SOME LIMITATIONS OF CLASSIFICATION IN COMPARATIVE STUDIES EXEMPLIFIED BY THE ANALYSIS OF PARALLEL ECONOMIES, WITH IMPLICATIONS FOR DEVELOPMENT THEORY*

Cyril S. Belshaw

Without challenging the obvious need for classification as an instrument of anthropological thought, I have in the past drawn attention to the way uncritical methods can mask potentialities of examining the force of variables in comparative studies (Belshaw 1969). In this paper I return to the issue by examining another example of the limitations of classification. The example is the analysis of economies parallel to industrial and commercial monetary economies (whether "capitalist" or "socialist"). My intent is to ask (a) whether clarity and accuracy are lost through present methods of classification, (b) whether there is an alternative method, and (c) whether such an alternative method might be useful in examining propositions relevant to development studies.

The stimulus for the paper is derived from an important study undertaken by Veechibala Das (1986) entitled *The urban informal sector: an alternative analysis* carried out for Ph. D. work in Community and Regional Planning. The study draws heavily on work in economic development, human geography and planning, and to some extent on anthropology, revealing a plethora of classifications, and attempting to resolve the confusions and inconsistencies in the literature, related to a dynamic of origins and functions of informal sectors, world-wide. It is the global implication that something called "an informal sector" always exists in parallel with capitalist market or socialist centrally planned economies that provides the challenge. For this provides opportunities for comparative examination of forces which affect

* This paper was completed under the terms of a Senior Research Stipend of the Wenner-Gren Foundation for Anthropological Research, New York.

the total performance of economies, polities and societies, whereas discussion of the market or planned economy alone provides only part of the answer.

I choose the therm "parallel economies" only for heuristic purposes, to distance myself from the terms already established in the literature, and because a short-hand reference is needed (though I will abandon it by the end of the paper.) All cultures are now part of nation state systems. The examination of the economy of relationships has largely concentrated on the dominant ideological perspective of the system, that is market capitalism or state socialism, or some combination. Nevertheless, ever since the sunset of colonial rule, minority attention has suggested that activity lying "outside" the dominant systems is of very great importance, even in the most advanced of capitalist and socialist countries. It is this activity which the Das thesis and major contemporary literature outside of anthropology dubs as "informal", and which I am calling "parallel", at least for the nonce.

I admit immediately that the term "parallel economies" has all the difficulties associated with the plethora of other terms that are favoured by this or that author. "Parallel" implies a social equivalence which may or may not be present, and begs the issue as to whether the activity in question is, for example, subordinate, dominant, intertwined with, or detached from, the capitalist or socialist activity. Much of the early literature suggests subordination, though more recent studies are more consistent with intertwining at various levels.

Further, the word "economy" suggests a closed system, again an issue that should be subject to empirical enquiry rather than being assumed. While few would doubt that one can normally abstract systemic and closed characteristics of capitalist and socialist polities, it must be admitted that such an abstraction defies reality. International forces have always been recognized, and now in the main body of economics the significance of exogenous variables and of parallel activities is getting considerable reinforcement. If this is true of the orthodox economic systems, it is even more true of the parallel sector. It has to be determined empirically whether such a sector is or is not an economy in the systemic sense. That is, can an analyst armed with data determine a system of interacting relationships which he can consider closed, other things being equal? This can frequently be done, and is indeed a commonplace in anthropology; but it cannot be assumed. While the black market may evidence a system of its own, selfhelp bri-

colage activities may influence supply and demand in the marketplace more than with each other.

With such cautions in mind I shall nevertheless retain the term for convenience, *pro tempore*.

The attention now being given to the role of parallel economies emerges from a gradual recognition that models limited to capitalist or socialist relationships are in fact ethnocentric or ideologically biased if they purport to deal with global phenomena. The origins of such a correction in perspective lie with the dual economy of Boeke (1942, 1953) and the perhaps more accurate plural society of Furnivall (1939), who held the capitalist colonial model could only be applied to a restricted range of behaviour. Yet even here the ethnocentrism is now apparent. Capitalism represented such a unique cultural mode that there was a sharp boundary between that and other cultures. Noncapitalism, or tradition, or the "Asian economy" was completely different in world view, in values, in social exchange relations, and particularly in the absence of a dynamic (this was more strongly stated in Boeke). In making comparisons, capitalism was the starting point.

That perspective remains true today. Fundamentally, the parallel or informal sector is that which economists classically do not model ... Historically, it has been left out. Now that it is being discovered and rediscovered, it turns out that it seems to operate with different rules, not merely empirically but also theoretically. Some anthropology, derived from Polanyi (e.g. 1957) and the substantivists (e.g. Paul Bohannan and George Dalton) reinforces such a position, suggesting that there are few if any "universal" models of behaviour, each culture providing a firm boundary, often with stasis on one side and dynamism on the other. Usually, this is to confuse empirical cultural variation with a presumed absence of universal, or broader, process, a confusion which makes the study of change and development almost deniable.

Before entering into the question of the validity or otherwise of boundary statements, let us overview the range of concepts, and the ideas behind them, in the literature. It must first be said that the literature is immense, and that neither Das nor I, nor any earlier author attempting a review, has covered all possible references. Computer searches (even if all references were in data banks) are necessarily limited because the full range of terms cannot be predetermined, and do not necessarily show up in titles or keywords. My own selection here will be to follow the literature which Das accessed and examined, and then look for similar con-

cepts in anthropology. In anthropology it appears that reference to even the plethora of terms used in other disciplines is minimal. This is not to say, however, that we do not examine the issues. Indeed we do, with different terms, and very frequently arriving at a consensus which is opposed to perspectives in other disciplines, but not directly confronting them.

In fact Das' review reveals an immediate weakness of any classificatory system, where the terms are not agreed upon, namely that it is difficult to be sure that all possible classifications have been taken into account in theoretical statements. A literature search must begin with what is known, and does not automatically lead into the discovery of what is not at first known.

I also impose a further restriction, following Das. I shall concentrate on that literature which deals with urban society. Das herself does not completely restrict the search to urban studies, taking into account, for example, wider national examinations; but it is the analysis of urban life that is her main goal. In a way this makes her task a little more difficult, at least on the surface, since it is perhaps easier to discern a certain kind of dualism based on the pair of concepts "city/town" and "rural/country". Indeed, talking about town also usually implies at least passing reference to country, as if country were an opposite, but interconnected, and Das of course does this. The dichotomy is one way or another inescapable in the anthropological studies. But within the city itself, the distinctions are not as superficially easy (I say "superficially" easy because the town/country distinction is not at all easy when one probes in depth).

What does this kind of sampling do to reveal issues of clarity and accuracy? From the Das monograph we can see that there is no agreed terminology, arguments abound as to whether the reality is one of dualism or pluralism, and what the criteria for determining separated systemic sectors should be. Here are some of the main classifications to which Das refers.

Most of the designations of sector I are, or can be, criteria used to determine whether an activity or a section of the population can be described by some such term as "informal" by contrast to the "formal" sector II. Each of the authors above uses the criterion or criteria as the primary determinant of classification, although he may elaborate by breaking up the criterion by means of lists of components, e.g. by listing appropriate occupations. It is quite evident that the method of arriving at the criteria is to determine on the basis of common sense, field experience, and the

Sector I	Sector II	Representative author ex Das
Traditional "eastern" limited needs	Changing "western" open needs	Boeke (1953) Furnivall (1939)
Production for use	For gain	Brookfield (1975)
Non-evolutionary	Dynamically changing	Chayanov (trans. 1966)
Rural	Industrial	Higgins (1968)
Bazaar sector	Firm-centered	Geertz (1963)
Peasant	Capitalist	McGee (1971)
Formal	Informal	Das (1986)
Unstructured	Structured	Emmerji (1974)
Unprotected	Protected	Mazumdar (1979)
Lower circuit	Upper circuit	Santos (1979)
Urban poor/squatters/immigrants in certain occupations	Others	Moser (1978)
Petty traders lacking skill and capital	Skilled capitalized firms	I.L.O. (1972)
Peasant using total family labour supply	Labour outside family	Franklin (1965)
Family entrepreneur	Individual and corporate entrepreneur	Friedmann & Sullivan (1974)
Unsupervised wages	Government or trade Union supervision	Mazumdar (1975)
Unprotected enterprise	Enterprise state protected	Weeks (1975)
Unregistered	Registered	Sethuraman (1976)
Self-employed	Wage employed	
Casual workers	Regular workers	Sethuraman (1976)
Wages below minimum	Wages at or over minimum	Schaefer (1976)

like, that there are at least two different kinds of life-styles in a city or group of cities which correlate to differing methods of production and/or distribution. When authors have engaged in empirical investigation their research method frequently requires

a precise determinant for counting or identification purposes: that determinant varies according to whether the study aims at quantitative precision, and/or relates to the examination of a predetermined theoretical position. Since each piece of research differs from all others in such matters, the criteria also differ.

At the same time, many authors are interested in globally applicable theories and concepts. The weight of the literature leads them to believe that, whether the dualism is called informal/formal or peasant/capitalist or traditional/modern or something else, it represents a universal reality which can be classified, with modifications of a single classification system to account for variants. Thus the selection of a single criterion by one author leads to the question, is that criterion suitable for the universalistic conception? The answer seems to be always no, yet the idea of a unifying term will not die. Authors are apparently committed to improving the criteria rather than abandoning the attempt at universalistic classification. For that classification attempt does permit them to examine and debate comparisons, and sometimes to disentangle functional themes.

The above classifications and criteria are drawn almost entirely from urban situations in the third world. This applies even to statements about "peasant" and "rural", since in such cases the urban sector of reference is deemed to have characteristics similar to those of rural life, even though physically present in the city. While some of the classifications can be considered as refinements of others, many groupings are mutually incompatible. The point does not have to be laboured. For example, unregistered enterprises do not necessarily have to be built around using a total family labour supply to the exclusion of non-family members. Some dichotomies are logically deficient. For example few anthropologists would find a lack of structure in the context to which Emmerji refers, and the dichotomy "production for use" and "production for gain" is at least ambiguous.

The search for a universal brings out such criticisms and many more. Das pays particular attention to amendments which authors have considered more recently, from the late seventies to about 1985. To begin with, the informal sector need not necessarily share each of the above criteria: it does not have to be low paying, it does not necessarily hold new migrants, in some instances it is not pre-capitalist but highly capitalist, the participants are not necessarily illiterate or unskilled, and insofar as it is defined in opposition to the formal, it may be itself fragmented, thus

pluralist rather than dualist. Some authors add an "intermediate sector" (Steel 1977) and others stress "petty commodity sector" (Forbes 1981, McGee 1979).

Again since the informal sector is sought in opposition to or by contrast with an apparently agreed upon formal sector, writers have discovered it outside the Third World. Similar sorts of confusions and disagreements abound, and authors select terms to make differentiations. Simon and Witt's (1982) underground economy is characterized elsewhere as a second economy, irregular economy, subterranean economy, black economy, characterized by clandestine employment, and equated with informal by Mingione (1985) and others. Some characterize it by referring to undeclared income activities plus crime (De Grazia 1984), or a combination of hidden, criminal and household activity, unmeasured, untaxed, und unregulated (Mattera 1985). Once such economies receive study they are found to constitute an enormous proportion of GDP (see Das 1986: 88 *et seq.*) In socialist societies, some of the criteria remain the same, but others are blurred. Mattera (1985) typically identifies private enterprise as the characterizing factor, but clearly there must be a distinction between that activity which is truly underground on the one hand and that which is state encouraged; and between the legal and the illegal.

I can see that most anthropologists, confronted with such identifications, would say that apples and oranges are being compared. Although household activity in industrialized societies may have some points of similarity with non-peasant traditional society, if and when exchange is involved, organized crime and the parallel internal dollar economy of Poland seem very different from informal urban sectors in Africa, Latin America, or Asia. On the other hand garage sales and second hand markets in France or Canada may have marked similarities with peasant markets which seem opposed to commercial firms in the Third World. At this stage I suggest we suspend our immediate impulse to criticism, and pursue the matter further.

Nevertheless, let us note that not a single one of the sets of criteria referred to above, except perhaps rural/urban, refers at all clearly to one major style of exchange which anthropologists study, which is at least equally worthy of being called informal (though in fact, like any style of economy, embodying rules which can be formalized). I refer of course to that nexus of behaviour which emerges from considerations of reciprocity and which can

be described as a social exchange system, whithout intervention of money.

What the anthropologists do with the same kind of issue? How do they handle the conceptual debates of their colleagues from other disciplines? Do we get closer through anthropology to an appropriate and existing classification system?

To partially answer these questions I consulted more or less at random a number of anthropological urban studies (Ansari & Nas 1982; Basham 1978; Cornelius & Trueblood 1974; Eames & Goode 1977; Geertz 1963; Gilbert 1982; Gmelch & Zenner 1980; Khuri 1975; Lewis 1959, 1966; Parkin 1975; Perlman 1976; Redfield 1930; Redfield & Singer 1954; Rew 1974; Spoehr 1963). Only one author in the sample (Cohen 1974) refers to the range of concepts associated with and including "the informal", although Perlman (1976) gives a thorough history of related concepts. In short, anthropologists have not, as a whole, entered into the debate and have developed their own terms, yet another set of concepts. This is despite the common evolutionary origin from Boeke and Furnivall, and the entry of anthropologists such as Geertz, Peattie and Perlman, into Das, the planner's, cognizance.

Two conceptual problems seem to dominate anthropologists' search for accuracy. The first is to carry forward the argument about the definition of "urban". In one sense the argument is independent of the sets of issues we have been examining, and has its own complexity. Also, just as formal/informal has to deal with a variety of ethnographic realities, so too does urban/non-urban: cities themeselves have to be sub-classified for they do not mirror each other in internal structure, in social objectives, in external relations, and in styles of life. Nevertheless, the urban/non-urban or urban/rural dichotomy is germane and sometimes fundamental to the equivalent of the formal/informal debate.

Anthropologists tend to take their first cue from the urbanist Louis Wirth (1938), who identifies two orders of criteria for the urban. Out of data referring to numbers of people, density of settlement, and degree of heterogeneity, towns will have a characteristic physical structure, and defined systems of social organisation, and a set of attitudes which lead to characteristic collective behaviour and social control. Since Wirth does not clearly establish how a generalized urban physical structure, social organisation and collective behaviour specifically differ from the rural, he cannot be said to define the urban. All he does is hint at the

dimensions that are relevant, leaving the field open to later writers.

In the process of elaboration and re-examination, anthropologists time and again refer to situations in which what they discover to be non-urban, or rural, in fact enters the physical and population entity that defines the city (cf. Ablon 1971, Abu-Lughod 1961, Levine & Levine 1979, Zenner 1980). Furthermore, movements across the boundary can occur in either direction. Thus the search for a designator is one thing when the referent is the physical city and another when the referent is a style of behaviour. It is not a paradox that, ethnographically and theoretically, rural life styles may be found in a city which is physically opposed to the countryside.

The second conceptual theme is that of marginality, a theme which may or may not link with that of the definition of poverty, and which tends to replace, in anthropology, the concept of informal, although it is by no means identical. Perlman (1976) gives an excellent account of the evolution of the concept, not only in anthropology (there is, of course, a great deal in sociology and in political science, as well as psychology). She also links it to a precise elaboration of the history of the rural/urban dichotomy itself. In addition, she is particularly concerned by the entry of the term into the political and bureaucratic life of Latin America, where it takes on a derogatory and social class connotation, sometimes going so far as to deny the possibility of social integration.

Once again, the criteria of marginality are numerous and inconsistent. Perlman points out that marginals may be defined as poor, as jobless, as immigrants, as different sub-cultures, as ethnic minorities, as illegal squatters, as any deviants, as not participating in the elite culture, as being below the standard class scale. While some of these criteria are part of what she calls the "myth of marginality", others can clearly be used as differing criteria for the objective determination of a style of life or a class of people.

Yet the selection of a criterion does not end the matter. Typical of the debates have been those centred upon modifying two classical approaches, the Redfield folk-urban continuum (Redfield 1930) and the Lewis culture of poverty (Lewis 1959, 1966).

Out of my sample I found the following points emerging. In almost all instances, the modifications come from the identification of places or times at which the Redfield or Lewis models do not seem appropriate. Thus Abu-Lughod (1961) argues convin-

cingly that rural immigrants to Cairo are more urbanized according to the Redfield criteria that longer-term residents of at least one of the eight census tracts of the city. In common with many other observers of international migration, Ablon (1971) shows that the participatory success of Western Samoans in Los Angeles relates directly to their use of modes of social networking they brought with them from Samoa, the opposite of the impersonality that both Wirth and Redfield posit as a characteristic of urban life.

As far as poverty is concerned, Lewis responded to initial criticism by creating a further classificatory distinction (Lewis 1966). He distanced himself from the idea that the occurrence of poverty necessarily implies a culture of poverty. Thus for any social group one can ask: Is there poverty? and: Is there a culture of poverty? as two separate questions. The distinction is typical of the method of correction of ethnographically inaccurate classifications; i.e. one adds another criterion which suggests a classificatory distinction. The method gives a hint towards the possible solution to the classificational dilemma. Apart from such considerations, much of the criticism surrounds the conception that there can be conservative values, and hence persistence deriving from the separated condition of poverty.

Yet when the identifier of the group under consideration is not "poverty" but some physical attribute which superficially seems to be linked with poverty, contemporary trends seem to be in a somewhat similar vein. Such an identifier would be residence in a shanty town, which to most observers implies both marginality and poverty, and many other criteria such as immigration. Observations indicate that not all shanty towns are composed of marginal people, not all residents are poor, and there may be both upward and downward mobility. The *favela*, *barrio*, or *bidonville* can be itself heterogeneous (Eames & Goode 1977) and may have a markedly coherent social organization (Peattie 1974, Perlman 1976).

Running through all the distinctions, and inherent in the concept of classification, is the idea that there is a boundary between those being grouped together and all others. This was highly evident in the writing of Boeke, whose work on Indonesia, extrapolated to cover "tropical economies", posited a dualism between the traditional and capitalist that was rigid and parallel, with almost no attention paid to cross-boundary phenomena. The rigid dualism was not fundamentally necessary to most of Boeke's argument and became a target of later critics, perhaps diverting

attention away from more subtle appraisals. Louis Wirth (cf. in Gmelch & Zenner 1980:11), the ancestor of urban studies, particularly in anthropology, specifically denied the rigidity of the boundary. As I have already noted, he himself, in his classical paper, did not arrive at clear cut characteristics, leaving it to his descendants to search for them. Wirth provided a check list, as it were, as to what to look for, without predetermining the answers. But when the descendants "discovered" the nature of the characteristics, and specified them as indicated above, they inevitably created classifications, which implied boundaries.

Anthropological classifications, however, are always subject to the confrontation with the ethnographic real. In this field, where boundaries emerge or are implied, scholars working on single ethnographies can, in the rural setting, abstract out the cross boundary events, or, as do economists with their models, regard them as exogenous. But the isolation of the single culture is becoming less and less acceptable, so that slowly it is being seen more and more as a figment of analysis, of the scholar's need to simplify. Nowhere is this more inescapable than in urban anthropology.

The urban ethnographic real is almost always, to coin a heavy phrase, multi-sub-cultural or even multi-cultural. This can express itself in class and educational differentiation, in life-style orientations symbolized by the physical manifestations of sections of the city, in ethnicity, in distinguishable migratory groups, and in numerous other features. Each of the classificatory exercises mentioned above, whether anthropological or not, can be closely correlated with such features, and others can be drawn peripherally into discussion.

It is thus not surprising that recent anthropological literature severely undermines the notion that somehow boundaries are rigid. Such criticism can take several forms. Peil (1981), for example (as do many writers with a sociological bent), goes almost to the fullest extreme. While writing of cities and suburbs in West African urban areas, on a comparative basis, almost all her discussion simply ignores the distinction, and her tables of characteristics are based on continuums, without boundary divisions. This result derives from her sociological method and the nature of the quantifications to her hand, and is followed by many writers who depend on statistical aggregates. It is some distance from a study based on ethnographic observation.

Where ethnographic observation stresses culture, values, be-

haviour drawn "from the ground up", rather than deduced from more macro aggregates, differentiation is plainly there and must be accounted for. But to go from there to rigid boundary is an *a priori* step. Numerous non-anthropologists have shown that under some circumstances the question of being in the formal or the informal sector (to use the writers' terms) is not necessarily a matter of destitution or inevitability but is a matter of choice (Sabot 1979, Sinclair 1978), that persons caught up in one sector may move in either direction (King 1975 for Kenya, Mazumdar 1981 for Malaysia). Thus there is what I have called elsewhere personnel transfer (Belshaw 1969). When such transfers occur, at least one writer (Rew 1974) has shown that alterations in norms and values take place, even suggesting that a single individual may move during his or her daily activity from one sector to another, easily adapting behaviour accordingly. Indeed, this must be the case, particularly if we include in our ethnographic sampling bricolage within a Western economy. It is also highly consistent with those studies of religious adaptation which suggest that syncretism is not the only response to pressures to change, but that individuals may move without stress between two religious systems operating in parallel.

Anthropologists also stress such matters as continuing rural linkages through traditional networks, and (e.g. Rhoades 1980) the importance for the total system of return migration, which can be not only of persons in the urban informal sector, marginals, and the like, but of persons who have been deeply involved in the highest ranks of the elite (the South Pacific is replete with examples).

For Marxist scholars the informal, the marginal, the poor, cannot exist in historical or social isolation. Just as Third World countries cannot be understood, according to such authors, outside of a dependency relationship with metropolitan power, so class differentiated cities must manifest a symbiotic relationship between the classes, or if you like the sectors. Non-Marxists make the same point. Perlman (1976), for example, makes it quite clear that the diversity of *favela* society involves major interactions with elite society, interactions which serve the total system, which help to characterize the total system, and that are handled with snobbish resentment by the elite who, however, cannot do without them. Such mutual dependencies are totally at variance with the separate dualism of Boeke, but do not derive from ethnographic differences between Latin America and Indonesia. Boeke

could be re-written in precisely the same terms, since it would not be difficult to demonstrate the same kinds of dependencies between the traditional Asian economy and the Western economy, once the latter had penetrated.

The study of boundaries in these and other ways is of fundamental importance to classificatory exercises, yet the number of studies based on classification which self-consciously examine boundary interactions is minimal. The mind set which opts for classification is more concerned, it appears, with identifying at least two differentiable groups, and defining them so that they must be separable in research operations. The boundary must be minimized in importance since it is posited to be there as part of the act of classification, and to modify its sharpness reduces the clarity of the classification. While classifications are clearly dependent on some empirical observation, the characteristics inferred from the observation may be selected almost arbitrarily, depending on the scholarly pursuit of the observer. Thus the differences and disagreements rife in the literature are less a matter of objective disagreement than the choice of differing objectives of research among the authors. There can in fact be as many differing concepts within this small field as there are researchers, provided the researchers are aiming at differing kinds of explanations as, indeed, most of them are. If this is so, quarrels about the validity or otherwise of an act of classification, based on the premise that what is to be classified consists of identical phenomena, are doomed, wasteful of scholarly effort, and misguided. This becomes even more valid when we realize that very few ethnographic realities are identical, taking into account the specifics of the characteristics that are being stressed.

Thus my answer to my first question must be that the method of applying standard classification to the set of problems involved is inaccurate, leads to confusion, is significantly misleading. This is not arrogantly to proclaim that it has been unproductive. Without such attempts we would not know that, and furthermore the arguments have served to show that there are conditions and forces at work which the earliest writers were unable to identify.

What then can we do? Is there an alternative method, and might it be more helpful? Southall (1975) gives us a hint of what to me seems a most profitable direction. He is looking at the kinds of cities in Africa and inescapably notes their enormous diversity, particularly if we include some of the very large population cen-

tres which have existed for centuries, without "benefit" of modern commerce or industrialisation. Seeking a method which will serve to explain the differences, particularly with regard to relations with the "country" (i.e. countryside), he writes:

> «... the apparent diversity ... can be shown to vary according to quite intelligible, orderly and consistent principles if the relevant variables are carefully sorted out, thus demonstrating that apparently unlike situations arise mainly from the same sets of factors combined and operating at different strengths» (Southall 1975).

If such an approach were practicable, it would avoid the necessity for the kind of classification we have been dealing with, except as a matter of ready reference, without scientific potency. If we applied the above method to all phenomena which one way or another go under the rough name "town" or "city", we could even apply the same variables to units of population which were not ascribed in the same way, were in fact rural. It might be that some of the variables showed up as zero; this then would indicate a substantial difference by comparison with other units being compared. Exactly what we want. Yet not dependent upon arguable classification.

This does not predict that all problems are over. Not at all. Issues will be debated around the selection, strength, measurement, and so forth, of variables. The identification of variables is itself arguable classification, albeit of a different kind. The selection of variables, also, will differ according to differing objectives, especially in identifying what the research problem is. But it is my belief that the method is inherently clearer methodologically, and is more directly related to acts of explanation which should seek to correlate and account for linkages in the movements of variables.

At this point I shall take up the issue of boundaries again to illustrate my meaning, though not to assert that this is the only way of proceeding.

In anthropology the discussion of boundaries as natural phenomena or artifacts of the scholar has received relatively little theoretical attention. The existence of a boundary is usually assumed to follow empirically from the identification of a named group, or from the allocation of a name to a group. Thus "doctors", "the peasants of Ecuador", "the Fulani", or in this context "city dwellers", "marginals", "the urban poor", "*barrio* dwellers",

by classifying if for no other reason, imply a boundary of some kind. Yet paradoxically, although questions about interactions or the lack of them across the boundaries are of crucial importance for systemic assessment, more frequently than not identification of interactions is based upon general thoughts rather than upon the detailed analysis of boundaries as being at the heart of the matter.

There is, however, on approach in anthropology that has become a locus classicus, with good reason. Barth's well-known essay on the boundaries of ethnic groups (1969) essentially attacks the problem of boundary-maintenance as a self-conscious socio-political process, a problem which had of course long been postulated as being at the heart of ethnicity. For in the theory of ethnicity lies the proposition that an ethnic group identifies itself by opposition to all others in such a way that its maintenance requires social and cultural instruments for the maintenance of the boundary between "we" and "they". This is true, even though in some instances the boundaries may be fuzzy or open to change. Barth (1969: 15) writes:

> «The critical focus of investigation from this point of view becomes the ethnic boundary that defines the group, not the cultural stuff that it encloses».

But alas this is exactly what he unfortunately does not do. He, his colleagues who contribute to his symposium, and the many writers who have taken his position as an excellent summing up of the state of the art, but who wish to add amending perspectives, concentrate instead on an important but alternative question: how do ethnic groups so identified maintain the boundary (allowing for adaptation over time)? This is not the same question, and it is approached with in fact very little attention, if any, to the mechanisms at the boundary itself.

It is clear that this approach will not help in the present exercise, except peripherally. If we posit the existence of a boundary, we should surely give it our full attention, as the centre of study, not as an assumed condition. Obviously, significant studies of activities across boundaries are commonplace in the discipline, going back all the way to diffusionism, and covering such topics as migration, inter-marriage, trade, political domination and many other themes. What has not happened, however, has been a systematic linkage of boundary issues to issues in the theory of classification, with a bearing upon the way we conceive of systems and

systemic relations, and ultimately the way we compare social groups.

One approach I have advocated (Belshaw 1969, Ch. VII) is to start from the assumption that all social groups, whether defined arbitrarily or according to some empirical criterion, have boundaries physically defined by the observer, which may or may not coincide with a "natural" boundary as experienced and/or defined by members of the group in question. An absolute boundary, in socio-cultural terms, would be one with no movement whatsoever across it; only completely isolated populations would meet this condition. In all other situations there is some cross-boundary activity, some set of observable transactions. In fact, boundaries consist of sets of variable movements, that is movements which are variable for the same group over time, and variable as between groups which are being compared. It follows that it would be legitimate and profitable to do what Southall suggested above, that is to see groups, not as ones falling into classificatory boxes, but as units characterized by the strength accorded to each of a number of boundary variables, i.e. a boundary profile. In such an approach, there is no longer need to argue whether a particular group falls into classificatory box A or B. Each group remains its individual and unique self.

Yet this does not abjure comparison; indeed it assists through the comparison of methodically appraised variables. As Southall suggests, this is in turn an essential step in creating explanations, presumably through the logically supported correlations or lack of them in the movement of the variables. In my tentative approach mentioned above, I suggested that the following variables would be relevant: the degree to which social roles were completely or incompletely acted out within the boundary; the degree of corporate solidarity addressed to boundary maintenance (Barth's problem); the social direction expressed by the boundary (e.g. parallel or hierarchical segmentation of the society); parity or non-parity in power relationships manifested by boundary interaction; the intensity and frequency of communication across boundaries; the scale and rate of value transfers; the scale and rate of personnel transfer (including such considerations as temporary versus permanent, inter-marriage, occupation); activity transfer; transfers of resource control. Such a list was *a priori*, not informed by any specific research goal, and not clear in terms of operational definitions and clarity. That, in my view, is not a fundamental long-term objection to the method, but merely a weakness in my

own presentation which was exploratory rather than definitive (as indeed, is the present essay).

Thus, there is an alternative to the kind of classificatory approach to the issues of pluralism and comparison of relationships between sectors of urban (and other) populations. How might it be pursued further to test its efficacy? There are two steps to the answer. The first is to determine whether the concepts being debated in the conceptual literature, such as the material cited in this paper, can be transformed into statements of variation instead of classification, and what the implications of such a transformation might be. The second is to determine whether the rearranged ideas can be linked, say, to propositions about development which would suggest the possibility of operational research. In some instances it will be convenient to consider the two steps together.

For example, as I have indicated, running through the literature is the issue, are urban sectors, however defined, separate or interacting? As the discussion on Latin American squatter settlements indicates, the answers given by different authors are diametrically opposed, especially when some authors are guided by political motives. And the answers tend to place the phenomena in one classification or the other. The method of comparative variation at least avoids this trap. The boundaries between defined sectors may be examined according to the variables I have mentioned two paragraphs above, with a standardized indicator of scale. Obviously, there will need to be thought and ingenuity in arriving at such scales, some of which will be ordinal, a task which lies beyond the scope of this paper. The creation of such scales will enable comparisons to be made, possibly from already existing studies, in terms of the interactions between numerous *barrios, favelas, bidonvilles*, shanty-towns, "rural elements" in Asian cities, suburbs, slums, well-to-do ethnic enclaves, informal sectors, and any other units on the one hand, and the rest of the urban society on the other. It really does not matter, from the point of view of this application, how the sectors are identified; comparisons can still be made. Some interactions will be low with respect to some variables and high with respect to others. Each set of phenomena being examined will have its own characteristics, laying a foundation for the question why?

I have emphasized the question of boundary for several reasons, one of the most important of which is the underlying belief that the scale and nature of interactions across boundaries

will be fundamental to development propositions, if by development we mean an increase in institutional complexity. It can also be fundamental for other questions based on other criteria of development, such as increase in income *per capita*, growth in productivity, increase in the scale of trade and exchanges, and so forth. Once the criteria have been selected for practical or theoretical reasons, those criteria of outcome can be matched with patterns of variables on a comparative (historical and geographical) basis.

But clearly boundary is not the only issue of importance, since most of the studies cited above have given only cursory attention to it. The internal operation of sectors, again however identified, can be treated in a similar way, and made subject to comparison. This means the transformation of the criteria of classification into criteria for the selection of variables, usually merely a matter of the phrasing of the words. Thus, when Emerji (1974) contrasts structured with unstructured sectors, one needs to establish the ideas behind structure and non-structure, and to conceptualize them from non-existent or very small to very large and significant. Franklin's (1965) distinction between peasants using a total family labour supply and a sector in which persons use labour outside the family, always leads to such arguments as, «If a non-relative drops in to undertake labour on a reciprocal basis, which box does the system fit?». The problem is avoided and the variables clarified if there is a scale which could summarize continuously the following kinds of conditions (and others): labour literally confined to the household; labour confined to the household and other lineal relatives; labour confined to all named relatives (a larger pool); labour selected from relatives and persons accorded fictive status as relatives; labour using non-relatives but limited to those with formalized partnership status; labour including the former but also including very occasional wage work outside the kinship and partnership system; labour entirely dependent on workers offering themselves for wages. As another example marginality can be broken into sets of variables, such as comparative income, weight accorded to identified values (ideal or action-governing), degree of political participation, degree of non-monetary exchange, scale of monetary exchange, rate of capital investment in housing, *ad infinitum*. Marginality in itself then becomes perhaps unimportant as a classificatory concept. Instead, the "sectors" which are labelled marginal or non-marginal or indeterminate can be examined and compared through the use of the

variables. Nevertheless, the initial act of classification served the purpose of bringing into the discussion the enumeration of factors in numerous empirical instances, out of which the identification of possibly significant variables emerges.

This is no place to list the whole range of propositions in the study of development. An example must suffice. But before exemplifying, one must record that the very concept of development has itself become a classificatory tool with all the ambiguities, inconsistencies and confusions that seem almost inevitably to arise after twenty or thirty years of debate. Put simply, it means different things to different people. Once again the scientific instrument has been blunted, and needs re-shaping in terms of variables, this time variables of output or result. Thus we can talk of such matters as the rate of increase in the complexity of institutional arrangements; the rate of growth in GNP *per capita*; the rate of increase in the articulation of institutions – these as outcomes of sets of variables, some of which have to do with the observation that boundaries exist within cities.

To conclude, I pose as an example an arbitrarily chosen set of questions which the study of comparative variation might help to answer. If a high degree of cultural difference is maintained with relatively frequent communication of ideas and techniques across the boundaries, will the city exhibit a high rate of innovation? If there is a high rate of population recruitment (natural increase, immigration, boundary transfer) in a sector, what factors affect the rate of un- or under-employment (not necessarily in market terms)? Does the size of the pool of un- or under-employed affect the rate of formation of new enterprises or the rate of expansion of quantities of production in some or all inter-acting sectors? Does the answer to the last question vary according to the volume of production?

Although existing field studies in anthropology have not normally been designed to contribute to the comparative examination of the kinds of variables we are now imagining, it is my belief that they do in fact contain a great deal of appropriate data, enough at least to warrant experiments in designing scales and indicators. It is of course essential not only that the scales and indicators are appropriate to the theoretical requirements of the research questions, but that they are based on ethnographic reality. As anthropologists we would not wish to be trapped like our economist colleagues into according priority to variables simply because measures exist a priori or are technically comfortable. Nor should we

shy away from developing scales of qualitative phenomena on the grounds that such scales are apparently less than precise. Precision and refined measurement can be chimerically misleading.

Sommario

In questo saggio Belshaw riprende un tema già affrontato nel 1969, riguardante i limiti della classificazione negli studi comparativi. La classificazione è, secondo l'autore, un metodo poco chiaro e impreciso, è inoltre limitante perché non permette l'esame della forza delle variabili essenziale nell'affrontare i problemi relativi allo sviluppo. Per mostrare la validità delle sue critiche, Belshaw propone come esempio l'analisi delle economie "parallele" e fissa come riferimento uno studio di V. Das del 1986 in cui l'autrice evidenzia l'importanza del settore da lei definito "informale" che esiste sempre in parallelo con economie capitalistiche di mercato o socialiste centralmente pianificate. Questo settore permette un esame comparativo delle forze che influenzano la totale *performance* dell'economia, della politica e della società, mentre la disamina della sola economia di mercato o pianificata può offrire una risposta parziale.

Questa attività che giace fuori dei sistemi dominanti e che Belshaw chiama "parallela", mentre in un primo momento veniva ritenuta subordinata alle attività capitaliste o socialiste, oggi da studi più recenti è considerata ad esse "intrecciata" a vari livelli.

L'attenzione rivolta al ruolo delle economie parallele emerge da un graduale riconoscimento che i modelli limitati alle relazioni capitaliste o socialiste nella loro pretesa di trattare fenomeni globali, peccano di etnocentrismo. Ritenere il capitalismo rappresentante di un unico modo culturale, significa tracciare un netto confine tra questo e altre culture.

Pur essendo stato ormai scoperto o riscoperto il settore parallelo, si tende sempre a ritenerlo operante con regole diverse. Una tale posizione è stata rinforzata da alcuni antropologi seguaci di Polanyi e dai sostantivisti che, opponendosi al riconoscimento di modelli universali di comportamento, vedono ciascuna cultura chiusa in un rigido confine. Ciò significa, secondo Belshaw, confondere una variazione culturale empirica con la presunta assenza di un processo universale: una confusione che rende lo studio del cambiamento e dello sviluppo quasi negabile.

Il concetto di confine, premessa o conseguenza della classificazione, è valido? Belshaw risponde citando studi, antropologici e non, su società urbane, intendendo dimostrare l'arbitrarietà dei criteri adottati dai vari autori per determinare se una attività o una sezione della popolazione può essere descritta come "informale" in contrasto con quella "formale". Mentre le classificazioni sono chiaramente dipendenti da qualche osservazione empirica, le caratteristiche dedotte dall'osservazione possono essere scelte arbitrariamente, dipendendo dagli scopi dell'osservatore. Cioè, secondo Belshaw, ciò che nel confronto fra due settori ogni autore vuol evidenziare come elemento determinante un netto dualismo, dipenderà dagli obiettivi scientifici della sua ricerca. Ciò spiega sia la quantità eccessiva e la diversità delle espressioni usate per specificare le caratteristiche di un settore in opposizione all'altro, sia il dibattito fra gli studiosi sulla ricerca di criteri per definizioni più concordi, per arrivare ad attribuire alle classificazioni un valore universalistico.

Come viene affrontato questo problema in ambiente più propriamente antropologico? Dagli studi esaminati da Belshaw risulta che gli antropologi hanno sviluppato una propria terminologia e quindi un altro insieme di concetti. Un esempio è la ricerca di una definizione dei concetti di "urbano" e di "marginale", e dei criteri per una precisa classificazione. Ma anche in questo caso i criteri sono numerosi e a volte inconsistenti, lasciando aperto e irrisolto il problema della loro scelta. Ed inoltre è comune a tutte le distinzioni, e quindi inerente al concetto di classificazione, l'esistenza di un confine, tra quelli che secondo qualche criterio sono raggruppati e gli altri. Inevitabilmente le classificazioni implicano la presenza di confini.

Le classificazioni antropologiche sono comunque sempre soggette al confronto con la realtà etnografica. La realtà urbana per esempio è quasi sempre *multi sub-cultural* o *multi-cultural*. Ma, dice il nostro autore, una cosa è mettere in risalto la cultura, i valori, il comportamento di ogni settore, altra cosa è creare rigidi confini tra gruppi o settori della popolazione.

Ci sono comunque degli antropologi che hanno mostrato che l'appartenenza a un settore (formale o informale) dipende da una scelta e che le persone possono muoversi da un settore all'altro (ciò che Belshaw chiama «transfer personale»). Rew (1974) ha dimostrato che questi transfer provocano alterazioni nelle norme e nei valori e che un individuo può muoversi durante la sua attività giornaliera da un settore all'altro, adottando il comportamento

corrispondente. Un esempio particolare è il sincretismo religioso che dimostra come gli individui possono muoversi tra due sistemi operanti in parallelo.

Riguardo al settore marginale, gli studiosi marxisti affermano che non può esistere in isolamento storico e sociale. Le città differenziate in classi possono infatti manifestare una relazione simbiotica fra le classi o i settori della popolazione.

Alle stesse conclusioni arrivano anche studiosi non marxisti, come Perlman (1976). Rimane comunque il fatto che studi basati sulle classificazioni che esaminano le interazioni fra i confini sono ancora troppo pochi.

Quindi, ricapitolando, per Belshaw la classificazione si dimostra un metodo inadeguato e poco chiaro prima di tutto perché le differenze ed i disaccordi fra gli autori nella scelta dei criteri dipendono dalla scelta di diversi obiettivi di ricerca, per cui si possono avere tanti concetti diversi quanti sono i ricercatori. Inoltre la condanna di un atto di classificazione, che si basa sulle premesse che ciò che è da classificare consiste di fenomeni identici, è resa più valida dal riconoscimento che poche realtà etnografiche sono identiche e dalla necessità di considerare invece proprio la specificità delle caratteristiche che si stanno evidenziando. Belshaw propone quindi un metodo alternativo.

L'approccio proposto parte del presupposto che tutti i gruppi sociali hanno confini fisicamente definiti dall'osservatore, che possono o non possono coincidere con un confine "naturale" definito dai membri del gruppo in questione. Solo popolazioni completamente isolate possono avere un confine assoluto in termini socioculturali. In tutte le altre situazioni esiste sempre qualche attività *cross-boundary*. In antropologia, sebbene la questione della interazione attraverso i confini sia di fondamentale importanza per una valutazione sistematica, non si fanno ancora dettagliate analisi del "confine". Una volta individuato, esso deve invece essere il centro del nostro studio. Southall (1975) di fronte alla diversità delle città dell'Africa, includendo anche i grossi centri che per secoli sono esistiti senza il "beneficio" del commercio moderno e dell'industrializzazione, cerca un metodo che serva a spiegare queste differenze, rivolgendosi soprattutto al diverso comportamento e al diverso tipo di rapporto con la "campagna". Se vengono selezionate attentamente le variabili più importanti, egli dice, si può dimostrare che la diversità varia secondo principi logici e che quindi situazioni apparentemente dissimili sorgono dallo stesso insieme di fattori combinati e operanti a diversi gradi di intensità. Un tale ap-

proccio eliminerebbe la necessità di ricorrere alla classificazione per evidenziare le differenze fra settori. Le differenze si evidenzierebbero applicando a settori diversi le stesse variabili.

Considerando i confini come variabili, nel tempo per lo stesso settore e in un confronto tra settori, si devono considerare i gruppi umani, o i vari settori, non come rinchiusi in scatole classificatorie, ma come unità, ciascuna caratterizzata dalla forza, maggiore o minore, di una o più variabili di confine (vedere nell'originale le variabili ritenute più importanti).

Non essendo compromessa la comparazione che può avvenire tra le variabili, il metodo di Belshaw può essere definito di "variazione comparativa". I confini tra settori possono essere esaminati secondo le variabili da lui menzionate, con un "indicatore standardizzato di scala" (*standardized indicator of scale*). La creazione di tali scale renderà possibili le comparazioni in termini delle interazioni, per esempio, tra i numerosi "elementi rurali", i settori informali o altre unità da una parte e il resto della società urbana dall'altra.

Una delle principali ragioni per cui Belshaw pone tanta attenzione sul problema del "confine", è che, secondo lui, la misura e la natura delle interazioni attraverso i confini sono fondamentali per i problemi dello sviluppo – inteso sia come accrescimento della complessità istituzionale, sia come aumento di reddito pro-capite, o crescita della produttività e degli scambi, ecc.

La trasformazione dei criteri di classificazione in criteri per la scelta di variabili, e la costruzione di scale e indicatori, che, oltre ad essere appropriati alle esigenze teoriche delle ricerche, siano basati sulle realtà etnografiche, portano la "variazione comparativa" ad essere l'unico metodo utile per rispondere a molti quesiti derivanti dal complesso problema dello sviluppo, e quindi utile per l'antropologia dello sviluppo.