three stages: In the 17th and 18th centuries, thanks to the colonization of America and its effects on the acceleration of the mercantilist proto-capitalism of Atlantic Europe, that part of the world acquired for the first time a decisive superiority over the old civilizations of the Orient which it prepared to attack, thus provoking a halt in their own proto-capitalist evolution (and even sometimes bringing about regressive involutions). In the 19th century the industrial revolution and then imperialism (in the classical Leninist sense of the term) accentuated this polarization of wealth and power, which became expressed in the contrast between industrialized and non-industrialized countries. I propose the thesis that the structural crisis of our epoch (starting with 1970) inaugurates a new stage in world domination (marked by new technologies, new forms of world-wide finance capital, etc.) which results not in a reduction but an aggravation of polarization. The peripheral industrialization of one part and the "Fourth-Worldization" of another part constitute the new forms corresponding to this last stage of polarization.

Does Blaut's reasoning imply that the transfer of value from the peripheries toward the centers constitutes the essential reason for the differential in remuneration of labor? It may, but only implicitly. If this is so then I cannot share this simplistic point of view. This is because the calculations which I have proposed show that the (high) level of wages in the center are principally explained by the (high) productivity of labor. However, the transfer of value from the peripheries toward the centers comprises a major obstacle to the acceleration of accumulation in the periphery, and particularly subordinates the latter to the requirements of its deployment in the center. Moreover, these transfers, like the other dimensions of polarization (above all, access to the natural resources of the whole globe to the exclusive profit of the centers), have largely contributed to making possible a continuous and ultimately prodigious amelioration of labor productivity in the center. I have expressed these conclusions in the following phrase: the development of the center explains the underdevelopment of the periphery, but the reverse is not true.

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CRISES LASTING FOR DECADES

Sometimes a theoretical debate will degenerate into a tower of babel because the participants, without being aware of the fact, are answering not the same question but different questions. It is therefore essential to be clear about what is being asked. In the case of my essay in *The Imperiled Economy* (Foster, 1987), which Howard Sherman criticizes in a recent article in *Science & Society* (Sherman, 1989), the question was given in the title: "What is Stagnation?" Moreover, stagnation is distinguished from the business cycle in a sentence that refers to the former as a "trend-line" of slow growth "around which the recurrent fluctuations of the business cycle occur" (Foster, 1987, 59). Similarly, in the other article that Sherman criticizes along with my own — "Power, Accumulation, and Crisis" by Gordon, Weisskopf and Bowles — the authors also make it clear that what they are trying to address at that point is "the stagnation of the United States economy over the last two decades . . ." (GWB, 1987, 53).

Sherman, however, treats these specific contributions as if they were concerned primarily with the very different (but of course related) subject of the business cycle, throwing a degree of confusion into the discussion as a result. Neither a demand-side nor a supply-side approach, Sherman tells us, constitutes a fully developed "profit-squeeze theory" of the cycle, since to be squeezed something has to be "squeezed from two sides." Both "supply-side Marxists" (a label that Sherman applies to Gordon, Weisskopf and Bowles), and "demand-side Marxists" (a label under which he includes Baran, Sweezy and myself) are said to reduce "the Marxist notion that profits are crushed between two inexorable forces in each expansion" to just "one essential aspect or relation." Neither of these theories, Sherman concludes, deserves the label "profit squeeze." Indeed, "Foster," he points out, "did not use the words profit squeeze" (Sherman, 1989, 65). This label Sherman then claims as his own after proposing a "two-horned" "general profit squeeze theory" or "nutcracker" model.

The best way to understand what is happening here, I think, is to view the problem in terms of Sherman's own previous work, particularly in relation to certain long-standing differences that he has with the theory associated with Baran and Sweezy's *Monopoly Capital* (1966). Sherman has always been first and foremost a theorist of the business cycle, and in this context, as well as in his studies of monopoly profits, has made major contributions. Nevertheless, the limits of his analysis can be seen in the fact that he has persistently and categorically denied any validity to theories of secular crisis. "[A]ll theories of long-run crises," he wrote at one point, "are incorrect . . ." (Sherman, 1984, 91). Furthermore, Sherman has frequently used this stance to distinguish himself from the tradition represented by Baran and Sweezy. This can be seen most clearly in the following quote from Sherman's review of *Monopoly Capital* for the *American Economic Review:*

> [W]here Marx argued that competitive capitalism suffers cycles of boom and bust (but said "there are no permanent crises . . ."), Baran and Sweezy see monopoly producing a permanent tendency toward slowed growth and stagnation. (Sherman, 1966, 920.)

The distinction drawn between Baran/Sweezy and Marx here is misleading, since the postulate of a more or less "permanent tendency toward slowed growth" (subject to various countervailing influences) under monopoly capitalism cannot be reasonably juxtaposed to the cryptic remarks that Marx directed at Adam Smith in a five-line footnote in *Theories of Surplus Value,* to which Sherman is alluding here (Marx, 1968, 497n). Certainly, Baran and Sweezy could never be accused of arguing that there is a permanent falling rate of profit, which is what Marx seems to be criticizing Smith for in that passage. Moreover, Marx himself was soon to develop a falling rate of profit theory of his own pointing to secular crisis tendencies. Finally, it is significant that the possibility of stagnation under late capitalism is already visible in Engels' 1884 preface to the first German edition of Marx's *Poverty of Philosophy.* Thus at the end of a discussion of sharp fluctuations in world trade, Engels adds the following footnote:

> At least, this was the case until recently. Since England's monopoly of the world market is being more and more shattered by the participation of France, Germany, and, above all, of America in world trade, a new form of equalization [between production and the market] appears to be operating. The period of general prosperity preceding the crisis still fails to appear. If it should fail altogether, then chronic stagnation would necessarily become the normal condition of modern industry, with only insignificant fluctuations. (Engels, 1963, 20n.)

The largely scholastic issue of whether or not Marx and Engels actually recognized the possibility of long-run crisis, is, however, a minor matter. More important is the fact that Sherman himself quite clearly has an aversion to any serious consideration of the problem of economic stagnation, or any other conceivable form of secular crisis. But in rejecting this he is closing his eyes to the significance that the larger historical environment has for the concrete functioning of the economy. This aspect of the modern stagnation dilemma is best grasped in terms of the following statement by Kalecki: "Our analysis shows . . . that long-run development is not inherent in the capitalist economy. Thus specific 'development factors' are required to sustain a long-run upward movement" (Kalecki, 1965, 161). In contrast, Sherman complains about theories that attribute "each recovery to an external shock (such as innovations or wars). It is far more impressive [he says] to present a theory that explains how the capitalist system leads to depressions, but also how depressions eventually tend to lead to recoveries" (Sherman, 1989, 67).

But is it really possible to argue so confidently that severe economic downturns always "eventually tend to lead" to full recoveries — as if the trend, as well as the cycle, were governed by some automatic mechanism? In the popular game "Trivial Pursuit" (if memory serves) the answer given to the question "when did the Great Depression end?" is "1933." This is when the recovery began "of itself," as Schumpeter said. Nevertheless, in 1937 the unemployment rate was still 14%, and in 1938 it was 19%. Eventually full recovery did come of course. But it came not as a result of the automatic functioning of either the regular business cycle or a mythical long cycle, but as a consequence of the Second World War! Sherman's argument would appear to agree with the establishment viewpoint that the problem would have "eventually" been solved through the automatic workings of the economy without any help from the state or the military. In contrast, stagnation theorists would argue that while this is possible the fact remains that the Second World War *was* capitalism's solution to the crisis, and this in itself should suggest that there is much more at issue here than simply the business cycle.

It is of course difficult to make sense of this in terms of conventional economic training since one is conditioned to think of the economy as "self-equilibrating" over the long run, as well as over the short run. Indeed, adherence to such views is often seen as a litmus test of respectability within the profession. Still, Marxist political economists should take seriously Kalecki's question about the secular trend: "Why cannot a capitalist system, once it has deviated downward from the path of expanded reproduction, find itself in a position of long-term simple reproduction?" (Kalecki, 1984, 164). Kalecki's own answer was that there is nothing in the nature of the capitalist economy that prevents this from happening. This being the case it becomes necessary to move away from mechanical economic models and examine economic history, where there is some hope of finding an answer. One need only add that the question would scarcely be asked, either in the 1930s or over the last two decades, if stagnation were not actually present and yet unaccounted for.

Once it is recognized that Sherman has always had a strong aversion to theories of secular crisis, one is in a position to understand more fully his own approach. In Sherman's numerous articles on economic crisis Marx's theory of the falling rate of profit based on rising organic composition is subsumed under the category of a cyclical theory which emphasizes "rising material costs." In like fashion, stagnation theory of the type advanced by Baran, Sweezy and Magdoff, which has investment as its central concern, gets subsumed under the category of "underconsumption theory" — which is said, virtually by definition, to ignore the problem of investment. Profit squeeze theories such as those of Gordon, Weisskopf and Bowles are generally treated more straightforwardly, since this is mainly a business cycle theory. Yet, even here Sherman characteristically tends to neglect their treatment of the long cycle — which is how they try to stretch their theory into a secular analysis.

In all of this Sherman is, I believe, trying to be fair. For although he does not give much credence to problems of secular crisis he wants to show that each of these approaches has some limited validity when placed in the context of the business cycle. Moreover, since the business cycle is by definition symmetrical, while the long-run trend of development follows a somewhat more indeterminate pattern, Sherman, with his eyes on the former, is inclined to think of theoretical contributions that were designed primarily in relation to the latter as one-sided. Indeed, he valiantly tries to overcome this one-sidedness by means of thought alone by proposing "two-horned dilemmas" in which all sides of the problem are given equal importance. Yet, to me this seems to be a clear case of what C. Wright Mills (1959) called "the democratic theory of knowledge" — a type of "liberal practicality" that always stresses balance, equilibrium and countervailing influences, while excluding, by general theoretical fiat, any possibility of long-term uneven development. In any case, for theorists focusing on secular crises of slow growth, Sherman's modifications in the name of the business cycle and balanced thinking remove from the problem all of its most critical aspects.

This is particularly the case where the tradition identified with Baran and Sweezy's *Monopoly Capital* is concerned. For thinkers of this kind the main theoretical concern is the historical determinants of the accumulation (or savings-and-investment) process. Although this has its short-run, cyclical aspect, meaningful historical discussions regarding investment — which involve the expansion of capacity, the projected use of that capacity, expectations, innovation, the relative development (and stages of development) of departments I and II in the Marxian reproduction schemes, etc. — are not easily dealt with in terms of a stylized, mechanical model of the business cycle. Accumulation is therefore treated predominantly in a long-run context. This is not to say that cyclical influences are unimportant, but simply that the nature of accumulation necessarily drives one toward a longer view, geared to the trend rather than the cycle.

Hence, it is not surprising that when Sherman takes a theory of this sort and tries to subsume it within an analysis that takes only the cycle seriously, *carefully removing any long-run influences,* he arrives at the conclusion that it one-sidedly neglects investment. Thus he writes that "J. B. Foster, following the views expressed by Sweezy and Baran in *Monopoly Capital,* sees the problem as a torrent of supply facing a very limited consumer demand" (Sherman, 1989, 64). This seems to reduce *Monopoly Capital,* in addition to my own derivative work, to a crude version of underconsumptionism. After all, even the most simple model has to encompass both consumption *and investment.* Sherman's interpretation therefore has to be held up against the actual fact that the bulk of what theorists like Baran, Sweezy and Magdoff have written has focused squarely on investment. If one looks at *Monopoly Capital* itself, *virtually nothing at all* (if one discounts the discussion of the sales effort) is really said about the consumption of the working class, since the focus of the entire book is on the absorption of the surplus, and workers, as is well known, have no access to surplus! While the key discussion of capitalist consumption occupies three pages, the bulk of the rest of their 367-page analysis is concerned with historical conditions governing investment, government spending, and international capitalist expansion.

It is therefore difficult to understand how Baran and Sweezy's analysis can be treated by one critic after another (for Sherman is not the first to do so) as if it one-sidedly stops at consumption. Indeed, by saying that the problem posed by the *Monopoly Capital* theory "is a torrent of supply facing a very limited consumer demand" Sherman himself equates the *starting point* of this theory — which says the surplus has to be absorbed by capital since the mass of consumers do not have access to surplus — with its main focus, completely ignoring the fact that the chief concern of the analysis is what governs investment itself.

The main evidence that Sherman points to in attempting to prove the existence of such a simplistic underconsumptionism, however, is drawn from my own essay. Sherman quotes from the following passage, which I will now requote in full, placing in italics those parts that he had replaced with ellipses:

[A]ny continual plowing back of profits into new investment would mean that the means of production *(Department 1 in the Marxian reproduction schemes, the* *demand for which comes out of gross profits)*, would expand very much faster than articles of consumption *(or Department 2, the demand for which comes mainly from wages)*. This is, in fact, the basic pattern of every accumulation boom. But it is a self-annihilating process. Sooner or later *(depending on historical conditions determining the degree to which the investment process is self-sustaining)* the means of production are built up to such a prodigious extent that a social disproportionality develops between the capacity to produce and the corresponding demand. A crisis of overaccumulation rooted in overexploitation then occurs. (Foster, 1987, 61.)

The following is the way in which Sherman summarizes my argument in the above quote:

In other words, workers' pay does not expand as rapidly as output because workers are too weak, capitalists are too strong, and the rate of exploitation is rising. Relatively less pay for workers means less consumer demand than available goods. (Sherman, 1989, 63.)

Now this may be what Sherman wants me to say, but it is not what was said, so the "in other words" is a *non sequitur*. What this does is give the false impression that the argument is concerned simply with consumption and neglects investment. Indeed, further down on the same page he states that "domestic demand is often seen [by "demand-side theorists"] as only consumer demand, with the assumption that investment is merely derivative of consumption." Yet, in the foregoing passage from my essay (which Sherman picked out precisely with the intention of backing up his own argument) I refer to aspects of accumulation (investment, accumulation, overaccumulation, Marx's department I) a number of times. In addition, in pointing to demand and not consumption as a constraint on the expansion of productive capacity I was explicitly formulating the matter in a way that encompassed investment. Nor is a simple functional relationship between consumption and investment postulated, since I refer to historical factors that determine the degree to which investment is self-sustaining in any given period. To be sure, Sherman is correct in suggesting that my argument points to a rising rate of exploitation as a key part of the dialectic of crisis. But to reduce the whole question to one of "consumer demand" not only distorts the analysis that I presented, but removes the very thing that constituted its central focus: capital formation.

What makes this doubly disturbing is that Sherman himself has virtually nothing meaningful to say in his article about the accumulation process as such. Investment is treated simply as a component in aggregate income or demand. Hence, accumulation does not seem to be an

object of analysis in and of itself in his model, despite the fact that this is the crux of the contemporary stagnation crisis. This seems to reflect a belief that once it is shown that there are two jaws (supply and demand) to the nutcracker that squeezes (cracks?) profits in each business cycle peak, one has solved the whole problem of economic crisis, including accumulation.

I was also perplexed when I read Sherman's contention that my analysis one-sidedly "neglects the role of the costs of supply," and wage costs in particular (Sherman, 1989, 64). The rate of exploitation, which Sherman himself says is at the core of my analysis, is Marx's main supply-side concept. Logically, to say that the rate of exploitation is rising no more "neglects" unit labor costs than to say that it is falling (the main contention of Gordon, Weisskopf and Bowles). To suggest as Sherman does that the whole truth is that the rate of exploitation both rises and falls over the course of the cycle (and further that cost and demand constraints assert themselves to different degrees at various points within an expansion), simply ignores the question of secular crisis. For a crisis of slow growth lasting for decades can only exist if either supply-side or demand-side constraints assert themselves disproportionately over the long haul. Otherwise there would simply be a moving equilibrium as postulated in neoclassical economics. Here Spinoza's famous phrase: "Omnis determinatio est negatio" (every determination is a negation) comes to mind.

What are the political implications that Sherman draws? In closing he says that his "nutcracker" model is superior to other Marxian crisis theories since it demonstrates that "only the end of capitalism will end the business cycle." In Sherman's view this is a valuable conclusion since Marxist supply-side theorists "imply that lower wages could prevent crises," while Marxist demand-side theorists "erroneously imply that higher wages could prevent crises" (Sherman, 1989, 70). Here I rubbed my eyes in disbelief! It is not my place to speak for Gordon, Weisskopf and Bowles, but I don't think they imply that lowering wages could *end the business cycle* — what Sherman is saying here.

However, I can speak for the tradition to which I belong, and can therefore state definitively that a policy of promoting higher wages would not end the business cycle under capitalism, which is an inherent part of the accumulation process. Nor are higher wages as such the best way to deal with present problems — though I would never attempt to discourage workers from fighting for them. There are of course other routes to redistributing income and wealth like overhauling a regressive tax structure, cutting military spending, and increasing state spending on education, health, and housing, as well as on combating poverty, cleaning up the environment and repairing infrastructure. These may not sound like revolutionary policies, but to be consistent they must be constructed within an anti-capitalist logic.

Having reached such a point in the discussion it is no doubt worthwhile to take a look at the world around us. We have been witnessing not only relative stagnation for two decades now, but also for most of that period a very deliberate strategy of supply-side restructuring designed to redress what the capitalist class itself sees as a problem of structural crisis. This strategy has included: breaking unions, promoting unemployment, driving down wages, cutting back on state spending that benefits the poor, altering the tax system to redistribute income and wealth from the poor to the rich, and forcibly eliminating obstacles to the free flow of capital in the underdeveloped regions of the globe. It is this overwhelming reality of our time that has most influenced my own thinking as a political economist, which has been governed by an attempt to resist these concrete practices by combating the ideological mantle of supply-side economics in which they are clothed.

And in the face of this determined global onslaught by capital the most powerful conclusion that Sherman himself is able to give us, in what purports to be a more developed theory of economic crisis, is that the business cycle is inherent to capitalism! I think this may have to do with the fact that he is asking the wrong question.

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SHERMAN ON CRISES: A COMMENT

Howard Sherman's recent communication in *Science & Society* (1989) presents a useful starting point for understanding the current state of Marxian crisis theory, as represented by his own work, and by the works he criticizes — by John Bellamy Foster, and by David Gordon, Tom Weisskopf, and Sam Bowles (GWB). The present comment focusses on what I see as the major contributions of Sherman's approach, and also on some weaknesses in his argument.

Agreements and Quibbles

First, I believe Sherman is correct in arguing that a complete theory of crisis must involve both "supply side" and "demand side" factors (Devine, 1983; 1987a; 1987b). But importing mainstream terminology into the theory (as popularized by GWB) can be confusing. While it is true that market-oriented jargon is less scary for outsiders and has obvious connections with mainstream macroeconomic debates, it hin-