Labour/Le Travailleur

"The Misery of Our Poor"
Recent Studies in Development Literature

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Volume 11, 1983

URI : https://id.erudit.org/iderudit/llt11re03

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Éditeur(s)

Canadian Committee on Labour History

ISSN

0700-3862 (imprimé)
1911-4842 (numérique)

Découvrir la revue

Citer ce compte rendu

Ommer, R. E. (1983). Compte rendu de ["The Misery of Our Poor": Recent Studies in Development Literature]. *Labour/Le Travailleur*, *11*, 196–203.

"The Misery of Our Poor":

Recent Studies in Development Literature

Rosemary E. Ommer

Jean Gottmann, ed. *Centre and Periphery: Spatial Variation in Politics* (Beverley Hills: Sage Publications 1980).

Dudley Seers, Bernard Schaffer, and Marja-Liisa Kiljunen, *Under-Developed Europe: Studies in Core-Periphery Relations* (Sussex: Harvester Press 1979).

Jose J. Villamil, ed. *Transnational Capitalism and National Development: New Perspectives on Dependence* (Sussex: Harvester Press 1979).

Walter L. Goldfrank, ed. *The World-System of Capitalism: Past and Present* (Volume 2 of the Political Economy of the World-System Annuals), (Beverley Hills: Sage Publications 1979).

Terence K. Hopkins and Immanuel Wallerstein, eds. *Processes of the World-System* (Volume 3 of the Political Economy of the World-System Annuals), (Beverley Hills: Sage Publications 1980).

Folker Fröbel, Jürgen Heinrichs, and Otto Kreye, *The New International Division of Labour* (Cambridge: Cambridge University Press 1980).

CORES AND PERIPHERIES, metropoles and hinterlands, development and underdevelopment, and dependency are related themes which have become increasingly prominent in the socio-economic literature of the last decade or so. These concepts arise out of a variety of disciplines, ideologies, and schools and amount at present to an inchoate collection of disparate ideas which cries out for overview, some way of welding a potentially valuable series of approaches into a cohesive and forceful whole. Certainly the social sciences urgently require a way of directing thought so that the world as it exists today becomes more comprehensible. A framework is needed within which multi-disciplinary studies of the current social and economic order can work with, rather than against, one another.

An enormous body of literature has been devoted to core-periphery, to the

Rosemary Ommer, " 'The Misery of Our Poor': Recent Studies in Development Literature," *Labour /Le Travailleur*, 11 (Spring 1983), 196-203.

point that even statesmen have adopted this terminology (Henrikson in Gottmann). While the idea offers the great attraction of a clear-cut dichotomy, this can all too easily be mistaken for explanation. In its simplest form, core-periphery is a structural relationship between the centre of a system and the part which surrounds it, a relationship which has come to be equated with dominance-and-dependence. Sociologists, however, remind us that the roots of the concept may well lie deep in the archetypes of western civilization (Strassoldo in Gottmann) with, for example, the mandala. This is worth taking time to consider, for the circle with its centre point and boundary expresses perfectly the conundrum which is still reflected in modern development thinking: it is not possible to have a centre without its periphery since that periphery is, by definition, a necessary condition for the existence of the centre. From this flows the kind of study which attempts to define the centre of, say, Europe; such a study requires the identification of socio-economic variables which may be used to establish centrality — high income, high GNP, low outmigration, and so on — all relative to surrounding areas. There are numerous examples of this technique in Seers, *et al*. The periphery thus defines the centre; but the centre, by contrast, does not define the periphery in a positive and limiting sense so much as it defines what is "not-centre" in the negative exclusive sense; the point is highlighted by the use of language such as "have" and "have not" in dependency writing, and the positing of that uncomfortable category the "semi-periphery" in the world-system approach.

Here it seems to me is an insight not sufficiently considered in the literature. The centre is, by definition, the cynosure of the periphery, but the reverse is not necessarily true: if you are on the periphery, you know it; if you are at the centre, you do not think about it unless you are threatened. This is where the modern academic literature on core-periphery starts, as Henrikson (in Gottmann) implies: with the geographical determinists. Writing at the end of the Victorian era, again after World War I and then, with some revision of previous assessments, during the crisis of World War II, Mackinder (quoted in map form in Henrikson's paper) and Huntington ("Geographical Optima of Civilization" [1919], *Mainsprings of Civilization* [1945]), for example, were worried about superiority, shifts in it, and threats to it. They represent the centre as it became self-conscious. Today, core-periphery most often represents the "view from the periphery": the polarity of the concept has reversed, so that it becomes an approach to inferiority — how to identify it and, all too infrequently, how to change it.

It is relatively easy to ascribe centrality, but peripherality is more complex, not in its acknowledged existence ("have-not," "marginal," "dependent") so much as in its boundaries. The literature speaks of "North/South," "developed/developing/under-developed," and now "core/periphery/semi-periphery."

"Core" rather than "centre" is useful because it acknowledges fuzziness in the real world at the edge of the centre "point." This allows scholars to deal with central areas (Seers, *et al.*) and to consider gradients, which Wallerstein attempts to recognize in his category of "semi-periphery" (see also a very good paper by Milkman on South Africa as semi-periphery, in Goldfrank).

Likewise, outer boundaries are problematic, not usually being as clear-cut as the line that defines the mathematical circle. Instead, as core-periphery, dependency, and world-system approaches all argue, there is a need to consider multiple cores and overlapping peripheries, networks of connectedness, and interdependencies (Gottmann); Wallerstein, of course, would argue that these all exist within one capitalist world system (Hop-

kins and Wallerstein).[1] The idea is fascinating: the paradox of centre/not-centre definition, the apparent simplicity, the actual complexity, the sense that there is order, process, explanation lurking in the mandala — always just out of reach. The search has something of the aura of a medieval Quest, with social justice as the Grail.

And that, of course, is one of the problems with this series of publications taken as a whole. With some notable exceptions, there is too little hard data, too much star-gazing, too much speculation at one end of the scale and case studies too narrowly conceived and lacking adequate synthesis into a wider framework at the other. Let us start with Gottman's collection of essays which all deal with a search for centrality: islands, socialist countries, politically-divided nations, and under conditions of post-imperial reorganization. Overall, one is left with a vague sense of unease: is there a useful concept here or a catch-all pseudo-explanation? This is not helped by pervasive problems of woolly language or woolly thinking — what are we to make of "Equality among people does not mean full uniformity; it is an aim that can be achieved to a relative degree, relative to a purpose of human activity" (19) or "usually these (powerful groups) are geographically identified with the place where the agencies are located through which dominance is exercised" (that is, they live where they live)? This kind of obscure language and incipient tautology is unfortunate, for the essays are driving at the oppositional and exploitative nature of the core-periphery syndrome: and I agree with Alexander (135-48) that it is a syndrome, not a model and certainly not a theory. There is, moreover, a serious lack of analysis and synthesis throughout the collection. The essay by Rokkan on Western Europe (163-204) is the best attempt, but even it is problematic, long-range, essentially descriptive. This volume is disappointing. One expects more from the marriage of geography and political science.

The volume by Seers, *et al.* on *Underdeveloped Europe* is better, but still suffers from the same kinds of problems: too little in the way of hard hitting analysis, too much speculation. Seers' curiously dated and defensive introduction points to the undeniable usefulness of recognizing the existence of peripheries even within core countries; Villamil gives a good summary of core-periphery relations in the western hemisphere. But there is nothing new here, despite Paine's claim to theoretical analysis of a "new" pattern in her essay on the replacement of migrant labour in western Europe by investment in the periphery. A similar demonstration of this phenomenon was given by Barnet and Muller as long ago as 1974 in a chapter entitled "The Obsolescence of American Labour."[2] Vaitsos on "Transnational Corporations and the Periphery" is better — technical, interesting, and tersely written; Stanton on Ireland is good though very depressing; Holland on Portugal is also good and the essay by Muñoz, Roldan, and Serrano on Spain and foreign investment shows the seductiveness of transnational corporations without the "evil plot" syndrome that characterizes so much analysis of transnational corporations' penetration into peripheral economies. The essay by Evangelinides on Greece offers a good description of "peripheral capitalism" (185) and Wade's article on Southern Italy balances economics and politics effectively, while Crotty's essay on Ireland is excellent in conception but sorely lacking in hard data.

The general impression gleaned from the book is that core-periphery scholars have progressed as far as a classification system, a first step on the road to diag-

[1] But see the objection in this in V. Navarro, "The Limits of the World System Theory in *Defining Capitalist and Socialist Formations*," *Science and Society*, 46 (1982), 77-90.

[2] R.J. Barnet and R.E. Muller, *Global Reach* (New York 1974).

nosis. In 1975, Brookfield commented that all societies contain some kind of inequity and while elimination of this should be sought, self-interested opposition from persons or groups will always be present. He proposed the identification of fundamental conflicts in various societies as a first step towards a theory of development.[3] This volume by Seers *et al.*, although it reads sometimes like a list of parallel complaints, is well on the way to achieving that aim.

Overall, Villamil's *Transnational Capitalism and National Development* is a much better collection. Whereas the core-periphery volumes examined above tended to leave out the world, this volume of dependency literature brings it back. The first two essays are essential reading as an introduction to the dependency approach. The essay by Sunkel on "The Development of Development Thinking" is a particularly impressive, concise historical review. Valenzuela and Valenzuela on "Modernization and Dependence" have produced a powerful article treating as ideal types the two basic and conflicting approaches to Latin American development, "two very different perspectives seeking to explain the same reality." (33) The contrasts drawn here between the two "types" are particularly useful, the modernization approach being seen as nationally focussed, reductionist, and "ideology disguised as theory" (55), the dependency approach as global, complex, descriptive, and propositional. Sunkel and Fuenzalida's essay on "Transnationalization and its National Consequences" is more problematic, as any attempt at global synthesis is bound to be. It argues differing historical paths, differences of time and scale, and an emerging framework more like a 3D Venn diagram than a centre-periphery in construct. This works better in the past than in the present, but perhaps this is because the world is already in the middle of a transnational crisis and even possibly moving on to another stage. If so, what? renewed nationalism? or global collapse? or salvation by the Third World?

The rest of the volume contains several strong essays. Luckham's paper on militarism makes a lucid statement about power, influence, and the limits to these in a world of nuclear deterrents and negotiated settlements. Godfrey uncovers the role of civil service salaries as one of the mechanisms of class formation created by the presence of transnational corporations in Third World countries. Fortin draws a neat model of power shifting in cycles from the transnational corporation to the Third World nation once the company is in production, but reverting to the corporation when new technology is introduced or expansion needed: but he fails to build into his model the fact that core-periphery relationships change in the capitalist system. This prevents his getting at the wider relationships involved. Langdon examines the role of the state as the meshing mechanism between settler capitalism and indigenous peasant production, while Villamil offers a good overview of the failure of neo-classical economics and modernization theory in a case study of Puerto Rico. The last two essays in the book are in strong contrast to each other. Oteiza's offering on collective self-reliance (must he refer to it as CSR?) is utopian and hopeless. Villamil's recipe for self-reliant growth is thoughtful, long-term planning with an emphasis on the need to create irreversible change towards independence when the opportunity arises. All in all, this is a lucid, well-organized volume, drawing together writings on a variety of related aspects of development. It is refreshingly free of dogmatic assertions and linguistic obfuscation; it is a pity that it is not similarly free of typographical error.

The remaining two volumes of essays come from the same "world-system" stable. Both of these collections are marred by a serious sense of cliqueishness and

[3] H. Brookfield, *Interdependent Development* (London 1975), 123.

obsessive proselytization: one occasionally has the feeling of being in on a diatribe. This is deeply unfortunate considering the potential importance of the approach and the indisputable quality of some of the contributions. In the Hopkins-Wallerstein volume, of particular note are Bosquet's essay on core cycles (hegemony to competition); Cronin's fine, thoughtful piece on the economics of unrest and the history of trade unions in the United Kingdom; Bergesen on cycles of formal colonial rule which makes a case for inter-dependency having been in existence for a long time; Clay's finely-drawn description of a Brazilian periphery within a periphery; Ragin's clearly-argued analysis of the relationship between nationalism and class conflict which is in strong contrast to a preceding doctrinaire essay by Jonas and Dixon.

In the earlier Goldfrank volume, Montejano's superb study of class/race labour oppression in South Texas is outstanding and shows what can be done when understanding is in charge of data rather than the reverse; Basu's study of the peripheralization of China is weak in argument but fascinating material; by contrast, Lubeck on Islam and resistance in Nigeria is an example of how such case studies can be integrated into the wider analytical framework. This is an excellent article using, not just invoking, the world-system approach and demonstrating a fine-grained empirical understanding of his subject. Weiskel's article on Baule, another good empirical study, is less well attached to the world-system approach. Milkman's essay on South Africa tests the world-system category of "semi-periphery" which she sees as not properly developed in the literature. She broadens the world-system arsenal with a variety of analytical tools which identify the relationship between linkage formation, technology dependence, demand structures, race relations, wage costs, and the expansion of capitalism in South Africa.

The rest is disappointing, for a variety of reasons. First of all, the language used in many of the papers is distracting or unclear. What are we to do with "neo-peripheralization" (Hopkins and Wallerstein, 257), a shame in an otherwise excellent essay; "pidginnization" (Goldfrank, 171), an ugly term but at least explained and defined; "eth-class" (Hopkins and Wallerstein, 268); and a whole section of Hopkins and Wallerstein's volume entitled "Proletarianization and Bourgeoisification." I'm not sure the latter is pronounceable let alone comprehensible, and it is certainly not necessary. What is the purpose of this abuse of the language? It does not add credence to the work done, but it does add confusion, makes the material more intractable than need be, erects a barrier between writer and reader, and intensifies the sense one has of being on the outside listening to a group of the elect speaking in code. This last point is further reinforced if one looks at references. In far too many cases these are restricted to a small number of writers with whom, after two volumes, the reader has become depressingly familiar. The broader, richer essays are in stark contrast. Compare, for example, the extremely narrow range of works cited in Jonas and Dixon's article with the spread of references in Cronin or Bosquet, all in the Hopkins and Wallerstein volume in which Wallerstein's own essay has no references whatsoever. Likewise, foonotes like footnote 49 in Gordon's article (Hopkins and Wallerstein, 40) are not helpful, referring to in-house objections rather than clarifying the discussion.

These kinds of problems may well be the result of the nature of these collections as proceedings of "workshops" but, while this is a possible explanation, it is not an absolution. In publishing proceedings of this kind for a wider audience, the editors should be striving to make their material more accessible since their presumed purpose is to reach beyond the small group who make up their immediate and assured audience. Indeed, if they perceive this work to be valuable and this message as one which should be heard, then it is in

their own interests to make the collection approachable, to cut back severely on the jargon, to define and explain terminology, and to advise authors to read and think about what is being said outside the limited pool of authorities who seem all too often to make up the sum of received wisdom among world-system scholars. Let them go talk, with Montejano, to the *braceros*. It would give their work credibility as being "scholarly." Like Marx's work, the world-system idea is at heart a moral stance and should not be obfuscated or downgraded by obscure specialist jargon, narrow reference bases, or superficial "cleverness."

The general critiques at the end of the Hopkins and Wallerstein collection are useful as assessments of the progress to date of world-system "theory." Chase-Dunn recommends model-building to provide a description of the relationship between cycles and trends, to get at the underlying causality, and to derive theory from this so as to see what reproduces capitalism and what transforms it. Mukherjee is more hesitant. He feels, as I do, that world-system scholars are really still involved in clarifying concepts. Methodological development belongs to the future. Hopkins notes that work will have to be done to enable world-system analysts to spell out operative processes "*in relation* to their oppositions and the indigenous forces . . . actively opposing." (318) To these comments I would add some others. The world-system approach, by its very nature, is complex. It deals with the world capitalist economy from inception to the present. Moreover, in line with good theory, it should be capable of prediction at some level and thus must also look to the future. That means identifying and manipulating massive amounts of historical and contemporary data, putting it through an analytical process drawn from a wide variety of disciplines and as yet not properly articulated, and finally reaching some conclusions. This is a vast, ambitious project, but that should not deter its followers, nor does it. However, it also means that world-system analysts must proceed carefully, rigorously, and thoughtfully, for the task is monumental and should not be brought into disrepute by sloppy scholarship. From these two volumes it becomes apparent that satisfactory formulation of the approach is still very far off, but at the same time there is an impression of people going too fast, offering superficial and facile solutions which are not carefully tested, producing a *deus ex machina* which resolves all issues. For example, what is the relationship of Kondratieff waves to world-system studies? Where do they fit into the emerging framework? How does one empirically test this? Gordon's essay on this topic (Hopkins and Wallerstein, 9-45) has the outward appearance of theory building, but is still too general to be really helpful. It is testable, but not tested (the amount of work would be enormous), although it desperately needs some data to tie it down. Yet real progress in knowledge does always come down to this: the ideas are propagated, discussed, refined, and then they have to be tested against the real world. There is no substitute for hard work here, nor for the time that that work will take.

There are also many unresolved issues lurking in the background. Let me take one example, that of "nationalism": is it relevant or irrelevant?[4] The paper of Jonas and Dixon insists on its being not only irrelevant but a capitalist distraction which ought not to mislead the international proletariat. Ragin's paper, by contrast, tests the manifestations of nationalism in Scotland, an area which is becoming increasingly more peripheral to a state economy. The result is convincing and useful. Nationalism is seen as a middle-class phenomenon, powerful only when class conflict is minimal and the middle class have no need to vote with the state élite. Likewise, Navarro, in his critique of world-system "theory" takes

[4] Whether this is doctrine, or feeling, or the nation-state itself, is often unclear.

Wallerstein to task for his emphasis on circulation and his lack of attention to national class struggle, which is based on production rather than exchange. The criticism is central, for it insists that Wallerstein's view of exchange as dominant weakens the "theory's" capacity to explain the shift from capitalism to socialism. If production is seen as dominant, however, then national class struggle becomes a vehicle for change. Navarro claims that

> the key issues in defining socialism or the transition to communism are (1) how does working class control over the state take place; and (2) how does that control shift the balance among the different modes of production to enable the eventual dominance of the communist mode of production.[5]

Wallerstein's explication of the world-system does not have room for either of these questions since it has no room for national class struggle. Navarro wants to know how, then, workers can seize a non-existent world state. He argues that there have to be national transitions to socialism based on relations of production.[6] One is reminded of Hobson's comment that "As individualism is essential to any sane form of national socialism, so nationalism is essential to internationalism: no organic conception of *world-politics can be framed on any other* supposition."[7]

In summary, then, the world-system approach as seen through these two collections still carries the hallmark of immaturity. Beleaguered by ideological in-fighting, deterministic tendencies, and jargon, it has the vulnerability of the sweeping overview. There is a great need for good, detailed analysis, for constructs which grow out of the careful consideration of hard data, for compelling evidence, rigorously presented, from the core, the periphery, and the semi-periphery. Linkages should be substantiated empirically between all three subsets of the hypothesized world system in order to demonstrate its existence, evolution, and *modus operandi*.

It was my hope that the last book, *The New International Division of Labour*, would do just this. Sadly, this has proven not to be the case. The authors have produced a major study of the transnational workings of the Federal German private sector and its employment implications for both the Federal German core and the peripheral world in which its companies are involved.[8] They have amassed some impressive data, but have *all* too often lessened the impact of their work with circular arguments and tendentious language. This is unfortunate, for studies of this kind, set in a world-wide framework, are desperately needed in order to document the existence of a capitalist world system. But they must be rigorous if they are to be convincing. The authors blithely assume that "The progress of the tendency towards a new international division of labour can be *read off* [my emphasis] from changes in the distribution of industrial labour." (183) No: it may be hypothesized, but cannot be "read off." We have to prove that statistics moving in this direction are caused by a new international division of labour and not by anything else. Possible alternatives must be considered. As it stands the sentence quoted above says that "the structure looks like this and therefore is this," instead of "if x is true, then a, b, and c will also occur," followed by a demonstration that this is the case, which will then permit some argument for causality. Likewise, the exclusion of EEC countries from the study of Federal German industry and the new international division of labour is dangerous, leaving the authors open to the accusation that they are accepting for analysis only that which

[5] Navarro, "Limits," 86.

[6] *Ibid.*, 90.

[7] J.A. Hobson, *Imperialism* (Ann Arbor 1978), 362-3.

[8] The English version of this book is somewhat abridged and so my comments should be read with this reservation in mind.

suits their thesis, for if there are major EEC shifts in labour then there is another movement of labour going on which may well affect the pattern under investigation. The existence of a labour shift from First World country to First World country might require modification of the authors' thesis, or might reinforce it. Either way, they (and we) need to know whether or not this is the case.

Another example of the kind of problem to be found in this study occurs in the section contained between pages 51 and 89, where the argument runs as follows: "There is a new international division of labour operating through the utilization of Third World dependent development structures. Could this actually be the start of complex industrial development in the Third World? No, because there is a tendency to dependent uneven development in the Third World which prohibits complex development." (That is: "This is happening; could something else be happening? No, because this is happening.") Again, the use of value-loaded terminology serves to discredit the study. Historians in particular will wince at the following comment on Hong Kong industrialization:

> As history shows, such a system has a particularly blatant tendency to regard the worker merely as an appendage of the machine, as a mere factor of production whose existence is only acknowledged as a necessary requirement for the making of profits. (90)

In short, while any detailed empirical study of this kind is a step in the right direction, this one is fraught with problems. There is a compelling logic to the argument as it is laid out in Chapter 1 and in the Conclusions, but there is nothing compelling about the way in which it is empirically demonstrated. Yet convincing demonstration is mandatory: transnational corporations and the capitalist world will defend their system thoroughly and competently. If they are to be challenged, that challenge must be equally well-founded and argued.

What, then, are we to make of these schools of thought as they are reflected in the volumes considered in this essay? First of all, their inter-relatedness would suggest that they are really different aspects of the same basic paradigm of a dominance-and-dependence perspective on the world's economic development. They have much of value to offer each other. The micro-studies of much core-periphery writing could usefully be incorporated into the search for methodology in world-system analysis; the experience of the dependency experts likewise has much to offer here. The world-system approach purports to be an alternative to the development approach, seeking to identify inter-relatedness rather than individual development paths.[9] It seems to me that inter-relatedness between these two approaches, rather than divergence, would be a more fruitful development path for world-system to take. If one studies the world, one needs all the help one can get.

The *interdisciplinary nature of this* kind of work can make it rich and complex, properly handled, but confusing and diffuse if an overall framework is not developed. The value of this work lies in *its perspective; the failure in the dearth of* careful empirical analysis, particularly in world-system studies, coupled with an excess of ideological posturing and the rigidity that follows. It is time to slow down, *re-think*, and work towards a clearer articulation of methodology; and it is worth the struggle. Charles Darwin commented in his *Voyage of the Beagle* that "If the misery of our poor be caused, not by the laws of nature, but by our institutions, great is our sin." That is the thrust of this literature. The sin will be all the greater if scholars fail to make their case through an unwillingness to do the requisite hard work.

[9] See Peter J. Taylor, "Geographical Scales Within the World-Economy Approach," *Review*, 5 (1981), 3-11.