The Stability Problem in Capitalism: Are Long-Term Positions the Problem? A Comment on Duménil and Lévy

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In the literature devoted to dynamics in the classical tradition, the works of Duménil and Lévy (DL hereafter) display a number of original features. The following seem to be of special interest:

(i) their models are related to a general approach called "disequilibrium microeconomics", which in their view constitutes the true classical tradition;

(ii) disequilibrium positions are held to be effective (and not only notional) and to lead to involuntary stockpiling and undesirable rates of capacity utilization;

(iii) the stability of the level of activity (what they call "stability in dimension") is dealt with as well as that of the structure of the economy ("stability in proportion");

(iv) the distinction between short term and long term is derived from at single general model;

(v) the results of the theoretical model are compared with historical data.

The paper presented by DL at this workshop does indeed exhibit all of these features at the same time. Since it is not possible to discuss all of them with great detail, I have chosen to limit my remarks to points (i) and (ii).

To summarize briefly, the main conclusion of the paper is that capitalism is stable in proportion and unstable in dimension. In other words capitalism is fairly efficient in the allocation of capital among industries, while its main weakness seems to be a lack of regularity in the overall level of activity, which tends to fluctuate widely. At the heart of the matter is the tradeoff between the rate of capacity utilization and involuntary stockpiling. This tradeoff takes the form of a stability frontier along which the economy moves back and forth.

Assuming that the approach advocated by DL (modeling effective

disequilibrium positions and deriving dynamic from adjustment reactions) is the right one, the first question to be raised concerns *the robustness of the conclusions*.

To my knowledge, there are no general results concerning the global stability properties of classical dynamic models with effective transactions (in contrast with nontâtonnement models in general equilibrium theory). What DL offer is a very careful treatment of the local stability of what they call the normal equilibrium, which is not the unique fixpoint of the recursion. It is therefore reasonable to ask whether the model is to be taken as it is or whether it is necessary to make some modifications with regard to its general inspiration. In addition, I would suggest that the simple reproduction hypothesis is highly questionable and that some revisions in the model might be welcome. Would these change DL's main conclusions? One would hope not...

A second interesting question concerns the foundations of the disequilibrium microeconomics. The type of coordination between individual actions which accounts for the existence of effective disequilibrium positions has to be elucidated. DL give some hints, but there is probably a lot of work to be done in this field.

One final point before embarking on commentary proposed above concerns the structure of the model itself.

Figure 1 shows the main relations concerning stability in dimension; the thick arrows represent the reactions to disequilibrium (for the sake of clarity, the actions of the banking system have been omitted).
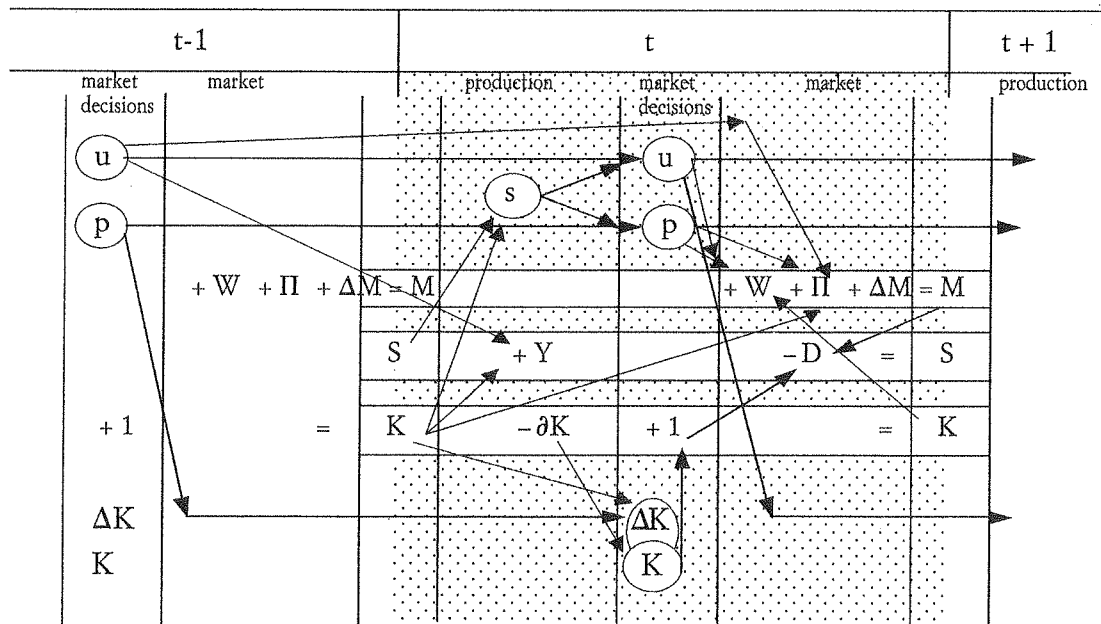

Figure 1

The market plays a very important role in the model. The agents' decisions cannot be carried out except in or through the market. This is true for the fixing of the levels of activity (u) and prices (p), and for the determination of investment as well. The outcome of these actions becomes known in the market only after the actions have been realized: the demand D determines the stock of capital available for the next production (K) and the stock of finished goods (S). The fact that the normal equilibrium of the model can be determined independently of the market thus appears as the consequence of a fixed coefficients assumption and of the fact that the targets s and u are known at the outset. This aspect of the model does not, however, reflect a "radical Neo-ricardian" point of view on gravitation according to which the market adjustment would drive the economy towards a position determined independently of the market.

The general structure of the model can also be expressed by the pattern of the relations between the agents, as shown in fig. 2.

Flows between agents
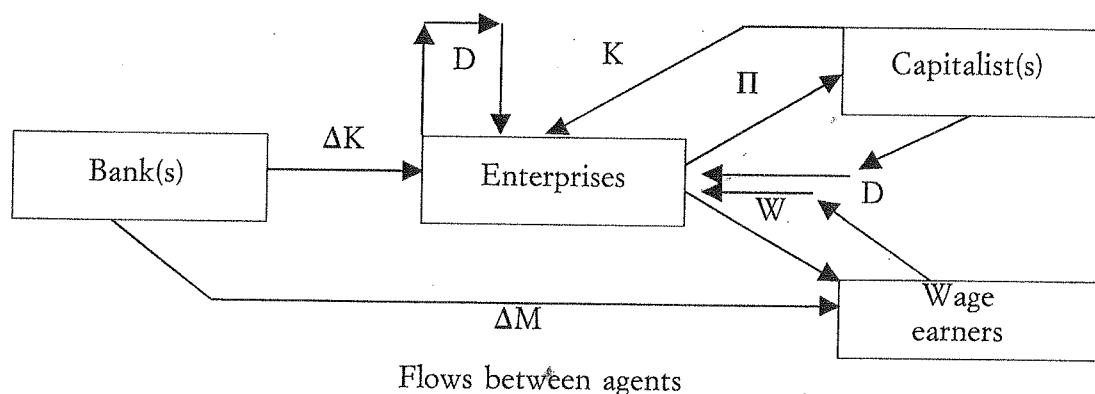
Figure 2

Let us now consider the first general issue raised above: is the model presented by DL so basic that their conclusions can be taken as well established and as constituting a general theory of capitalist instability?

One of the most striking features of the model is the way investment relations between capitalists, enterprises and banks are modeled. As figure 2 clearly demonstrates, capital is made available to enterprises through two different channels: one is the allocation of depreciation allowances (a consequence of the simple reproduction assumption) and the other banks' credit. The determinants of these two flows are different. On the global level, the first is determined by technique (∂) only, whereas the other depends on the capacity rate of utilisation u and the reaction of the bank to the price-level variations (not shown on figure 1). Profitability is not a direct argument in the reaction function of the enterprises. One could argue that there is a positive relation between profitability and u, but this is true only *ceteris paribus*.

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The reason for this special treatment (not common is the classical tradition) is likely to be found in the simple reproduction hypothesis. This assumption explains the special behaviour of capitalists, who care only about the allocation of ∂K between industries and not the overall profitability of investment (if they did, they would consume a variable fraction of their profits). Since the constancy of total capitalist investment is not compatible with the idea that volatility of investment is the key for understanding instability in the short run, DL have had to introduce a factor of variation in another way. So the banks fulfill the job. Reacting to a gap between the observed and desired capacity rates of utilization, enterprises raise funds for investment from the banks. The analysis of the conditions for local stability of the normal equilibrium confirms the strategic role played by ω, the reaction coefficient of enterprises that determines ΔK.

The question here is: what prevents the overall stock of fixed capital from growing over time, since ΔK is self liquidating as the loop D makes clear in figure 2. The credit ΔK is entirely used to finance investment expenditures. For each ΔK, there exists an equivalent revenue for the enterprises as a whole which allows them to pay back the banks (something like the Kalecki principle). This point is briefly touched upon in the paper by DL ["Strict simple reproduction is not guaranteed in a disequilibrium, because of the existence of loans for investment"].

It is true that such growth is not possible if one considers normal equilibrium (with $u = u$ and $s = s$). However, nonequilibrium paths, characterized by a growing stock of fixed capital never converging towards the normal equilibrium, are not ruled out. In a globally stable model, there would be no harm in allowing some variation in the investment, since the variation would be transient and the stock of capital would converge towards a constant. But we have no idea about the global stability of the model (maybe DL do...). For this reason, in fact, it might be better not to introduce a possibility of variation in the investment, because this might be inconsistent with the simple reproduction hypothesis.

Another way to react to the modeling of investment in the DL model is to suggest that either profitability should play a role in the variation of investment or investment should not be allowed to vary at all. If the instability of capitalism comes from investment (as DL maintain), simple reproduction models are not the best framework for studying it. Schumpeter, who associated credit and investment so intimately, argued that the "circuit economy" never experiences instability. The question of the articulation of the gravitation process (does competition work?) to the accumulation process (does the development of capitalism take place without crises?) is still pending. Nevertheless, DL should be crediter for a decisive step in the right direction.

The adoption of a simple reproduction framework is less indicative of a deliberate theoretical preference on the authors' part than of the intrinsic

difficulty of modeling expanded reproduction. Thus, the foregoing remarks should not be interpreted as a criticism of DL's paper. In fact, I am in general agreement with their many works, and I find this particular text very interesting and stimulating.

The modeling of expanded reproduction could provide an opportunity to return to the classical idea of capital accumulation out of profits (or credit) ruled by profitability. An objection raised by DL (during a private discussion) is that this would require the introduction of a positive rate of interest (to be compared with a rate of profit), with all the difficulties owing to infinite leverage and credit rationing. They know better than I do the intricacies of such a treatment of investment. I would simply remark that the limits of accumulation, when credit comes into the picture, are determined by the general conditions of the monetary and financial system (which are already taken into consideration in their model). This would be a good incentive to develop that part of the model (which constitutes one of its most valuable features).

Another remark relates to the treatment of profits. DL calculate profits as the difference between sales on the one hand and wages and depreciation allowances on the other, a fairly usual definition. But DL take sales as being defined by Kbup, which at best can be interpreted only as expected and not realized sales. Profits, in DL's model, are not realized profits: they do not reflect involuntary stockpiling. Since the latter plays a very important role in other equations, this observation may appear minor. However, it leads to an important discussion. If the model is to be corrected to take into account the direct influence of actual expenditure on profits, an arrow must be added between D and Π on figure 1. This would give rise to something like a Kalecki's principle phenomenon: in part, profit would depend directly on investment. It is fair to add that this line of reasoning is not in the classical tradition, but after all, nothing prevents one from aiming at a synthesis...

A final point concerns inventories. Figure 1 shows that decisions about the rate of capacity utilization take place *before* the market (determining the demand for inputs) whereas the level of capital available at the end of the period is known *after* the market. This inconsistency (how can one decide the rate of utilization of a capacity not yet known?) disappears if it assumed that desired investment I, decided at the same time as u, is always equal to realized investment. But why could not demand for investment exceed supply, if inventories are insufficient to fill the gap? More generally, nothing is said about s being equal to zero with an excess demand in the market. What would happen in such a case? Should we consider that unfilled orders are of the same nature as negative inventories?

Let me come now to the second issue mentioned above. This concerns the very foundations of the disequilibrium microeconomics advocated by DL. Some of the points made by DL are very important (e.g., the

effectiveness of disequilibrium positions), and I would like to suggest that they deserve more detailed explanations and justifications. The problem here is not so much that of the rationality of the behaviour assumed by DL as that of the special coordination which accounts for the effectiveness of disequilibrium positions.

DL have shown elsewhere that, under some not too restrictive assumptions, it is possible to derive the reaction equations of their model from a set of rational behaviour hypotheses. Therefore, the point I would like to emphasize is quite different and concerns the way people meet on the market and why it is consistent to conceive of effective disequilibrium positions. Such nonequilibrium positions are not incompatible with the fundamental hypothesis of voluntary exchange.

A we know, and as Lucas reminded us some years ago, traditional dynamic models exhibiting disequilibrium paths are based on a nonoptimal use of the information available on the market (adaptative expectations). Moreover, we have to explain why people confronting undesirable situations agree to enter into transactions. In the traditional framework, transactions out of equilibrium are excluded since they contradict the voluntary exchange hypothesis. When they are taken into account as in non-tâtonnement models, a contradiction arises between the rules of exchange (efficiency of markets), the pattern of the excess demand matrix (absence of feasibility of transaction) and the principle of voluntary exchange. Thus, only equilibrium positions can be conceived of. As a consequence, theory can deal with actual, observed situations only by modeling them as equilibria. How then can one account for effective disequilibrium positions?

The key to this point lies in money conceived as a means of coordination between individual actions (and not as an additional good). In this view, money is an alternative concept to that of equilibrium. Money is in some sense an institution, a set of rules determining the way individuals are allowed to carry out their economic decisions.[1]

Briefly stated, when a monetary and financial system does exist, agents can act and transact without the authorization of the auctioneer. The counterpart of this freedom is that they do not know whether or not the economy is in a state of equilibrium (in which individual actions are mutually compatible). Transactions made by an agent are split between didfferent markets, each working according to different rules. It is not generally possible to subject the execution of one order to that of another. Since individual (optimal) plans are not mutually compatible and agents have the means of payment to carry them out, the outcome of the market is the existence of unsatisfied supplies and demands closely related to monetary excesses or deficits. Involuntary stockpiling is not the direct consequence of an

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[1] CARTELIER, JEAN, "Money and Equilirbium: Two Alternative Modes of Coordination of Economic Activities". *Jerome Levy Economics Institute*, Working Paper no. 27, (1989).

incorrect behaviour. Rather, it expresses the very conditions under which plans are carried out. It must be recalled that, even in equilibrium, transactions are not generally feasible without money. Everything being equal, involuntary stockpiling is to be associated with a monetary deficit on the part of the enterprise. These deficits (which, taken as a whole, are identical to excesses) must be paid in order to avoid bankruptcy. Such a financial closure of the system occurs through direct or indirect finance from excess agents to deficit agents; whether or not this is workable depends heavily on the financial state of the economy, on the rules of its working and on the policy of the monetary authorities. These conditions may be of some importance for decisions concerning the level of activity for the next period.

The spirit of the model presented by DL is perfectly consistent with my second point, as is the paper presented by Ingrid Kubin.[2] The fact that DL put banks and credit into the picture is not coincidental. That money figures in "the 'smallest' framework in which the stability problem can be discussed globally" is to be taken seriously.

In this view, one may feel a bit disappointed by this modeling of the reactions of the banking system, which is more in accordance with orthodox central banking than with an analysis of the role of money in disequilibrium transactions. My suggestion here is that future versions of the model should develop the monetary underpinnings of the approach, even if, in so doing, the Keynesian flavour may supersede the Classical one.

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[2] KUBIN, INGRID, "Market Prices and Natural Prices: a Model with a Value Effective Demand", mimeo, (1990).