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CONCEPT MISFORMATION IN COMPARATIVE POLITICS*

GIOVANNI SARTORI

University of Florence

"To have mastered 'theory' and 'method' is to have become a *conscious thinker*, a man at work and aware of the assumptions and implications of whatever he is about. To be mastered by 'method' or 'theory' is simply to be kept from working."¹ The sentence applies nicely to the present plight of political science. The profession as a whole oscillates between two unsound extremes. At the one end a large majority of political scientists qualify as pure and simple unconscious thinkers. At the other end a sophisticated minority qualify as overconscious thinkers, in the sense that their standards of method and theory are drawn from the physical, "paradigmatic" sciences.

The wide gap between the unconscious and the overconscious thinker is concealed by the growing sophistication of statistical and research techniques. Most of the literature introduced by the title "Methods" (in the social, behavioral or political sciences) actually deals with survey techniques and social statistics, and has little if

anything to share with the crucial concern of "methodology," which is a concern with the logical structure and procedure of scientific enquiry. In a very crucial sense there is no methodology without *logos*, without thinking about thinking. And if a firm distinction is drawn—as it should be—between methodology and technique, the latter is no substitute for the former. One may be a wonderful researcher and manipulator of data, and yet remain an unconscious thinker. The view presented in this article is, then, that the profession as a whole is grievously impaired by methodological unawareness. The more we advance technically, the more we leave a vast, uncharted territory behind our backs. And my underlying complaint is that political scientists eminently lack (with exceptions) a training in logic—indeed in elementary logic.

I stress "elementary" because I do not wish to encourage in the least the overconscious thinker, the man who refuses to discuss heat unless he is given a thermometer. My sympathy goes, instead, to the "conscious thinker," the man who realizes the limitations of not having a thermometer and still manages to say a great deal simply by saying hot and cold, warmer and cooler. Indeed I call upon the conscious thinker to steer a middle course between crude logical mishandling on the one hand, and logical perfectionism (and paralysis) on the other hand. Whether we realize it or not, we are still swimming in a sea of naivete. And the study of comparative politics is particularly vulnerable to, and illustrative of, this unfelicitous state of affairs.

I. THE TRAVELLING PROBLEM

Traditional, or the more traditional, type of political science inherited a vast array of concepts which had been previously defined and refined—for better and for worse—by generations

* An earlier draft, "Theory and Method in Comparative Politics," was submitted as a working paper to the IPSA Torino Round Table of September, 1969. I wish to thank, in this connection, the Agnelli Foundation which provided the grant for the Torino panel. I am particularly indebted to David Apter, Harry Eckstein, Carl J. Friedrich, Joseph LaPalombara, Felix Oppenheim and Fred W. Riggs for their critical comments. I am also very much obliged to the Concilium on International and Area Studies at Yale University, of which I was a fellow in 1966-67. This article is part of the work done under the auspices of the Concilium.

¹C. Wright Mills, "On Intellectual Craftsmanship," in Llewellyn Gross (ed.), *Symposium on Sociological Theory* (New York: Harper & Row, 1959) p. 27 (My emphasis).

of philosophers and political theorists. To some extent, therefore, the traditional political scientist could afford to be an "unconscious thinker"—the thinking had already been done for him. This is even more the case with the country-by-country legalistic institutional approach, which does not particularly require hard thinking.² However, the new political science engages in reconceptualization. And this is even more the case, necessarily, with the new comparative expansion of the discipline.³ There are many reasons for this *renovatio ab imis*.

One is the very "expansion on politics." To some extent politics results *objectively* bigger on account of the fact that the world is becoming more and more politicized (more participation, more mobilization, and in any case more state intervention in formerly non-governmental spheres). In no small measure, however, politics is *subjectively* bigger in that we have shifted the focus of attention both toward the periphery of politics (*vis-à-vis* the governmental process), and toward its input side. By now—as Macridis puts it—we study everything that is "potentially political."⁴ While this latter aspect of the expansion of politics is disturbing—it ultimately leads to the disappearance of politics—it is not a peculiar concern for comparative politics, in the sense that other segments of political science are equally and even more deeply affected.⁵

² This is by no means a criticism of a comparative item by item analysis, and even less of the "institutional-functional" approach. On the latter see the judicious remarks of Ralph Braibanti, "Comparative Political Analytics Reconsidered," *The Journal of Politics*, 30 (February 1968), 44-49.

³ For the various phases of the comparative approach see Eckstein's perceptive "Introduction," in H. Eckstein and D. E. Apter (eds.), *Comparative Politics* (Glencoe: Free Press, 1963).

⁴ "Comparative Politics and the Study of Government: The Search for Focus," *Comparative Politics*, (October 1968), p. 81.

⁵ On the "fallacy of inputism" see again the remarks of Roy C. Macridis, *loc. cit.*, pp. 84-87. In his words, "The state of the discipline can be summed up in one phrase: the gradual disappearance of the political." (p. 86). A cogent statement of the issue is Glenn D. Paige, "The Rediscovery of Politics," in J. D. Montgomery and W. I. Siffin (eds.), *Approaches to Development* (New York: McGraw Hill, 1966), p. 49 ff. My essay "From the Sociology of Politics to Political Sociology," in S. M. Lipset (ed.), *Politics and the Social Sciences* (New York: Oxford University Press, 1969), pp. 65-100, is also largely concerned with the fallacy of inputism viewed as a sociological reduction of politics.

Aside from the expansion of politics, a more specific source of conceptual and methodological challenge for comparative politics is what Braibanti calls the "lengthening spectrum of political systems."⁶ We are now engaged in world-wide, cross-area comparisons. And while there is an end to geographical size, there is apparently no end to the proliferation of political units. There were about 80 States in 1946; it is no wild guess that we may shortly arrive at 150. Still more important, the lengthening spectrum of political systems includes a variety of primitive, diffuse polities at very different stages of differentiation and consolidation.

Now, the wider the world under investigation, the more we need conceptual tools that are able to travel. It is equally clear that the pre-1950 vocabulary of politics was not devised for world-wide, cross-area travelling. On the other hand, and in spite of bold attempts at drastic terminological innovation,⁷ it is hard to see how Western scholars could radically depart from the political experience of the West, i.e., from the vocabulary of politics which has been developed over millennia on the basis of such experience. Therefore, the first question is: how far, and how, can we travel with the help of the available vocabulary of politics?

By and large, so far we have followed (more or less unwittingly) the line of least resistance: broaden the meaning—and thereby the range of application—of the conceptualizations at hand. That is to say, the larger the world, the more we have resorted to *conceptual stretching*, or conceptual straining, i.e., to vague, amorphous conceptualizations. To be sure, there is more to it. One may add, for instance, that conceptual stretching also represents a deliberate attempt to make our conceptualizations value free. Another concurrent explication is that conceptual straining is largely a "boomerang effect" of the developing areas, i.e., a feedback on the Western categories of the diffuse polities of the Third

⁶ "Comparative Political Analytics Reconsidered," *loc. cit.*, pp. 36-37.

⁷ The works of Fred W. Riggs are perhaps the best instance of such bold attempts. For a recent presentation see "The Comparison of Whole Political Systems," in R. T. Holt and J. E. Turner (eds.), *The Methodology of Comparative Research* (New York: Free Press, 1970), esp. pp. 95-115. While Riggs' innovative strategy has undeniable practical drawbacks, the criticism of Martin Landau ("A General Commentary," in Ralph Braibanti (ed.), *Political and Administrative Development* (Durham: Duke University Press, 1969), pp. 325-334.) appears somewhat unfair.

World.⁸ These considerations notwithstanding, conceptual stretching does represent, in comparative politics, the line of least resistance. And the net result of conceptual straining is that our gains in extensional coverage tend to be matched by losses in connotative precision. It appears that we can cover more—in travelling terms—only by saying less, and by saying less in a far less precise manner.

A major drawback of the comparative expansion of the discipline is, then, that it has been conducive to indefiniteness, to undelimited and largely undefined conceptualizations. We do need, ultimately, “universal” categories—concepts which are applicable to any time and place. But nothing is gained if our universals turn out to be “no difference” categories leading to pseudo-equivalences. And even though we need universals, they must be *empirical* universals, that is, categories which somehow are amenable, in spite of their all-embracing very abstract nature, to empirical testing. Instead we seem to verge on the edge of *philosophical* universals, understood—as Croce defines them—as concepts which are by definition *supra-empirical*.⁹

That the comparative expansion of the discipline would encounter the aforementioned stumbling block was only to be expected. It was easy to infer, that is, that conceptual stretching would produce indefiniteness and elusiveness, and that the more we climb toward high-flown universals, the more tenuous the link with the empirical evidence. It is pertinent to wonder, therefore, why the problem has seldom been squarely confronted.

Taking a step back, let us begin by asking whether it is really necessary to embark in hazardous world-wide comparisons. This question hinges, in turn, on the prior question, Why compare? The unconscious thinker does not ask himself why he is comparing; and this neglect goes to explain why so much comparative work provides extensions of knowledge, but hardly a strategy for acquiring and validating new knowledge. It is not intuitively evident that to compare is to control, and that the novelty, distinctiveness and importance of comparative politics consists of a systematic testing, against as many cases as possible, of sets of hypotheses, generalizations and laws of the “if . . . then”

type.¹⁰ But if comparative politics is conceived as a method of control, then its generalizations have to be checked against “all cases,” and therefore the enterprise must be—in principle—a global enterprise. So the reason for world-wide comparisons is not simply that we live in a wider world; it is also a methodological reason.

If two or more items are identical, we do not have a problem of comparability. On the other hand, if two or more items have nothing, or not enough in common, we rightly say that stones and rabbits cannot be compared. By and large, then, we obtain comparability when two or more items appear “similar enough,” that is, neither identical nor utterly different. But this assessment offers little positive guidance. The problem is often outflanked by saying that we make things comparable. In this perspective to compare is “to assimilate,” i.e., to discover deeper or fundamental similarities below the surface of secondary diversities. But this argument equally affords little mileage and conveys, moreover, the misleading suggestion that the trick resides in making the unlike look alike. Surely, then, we have here a major problem which cannot be disposed of with the argument that political theorists have performed decently with comparing since the time of Aristotle, and therefore that we should not get bogged by the question “What is comparable?” any more than our predecessors. This argument will not do on account of three differences.

In the first place if our predecessors were culture bound this implied that they travelled only as far as their personal knowledge allowed them to travel. In the second place, our predecessors hardly disposed of quantitative data and were not quantitatively oriented. Under both of these limitations they enjoyed the distinct advantage of having a substantive understanding of the things they were comparing. This is hardly possible on a world wide scale, and surely becomes impossible with the computer revolution. A few years ago Karl Deutsch predicted that by 1975 the informational requirements of political science would be satisfied by some “fifty million card-equivalents [of IBM standard cards] . . . and a total annual growth rate of perhaps as

¹⁰ For the comparative method as a “method of control” see especially Arend Lijphart, *Comparative Politics and the Comparative Method*, (mimeographed) paper presented at the Torino IPSA Round Table, September, 1969. According to Lijphart the comparative method is a “method of discovering empirical relationships among variables” (p. 2); and I fully concur, except that this definition can be entered only at a later stage of the argument.

⁸ On the boomerang effect of the developing areas more in the final section.

⁹ More precisely in B. Croce, *Logica come Scienza del Concetto Puro*, (Bari: Laterza, 1942), pp. 13-17, universals are defined *ultrarappresentativi*, as being above and beyond any conceivable empirical representability.

much as five million."¹¹ I find the estimate frightening, for computer technology and facilities are bound to flood us with masses of data for which no human mind can have any substantive grasp. But even if one shares the enthusiasm of Deutsch, it cannot be denied that we have here a gigantic, unprecedented problem.

In the third place, our predecessors were far from being as unguided as we are. They did not leave the decision about what was homogenous—i.e., comparable—and what was heterogeneous—i.e., non-comparable—to each man's genial insights. As indicated by the terminology, their comparisons applied to things belonging to "the same genus." That is to say, the background of comparability was established by the *per genus et differentiam* mode of analysis, i.e., by a taxonomical treatment. In this context, comparable means something which belongs to the same genus, species, or sub-species—in short to the same class. Hence the class provides the "similarity element" of comparability, while the "differences" enter as the species of a genus, or the sub-species of a species—and so forth, depending on how fine the analysis needs to be. However, and here is the rub, the taxonomical requisites of comparability are currently neglected, if not disowned.

We are now better equipped for a discussion of our initial query, namely, why the travelling problem of comparative politics has been met with the poor remedy of "conceptual stretching" instead of being squarely confronted. While there are many reasons for our neglect to attack the problem frontally, a major reason is that we have been swayed by the suggestion that our difficulties can be overcome by switching from "what is" questions to "how much" questions. The argument runs, roughly, as follows. As long as concepts point to differences of *kind*, i.e., as long as we pursue the either-or mode of analysis, we are in trouble; but if concepts are understood as a matter of more-or-less, i.e., as pointing to differences in *degree*, then our difficulties can be solved by measurement, and the real problem is precisely how to measure. Meanwhile—waiting for the measures—class concepts and taxonomies should be looked upon with suspicion (if not rejected), since they represent "an old fashioned logic of properties and attributes not well adapted to study quantities and relations."¹²

¹¹ "Recent Trends in Research Methods," in J. C. Charlesworth (ed.), *A Design for Political Science: Scope, Objectives and Methods* (Philadelphia: American Academy of Political and Social Science, 1966), p. 156.

¹² Carl F. Hempel, quoted in Don Martindale, "Sociological Theory and the Ideal Type," in

According to my previous analysis, a taxonomic unfolding represents a requisite condition for comparability, and indeed a background which becomes all the more important the less we can rely on a substantive familiarity with what is being compared. According to the foregoing argument, instead, quantification has no ills of its own; rather, it provides a remedy for the ills and inadequacies of the *per genus et differentiam* mode of analysis. My own view is that when we dismiss the so-called "old fashioned logic" we are plain wrong, and indeed the victims of poor logic—a view that I must now attempt to warrant.

II. QUANTIFICATION AND CLASSIFICATION

What is very confusing in this matter is the abuse of a quantitative idiom which is nothing but an idiom. All too often, that is, we speak of degrees and of measurement "not only without any actual measurements having been performed, but without any being projected, and even without any apparent awareness of what must be done before such measurements can be carried out."¹³ For instance, in most standard textbooks one finds that nominal scales are spoken of as "scales of measurement."¹⁴ But a nominal scale is nothing else than a qualitative classification, and I fail to understand what it is that a nominal scale does, or can, measure. To be sure classes can be given numbers; but this is simply a coding device for identifying items and has nothing to do with quantification. Likewise the incessant use of "it is a matter of degree" phraseology and of the "continuum" image leave us with qualitative-impressionistic statements which do not advance us by a hair's breadth toward quantification. In a similar vein we speak more and more of "variables" which are not variables in any proper sense, for they are not attributes permitting gradations and implying

Gross, *Symposium on Sociological Theory*, p. 87. Martindale aptly comments that "Hempel's judgments are made from the standpoint of the natural sciences." But the vein is not dissimilar when the statistically trained scholar argues that "whereas it is admittedly technically possible to think always in terms of attributes and dichotomies, one wonders how practical that is": Hubert M. Blalock, Jr., *Causal Inferences in Nonexperimental Research* (Chapel Hill: University of North Carolina Press, 1964, p. 32).

¹³ Abraham Kaplan, *The Conduct of Inquiry* (San Francisco: Chandler, 1964), p. 213.

¹⁴ Eg., L. Festinger and D. Katz (eds.), *Research Methods in the Behavioral Sciences* (New York: Dryden Press, 1953); and Selltitz, Jahoda et al., *Research Methods in Social Relations* (rev. ed., New York: Holt, Rinehart & Winston, 1959).

measurability. No harm necessarily follows if it pleases us to use the word variable as a synonym for the word concept; but we are only deluding ourselves if we really believe that by saying variable we *have* a variable.

All in all, coquetting (if not cheating) with a quantitative idiom grossly exaggerates the extent to which political science is currently amenable to quantification, and, still worse, obfuscates the very notion of quantification. The dividing line between the jargon and the substance of quantification can be drawn very simply: quantification begins with numbers, and when numbers are used in relation to their arithmetical properties. To understand, however, the multifaceted complexities of the notion beyond this dividing line is a far less simple matter. Nevertheless one may usefully distinguish—in spite of the close interconnections—among three broad areas of meaning and application, that is, between quantification as i) measurement, ii) statistical manipulation and, iii) formal mathematical treatment.

In political science we generally refer to the first meaning. That is to say, far more often than not the quantification of political science consists of (a) attaching numerical values to items (pure and simple measurement), (b) using numbers to indicate the rank order of items (ordinal scales) and (c) measuring differences or distances among items (interval scales).¹⁵

Beyond the stage of measurement we do own, in addition, powerful statistical techniques not only for protecting ourselves against sampling and measurement errors, but also for establishing significant relationships among variables. However, statistical processing enters the scene only when sufficient numbers have been pinned on sufficient items, and becomes central to the discipline only when we dispose of variables which measure things that are worth measuring. Both conditions—and especially the latter—are

¹⁵ There is some question as to whether it can really be held that ordinal scales are scales of measurement: most of our rank ordering occurs without having recourse to numerical values, and whenever we do assign numbers to our ordered categories, these numbers are arbitrary. However, there are good reasons for drawing the threshold of quantification between nominal and ordinal scales rather than between ordinal and interval scales. (See Edward R. Tufte, "Improving Data Analysis in Political Science," *World Politics*, 21 (July 1969), esp. p. 645.) On the other hand, even if the gap between ordinal scales and interval measurement is not as wide in practice as it is in theory, nonetheless from a mathematical point of view the interesting scales are the interval and even more, of course, the cardinal scales.

hard to meet.¹⁶ Indeed, a cross-examination of our statistical findings in terms of their theoretical significance—and/or of a "more relevant" political science—shows an impressive disproportion between bravura and relevance. Unfortunately, what makes a statistical treatment theoretically significant has nothing to do with statistics.

As for the ultimate stage of quantification—formal mathematical treatment—it is a fact that, so far, political science and mathematics have engaged only "in a sporadic conversation."¹⁷ It is equally a fact that we seldom, if ever, obtain isomorphic correspondences between empirical relations among things and formal relations among numbers.¹⁸ We may well disagree about future prospects,¹⁹ or as to whether it

¹⁶ Otherwise the comparative method would largely consist of the statistical method, for the latter surely is a stronger technique of control than the former. The difference and the connections are cogently discussed by Lijphart, "Comparative Politics and the Comparative Method," *op. cit.*

¹⁷ Oliver Benson, "The Mathematical Approach to Political Science," in J. C. Charlesworth (ed.), *Contemporary Political Analysis* (New York: Free Press, 1967), p. 132. The chapter usefully reviews the literature. For an introductory treatment see Hayward R. Alker, Jr., *Mathematics and Politics* (New York: Macmillan, 1965). An illuminating discussion on how quantification enters the various social sciences is in Daniel Lerner (ed.), *Quantity and Quality* (Glencoe: Free Press, 1961), *passim*.

¹⁸ A classic example is the (partial) mathematical translation of the theoretical system of *The Human Group* of George C. Homans by Herbert A. Simon, *Models of Man* (New York: Wiley, 1967), Chap. 7. No similar achievement exists in the political science field. To cite three significant instances, political science issues are eminently lacking in Kenneth J. Arrow, "Mathematical Models in the Social Sciences," in D. Lerner and H. D. Lasswell (eds.), *The Policy Sciences* (Stanford: Stanford University Press, 1951), Chap. 8; in the contributions collected in P. F. Lazarsfeld (ed.), *Mathematical Thinking in the Social Sciences* (Glencoe: Free Press, 1954); in J. G. Kemeny and J. L. Snell, *Mathematical Models in the Social Sciences* (Boston: Ginn, 1962).

¹⁹ Perhaps the mathematical leap of the discipline is just around the corner waiting for non-quantitative developments. If one is to judge, however, from the "mathematics of man" issue of the *International Social Science Bulletin* introduced by Claude Levi-Strauss (IV, 1954), this literature is very deceiving. More interesting is John G. Kemeny, "Mathematics without Numbers," in Lerner, *Quantity and Quality*, pp. 35–51; and the modal logic developed by the Bourbaki group,

makes sense to construct formalized systems of quantitatively well defined relationships (mathematical models) so long as we wander in a mist of qualitatively ill-defined concepts. If we are to learn, however, from the mathematical development of economics, the evidence is that it "always lagged behind its qualitative and conceptual improvement."²⁰ And my point is, precisely, that this is not a casual sequence. It is for a very good reason that the progress of quantification should lag—in whatever discipline—behind its qualitative and conceptual progress.

In this messy controversy about quantification and its bearing on standard logical rules we simply tend to forget that *concept formation stands prior to quantification*. The process of thinking inevitably begins with a qualitative (natural) language, no matter at which shore we shall subsequently land. Correlatively, there is no ultimate way of bypassing the fact that human understanding—the way in which our mind works—requires cut-off points which basically correspond (in spite of all subsequent refinements) to the slices into which a natural or qualitative language happens to be divided.

There is a fantastic lack of perspective in the argument that these cut-off points can be obtained via statistical processing, i.e., by letting the data themselves tell us where to draw them. For this argument applies only *within* the frame of conceptual mappings which have to tell us first of what reality is composed. Let it be stressed, therefore, that long before having data which can speak for themselves the fundamental articulation of language and of thinking is obtained logically—by cumulative conceptual refinement and chains of coordinated definitions—not by measurement. Measurement of what? We cannot measure unless we know first what it is that we are measuring. Nor can the degrees of something tell us what a thing is. As Lazarsfeld and Barton neatly phrase it, "before we can investigate the presence or absence of some attribute . . . or before we can rank objects or measure them in

Eléments de Mathématique, appearing periodically (Paris: Hermann). For a general treatment see J. G. Kemeny, J. L. Snell, G. L. Thompson, *Introduction to Finite Mathematics* (Englewood Cliffs: Prentice Hall, 1957).

²⁰ Joseph J. Spengler, "Quantification in Economics: Its History," in Lerner, *Quantity and Quality*, p. 176. Spengler equally points out that "the introduction of quantitative methods in economics did not result in striking discoveries" (*ibid.*). While formal economic theory is by now highly isomorphic with algebra, mathematical economics has added little to the predictive power of the discipline and one often has the impression that we are employing guns to kill mosquitos.

terms of some variable, *we must form the concept of that variable.*"²¹

The major premise is, then, that quantification enters the scene after, and only after, having formed the concept. The minor premise is that the "stuff" of quantification—the things underpinned by the numbers—cannot be provided by quantification itself. Hence the rules of concept formation are independent of, and cannot be derived from, the rules which govern the treatment of quantities and quantitative relations. Let us elaborate on this conclusion.

In the first place, if we never really have "how much" findings—in the sense that the prior question always is how much *in what*, in what conceptual container—it follows from this that how much quantitative findings are an internal element of "what is" qualitative questions: the claim that the latter should give way to the former cannot be sustained. It equally follows, in the second place, that "categoric concepts" of the either-or type cannot give way to "gradation concepts" of the more-than-less-than type.

What is usually lost sight of is that the either-or type of logic is the very logic of classification building. Classes are required to be mutually exclusive, i.e., class concepts represent characteristics which the object under consideration must either have or lack. Two items being compared must belong first to the same class, and either have or not have an attribute; and only if they have it, the two items can be matched in terms of which has it *more* or *less*. Hence the logic of gradation belongs to the logic of classification. More precisely put, the switch from classification to gradation basically consists of replacing the signs "same-different" with the signs "same-greater-lesser," i.e., consists of introducing a quantitative differentiation within a qualitative sameness (of attributes). Clearly, then, the sign "same" established by the logic of classification is the requisite condition of introducing the signs "plus-minus."

The retort tends to be that this is true only as long as we persist in thinking in terms of attributes and dichotomies. But this rejoinder misses the point that—aside from classifying—we dispose of no other unfolding technique. Indeed, the taxonomical exercise "unpacks" concepts, and plays a non-replaceable role in the process of thinking in that it decomposes mental compounds into orderly and manageable sets of component units. Let it be added that at no stage of the methodological argument does the taxonomical unpacking lose weight and impor-

²¹ "Qualitative Measurement in the Social Sciences: Classifications, Typologies and Indices," in D. Lerner and H. D. Lasswell (eds.), *The Policy Sciences*, *op. cit.*, p. 155 (my emphasis).

tance. As a matter of fact, the more we enter the stage of quantification, the more we need unidimensional scales and continua; and dichotomous categorizations serve precisely the purpose of establishing the ends, and thereby the uni-dimensionality, of each continuum.

Having disposed of the fuzziness brought about by the abuse of a quantitative idiom, attention should immediately be called to the fact-finding side of the coin. For my emphasis on concept formation should not be misunderstood to imply that my concern is more theoretical than empirical. This is not so, because the concepts of any social science are not only the elements of a theoretical system; they are equally, and just as much, data containers. Indeed data is information which is distributed in, and processed by, "conceptual containers." And since the non-experimental sciences basically depend on fact-finding, i.e., on reports about external (not laboratory) observables, the empirical question becomes what turns a concept into a valuable, indeed a valid, fact finding container.

The reply need not be far-fetched: the lower the discriminating power of a conceptual container, the more the facts are misgathered, i.e., the greater the misinformation. Conversely, the higher the discriminating power of a category, the better the information. Admittedly, in and by itself this reply is not very illuminating, for it only conveys the suggestion that for fact-finding purposes it is more profitable to exaggerate in over-differentiation than in over-assimilation. The point is, however, that what establishes, or helps establish, the discriminating power of a category is the taxonomical infolding. Since the logical requirement of a classification is that its classes should be mutually exclusive and jointly exhaustive, it follows from this that the taxonomical exercise supplies an orderly series of well sharpened categories, and thereby the basis for collecting adequately precise information. And this is indeed how we know whether, and to what extent, a concept has a fact-gathering validity.

Once again, then, it appears that we have started to run before having learned how to walk. Numbers must be attached—for our purposes—to "things," to facts. How are these things, or facts, identified and collected? Our ultimate ambition may well be to pass from a science "of species" to a science of "functional co-relations."²² The question is whether we are not repudiating a science of species in exchange for nothing. And it seems to me that premature haste combined with the abuse of a quantitative

idiom is largely responsible not only for the fact that much of our theorizing is muddled, but also for the fact that much of our research is trivial and wasteful.

Graduate students are being sent all over the world—as LaPalombara vividly puts it—on "indiscriminate fishing expeditions for data."²³ These fishing expeditions are "indiscriminate" in that they lack taxonomical backing; which is the same as saying that they are fishing expeditions without adequate nets. The researcher sets out with a "checklist" which is, at best, an imperfect net of his own. This may be an expeditious way of handling his private research problems, but remains a very inconvenient strategy from the angle of the additivity and the comparability of his findings. As a result, the joint enterprise of comparative politics is menaced by a growing potpourri of disparate, non-cumulative and—in the aggregate—misleading morass of information.

All in all, and regardless of whether we rely on quantitative data or on more qualitative information, in any case the problem is the same, namely, to construct fact-finding categories that own sufficient discriminating power.²⁴ If our data containers are blurred, we never know to what extent and on what grounds the "unlike" is made "alike." If so, quantitative analysis may well provide more misinformation than qualitative analysis, especially on account of the aggravating circumstance that quantitative misinformation can be used without any substantive knowledge of the phenomena under consideration.

To recapitulate and conclude, I have argued that the logic of either-or cannot be replaced by the logic of more-and-less. Actually the two logics are complementary, and each has a legitimate field of application. Correlatively, polar oppositions and dichotomous confrontations cannot be dismissed: they are a necessary step in the process of concept formation. Equally, impatience with classification is totally unjustified. Rather, we often confuse a mere enumera-

²² "Macrotheories and Microapplications in Comparative Politics," *Comparative Politics*, (October 1968), p. 66.

²⁴ It hardly needs to be emphasized that census data—and for that matter most of the data provided by external agencies—are gathered by conceptual containers which hopelessly lack discrimination. The question with our standard variables on literacy, urbanization, occupation, industrialization, and the like, is whether they really measure common underlying phenomena. It is pretty obvious that, across the world, they do not; and this quite aside from the reliability of the data gathering agencies.

²² Harold D. Lasswell and Abraham Kaplan, *Power and Society* (New Haven: Yale University Press, 1950), pp. XVI-XVII.

tion (or checklist) with a classification, and many so called classifications fail to meet the minimal requirements for what they claim to be.

The overconscious thinker takes the view that if the study of politics has to be a "science," then it has to be Newton (or from Newton all the way up to Hempel). But the experimental method is hardly within the reach of political science (beyond the format of small group experimentation) and the very extent to which we are systematically turning to the comparative method of verification points to the extent to which no stronger method—including the statistical method—is available. If so, our distinctive and major problems begin where the lesson of the more exact sciences leaves off. This is tantamount to saying that a wholesale acceptance of the logic and methodology of physics may well be self-defeating, and is surely of little use for our distinctive needs. In particular, and whatever their limits, classifications remain the requisite, if preliminary, condition for any scientific discourse. As Hempel himself concedes, classificatory concepts do lend themselves to the description of observational findings and to the formulation of initial, if crude, empirical generalizations.²⁵ Moreover, a classificatory activity remains the basic instrument for introducing analytical clarity in whatever we are discussing, and leads us to discuss one thing at a time and different things at different times. Finally, and especially, we need taxonomical networks for solving our fact-finding and fact-storing problems. No comparative science of politics is plausible—on a global scale—unless we can draw on extensive *information* which is sufficiently *precise* to be *meaningfully compared*. The requisite condition for this is an adequate, relatively stable and, thereby, *additive filing system*. Such a filing system no longer is a wild dream, thanks to computer technology and facilities—except for the paradoxical fact that the more we enter the computer age, the less our fact-finding and fact-storing methods abide by any logically standardized criterion. Therefore, my concern with taxonomies is also a concern with 1) the data side of the question, and 2) our failure to provide a filing system for computer exploitation. We *have* entered the computer age—but with feet of clay.

III. THE LADDER OF ABSTRACTION

If quantification cannot solve our problems, in that we cannot measure before conceptualizing, and if, on the other hand, "conceptual stretching" is dangerously conducive to the Hegelian

²⁵ *Fundamentals of Concept Formation in Empirical Science* (Chicago: University of Chicago Press, 1952), p. 54.

night in which all the cows look black (and eventually the milkman is taken for a cow), then the issue must be joined from its very beginning, that is, on the grounds of concept formation.

A few preliminary cautions should be entered. Things conceived or meaningfully perceived, i.e., concepts, are the central elements of propositions, and—depending on how they are named—provide in and by themselves guidelines of interpretation and observation. It should be understood, therefore, that I shall implicitly refer to the conceptual element problems which in a more extended treatment actually and properly belong to the rubric "propositions." By saying concept formation I implicitly point to a proposition-forming and problem-solving activity. It should also be understood, in the second place, that my focus will be on those concepts which are crucial to the discipline, that is, the concepts which Bendix describes as "generalizations in disguise."²⁶ In the third place, I propose to concentrate on the vertical members of a conceptual structure, that is, on 1) *observational terms*, and 2) the vertical disposition of such terms along a *ladder of abstraction*.

While the notion of abstraction ladder is related to the problem of the levels of analysis, the two things do not coincide. A highly abstract level of analysis may not result from "ladder climbing." Indeed a number of universal conceptualizations are not abstracted from observables: they are "theoretical terms" defined by their systemic meaning.²⁷ For instance the meaning of isomorphism, homeostasis, feedback, entropy, etc., is basically defined by the part that each concept plays in the whole theory. In other instances, however, we deal with "observational terms," that is, we arrive at highly ab-

²⁶ Reinhard Bendix, "Concepts and Generalizations in Comparative Sociological Studies," *American Sociological Review*, 28 (1963), p. 533.

²⁷ See Abraham Kaplan, *The Conduct of Inquiry*, pp. 56-57, 63-65. According to Hempel theoretical terms "usually purport to not directly observable entities and their characteristics. . . . They function . . . in scientific theories intended to explain generalizations": "The Theoretician's Dilemma," in Feigl, Scriven and Maxwell (eds.), *Minnesota Studies in the Philosophy of Science* (Minneapolis: University of Minnesota Press, 1958), vol. II, p. 42. While it is admittedly difficult to draw a neat division between theoretical and observational terms, it is widely recognized that the former cannot be reduced to, nor derived from, the latter. For a recent assessment of the controversy, see A. Meotti, 'L'Eliminazione dei Termini Teorici,' in *Rivista di Filosofia*, 2 (1969), pp. 119-134.

stract levels of conceptualization via ladder climbing, via abstractive inferences from observables. For instance, terms such as group, communication, conflict, and decision can either be used in a very abstract or in a very concrete meaning, either in some very distant relation to observables or with reference to direct observations. In this case we have, then, "empirical concepts" which can be located at, and moved along, very different points of a ladder of abstraction. If so, we have the problem of assessing the level of abstraction at which observational or (in this sense) empirical concepts are located, and the rules of transformation thus resulting. And this seems to be the pertinent focus for the issue under consideration, for our fundamental problem is how to make extensional gains (by climbing the abstraction ladder) without having to suffer unnecessary losses in precision and empirical testability.

The problem can be neatly underpinned with reference to the distinction, and relation, between the *extension* (denotation) and *intension* (connotation) of a term. A standard definition is as follows: "The extension of a word is the class of *things* to which the word applies; the intension of a word is the collection of *properties* which determine the things to which the word applies."²⁸ Likewise, the denotation of a word is the totality of objects indicated by that word; and the connotation is the totality of characteristics anything must possess to be in the denotation of that word.²⁹

Now, there are apparently two ways of climbing a ladder of abstraction. One is to broaden the extension of a concept by diminishing its attributes or properties, i.e., by reducing its connotation. In this case a more "general," or more inclusive, concept can be obtained without any loss of precision. The larger the class, the lesser its differentiae; but those differentiae that remain, remain precise. Moreover, following this procedure we obtain conceptualizations which, no matter how all-embracing, still bear a traceable relation to a collection of specifics, and—out of being amenable to identifiable sets of specifics—lend themselves to empirical testing.

On the other hand, this is hardly the procedure implied by "conceptual stretching," which adds up to being an attempt to augment the ex-

ension without diminishing the intension: *the denotation is extended by obfuscating the connotation*. As a result we do not obtain a more general concept, but its counterfeit, a mere generality (where the pejorative "mere" is meant to restore the distinction between correct and incorrect ways of subsuming a term under a broader genus.) While a general concept can be said to represent a collection of specifics, a mere generality cannot be underpinned, out of its indefiniteness, by specifics. And while a general concept is conducive to scientific "generalizations," mere generalities are conducive only to vagueness and conceptual obscurity.

The rules for climbing and descending along a ladder of abstraction are thus very simple rules—in principle. We make a concept more abstract and more general by lessening its properties or attributes. Conversely, a concept is specified by the addition (or unfolding) of qualifications, i.e., by augmenting its attributes or properties. If so, let us pass on to consider a ladder of abstraction as such. It is self-evident that along the abstraction ladder one obtains very different degrees of inclusiveness and, conversely, specificity. These differences can be usefully underpinned—for the purposes of comparative politics—by distinguishing three levels of abstraction, labeled, in shorthand, HL (high level), ML (medium level), and LL (low level).

High level categorizations obtain universal conceptualizations: whatever connotation is sacrificed to the requirement of global denotation—either in space, time, or even both.³⁰ HL concepts can also be visualized as the ultimate genus which cancels all its species. Descending a step, medium level categorizations fall short of universality and thus can be said to obtain general classes: at this level not all differentiae are sacrificed to extensional requirements. Nonetheless, ML concepts are intended to stress similarities at the expense of uniqueness, for at this level of abstraction we are typically dealing with generalizations. Finally, low level categories obtain specific, indeed configurative conceptualizations: here denotation is sacrificed to accuracy of connotation. One may equally say that with LL categories the differentiae of individual settings are stressed above their similarities: so much so that at this level definitions are often contextual.

A couple of examples may be usefully entered. In a perceptive essay which runs parallel to my

³⁰ The space and time dimensions of concepts are often associated with the geography versus history debate. I would rather see it as the "when goes with when?" question, that is, as a calendar time versus historical time dilemma. But this line of development cannot be pursued here.

²⁸ I quote from Wesley C. Salmon, *Logic* (Englewood Cliffs: Prentice-Hall, 1963), pp. 90–91. The distinction is more or less the same in any textbook of logic.

²⁹ "Connotation" is also applied, more broadly, to the associations, or associated conceptions brought to mind by the use of a word. As indicated by the text, I intend here the narrower meaning.

line of thinking Neil J. Smelser makes the point that, for purposes of comparability, "staff is more satisfactory than administration . . . , and administration is more satisfactory than civil service."³¹ This is so, according to Smelser, because the concept of civil service "is literally useless in connection with societies without a formal state or governmental apparatus." In this respect "the concept of administration is somewhat superior . . . but even this term is quite culture-bound." Hence the more helpful term is "Weber's concept of staff . . . since it can encompass without embarrassment various political arrangements . . ."³² In my own terms the argument would be rephrased as follows. In the field of so-called comparative public administration, "staff" is the high level universal category. "Administration" is still a good travelling category, but falls short of universal applicability in that it retains some of the attributes associated with the more specific notion of "bureaucracy." Descending the ladder of abstraction further we then find "civil service," which is qualified by its associations with the modern State. Finally, and to pursue the argument all the way down to the low level of abstraction, a comparative study of, say, French and English state employees will discover their unique and distinguishing traits and would thus provide contextual definitions.

The example suggested by Smelser is fortunate in that we are offered a choice of terms, so that (whatever the choice) a different level of abstraction can be identified by a different denomination. The next example is illustrative, instead, of the far less fortunate situation in which we may have to perform across the whole ladder of abstraction with one and same term. In illustrating his caution that many concepts are "generalizations in disguise," Bendix comes across such a simple concept as "village." Yet he notes that the term village may be misleading when applied to Indian society, where "the minimum degree of cohesion commonly associated with this term is absent."³³ Even in such a simple case, then, a scholar is required to place the various associations of "village" along an abstraction ladder in accord with the travelling extension afforded by each connotation.

Clearly, there is no hard and fast dividing line between levels of abstraction. Borders can only be drawn very loosely; and the number of slices

³¹ "Notes on the Methodology of Comparative Analysis of Economic Activity," *Transactions of the Sixth World Congress of Sociology*, 1967, International Sociological Association, vol. II, p. 103.

³² *Ibid.*

³³ Bendix, "Concepts and Generalizations. . . ," p. 536.

into which the ladder is divided largely depends on how fine one's analysis needs to be. Three slices are sufficient, however, for the purposes of logical analysis. And my major concern is, in this connection, with what goes on at the upper end of the ladder, at the crucial juncture at which we cross the border between medium level general concepts and high level universals. The issue may be formulated as follows: how far up can an observational term be pushed without self-denying results?

In principle the extension of a concept should not be broadened beyond the point at which at least one relatively precise connotation (property or attribute) is retained. In practice, however, the requirement of positive identification may be too exacting. But even if no minimal positive identification can be afforded, I do not see how we can renounce the requirement of negative identification. The crucial distinction would thus be between 1) concepts defined by negation or *ex adverso*, i.e., by saying what they are *not*, and 2) concepts *without negation*, i.e., no-opposite concepts, conceptions without specified termination or boundaries. The logical principle involved in this distinction is *omnis determinatio est negatio*, that is, any determination involves a negation. According to this principle the former concepts are, no matter how broad, *determinate*; whereas the latter are indeterminate, literally *without termination*.

If this principle is applied to the climbing process along a ladder of abstraction, and precisely to the point at which ML categories are turned into HL universals, in the first instance we obtain *empirical universals*, whereas in the second instance we obtain universals which lack empirical value—*pseudo-universals* for an empirical science. The reason for this is that a concept qualified by a negation may, or may not, be found to apply to the real world; whereas a non-bounded concept always applies by definition: having no specified termination, there is no way of ascertaining whether it applies to the real world or not. An empirical universal is such because it still points to *something*; whereas a non-empirical universal indiscriminately points to *everything* (as any researcher on the field soon discovers).

The group concept lends itself nicely as an illustration of the foregoing (other examples will be discussed in greater detail later), and is very much to the point in that it represents the first large scale attempt to meet the travelling problem of comparative politics. In the group theory of politics (Bentley, David Truman, and Earl Lathan being the obvious references) it is clear enough that "group" becomes an all-embracing category: not only an analytical construct (as the

queer and unclear terminology of the discipline would have it), but definitely a universal construct. However, we are never really told what group is *not*. Not only "group" applies *everywhere*, as any universal should; it equally applies to *everything*, that is, never and nowhere shall we encounter non-groups.³⁴ If so, how is it that the group theory of politics has been followed—in the fifties—by a great deal of empirical research? The reply is that the research was not guided by the universal construct but by intuitive concrete conceptualizations. Hence the "indefinite group" of the theory, and the "concrete groups" of the research, fall wide apart. The unfortunate consequences are not only that the research lacks theoretical backing (for want of medium level categories, and especially of a taxonomic framework), but that the vagueness of the theory has no fit for the specificity of the findings. We are thus left with a body of literature that gives the frustrating feeling of dismantling theoretically whatever it discovers empirically.

There is, then, a break-off point in the search for universal inclusiveness beyond which we have, theoretically, a "nullification of the problem" and, empirically, what may be called an "empirical vaporization." This is the point at which a concept is not even determined *ex adverso*. By saying that no-opposite universals are of no empirical use I do not imply that they are utterly useless. But I do wish to say that whenever notions such as groups or—as in my subsequent examples—pluralism, integration, participation, and mobilization, obtain no termination, i.e., remain indeterminate, they provide only tags, chapter headings, i.e., the main entries of a filing system. From an empirical point of view pseudo-universals are only funnels of approach and can only perform, so to speak, an allusive function.

Turning to the middle slice—the fat slice of the medium level categories—it will suffice to note that at this level we are required to perform the whole set of operations that some authors call "definition by analysis," that is, the process of defining a term by finding the genus to which the object designated by the word belongs, and then specifying the attributes which

distinguish such object from all the other species of the same genus. When Apter complains that our "analytical categories are too general when they are theoretical, and too descriptive where they are not,"³⁵ I understand this complaint to apply to our disorderly leaps from observational findings all the way up to universal categories—and vice versa—by-passing as it were the stage of definition by analysis. Apter is quite right in pleading for "better intermediate analytical categories." But these intermediate categories cannot be constructed, I fear, as long as our contempt for the taxonomical exercise leaves us with an atrophied medium level of abstraction.

The low level of abstraction may appear uninteresting to the comparative scholar. He would be wrong, however, on two counts. First, when the comparative scholar is engaged in field work, the more his fact-finding categories are brought down to this level, the better his research. Second, it is the evidence obtained nation-by-nation, or region-by-region (or whatever the unit of analysis may be) that helps us decide which classification works, or which new criterion of classification should be developed.

While classifying must abide by logical rules, logic has nothing to do with the usefulness of a classificatory system. Botanists, mineralogists and zoologists have not created their taxonomical trees as a matter of mere logical unfolding; that is, they have not imposed their "classes" upon their animals, any more than their animals (flowers or minerals) have imposed themselves upon their classifiers. Let it be added that the information requirements of such an unsettled science as a science of politics can hardly be satisfied by single-purpose classifications (not to mention single-purpose checklists). As I have stressed, we desperately need standard fact-finding and fact-storing containers (concepts). But this standardization is only possible and fruitful on the basis of "multi-purpose" and, at the limit, all-purpose classifications. Now, whether a classification may serve multiple purposes, and which classification fits this requirement best, this is something we discover inductively, that is, starting from the bottom of the ladder of abstraction.

The overall discussion is recapitulated in Table 1 with respect to its bearing on the problems of comparative politics. A few additional comments are in order. In the first place, reference to three levels of abstraction brings out the inadequacy of merely distinguishing between

³⁴ This criticism is perhaps unfair to David Truman's *The Governmental Process* (New York: Knopf, 1951). However, in spite of its penetrating anatomy the pace of the enquiry is set by the sentence that "an excessive preoccupation with definition will only prove a handicap" (p. 23). For a development of this line of criticism see G. Sartori, "Gruppi di Pressione o Gruppi di Interesse?," *Il Mulino*, 1959, pp. 7-42.

³⁵ David E. Apter, "Political Studies and the Search for a Framework," (pp. 15-16 mns.) to be published in C. Allen, W. Johnson (eds.), *African Perspectives*, Cambridge University Press.

TABLE I. LADDER OF ABSTRACTION

Levels of Abstraction	Major Comparative Scope and Purpose	Logical and Empirical Properties of Concepts
HL: <i>High Level Categories</i> Universal conceptualizations	Cross-area comparisons among heterogeneous contexts (global theory)	Maximal extension Minimal intension Definition by negation
ML: <i>Medium Level Categories</i> General conceptualizations and taxonomies	Intra-area comparisons among relatively homogeneous contexts (middle range theory)	Balance of denotation with connotation Definition by analysis, i.e. per genus et differentiam
LL: <i>Low Level Categories</i> Configurative conceptualizations	Country by country analysis (narrow-gauge theory)	Maximal intension Minimal extension Contextual definition

"broad" and "narrow" meanings of a term.³⁶ For this does not clarify, whenever this is necessary, whether we distinguish, 1) between HL universal and ML general conceptualizations, or 2) between ML genres and species or, 3) between ML and LL categories, or even 4) between HL universal and LL configurative conceptualizations.

In the second place, and more important, reference to the ladder of abstraction forcibly highlights the drastic loss of logical articulation, indeed the gigantic leap, implied by the argument that *all* differences are "a matter of degree." This cannot be conceded, to begin with, at the level of universal categories. But all differences cannot be considered a matter of more-or-less at the medium level either. At the top we inevitably begin with opposite pairs, with polar opposites, and this is tantamount to saying that the top ML categories definitely and only establish differences in *kind*. From here downwards definitions are obtained via the logic of classification, and this implies that a logic of gradation cannot be applied as long as we establish differences between species. Differences in degree obtain only after having established that two or more objects have the same attributes or properties, i.e., belong to the same species. Indeed, it is only *within* the same class that we are entitled—and indeed required—to ask which object has more or less of an attribute or property.

In principle, then, it is a fallacy to apply the logic of gradation whenever ladder climbing (or descending) is involved. If we are reminded that along the ladder we augment the extension by diminishing the denotation (and vice versa), what is at stake here is the presence or absence of a given property; and this is not a matter of

degree, but a matter of establishing the level of abstraction. Hence it is only after having settled at a given level of abstraction that considerations of more-and-less correctly apply. And the rule of thumb seems to be that the higher the level of abstraction, the less a degree language applies (as anything but a metaphor); whereas the lower level of abstraction, the more a degree optics correctly and necessarily applies, and the more we profit from graduation concepts.

In the third place, and equally important, reference to the ladder of abstraction casts many doubts on the optimistic view—largely shared by the methodological literature—that "The more universal a proposition, i.e., the greater the number of events a proposition accounts for, the more potential falsifiers can be found, and the more informative is the proposition."³⁷ The sentence suggests a simultaneous and somewhat natural progression of universality, falsifiers and informative content. It seems to me, instead, that reference to the correct technique of ladder climbing (and descending) confronts us at all points with choosing between range of explanation (thereby including the explanation of the relationships among the items under investigation), and accuracy of description (or informative accuracy). By saying that the "informative content" of a proposition grows by climbing the abstraction ladder, we should not be misled into understanding that we are supplying more descriptive information. Hence it is dubious whether we are really supplying more potential falsifiers (let alone the danger of "overly universal" propositions of no informative value for which falsifiers cannot be found).

Before concluding it should not pass unno-

³⁶ The same caution applies to the distinctions between micro and macro, or between molecular and molar. These distinctions are insufficient for the purpose of underpinning the level of analysis.

³⁷ I quote Erik Allardt, "The Merger of American and European Traditions of Sociological Research: Contextual Analysis," *Social Science Information*, 1 (1968), p. 165. But the sentence is illustrative of a current mood.

ticed that in this section I have never used the word "variable," nor mentioned operational definitions, nor invoked indicators. Equally, my reference to gradation concepts and to considerations of more-or-less has been, so far, entirely pre-quantitative. What is noteworthy, then, is the length that has been travelled before entering the problems which seem to monopolize our methodological awareness. There is nothing wrong, to be sure, in taking up an argument at whichever point we feel that we have something to say—except that the tail of the methodological argument should not be mistaken for its beginning. Since I have taken up the issue at an early stage, I cannot possibly carry it through to its end. It behooves me, nonetheless, to indicate how I would plug what I have said into what shall have to remain unsaid.³⁸

For one thing, it should be understood that by considering concepts—the genus—I have not excluded the consideration of variables, which are a species. That is, a variable is still a concept; but a concept is not necessarily a variable. If all concepts could be turned into variables, the difference could be considered provisional. Unfortunately, as a scholar well versed in quantitative analysis puts it, "all the most interesting variables are nominal."³⁹ Which is the same as saying that all the most interesting concepts are not variables in the proper, strict sense of implying "the possibility of measurement in the most exact sense of the word."⁴⁰

A closely linked and similar argument applies to the operationist requirement. Just as concepts are not necessarily variables, definitions are not necessarily operational. The definitional requirement for a concept is that its *meaning* is declared, while operational definitions are required to state the conditions, indeed the operations, by means of which a concept can be *verified* and, ultimately, measured. Accordingly we may usefully distinguish between definition of meaning and operational definition. And while it is obvious that an operational definition still is a declaration of meaning, the reverse is not true.

³⁸ In this latter connection an excellent reader still is P. F. Lazarsfeld and M. Rosenberg (eds.), *The Language of Social Research* (Glencoe: The Free Press, 1955). See also its largely revised and updated revision, R. Boudon and P. F. Lazarsfeld, *Méthodes de la Sociologie*, 2 Vols. (Paris and La Haye: Mouton, 1965–1966).

³⁹ Richard Rose, "Social Measure and Public Policy in Britain—The Empiricizing Process," *mns.* p. 8.

⁴⁰ Lazarsfeld and Barton in Lerner and Lasswell, *The Policy Sciences*, p. 170. This notably excludes, for the authors, the application of "variable" to items that can be ranked but not measured.

The contention often is that definition of meaning represents a pre-scientific age of definition, which should be superseded in scientific discourse by operational definitions. However, this contention can hardly meet the problems of concept formation, and indeed appears to ignore them. As the ladder of abstraction scheme helps to underline, among the many possible ways and procedures of defining the *ex adverso* definitions and taxonomic unfoldings (or definition by analysis) some correspond to different levels of analysis and play, at each level, a non-replaceable role. Moreover operational definitions generally entail a drastic curtailment of meaning for they can only maintain those meanings that comply with the operationist requirement. Now, we are surely required to reduce ambiguity by cutting down the range of meanings of concepts. But the operational criterion of reducing ambiguity entails drastic losses in conceptual richness and in explanatory power. Take, for instance, the suggestion that "social class" should be dismissed and replaced by a set of operational statements relating to income, occupation, educational level, etc. If the suggestion were adopted wholesale, the loss of conceptual substance would be not only considerable, but unjustified. The same applies, to cite another instance, to "power." To be concerned with the measurement of power does not imply that the meaning of the concept should be reduced to what can be measured about power—the latter view would make human behavior in whatever collective sphere almost inexplicable.

It should be understood, therefore, that operational definitions implement, but do not replace, definitions of meaning. Indeed there must be a conceptualization before we engage in operationalization. As Hempel recommends, operational definitions should not be "emphasized to the neglect of the requirement of systematic import."⁴¹ This is also to say that definitions of meaning of theoretical import, hardly operational definitions, account for the dynamics of intellectual discovery and stimulation. Finally, it should be understood that empirical testing occurs before, and also without, operational definitions. Testing is any method of checking correspondence with reality by the use of pertinent observations; hence the decisive difference brought

⁴¹ *Fundamentals of Concept Formation in Empirical Science*, p. 60. At p. 47 Hempel writes: "it is precisely the discovery of concepts with theoretical import which advances scientific understanding; and such discovery requires scientific inventiveness and cannot be replaced by the—certainly indispensable, but also definitely insufficient—operationist or empiricist requirement of empirical import alone."

about by operationalization is verification, or falsification, by measurement.⁴²

Speaking of testing, indicators are indeed precious "testing helpers." As a matter of fact it is difficult to see how theoretical terms could be empiricized and tested otherwise, that is, without having recourse to indicators. Indicators are also expedient shortcuts for the empirical checking of observational terms. Yet the question remains: Indicators of what? If we have fuzzy concepts, the fuzziness will remain as it is. That is to say that indicators cannot, in and by themselves, sharpen our concepts and relieve us from composing and decomposing them along a ladder of abstraction.

IV. COMPARATIVE FALLACIES: AN ILLUSTRATION

We may now confront in more detail how the ladder of abstraction scheme brings out the snares and the faults of our current way of handling the travelling problem of comparative politics. For we may now settle at a less rarified level of discussion and proceed on the basis of examples. It is pretty obvious that my line of analysis largely cuts across the various theories and schools that propose themselves for adoption in comparative politics, for my basic preoccupation is with the ongoing work of the "normal science," i.e., with the common conceptual problems of the discipline. Nonetheless it will be useful to enter here a somewhat self-contained illustration which bears not only on discrete concepts, but equally on a theoretical framework. I have thus selected for my first detailed discussion the categories of "structure" and "function," and this precisely on account of their crucial role in establishing the structural-functional approach in the political science setting.⁴³

In introducing his pioneering comparative volume, Almond boldly asserts: "What we have

⁴² This is not to say that operationalization allows *eo ipso* for quantitative measurements, but to suggest that either operational definitions are ultimately conducive to measurement, or may not be worthwhile.

⁴³ I specify political science setting to avoid the unnecessary regression to Malinowski and Radcliff-Brown. This is also to explain why I set aside the contributions of Talcott Parsons and of Marion J. Levy. Flanigan and Fogelman distinguish between three major streams, labeled 1) eclectic functionalism, 2) empirical functionalism (Merton) and 3) structural-functional analysis. ("Functional Analysis," in Charlesworth, *Contemporary Political Analysis*, pp. 72-79). My discussion exclusively applies to part of the latter.

done is to separate political function from political structure."⁴⁴ This separation is indeed crucial. But ten years have gone by and the assignment remains largely unfulfilled. Indeed the structural-functional school of thought is still grappling—with clear symptoms of frustration—with the preliminary difficulty of defining "function"—both taken by itself and in its relation to "structure."⁴⁵

Whether function can be simply conceived as an "activity" performed by structures; or whether it is more proper to construe function as an "effect";⁴⁶ or whether function should be conceived only as a "relation" among structures⁴⁷—this controversy turns out to be largely immaterial in the light of our substantive performance. That is to say, if our attention turns to the functional vocabulary in actual use, a perusal of the literature quickly reveals two

⁴⁴ Gabriel A. Almond and James S. Coleman, *The Politics of the Developing Areas* (Princeton: Princeton University Press, 1960), p. 59.

⁴⁵ It should be understood that by now the structural-functional label applies to a widely scattered group operating on premises which are largely at variance.

⁴⁶ This focus was suggested by R. K. Merton, whose concern was to separate function—defined as an "observable objective consequence"—from "subjective disposition," i.e., aims, motives and purposes (*Social Theory and Social Structure*, Glencoe: The Free Press, rev. ed., 1957, p. 24 and, *passim*, pp. 19-84.) In attempting to meet the difficulties raised by the Mertonian focus, Robert T. Holt construes functions as "sub-types" of effects, and precisely as the "system-relevant effects of structures"; understanding system-relevance as the "system-requiredness" which is determined, in turn, by the "functional requisites" of a given system. ("A Proposed Structural-Functional Framework," in Charlesworth, *Contemporary Political Analysis*, pp. 88-90). My own position is that Merton overstated his case thereby creating for his followers unnecessary and unsettled complications.

⁴⁷ This is the mathematical meaning of function. E.G. according to Fred W. Riggs in systems theory function refers to "a relation between structures." ("Some Problems with Systems Theory—The Importance of Structure," mimeographed p. 8. A redrafted version is scheduled for publication in Michael Haas and Henry Kariel (eds.), *Approaches to the Study of Political Science*, (Chandler Publishing Co.) There are problems, however, also with this definition. In particular, while the mathematical meaning of function is suited for whole systems analysis, it hardly suits the needs of segmented systems analysis.

things: a tantalizing anarchy (on this more later), and, second, that the functional terminology employed most of the time by most practitioners definitely carries a purposive or teleological connotation. Skillful verbal camouflage may well push the teleological implication in the background. Yet it is hard to find a functional argumentation which really escapes, in the final analysis, *Zweckrationalität*, what Max Weber called rationality of ends.⁴⁸ We may well quarrel about the definition;⁴⁹ yet the substance of the matter remains that the definitional controversy has little bearing on our subsequent proceedings. If so, it suits my purposes to settle for the way in which most people use "function" in practice (regardless of how they theorize about it), and thereby to settle for the common sense, unsophisticated meaning.

When we say, somewhat naively, that structures "have functions," we are interested in the reason for being of structures: we are implying, that is, that structures exist *for* some end, purpose, destination or assignment.⁵⁰ This is tantamount to saying that "function" points to a means-end relationship (which becomes, from a systemic viewpoint, also a part-whole relationship), i.e., that function is the activity performed by a structure—the means—vis-à-vis its

⁴⁸ Rationality of ends should not be confused with *Wertrationalität*, value rationality, among other reasons because in the former perspective all conceivable ends can be hypothesized as being equally valid. Hence in the *Zweckrationalität* perspective there is little point in unmasking functions as "eu-functions" or, conversely, as "caco-functions." Whether the good goals of one man are the bad goals of the next man becomes relevant only if we enter a normative, *Wertrationalität* discussion.

⁴⁹ For the many additional intricacies of the subject that I must neglect, a recent, interesting reader largely focussed on the "debate over functionalism" is N. J. Demerath and R. A. Peterson (eds.), *System, Change and Conflict* (New York: Free Press, 1967). For a critical statement of the inherent limitations of functionalism see W. C. Runciman, *Social and Political Theory* (Cambridge: Cambridge University Press, 1963), pp. 109-123. Hempel equally takes a critical view of "the logic of functional analysis" (in Gross, *Symposium on Sociological Theory*, pp. 271-307), but his standpoint is often far removed from our problems.

⁵⁰ This is not to fall prey to the subjectivistic fallacy on which Merton builds his case (*supra*, note 46). Purpose may be a "motivation" of the actor, but may equally be—as it is in teleological analysis—an "imputation" of the observer.

ascribed or actually served purpose.⁵¹ Conversely, dis-function, non-functionality, and the like, indicate—from different angles—that the assigned purpose is not served by a given structure. And this current usage of function goes a long way to explain, in turn, our difficulties with structure.

The major problem with "structure" is, in fact, that political bodies and institutions largely bear, if not a functional denomination, a functional definition. Either under the sheer force of names—which is in itself a tremendous force—or for the sake of brevity, political structures are seldom adequately defined on their own terms—*qua* structures. That is to say, on the one hand, that we dispose of a functional (purposive) vocabulary, whereas we badly lack a structural (descriptive) vocabulary; and that, on the other hand, even when we deliberately ask "what is," we are invariably prompted to reply in terms of "what for." What is an election? A means (a structure) *for* electing office holders. What is a legislature? An arrangement *for* producing legislation. What is a government? A set-up *for* governing. The structure is almost invariably perceived and qualified by its salient function.⁵² This makes a great deal of sense in practical politics, but represents a serious handicap for the understanding of politics.

The plain fact is, then, that the structural-functional analyst is a lame scholar. He claims to walk on two feet, but actually stands on one foot—and a bad foot at that. He cannot really visualize the interplay between "structure" and "function" because the two terms are seldom, if ever, neatly disjoined: the structure remains throughout a twin brother of its imputed func-

⁵¹ "Unintended functions"—the fact that structures may serve ends and obtain results which were neither foreseen nor desired by the structure builders—can be entered, for the economy of my argument, into the list of the purposes actually served. Likewise "latent functions" are immaterial to my point.

⁵² Riggs makes the same point, namely, that "current terminology quite confusingly links structural and functional meanings" from the opposite angle that expressions such as "legislature and public administrator . . . are normally defined structurally, the first as an elected assembly, the second as a bureaucratic office"; but then goes on to say that "the words . . . also imply functions" (p. 23 of the paper cit. *supra*, note 47). It should be understood, therefore, that my "structural definition" calls for a thorough structural description. If the argument were left at defining a legislature as an elected assembly, then it can be made either way, as Riggs does.

tional purposes. And here we enter a somewhat vicious whirl which leads the approach to conclusions which, if true, would be self-denying.

Whatever else the structural-functional scholar may have failed to discover, he feels pretty sure about three points: first, no structure is unifunctional, i.e., performs only one function; second, the same structure can be multifunctional, i.e., can perform across different countries widely different functions; third, and therefore, the same function has structural alternatives, i.e., can be performed by very different structures. Now, to some extent these points are undeniable—but only to the extent sensed at any time by any perceptive comparative scholar. My quarrel is with the emphasis, which is unwarranted and positively misleading.

Is it really the *same* structure that functions differently? Or is the functional performance different because the structure is not the same? The thesis generally lacks adequate evidence on the structural side. For instance, "elections" are multifunctional (they may well serve the purpose of legitimizing a despot), but "free elections" are not.⁵³ That is to say, as soon as the electoral process obtains a structural underpinning—the minute and multiple structural conditions that make for free voting—electoral multifunctionality rapidly comes to an end. If the voter *is* offered alternatives, if the candidates *are* free to compete, if fraudulent counting *is* impossible, then free elections do serve—everywhere—the purpose of allowing an electorate to select and dismiss office holders. In view of this primary, fundamental purpose the *same* electoral structure (same in providing all the necessary safeties) either approaches uni-functionality, or leaves us with non-functionality, e.g., with the finding that illiterate voters are unable to use electoral mechanisms which presuppose literacy.

While the most serious problem and default is that the structures are inadequately pinpointed and described, let me hasten to add that we are not performing much better from the functional end of the argument. For our functional categories also generally lack adequate underpinning. Surprisingly enough—if one considers the far greater ease with which the functional side of the problem can be attacked—our functions tend to be as unhelpful as our structures.

For instance, if one asks, "Why a party system?" the least challengeable and most inclusive reply might be that parties perform a communi-

cation function. And if the problem is left at that, it easily follows that the authorities and the citizens "communicate," in some sense, in any polity, i.e., even when no party system exists. Hence party systems have structural alternatives—*quod erat demonstrandum*. But the problem cannot be left at that, i.e., with an unbounded, no-difference notion of communication which nullifies the problem. And the underpinning of communication brings out, first, that there is an essential difference between up-going and descending communication, and, second, that it is equally important to distinguish between "communication-information" and "communication-pressure." If so, to define a party system as an instrument for "communicating" demands and conveying "information" to the authorities, is to miss the point. A party system is, in reality, a mechanism for *sustaining* demands—and *pressing* demands—all the way through to policy implementation. What is at stake, then, is the passage from a two-way (reversible) communication-information to a prevalence of up-going communication-pressure. And for this latter purpose we have not devised, so far, any structural alternative. A party system turns out to be, therefore, a non-replaceable, *unique structure* as soon as we spell out its distinctive, crucial reason for being.

A more careful scrutiny goes to show, then, that the multi-functional, multi-structural argument has been pushed far too far, indeed to the point of becoming erroneous. Aside from the error, the irony of the situation is that, as it stands, the thesis appears self-defeating. If the same structure performs utterly different functions in different countries, and if we can always find structural alternatives for whatever function, what is the use of structural-functional analysis?

Pulling the threads together, I need not spend much time in arguing that the stalemate and the mishandlings of the structural-functional approach have a lot to do with the ladder of abstraction.

On the functional side of the coin we are encumbered by a wealth of haphazard functional categories which are merely enumerated (hardly classified according to some criterion, and even less according to the logical requirements of a taxonomical tree-type unfolding), and definitely provide no clues as to the level and type of analysis (e.g., total versus partial systems analysis) to which they apply.⁵⁴ As a result the global

⁵³ I cite the title of W. J. M. MacKenzie's book *Free Elections*, (London: Allen & Unwin, 1958) to imply that a real structural underpinning may well presuppose a hundred-page description.

⁵⁴ A sheer list of the functional denominations, roles or attributions scattered throughout the literature on political parties suffices to illustrate the point, and would be as follows: participation,

functional argument developed by a number of structural-functionalists remains suspended in mid-air—for lack of a coordinated medium level taxonomic support—and is left to play with over-stretched, if not contentless, functional universals.

On the structural side of the coin we are confronted, instead, with little more than nothing. Structures qualified on their own right hardly exist—at least in the Almond line of thinking.⁵⁵ This is all the more regrettable in view of the fact that while functions are meant to be (at least in global comparative politics) broad explanatory categories which do not require a low level specification, structures bear, instead, a closer relation to observables, and definitely need under-pinning all the way down the ladder. With structures understood as organizational structures we are required, in fact, to descend the ladder all the way down to low level configurative-descriptive accounts.

Starting from the top, one can identify—with the help of minor terminological devices—at least four different levels of analysis: 1) structural principles (e.g., pluralism), 2) structural conditions (e.g., the class or the economic structure), 3) organizational patterns (with relation to membership systems), 4) specific organizational structures (e.g. constitutions). By saying “structural principles” I mean that as an HL category the notion of structure can only point to the principles according to which the component part of politics, or of societies, are related to each other. With reference, instead, to the low level of abstraction it should be clear that con-

electioneering, mobilization, extraction, regulation, control, integration, cohesive function, moderating function, consensus maintenance, simplification of alternatives, reconciliation, adaptation, aggregation, mediation, conflict resolution, brokerage, recruitment, policy making, expression, communication, linkage, channelment, conversion, legitimizing function, democratization, labelling function.

⁵⁵ I make specific reference to Almond because I believe that his very conception of structure is largely responsible for this outcome. For instance, “By structure we mean the observable activities which make up the political system. To refer to these activities as having a structure simply implies that there is a certain regularity to them.” (Almond and Powell, *Comparative Politics: A Developmental Approach*, Boston: Little, Brown, 1966, p. 21). In the subsequent paragraph one reads: “We refer to particular sets of roles which are related to one another as *structures*.” Under such porous and excessively sociological criteria, “structure” becomes evanescent.

stitutions and statutes are not the “real structure. Nonetheless behavior under written rules is easier to pin down than behavior under diffuse roles, and excessive anti-formalism leads us to neglect organizational theory and the extent to which legally enforced regulations do mold behavior.

In summing up, not only has the structural-functional scholar ignored the ladder of abstraction, but he has inadvertently destroyed, during his reckless climbing, his own ladder.⁵⁶ So much so that the approach encounters exactly the same perplexity as, say, general systems theory, namely, “Why has no scholar succeeded in presenting a structural-functional formulation which meets the requirements of empirical analysis.”⁵⁷ Now, it is hardly surprising that the general systems theorist should encounter great difficulties in deriving testable propositions about politics, since he is required to proceed deductively on the basis of theoretical primitives.⁵⁸ But this is not the case with the structural-functional approach, which is not necessarily committed to whole systems analysis and enjoys the distinctive empirical advantage of leaning extensively—especially with segmented systems analysis—on observational terms.⁵⁹ So, why should the struc-

⁵⁶ This complaint is ad hoc, but could be expanded at length. On the general lack of logical and methodological status of the approach two strong critical statements are: R. E. Dowse, “A Functionalist’s Logic,” *World Politics*, (July 1966), 607–622; and A. L. Kalleberg, “The Logic of Comparison,” *World Politics*, 18 (October 1966), 69–82. While the two authors are overconscious thinkers, I would certainly agree with Dowse’s concluding sentence, namely, that “to ignore trivial logical points is to risk being not even trivially true” (p. 622).

⁵⁷ Flanigan and Fogelman in Charlesworth, *Contemporary Political Analysis* pp. 82–83.

⁵⁸ On general systems theory one may usefully consult Oran R. Young, *Systems of Political Science* (Englewood Cliffs: Prentice-Hall, 1968), Chap. 2. See also Giuliano Urbani, “General Systems Theory: Un Nuovo Strumento per l’Analisi dei Sistemi Politici?,” *Il Politico*, 4 (1968), 795–819.

⁵⁹ While there is some controversy on the respective merits and shortcomings of the two strategies, the structural-functional approach is not inherently tied to either one. For the partial versus whole systems controversy the two stances are well represented by J. LaPalombara, who favors the segmented approach. (cf. esp. “Parsimony and Empiricism in Comparative Politics: An Anti-Scholastic View,” in R. T. Holt and J. E. Turner (eds.), *The Methodology of Comparative Re-*

tural-functional scholar remain tied to "a level of analysis which [does] not permit empirical testing?"⁶⁰ According to my diagnosis there is no intrinsic reason for this. Quite to the contrary, we may expect very rewarding returns, and the empirical promise (and distinctiveness) of the approach may well near fulfillment, if we only learn how to maneuver along a ladder of abstraction.

Let us now pass on to a more loose discussion—the second part of this illustration—for which I have selected a somewhat different family of categories: pluralism, integration, participation and mobilization.⁶¹ While one may think of many other examples that would suit my purposes just as well, the four categories in question are representative in that they are used for significant theoretical developments not only under a variety of different frameworks, but also by the non-affiliated scholar, thereby including—in the case of participation and mobilization—the scholar who happens to be interested only in statistical manipulations.

Given the fact that pluralism, integration, participation and mobilization are culture-bound concepts which may reflect—as far as we know at the outset—a distinctive Western experience, the methodological caveat here is that the reference area should make for the starting point of the investigation. So to speak, we are required to elaborate our culture-bound concepts in a "we-they" clockwise direction. It is proper, therefore, to start with the question: How do we understand pluralism, integration, participation and mobilization in their domestic, original context?

At home "pluralism" does not apply to societal and/or political structure, nor to interplay between a plurality of actors. Pluralism came to be used, in the Western literature, to convey the idea that a pluralistic society is a society whose structural configuration is shaped by pluralistic beliefs, namely, that all kinds of autonomous sub-units should develop at all levels, that interests are recognized in their legitimate diversity, and

that dissent, not unanimity, represents the basis of civility. Pluralism is indeed—as already noted—a highly abstract structural *principle*. Yet the term points to a *particular* societal structure—not merely to a developed stage of differentiation and specialization—and does retain a wealth of characterizing connotations whenever we discuss, in the Western democracies, our internal policies and problems.

"Integration" can be conceived as an end-state, as a process, or as a function performed by integrating agencies (parties, interest groups, etc.). In any case, in the Western politics integration is not applied to *whatever* kind of "putting together," to *whatever* state of amalgamation. For instance, when American scholars discuss their own domestic problems, they have definite ideas of what is, and what is not, integration. They would deny that integration has anything to do with "enforcing uniformity." They are likely to assume, instead, that integration both presupposes and generates a pluralistic society (as qualified above). And, surely, an integrative agency is required to obtain a maximum of coalescence and solidarity with a minimum of coercion.⁶²

Similar points can be made with regard to participation and mobilization. Regardless of whether "participation" is used normatively (as pointing to a basic tenet of the democratic ideal) or descriptively (as reflecting a democratic experience), in either case in our domestic discussions participation is not any such kind of "taking part." Thus the advocates of a participatory democracy are hardly satisfied by any kind of involvement in politics. To them participation means *self-motion*; it does not mean being manipulated or coerced into motion. And surely the original definite meaning of the term conveys the idea of a free citizen who acts and intervenes—if he so wishes—according to *his* best judgement. So conceived, participation is the very opposite, or the very reverse, of mobilization. Mobilization does not convey the idea of individual self-motion, but the idea of a malleable, passive collectivity which is being *put into motion* at the whim of persuasive—and more than persuasive—authorities. We say that indi-

search, op. cit., pp. 125-149); and, for the contrary view, Fred W. Riggs (cf. especially his forthcoming essay in Haas and Kariel, *Approaches to the Study of Political Science*.)

⁶⁰ Flanigan and Fogelman, *op. cit.*

⁶¹ The relevant "family difference" is that structure and function are not culture-bound concepts, while the four other categories are. This is also to note that the travelling problem of comparative politics cannot be reduced to the construction of "non-culture bound" conceptualizations. How to use those conceptualizations which cannot help being culture bound is equally a problem.

⁶² Since we are discussing here macro-problems and macro-theory I need not follow the concepts under investigation all the way down the ladder of abstraction. I should not let pass unnoticed, however, that "integration" also belongs to the vocabulary of sociology and psychology, thereby lending itself to very fine lower level distinctions. See e.g., W. S. Landecker, "Types of Integration and their Measurements," in *The Language of Social Research, op. cit.*, pp. 19-27.

viduals "participate," but we cannot say about the same individuals that they "mobilize"—they *are mobilized*.

It is quite clear, then, that pluralism, integration, participation and mobilization all have specific connotations which can be pinned down, and are in fact retained—no matter how implicitly—in our Western enquiries and controversies. However, in the context of global comparative politics the specificity of these notions gets lost: there is no end to pluralism; integration is applied indifferently to pluralistic and non-pluralistic settings; and participation and mobilization are turned into largely overlapping notions. There is no end to pluralism, for we are never told what is non-pluralism. Since pluralism exists somewhere, the assumption appears to be that "to a different degree" pluralism will be found to exist everywhere. However, a different degree of *what?* This is indeed the irony of using a degree language—intended when used appropriately to convey precision—for conveying elusiveness. Likewise the meaning of integration changes, and eventually evaporates, en route. Finally, and similarly, the distinction between participation and mobilization only holds at home. With most comparative oriented scholars mobilization comes to mean whatever process of social activation; and participation is currently applied by the discipline at large both to democratic and mobilizational techniques of political activation.

At this stage of the argument I need not labor at explaining why and how we obtain these drastic losses of specificity. They result, as we know, from conceptual stretching, which results, in turn, from incorrect ladder climbing: the clumsy attempt to arrive at "travelling universals" at the expense of precision, instead of at the expense of connotation (i.e., by reducing the number of qualifying attributes). What remains to be highlighted are the consequences of this state of affairs.

Take, for instance, the formidable errors in interpretation and prediction which are suggested by the universal, unspecified application of "pluralism" and "integration." If we say that African societies are not pluralistic but "tribalistic," the argument is likely to be that a situation of tribalistic fragmentation hardly provides the structural basis not only for integrative processes to occur, but also for bringing integrative agencies to the fore. Indeed my argument would be that the functional needs, or feedbacks, of a fragmented society are at odds with the functional feedbacks, or needs, of a pluralistic society. In Europe, for instance, medieval fragmentation generated monarchical absolutism. However, if pluralism is vaporized into an empty generality, then we are authorized to

call African societies pluralistic, and the unfortunate implication may well be that we expect Africans to solve their problems as if they had to deal with Western-type societies.⁶³

"Mobilization" is also a worthwhile example in that it confronts us with a problem that has only been mentioned, so far, in passing. While pluralism, integration and participation are derived from our experience with democracy—i.e., from the context of the democratic polities—we also dispose of a limited set of terms which originate from a totalitarian context. This is the case of the term mobilization, which derives from military terminology—especially the German total mobilization of World War I—and enters the vocabulary of politics via the militia type of party (as Duverger calls it), and specifically via the experience of fascism and of nazism.⁶⁴ Nonetheless the term is currently applied also to the democratic polities—and this means that we have drawn a "reversed extrapolation" (i.e., a counter-clockwise extrapolation). And since we often complain that our terminology is democracy-centered, my first complaint is that we fail to take advantage of the fact that we do have terms which escape the democratic bias. However, the inconvenience resulting from reversed extrapolations are seen best on a broader scale, and with particular reference to what I call the "boomerang effect" of the developing areas.⁶⁵

Western scholars travelling across Africa or South-East Asia discover that our categories hardly apply, which is hardly surprising. From this they conclude—and this is the boomerang effect—that the Western categories also should not be applied to the West. But this is a strange inference. Granted that global comparative poli-

⁶³ The point could be extended at great length. E.g., I would assume that only in a truly pluralistic society (i.e., qualified by the characteristics conveyed by the Western use of term) may differentiation result in, and join forces with, integration. But much of the literature on political development seems to miss this essential condition.

⁶⁴ Shils and Deutsch relate the notion also to Mannheim's "fundamental democratization" (see esp. K. W. Deutsch "Social Mobilization and Political Development," this REVIEW, 55, September 1961, p. 494). But while Mannheim may well have provided the bridge across which "mobilization" entered the vocabulary of democracy, the fact remains that the term was commonly used in the early thirties, in Italy and in Germany, as reflecting a distinctly totalitarian experience.

⁶⁵ The boomerang effect is also responsible, in part, for the disappearance of politics (*supra*, note 5).

tics requires minimal common denominators, it does not follow that we should escape Western parochialism by masquerading in non-Western clothes. For one thing, it may well be that a number of ancient civilizations appear diffuse and amorphous to the Western observer precisely because he lacks the categories for coping with devious, overly sedimented, "non-rational" structural patterns. On the other hand, and assuming that underdeveloped political societies may be far less structured than others, this is no reason for feeding back shapelessness where structural differentiation does exist. Hence, reversed extrapolations are a fallacy, and the problem of establishing a minimal common denominator does not authorize us to feed primitivism and formlessness into non-primitive settings.

If I may generalize from the foregoing, it appears that much of the ongoing work of the discipline is plagued by "meaningless togetherness," and thereby by dangerous equivocations and distortions. In particular, and especially important, under these conditions we are dangerously exposed to "begging the question," i.e., to assuming what we should be proving: the *petitio principii* fallacy. For instance, if "mobilization" is applied to a democratic polity the suggestion is that democracies mobilize more or less as totalitarian regimes do. Conversely, if "participation" is applied to a totalitarian system the suggestion is that democratic participation also occurs, to some extent at least, in nondemocratic settings. Now this may well be the case. But the case cannot be proven by *transferring the same denomination* from one context to another. For this amounts to pure and simple terminological camouflage: things are declared alike by making them *verbally* identical.

All in all, then, it can hardly be held that our "losses of specificity" are compensated by gains in inclusiveness. I would rather say that our gains in travelling capacity, or in universal inclusiveness, are verbal (and deceptive) while our "gains in obfuscation" are very substantial.

I cannot discuss this further. As LaPalombara vividly puts it, "so many of our generalizations about the political process move with apparent randomness from the micro to the macroanalytic levels"—the result being "messiness caused by confusion as to the level of analysis."⁶⁶ Following this line of complaint I have argued that confusion as to the level of analysis brings about these unfortunate results: 1) at the higher levels, macroscopic errors of interpretation, explanation and prediction; 2) at the lower levels, a great deal of wasteful data misgathering; 3) at all

levels, confusion of meaning and destruction of the sharpness of our concepts. We do lack words. But conceptual stretching and poor logic have largely impoverished the analytical articulation and the discriminating power of the words that we do have. And my feeling is that only too often major differences are being cancelled on the thin basis of secondary, trivial similarities. It would hardly make sense to say that men and fishes are alike in that both classes share a "swimming capability." Yet much of what we are saying in global comparative politics may not make much more sense.

Let me stress, to conclude, that according to my scheme of analysis all of this is unnecessary. Awareness of the ladder of abstraction shows that the need for highly abstract, all-embracing categories does not require us to inflate, indeed to evaporate, the observational, empirically-linkable, categories that we do have. Moreover, if we know how to climb and descend along a ladder of abstraction—and thereby know where we stand in relation to the "property space" of the analysis that we are pursuing—not only conceptual stretching is ruled out, but also faulty analogies and the begging-the-question fallacy can be disposed of.

V. SUMMARY

Especially during the last decade comparative politics as a substantive field has been rapidly expanding. The scale, if not the scope, of this expansion raises thorny and unprecedented problems of method. But we seem to embark more and more in comparative endeavors without *comparative method*, i.e., with inadequate methodological awareness and less than adequate logical skills. That is to say, we seem to be particularly naive vis-à-vis the logical requirements of a world-wide comparative treatment of political science issues.

My focus is conceptual—about concepts—under the assumption that concepts are not only elements of a theoretical system, but equally tools for fact-gathering, data containers. The empirical problem is that we badly need information which is sufficiently precise to be meaningfully comparable. Hence we need a filing system provided by discriminating, i.e., taxonomic, conceptual containers. If these are not provided, data misgathering is inevitable; and statistical, computerized sophistication is no remedy for misinformation. The theoretical problem can be stated, in turn, as follows: we grievously lack a disciplined use of terms and procedures of comparison. This discipline can be provided, I suggest, by awareness of the ladder of abstraction, of the logical properties that are implied, and of

⁶⁶ *Comparative Politics* (October 1968), p. 72.

the rules of composition and decomposition thus resulting. If no such discipline is obtained, conceptual mishandling and, ultimately, conceptual misformation is inevitable (and joins forces with data misgathering).

Thus far the discipline has largely followed the line of least resistance, namely, "conceptual stretching." In order to obtain a world-wide applicability the extension of our concepts has been broadened by obfuscating their connotation. As a result the very purpose of comparing—control—is defeated, and we are left to swim in a sea of empirical and theoretical messiness. Intolerably blunted conceptual tools are conducive, on the one hand, to wasteful if not misleading research, and, on the other hand, to a meaningless togetherness based on pseudo-equivalences.

The remedy resides—I submit—in our combined ability 1) to develop the discipline along a medium level of abstraction with better intermediate categories, and 2) to maneuver, both upwards and downwards, along a ladder of abstraction in such a way as to bring together assimilation and differentiation, a relatively high explanatory power and a relatively precise descriptive content, macro-theory and empirical

testing. To be sure, no level of analysis can be exactly translated and converted into the next level. In this sense, something is always lost (and gained) along the ladder. But a disciplined use of terms and procedures of comparison generates, at each level, sets of propositions which either reinforce or contradict the propositions of the neighboring levels.

The suggestion has recently been put forward that "political scientists turn to mathematics for [the] rules of logic" required "to introduce the necessary deductive power into a paradigm."⁶⁷ I have taken the more sober, and indeed counter-perfectionistic view that we should not encourage the "overconscious thinker" paralyzed by overly ambitious standards. But surely we cannot expect an unconscious thinker lacking elementary logical training and discipline to meet the intricate new problems arising from global comparisons.

⁶⁷ Holt and Richardson, "Competing Paradigms in Comparative Politics," in Holt and Turner, *The Methodology of Comparative Research*, cit., p. 70. The chapter is perhaps perfectionistic, but surely a very intelligent and stimulating "stock taking" overview.