

**WORLD-SYSTEMS ANALYSIS**  
***Methodological Issues***

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The topic is methodological issues. It is a brave one to attempt to address. I shall try to do so in two steps. After some preliminary observations, I shall try to speak to what I think are three issues under the general heading of concept formation and measurement. Then I shall speak on two or three under the heading of explanation and interpretation. I take it that the issues concern the study of long-term large-scale change—in that sense, historical transformations—and that the job of someone commenting methodologically on such work is to raise questions not about *what* we think in the course of this work but about *how* we think as we proceed. I shall try to do that. What I have to say will thus inevitably be distant from the kind of discourse we have been having. My apologies in advance.

I would like to pose an opening query which I cannot answer, but which I think any adequate methodological discussion would have to address: Why are these matters up for discussion? Why do we have the kinds of issues we have? What is our intellectual world—in that sense, our consciousness—which leads us to be concerned, now, with these kinds of discussions? I cannot answer these questions, but I believe an adequate epistemology would at least address them.

That kind of question it seems to me raises additional ones about our understanding of inquiry. Ours is academic inquiry; this is an academic conference. There are therefore severe limitations on the kinds of queries that can be addressed and on the criteria of adequacy that can be appropriately used. I think we have a very deep need to put in front of us

how we are, in fact, thinking. Because we come out of particular settings, we have learned particular ways to think, and it is not at all obvious that they are the most useful ones for our work. I think we need a lot of help here, from one another, from others, in trying to visualize the changing shape of the intellectual terrain we perform work in.

### Concepts and Conceptualization

Forcing ourselves to think in ways we are not used to. . . Here let me turn to the first of the broad headings, concept formation and measurement. Those familiar with the analytical-philosophical headings of the 1950's and 1960's will recognize this one. There are three subjects I wish to deal with, three movements I think we perform in the course of inquiry into long-term large-scale change.

For the first I will use the expression "movement from the abstract to the concrete." One general form of this movement is from concept (abstract) to indicator (concrete). I think the concept-indicator "movement of thought" is so ingrained in our understanding of inquiry that it takes an act of will not to proceed straight off in that fashion. The format is reasonably clear; there are marvelous descriptions of it; it is an important way of thinking, a very important way. I think Lazarsfeld, in every third publication on methodological matters, goes into the movement from formulation of a concept through to associated ideas, through to selection of indicators, through to their combination into indices, and so forth. More abstractly, there is the Hempel-like formulation, the imagery of a theory as a network of substantive concepts (nodes) and logical relations between them (threads), floating, as it were, above a world of reality to which the theory is linked by "rules of interpretation"; the rules tell one how to descend from the concepts to the observations that indicate or measure the concepts and how to ascend from observations by conceptualizing, interpreting them. The movement here is analogous, then, to the movement in a relation of logical inclusion, from abstract concept via successive additions of specifics or attributes to the concrete, the "real indicators"; from the concrete via the dropping of these attributes, to the abstract. That is one sense of this movement.

There is a very different form of the movement which is analogous not to inclusion-relations but to part-whole relations. In this case, the part (a theoretical process) is the abstract, the whole is the concrete. Concrete here is a level of conceptualization, it is not the "real world". This sense of the movement from abstract to concrete is the one Marx discusses in his

very brief and elliptical remarks on "The Method of Political Economy." As a methodological directive, it is extremely demanding. Consider, as an abstract formulation, the reproduction cycle (basically the array of arguments set forth in Volume I of *Capital*) and take the concept "capital accumulation"—how do you proceed from there? Well, if you work with the notion of the concept-indicator interpretation of abstract-concrete, you have one set of directions. You are to begin looking for indicators of degree-of or amount-of capital accumulation at some place(s) in some period(s), presuming there are ways of measuring it straight off, more or less validly, more or less reliably. The part-whole directive gives one an utterly different set of directions. It says to keep moving out by successive determinations, bringing in successive parts—themselves abstract processes—in continuous juxtaposition and in this way form the whole which you need for interpreting and explaining the historical changes or conditions under examination. I will come back to this later, when I take up some topics under the heading of explanation-interpretation.

Now, in this context, I have difficulties with, for example, Theda Skocpol and Kay Trimberger's (1978) notion of revolutions, because I sense that the term is used as a category ("abstract") into which events ("concrete") can be classified. And I have doubts about proceeding that way in the kind of work we do. Revolutionary transformations of social relations—of production and, thus, of all others—seem to me very abstract "parts" (theoretical processes) which have to be brought into successive relation with other "parts" (other theoretical processes) in order to move toward the concrete. That is *still* a conceptualization (or an interpretation-sketch); but then, in the fullness of the whole so formed, one "interprets" observational statements; or, alternatively, one "measures" selected and partial "outcomes" of the complex of processes.

I guess I have similar difficulties, therefore, with Walter Goldfrank's (1978) "fascism". If it is just a condition, an attribute of a regime, it is not very interesting. *If* it is a theoretical process (a part) dealing with rule and therefore relational, I need to know far more about the movement he takes in going from the abstract to the concrete, toward a fuller whole, and within that, from observational statements (about "German rearmament") to the now-formed social whole in which the "rearmament" has meaning, from which it derives meaning. That is one topic under this heading.

A second concern and movement is quite different. It has to do with our locating ourselves "in time" in relation to our subject matter. One notion of time is that it is an objective time scale—it is "out there",

forming a coordinate of our observations and ourselves. We can position ourselves and our subject matter—it is in the past, it is going to be in the future, so many years, and so forth—and so fix our respective places. This has the great advantage that we can always undo the relation so formed; we can shift ourselves about, in the way anthropologists do when writing in “the ethnographic present,” which of course simply eliminates the real relations between the researcher and the researched.

I do not think the world-system perspective, as it is developing and as we begin to reread and runderstand matters from this angle of vision, permits this. I think the perspective we are trying to develop premises a multi-level, complex system of social action that is comprehensive and singular not only in scope—and so forms a spatial “world” with its own changing geo-political boundaries—but also in time—and so forms a temporal “world” with its own irreversible sequences and non-arbitrary periodicities. It forms, that is, a single temporal, developing world, through its deepening synchronization, its chronologically ordered thrust, its cycles of expansion and contraction, and its secular trends.

This is not a concept that is easy to grasp or to work with. Most of us do not consider “time” in this sense as an integral dimension of the systems we predicate and examine and, *inseparately*, of predicating and examining them. Instead, we think of it, as I indicated, as an independently given ordering dimension in terms of which we make and array observations bearing on the systems we are examining. The modern world-system, however, cannot be usefully thought of as analogous to a racing car that is “started” and then “timed” over the course it ran and continues to run. As we conceive it, it does not function so much *over* time as *through* time. Time in the form of its trends and cycles is, like space, constitutive of it as a system, not merely a coordinate of its properties’ variations. It does not have a history or set of histories so much as it constitutes a history or set of histories.

Now, that is obviously a difficult epistemological premise, but if we can grant it for now, we can see that we are strictly limited in the ways in which we may form working concepts. We cannot proceed, as Weber did, in sketching the dimensions, features, and aspects of, for example, the social relations of domination—that is, bureaucracy—to pull out an attribute from here, an attribute from there, an attribute from somewhere else, and form with them an “ideal-type” construct. That way of proceeding is precisely to deny a central feature of what we are trying to study and a central premise of how we are trying to study it. Or maybe a better example. We cannot construct a typology of the social aspects of the

division of labor, as Weber did in those marvelous sections 15-24a of the second chapter of *Economy and Society*, where he takes up just about all of the distinctions any of us might imagine. For, as he says, he took most of the ideas from Bücher *except* their formulation as integral to definite historical developments. Instead, if we want to use the ideas, we have to use them in the form in which Bücher presents them, as cumulating *processes* of division of labor in their historical sequences, so that the understanding of something called “division of labor” is as a set of processes, *not* as a set of conditions. Bücher depicts an overall developmental movement carried forward by one major form of the process, then a second one, with the first still going on, then a third form, all intersecting, and so on. He gets to five forms of division of labor as process. I am sure we will be able to find more or maybe collapse some, but the point here is that the concept is itself time-developmental or “historical.” Also, and in a different vein, there is Lenin’s review in *State and Revolution* of the formulations of the state, in the sense of post-seizure of state power, in Marx and Engels’s changing understanding of it as they were analyzing first the 1850’s then the 1870’s. Something along these lines seems to be necessary if we are to build up and to deepen the kinds of concepts we need in order to apprehend the kinds of real movements we seek to analyze and interpret.

The third mental movement in this area that I wish to discuss I will introduce with an analogy. (The exposition here, I might insert, will run on a bit.) I have in mind the figure-ground movement where if one refocuses, what was figure becomes ground and when one refocuses again, what was ground becomes figure. For us, the figure-ground movement seems to take place centrally between social relations and agencies of action, between role and role relation. I think the methodological directive with which we work is that our acting units or agencies can only be thought of as *formed*, and continually re-formed, by the relations between them. Perversely, we often think of the relations as only going between the end points, the units or the acting agencies, as if the latter made the relations instead of the relations making the units. Relations, generally, are our figures and acting agencies are our backgrounds. At certain points in conducting analyses, it is of course indispensable to shift about and focus on acting agencies; but I think we too often forget what we have done and fail to shift the focus back again. Let me give here one formal illustration of what I mean and then two of a more substantive sort.

We often address ourselves in inquiry to conditions we call “distributions”. One has a set of, say, households, and each exhibits more or less of

some condition, say, material well-being, so that we can then say the condition is unequally distributed over the households. We then usually ask questions like "How come?" "What produced the inequality?" "What are the effects?" I think we commonly forget that the directives we work with tell us to envision these distributions as "produced by" the relations among the units, i.e., as themselves merely the totals rows or columns of relational matrices (in the common formats we are used to: input-output tables, who-to-whom interaction summaries, origin-destination arrays, and the like). The same units are arrayed horizontally and vertically, and the resulting cells give the formal locus of their respective dyadic relations, i.e., the formal locus of the ongoing processes. The totals rows and columns, which one literally obtains by some summing operation across rows or down columns, are the "distributions". And so, when we address ourselves to distributions without remembering to see them as summaries of conditions continually resulting from processes among the units, we give up our central focus on relations and perforce become eclectic and ad hoc in our efforts to set forth coherent accounts of "distributions".

- Let me give a substantive example. In Chapter 2 of *Development of Capitalism*, Lenin addresses himself to a matter then under debate in Russian intellectual circles: "Why was there increasing inequality among the peasants? Why were some peasants becoming quite rich and some poorer and poorer?" (There were becoming available at the time a growing number of reports containing what are known as the Zemstvo statistics; and the chapter consists in large measure of a careful, critical review of a sizeable number of these reports.) Lenin's answer, to summarize it very briefly, is that the observed "differentiation" of the peasantry into "rich", "middle", and "poor" results from an ongoing transformation of the social relations of production in the Russian countryside. The "middle" stratum (30 percent) for the most part is what remains of the peasantry proper, those still in the lord-peasant relation of production (in its post-emancipation forms). This stratum (and the social structure it is integral to), however, is progressively being eliminated through the increasing formation of capitalist relations of production between the few (20 percent), who as a result are becoming a class of small bourgeoisie (the "rich"), and the many (50 percent), who as a result are becoming a class of rural proletarians (the "poor"). The few increasingly concentrate the means of production in their hands (land, draft animals, implements) and so also the means of subsistence, increasingly engage others to work for them, and increasingly produce "for the market". The many increasingly lose possession and control of means of production, increasingly work for others,

increasingly purchase their means of subsistence "on the market". The two seemingly different trends—some becoming rich and others becoming poor—are only the necessarily opposite tendencies produced by the single relational development. Accordingly, what is occurring is not the peasantry's differentiation but its disintegration. And it is this ongoing transformation of the relational structure that brings about, and is in part reflected in, the increasingly unequal distribution of material well-being over rural households, which the Zemstvo reports document.

The data here thus come to the analyst in the form of distributions of features (e.g., amount of land leased) over acting agencies (in this case, households) and not in the form of relational matrices (showing, for example, the lease-from/rent-to relations among the households of an area). Nevertheless the analyst keeps his attention throughout, not on the acting agencies (households), or on abstract categories ("rich", "peasants", and so forth) in which they have been classified, but on the developing relations among them and on the processes integral to those relations and their development. The result is a sustained, comprehensive interpretation not otherwise likely.

A second case in point is the core-periphery relational conception. Here, unfortunately, the end-terms "core" and "periphery" all too often become themselves respective foci of attention, categories in their own right, as it were. And the relation which the joined terms designate slips into the background, sometimes out of sight entirely. When that happens the processes continually reproducing the relation, and hence the relational categories, also drop from sight, and we are left with only the categories, which, as a result, are now mere classificatory terms, neither grounded theoretically nor productive analytically.

Capital accumulation, concentration, and centralization (through the "broadening" as well as the "deepening" of capitalist development, to use Lenin's apposite pairing) are, in the forms in which we know them, the most general and comprehensive of the processes reproducing core-periphery relations (themselves a definite variety of "uneven and combined development"). But more immediately related to the conception, as we work with it, is division and integration of labor as processes. For these have as their general direct effect the reproduction and further development of specialized (therefore partial) production-operations—hence, of production-communities—as distinguishable but not separable production-activities of a more or less integrated, continually expanding and deepening world-scale social economy (in that sense, world-economy). Core-periphery relations, or derivatively core-and-periphery formations, are

thus, in the first instance, categories of the world-scale social division of labor. Those latter processes, though, are in turn aspects—or better, forms of specification—of the most general and most abstract process, the accumulation of capital (including here the ongoing so-called “primitive” accumulation, i.e., “original” expropriation as a continuing historical process, as well as the self-expansion of capital).

Accordingly, to let the relation which “core-and-periphery” designates slip into the background is to let the labor process as it operates on a world scale slip into the background as well. One place in particular where this sort of slippage seems to occur frequently is in discussions of “trade” between “core” and “periphery”. With the latter pair as classificatory terms, we say, “Here’s a core-country and here’s a periphery-country; now, how are they related? Why, through ‘trade’.” And with that, a set of activities and interactions we call “trade” ceases to be just one of many ways in which the interrelations linking the partial-production-operations formative of “cores” and those formative of “peripheries” are actualized, in given times and places. And instead “trade” (almost invariably as “market-trade”) becomes *the* form of *the* relationship between *the* core and *the* periphery. We now have two kinds of “things” (world-regions), “cores” and “peripheries”, related by (usually) international trade—hence, exchange rates—a world market, terms of trade, and the like. With that the figure-ground inversion is complete, and the basic conception ceases to frame analysis or to guide interpretation.

### Explanation and Interpretation

The second heading under which I want to discuss some issues of method is explanation and interpretation.

Let me turn first to the activity of explaining. As working social scientists, we engage in it all the time; sometimes we do so quite self-consciously, sometimes in a rather off-handed fashion.

Whether or not we are attentive to what we do when we explain, however, may not matter very much. For a particular conception of explaining, seen not as an ongoing activity but as a more or less adequate performance, has become dominant in American philosophical circles—dominant in the sense that, whether you are for that conception or opposed to it, you address yourself to it. This is the “covering law” notion of explanation (Hempel) which, although formulated to facilitate philosophical studies of scientific work, has become, for many social scientists, prescriptive for scientific work.

The nub of the covering-law notion of explanation is exceedingly simple (although after that the idea becomes quite complex). And it looks suspiciously like an elementary syllogism. One wants to explain a described condition, *b*. This requires a law (one covering the condition) stating, in effect, that if *a*, then *b*; it also of course requires an existence statement to the effect that *a* exists. The full account then consists of statements to the effect that (1) *a* exists, (2) a law obtains such that if *a*, then *b*, and (3) *b* exists. In this version, the existence statement about *a*, coupled with the statement of the covering law relating *a* to *b*, *together* “explain” *b*, i.e., logically “produce” (by one calculus or another) the existence statement about *b*.

This schematic notion of explanation may be useful in summarizing explanatory arguments, as I shall shortly illustrate. It may also, like truth-tables, help to make internally consistent one’s own explanatory accounts. But I doubt very much that in itself it is of much help in the actual *activity* of “explaining”. For our difficulties lie, not so much in knowing how to cast our interpretive arguments, once they are formulated, as in figuring out how to formulate the arguments in the first place.

But let me first illustrate the claim that interesting explanatory accounts of complex social changes are not necessarily inconsistent with the covering-law formalization of explanation. Let us sketchily restate Lenin’s account, mentioned earlier, of why some peasant households were becoming poorer and some richer. The “dependent variable” or, here, the explicandum, is the growing “differentiation of the peasantry” or the increasing inequality of well-being among peasant households. The answer Lenin gave is, in summary, this: new social relations of production are forming among peasant households such that, in relation to one another (and not merely relative to one another), some are becoming small-capitalist households and others proletarian households; such relations presuppose and carry further an unequal distribution over the households of, primarily, means of production and, secondarily, means of subsistence (well-being); the latter inequality constitutes what is seen as the growing “differentiation of the peasantry”. Thus we have:

- (1) the existence statement that capitalist relations of production are developing in rural Russia;
- (2) the law-like statement that the development of capitalist relations of production presupposes and furthers centralization of capital among households and, in addition, an unequal distribution of means of subsistence (well-being) among them;

- (3) the derived existence statement is, therefore, that there is an increasing inequality of well-being over peasant households, or a growing differentiation of the peasantry, in rural Russia.

(This is not a particularly precise or elegant version, but it will serve for present purposes.)

How useful is this notion of explanation to working social scientists? My guess is, not very (except perhaps in checking out the coherence of one's own, or another's, interpretative argument). Why? Because it presupposes what is at the heart of our working difficulties, namely, how to formulate what seems in need of explanation or interpretation and what laws to use in constructing the explanatory or interpretative account. Or, alternatively, it presupposes laws to work with in seeking to identify what is in need of interpretation and a procedure for moving from the "abstract" law-like formulations to defensible "concrete" interpretative accounts. An analytic philosopher would say, I imagine, that of course such concerns are "presupposed"; they are substantive matters, and we can have nothing of import to say about such things. Which is exactly the first point I wish to make under this heading: namely, the "covering-law" conception of "explanation" may provide some guidelines for writing up the results of an inquiry, once they have been obtained; it provides no guidelines whatsoever to the actual conduct of inquiry, "legitimatizing" footnotes to philosophical writings to the apparent contrary notwithstanding. We are, in short, much more on our own here than we may think or than we may have been led to think.

We then have two distinguishable, if in practice invariably highly interrelated, steps under this heading, the selection of problems and the construction of explanations. In the nature of the case, I can do no more than touch on one or two points in connection with each.

Regarding the selection of problems for inquiry, in studies of long-term, large-scale change, let me start somewhat negatively. A practice has grown up, largely the product of our semi-statistical way of imagining problems of inquiry, which we are probably all engaged fully in at one time or another and probably engage in to some extent most of the time. In this practice, one rapidly (sometimes thoroughly—it does not matter) scans a set of "cases" and classifies them either as instances under some general rubric ("revolutions") or as instances not under it. Alternatively, one scans them with a view to uncovering what a subset of them has in common (which becomes the equivalent of the general rubric) and the remainder lack. Have they or have they not had a revolution? Do they or do they not exhibit democracy (or fascism)? Is there or is there not a true national

bourgeoisie? Is the peasantry a decisive social force there or is it not? And so forth. One then, almost without thinking about it, inverts the subject and the predicate: one moves from this "case" exhibiting this "condition" to this "condition" having a "case", as an instance. Now, in so moving, one has "abstracted" the condition and made it, in its now categorical form, the focus of attention and inquiry. But what is there to say about "revolutions", "democracies" (or "fascisms"), "true national bourgeoisie," or "socially decisive peasantries"? Not much (just as there is not much to say about "anomie" or "cross-pressures"). And so one invokes the life-saving queries, "Under what conditions does X occur?" or "Given X, what conditions account for its having different effects?" And all manner of "hypotheses", "theories", and doctoral theses are thereby licensed.

From there it is but a small step to rounding up a set of cases; predicating and measuring properties of each; tracking the observed relations among the measurements; checking out the recurrence of the observed relations under various conditions (the analysis of statistical relations); generalizing the observed relations; and concluding with several new laws of the form: for any  $s$ , if  $x$ , then (probably)  $y$ .

From what has been said above about the singularity of the modern world-system as a complex of spatio-temporal processes (social relations), it should be evident that this way of choosing a subject of inquiry—this highly conventionalized, and convenient, practice—is fundamentally inappropriate in the study of social change from the world-system angle of vision. For at base the practice presumes a degree (quantitative) and a kind (qualitative) of repetition that the construct—"modern world-system"—flatly rules out. To focus on certain seemingly similar conditions in various places at various times; to abstract those conditions from their place-time settings; and to inquire, abstractly, into the causes or consequences of the conditions is to proceed precisely in the one way clearly ruled out of court by the world-system or world-historical perspective on social change.

(The preceding is *not* intended to suggest that the presentation of statistical relations, of the sort alluded to, has no place in world-system studies; that would be silly as well as wrong. It is intended to say that the role of such presentations in world-historical studies is decidedly different from their role in ahistorical inquiries, including studies that come under the heading of the sociology of the past. In much current work, they are used to establish general propositions or to disconfirm them. In world-system studies, their role is fundamentally different. They may function to set up a problem for case-by-case examination; or, at the opposite end, they may be used to summarize results from a case-by-case inquiry. But they can never legitimately serve, as they commonly do in much con-

temporary inquiry, as substitutes for successive detailed studies of each case. It is the a priori elimination of each case's distinctiveness that the world-system's approach rules out, not the claim that there are comparabilities or similarities.)

As to ways in which to choose a problem for inquiry, we are just beginning to learn them. Let me briefly describe two. One is illustrated by Lenin's study, mentioned above. The general format here is "the development of capitalism in. . . ." Within that there is a focus, given in that study by its subtitle, "The process of the formation of a home market for large-scale industry." That, in turn, has an origin simultaneously in a general theoretical problem current at the time (and in the place, Russia), concerning "the realization of the product" and in an historically specific interpretative problem, "how and in what direction are the diverse aspects of the Russian national economy developing?" Here a definite theoretical-interpretative concern—how to bring into relation (1) a clear but highly general (abstract) theoretical account and (2) a body of apparently pertinent information about economic activities within a particular place over a particular period—organizes the analysis. The study's conclusion, just to be clear on the matter, concerns the plausibility, not of the abstract theoretical argument but of the interpretative argument (regarding "the formation of the home market").

Another is illustrated by Frances Moulder's slim monograph, *Japan, China and the Modern World Economy* (1977). At the time she began work on it in the mid-sixties as a doctoral research project, the accepted query regarding China and Japan was: Why did Japan modernize and China not do so? Which, in its formulation, directed inquiry inward: What was there about "traditional" Japanese society, in contrast to "traditional" Chinese society, that led the one to modernize and the other to fail to? Moulder, working from the world-system perspective, sharply restated the question: How did Japan come to occupy the place it has in the world-economy and China to occupy the place it has? Here, then, we have a problem generated, and a line of interpretation suggested, by the world-historical perspective but, in this case, without there being, as well, a governing general theoretical problem (such as the "realization problem"). More generally, perhaps, studies of the development of capitalism that are oriented to its "deepening", such as Lenin's, may be more explicitly theoretically oriented to begin with than studies of its development oriented to its "broadening," owing to the scope and limiting assumptions of *Capital*. Moulder subtitles her study, "Toward a reinterpretation of East Asian development ca. 1600 to ca. 1918." We shall probably need a number of such polemically framed interpretative works before a theory

of noncapitalist expansion of capital, comparable in generality to and integrated with the theory of the self-expansion of capital, can be stated sufficiently fully and abstractly to serve as the guide to orienting *theoretical* problems.

I now turn to the last topic, the construction of explanations. Two matters concern me here, the first being the use of "laws" in interpretative accounts. Christopher Chase-Dunn (1978) sketched a set of law-like statements. The construction of such arguments-in-the-abstract is an indispensable step in inquiry and a needed intermediate activity we all should engage in. But Chase-Dunn was also worried about "testing" his interpretation-sketch. And that I have misgivings about. For he may have meant: constructing Hempel-like rules of interpretation linking his concepts to indicators of them; making (obtaining) the thereupon requisite kinds of observations (measurements) and examining the observable patterns among interrelated arrays; and "concluding" from all this that his interpretation-sketch is warranted, in some general sense, by this sequence of activities or made abstractly plausible by it. For if that is what he did mean by "testing", then in the light of what I have been saying, I evidently think he is on the wrong track. I do not know that that is what Chase-Dunn meant; I only want to express my misgivings if it *is* what he meant.

It seems to me that Richard Rubinson (1978) carried a Chase-Dunn-like argument in the direction it should go, that is, he puts a general argument to use in (for a short paper) a reasonably specific way in order to interpret ("explain") a particular development. Both the United States and Germany, he observes, became core powers in about the same period. From a world-system perspective (more specifically, assuming *some* proportionality in core-periphery relations), this "expansion" of the core-arena of the system would seem to entail (imply) an "expansion" of the periphery-arena. This he can document, and he then turns to the related processes through which center-coalitions both form within the two core-area jurisdictions under discussion and proceed, simultaneously with the system's "expansion", to develop relatively strong state structures (or, better, processes). I don't think the method here is at all mystical. He moves toward his subject matter, making tactical use of contradictions in his exposition, in what seems to me a rather sound scientific fashion.

I do, however, miss one important kind of consideration in his account, and noting that will wind up these comments. I have in mind the relation between "internal" contradictions and "external" contradictions. Specifically, I could detect no connection, in the account as given by Rubinson, between the balance of forces (processes) accounting for center-coalition formation and expansion and the balance of forces (processes) accounting

for core-arena (and therefore periphery-arena) expansion. That seems to me an analytic weakness in the account as given; I do not think our rules of procedure permit us *this* kind of incompleteness in our accounts.

I want to conclude, with an illustration of the point, by bringing these two sets of contradictions into relation with one another in the course of constructing explanatory accounts, but I shall have to reach for a far-removed example. (Mao's essay "on contradiction" sketches the general, schematic considerations more fully than any other single source I know.) At the time the British were forming a colonial unit they would call Uganda, there was in what became that colony's southwest corner a centrally organized "kingdom", Ankole. Internal to this kingdom were many tensions, among which two are pertinent here. One was between the Omugabe ("king") and his clan brothers (the "aristocracy"), each of whom was by birth if not by events as qualified as the incumbent to rule. Another was between these "princes of the drum", collectively, and the powerful, but by clan ineligible, "commoner" chiefs. In the century preceding the establishment of British overrule, both had come to be dominant tensions within the kingdom, for the incumbent kings had worked successfully with the commoner chiefs to strengthen considerably the "royal" power at the expense of the aristocratic power, and hence to strengthen commoner chiefly power against aristocratic chiefly power. Externally, the kingdom was under pressure from several sides, one of which was the kingdom of Buganda. The British then appeared, on the side of Buganda (itself a complicated story), altering the balance of forces "externally". The second in command in the kingdom, as it were, who was a commoner, made arrangements with the Baganda and the British, altering the balance of forces "internally". Then, as overrule took effect through the Omugabe and (primarily) the commoner chiefs, the pressures on the aristocratic chiefs grew, and eventually they fled, fundamentally altering the balance of forces ("contradictions") within the now-incorporated kingdom. Coupled with this was the formation of Uganda, and so a whole structurally new set of "internal" and "external" contradictions began to form (and mature). Or at least—and this is the point, of course—that is one way of beginning to organize an account of the establishment/elaboration of imperial overrule in one particular instance. More generally, perhaps, anti-overrule begins with overrule; or, anti-imperialism with imperialism.

So far as constructing interpretative accounts is concerned, we have barely begun to appreciate the power of the "internal"/"external" sets of contradictions—let alone sketch a "logic" for them.

## *ON THE HOLISM OF A WORLD-SYSTEM PERSPECTIVE*

Robert L. Bach

There is a great big something wanting, I don't quite know what it is. Please walk thirty yards, so that I can get a perspective on the thing. . . . Now, then, your head's right—everything's right! And yet the fact remains, the aggregate's wrong. The account don't balance.

—Mark Twain

This essay is an attempt to gain some perspective on the methodological issues of a world-system approach. Of particular concern is how to proceed analytically given the theoretical insistence that social action takes place at the level of a world-system as a social whole—"not 'society' in the abstract, but a definite 'world', a *spatio-temporal whole*" (Hopkins & Wallerstein, Chapter 2, this volume). For some (Rubinson, 1977); Chase-Dunn, 1979a, b), the theoretical issues derived from the challenge to developmentalism do not suggest and certainly do not require a reconsideration of methodological traditions. I disagree. While many of their techniques may be useful, for instance, relational network analysis (see Snyder & Kick, 1979), few are sufficiently grounded in the "logic of inquiry" suggested by a world-system approach. My intent in this chapter

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