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COMPARING AND MISCOMPARING

Giovanni Sartori

ABSTRACT

I seek to explain the disappointing performance of the field of comparative politics addressing the three basic questions: Why compare? What is comparable? and How? I also challenge the view that the methodology of comparison is pretty well known and established. Hosts of unsettled issues remain, while a growing cause of frustration and failure is the undetected proliferation of 'cat-dogs' (or worse), that is, nonexistent aggregates which are bound to defy, on account of their non-comparable characteristics, any and all attempts at law-like generalizations. The bottom line is that the comparative endeavor suffers from loss of purpose.

KEY WORDS • comparative method • conceptual stretching • degreeism • logic of comparison • misclassification • parochialism

In the early 1950s, when Roy Macridis blasted (1953 and 1955) at the traditional comparative politics of the time, his first and major charge was that it was 'essentially noncomparative'. Much, or indeed more of the same can be said today, since the field defines itself (in the United States) as studying 'other countries', generally just one. Thus, a scholar who studies only American presidents is an Americanist, whereas a scholar who studies only French presidents is a comparativist. Do not ask me how this makes sense – it does not.¹ The fact remains that a field called comparative politics is densely populated by non-comparativists, by scholars who have no interest, no notion, no training in comparing. The preliminary point must thus be to establish the distinctiveness of comparative politics as a field characterized by a method.²

It is often held that comparisons can be 'implicit' and/or that the scientific approach per se is inherently comparative. I certainly grant that a scholar *can be* implicitly comparative without comparing, that is, provided that the one-country or one-unit study *is* embedded in a comparative context; but how often is this really the case?³ I equally grant that, in some sense, the scienti-

1. As Sigelman and Gadbois correctly put it, 'comparison presupposes multiple objects of analysis . . . one compares something to or with something else' (1983: 281).

2. Indeed, comparative politics is the one field of political science that defines itself by 'a methodological instead of substantive label' (Lijphart, 1971: 682). Similarly, in Holt and Turner (1970: 5): 'the common-sense meaning of the term *comparative* . . . refers to a method of study and not to a body of substantive knowledge.'

3. Not often, as one can easily infer from a skimming of the bibliographies. Most single nation studies plainly and wholeheartedly ignore the comparative frameworks and literature that bear on their topics.

fic method itself assumes a comparing; but this is a long shot. The short of the matter is that *if* a scholar is implicitly (though not unwittingly) comparative, this doubtlessly makes him or her a better scholar. But the difference between the implicit and the explicit cannot be slighted to the point of automatically making the 'unconscious comparativist' a comparativist. On this criterion there has never been a behavioral revolution because students of politics have always, implicitly, observed behavior; there has never been a quantitative revolution because even the simpletons of the past said much, little, greater, lesser, and were thus, implicitly, quantitativists. How absurd – as is attested by the blatant fact that under the 'inevitably comparative' cover-up the social sciences are actually inundated with parochial yardsticks and hypotheses that would founder in a second if ever exposed to comparative checking.

Why Compare?

Indeed, comparative *checking*. Have I not already answered my first question, namely, *Why compare?* While I am surely not the first to assert that comparisons *control* – they control (verify or falsify) whether generalizations hold across the cases to which they apply – nonetheless this is a seemingly forgotten answer. According to Przeworski, a 'consensus exists that comparative research consists *not of comparing but of explaining*. The general purpose of cross-national research is to understand . . .' (1987: 35, my emphasis). Przeworski appears to be right about the current consensus. In similar fashion Ragin (1987: 6) holds that comparative knowledge 'provides the key to understanding, explaining and interpreting'; and Mayer (1989: 12) 'redefines' (in his title) comparative politics as a field whose goal is 'the building of empirically falsifiable, explanatory theory'. Well, one can hardly disagree with an intent described as understanding and explaining; for all knowledge, none excluded, is aimed at understanding and all knowledge seeks to explain.⁴ But then, why compare? What is the specific reason for being of a comparative route to knowledge? What are comparisons *for*? Against the loss of purpose that appears by now to dominate the field, let a purpose be forcefully reinstated, namely, and to repeat, that *comparing is controlling*. To be sure, one may engage in comparative work for any number of reasons; but *the* reason is control.⁵

4. Note that even 'explanation' appears too strong a requirement to Cantori. In his appraisal, 'comparative politics is more inclined towards interpretation than explanation', the difference between the two being that explanation 'seeks to *demonstrate* the validity of its conclusion', whereas interpretation 'seeks to *convince* only by means of persuasion' (Cantori and Ziegler, 1988: 418).

5. It should be understood that the point bears on the normal science. In authors of the stature of Tocqueville, Durkheim or Max Weber, the comparative component of their work is part and parcel of the richness of their thinking. As all my examples indicate, I am not speaking to 'grand schemes' but to the single generalizations (causal-like hypotheses) that authors would 'normally' formulate in pursuing their subject matter.

Take the statement, 'Revolutions are caused by relative deprivation'; or the statement, 'Presidential systems are conducive to effective government, while parliamentary systems result in feeble government'. True or false? How do we know? We know by looking around, that is, by comparative checking. Granted, comparative control is but *one* method of control. It is not even a strong one. Surely experimental controls and, presumably, statistical controls are more powerful 'controllers'.⁶ But the experimental method has limited applicability in the social sciences, and the statistical one requires many cases.⁷ We are often faced, instead, with the 'many variables, small *N*' problem, as Lijphart (1971: 686) felicitously encapsulates it; and when this is the case our best option is to have recourse to the comparative method of control.⁸ *The* reason for comparing is thus, in its basic simplicity, a compelling one. To this one may sensibly add that comparing is 'learning' from the experience of others and, conversely, that he who knows only one country knows none. Quite so.

What Is Comparable?

Second question, *What is comparable?* We frequently argue that apples and pears are 'incomparable'; but the counterargument inevitably is: How do we know unless we compare them? Actually, with pears and apples the issue is easily solved. But are stones and monkeys comparable? We may still reply that in order to declare them 'incomparables' we have, if for only one second, compared them. Nonetheless, if the entities⁹ being compared have nothing in common, there is nothing more to be said, and this is what we mean when we declare that stones and monkeys are not comparable: The comparison is of no interest, it ends where it begins. Returning to pears and apples, are they comparable or not? Yes, they are comparable with respect to some of their properties, i.e. the properties they share, and non-comparable with respect

6. I say 'presumably' to account for the counterarguments of Frensdreis (1983: 258), and especially of Ragin (1987: 15-16), who contends that 'the comparative method is superior to the statistical method in several important respects'.

7. Lijphart and Smelser take a different view as to whether the experimental statistical and comparative methods are distinct methods (Lijphart), or simply different implementations of a same comparative logic (Smelser, 1976: 158). Since the methods in question are not equivalent, in my opinion their distinctiveness matters more than their similarity.

8. Of course, in some instances one may control both across a relatively small and a relatively large number of cases, that is, statistically. Let the hypothesis be: party cohesion is a direct function of the degree of inter-party competition (and, thus, the lesser the competition, the higher the degree of intra-party factionism). Here comparative checking will help refine the hypothesis, so that a statistical control may subsequently become correctly applicable.

9. Entity stands here for whole systems, subsystemic 'segments' (vigorously upheld by LaPalombara, 1970: 123ff.), processes or even, at the limit, for a single property or characteristic of a universe.

to the properties that they do not share. Thus, pears and apples are comparable as fruits, as things that can be eaten, as entities that grow on trees; but incomparable, e.g., in their respective shapes. Making the point in general, the question always is: *comparable with respect to which properties or characteristics*, and incomparable (i.e. too dissimilar) with respect to which other properties or characteristics?

It will be appreciated that the foregoing establishes that to compare is both to assimilate and to differentiate *to a point*. If two entities are similar in everything, in all their characteristics, then they are a same entity – and that is that. If, on the other hand, two entities are different in every respect, then their comparison is nonsensical – and that is again that. The comparisons in which we sensibly and actually engage are thus the ones between entities whose attributes are in part shared (similar) and in part non-shared (and thus, we say, incomparable).

Does the above simply push the problem back to the Osgood question, namely, ‘when is the same really the same?’ and, conversely, ‘when is different really different?’ (1967: 7). Many authors have been struggling without avail with this question, and much research has foundered on its reefs. Yet the question does have a sound answer if we remember that it is answered by classifications and/or by the *per genus et differentiam* mode of analysis. To classify is to order a given universe into classes that are mutually exclusive and jointly exhaustive. Hence, classifications do establish what is same and what is not. ‘Same’ brings together whatever falls into a given class; ‘different’ is what falls under other classes.¹⁰ Let it also be underscored that classes do not impute ‘real sameness’, but *similarity*. The objects that fall into a same class are more similar among themselves – with respect to the criterion of the sorting – than to the objects that fall into other classes. But this leaves us with highly flexible degrees of similarity. As a rule of thumb, the smaller the number of classes yielded by a classification, the higher its intra-class variation (its classes incorporate, so to speak, very different sames). Conversely, the greater the number of the classes, the lesser the intra-class variance. If we divide the world just into monarchies and republics, we have two classes that are, if anything, impossibly large and excessively varied. Still, the example shows that there is no merit in the objection that to classify is to freeze sameness. Any class, no matter how minute, allows for intra-class variations (at least of degree); and it is up to the classifier to decide how much his classes are to be inclusive (broad) or discriminating (narrow).

10. The point is made by Kalleberg (1966: 77–8) as follows: ‘Truly comparative concepts . . . can only be developed after classification has been completed. Classification is a matter of ‘either-or’; comparison is a matter of more or less’. I concur up until the last sentence; but why *must* comparisons be a matter of more-or-less? Possibly Kalleberg has in mind, here, intra-class (not inter-class) comparing.

The gist is – let it be reiterated – that what is comparable is established by putting the question in its proper form, which is: comparable *in which respect*? Under this formulation, pears and apples are, in a number of respects (properties), comparable. So are, but less so, men and gorillas (they are, e.g., both erect animals with *prensilis* hands); and so are, at the limit, even men and whales (both are mammals, and neither can breathe under water). But, of course, the incomparables grow as we pass from the first to the third example. When and how is it, then, that we go wrong? I do not claim that the only way of playing the comparative game without error is to rely on classificatory orderings. However, the route along which much of the profession has chosen to embark in the last twenty years or so is clearly unsafe and easily conducive to shipwreck. I call it the cat-dog route and make it into a story (I hope an amusing one).

The Cat-Dog

Mr Doe is ready for his dissertation, but he must be original and must have, he is insistently told by his advisers, a hypothesis. His subject is the cat-dog (one cannot be original, nowadays, just with cats or just with dogs), and his hypothesis, after much prodding, is that all cat-dogs emit the sound ‘bow wow’. The adviser says ‘interesting’, and a foundation gives him \$100,000 for world-wide research. Three years later Mr Doe shows up in great dismay and admits: many cat-dogs do emit the sound ‘bow wow’, but many do not – the hypothesis is disconfirmed. However, he says, I now have another hypothesis: all cat-dogs emit the noise ‘meow meow’. Another three years go by, another \$100,000 are dutifully spent in researching, and yet, once again, the hypothesis is not sustained: many cat-dogs do emit the noise ‘meow meow’ but many do not. In deep despair Mr Doe visits, in her cave, at dusk, the oracle of Delphi, who on that day had grown tired of making up sibylline responses. My friend, the oracle says, to you I shall speak the simple truth, which simply is that the cat-dog *does not exist*. End of story, and back to non-fiction.

How does the cat-dog come about? It is fathered, I submit, by four mutually sustaining sources: (i) parochialism, (ii) misclassification, (iii) degreeism, and (iv) conceptual stretching.

Parochialism refers, here, to single-country studies *in vacuo*, that purely and simply ignore the categories established by general theories and/or by comparative frameworks of analysis, and thereby unceasingly invent, on the spur of the moment, an ad hoc, self-tailored terminology. For example, a recent article by Sundquist (1988) deals – the wording of the title – with ‘coalition government in the United States’. Now, throughout the world ‘coalition government’ stands for parliamentary systems (not American-type presidential systems), in which governments are voted into office and supported by parliaments, and happen not to be single-party governments. Not

one of these characteristics obtains in what Sundquist calls coalition government. Thus a cat-dog (or, worse, a dog-bat) is born; and as soon as the misnomer is entered into our computers it is bound to mess up whatever is correctly known of coalition governments (proper).

Misclassification and Degreeism

In the example above a cat-dog results from a mislabeling which results, in turn, from parochialism. A second source is misclassification, pseudo-classes. Take the one-party category in the literature on political parties, a huge basket that has long included: (1) the so-called one-party states in the United States, Japan and, off and on, Sweden, Norway and India; (2) Mexico; (3) the Soviet Union, China and Eastern European countries. The above collapses into one three utterly different animals, and thus is, I submit, a cat-dog-bat.¹¹ Suppose now that we wish to explain what causes unipartism. Huntington suggests that 'the social origins of one-party systems are to be found . . . in bifurcation', that is to say, 'one-party systems . . . tend to be the product of either the cumulation of cleavages . . . or the ascendancy in importance of one line of cleavage over the others' (Huntington and More, 1970: 11). Right, wrong? We shall never find out. Neither his hypothesis nor any other will ever pass the test of the cat-dog-bat, for no generalization can conceivably hold up under the joint assaults of such a three-headed monster. What might apply to cats will apply only in part to dogs, and in almost no respect whatsoever to bats (and vice versa). One may wonder why, here, the culprit is misclassification. Well, because classifications are orderings derived from a single criterion. In the case in point, under a correct classificatory treatment 'one' will include only the polities in which 'second' (more than one) parties neither exist nor are permitted to exist. Under a classificatory treatment, therefore, the United States, Japan, India, etc., could not possibly fall into the one-party box. But under a pseudo-class anything goes.

A third producer of cat-dogs and further – in increasing order of teratological messiness – of dog-bats and even fish-birds is, I have suggested, 'degreeism'. By this I mean the abuse (uncritical use) of the maxim that differences in kind are best conceived as differences of degree, and that dichotomous treatments are invariably best replaced by continuous ones. To exemplify, under a continuous or continuum-based treatment, democracy cannot be separated from non-democracy; rather, democracy is a property that to some (different) degree can be predicated of all political systems and, conversely, non-democracy is always more or less present in any polity. We

11. To specify, the first group of countries are predominant party systems that belong to a competitive setting (Sartori, 1976: 192-201); Mexico is a hegemonic party polity that 'licenses' a limited competition (230-8); and the third group is (was) one-party proper, in that it impedes competition and any other party (221-30).

may thus obtain a world-wide continuum ranging, say, from 80 percent democracies, across semi-democracies, to 80 percent non-democracies, whose cut-off points are stipulated arbitrarily and can, therefore, be moved around at whim. This is wonderful, for the exceptions that might cripple a hypothesis generally lie in the vicinity of the cutting points. Thus, in the continuous treatment the exceptions (disconfirmations) can simply be made to disappear by cutting a continuum at astutely doctored points. Along this route we obtain, then, the Cheshire cat-dog – it appears, grins at us, and vanishes before we catch it.

Concept Stretching

Fourthly, comparative futility and fallacies simply and generally result from definitional sloppiness and ‘conceptual stretching’ (Sartori, 1970). Take *constitution*. If the term is stretched to mean ‘any state form’, then the generalization ‘constitutions obstruct tyranny’ would be crushingly disconfirmed (while it would be confirmed under a narrower meaning). Take *pluralism*. If all societies are declared, in some sense, pluralistic, then the generalization ‘pluralism falls and stands with democracy’ no longer holds. Another good example is *mobilization*. If the concept is stretched to the point of including both self-motion (participation as a voluntary act) and its obverse, namely, hetero-motion, being coerced into motion (mobilization proper), then we do not only have a perfect cat-dog but we may also end up with a cat-dog fight in which the two components eat up each other. *Ideology* would be a further excellent instance of a concept deprived of all heuristic validity, let alone testability, by having been stretched to a point of meaninglessness. As currently used and abused, the word ideology never ceases to apply (it has no opposite), everything is ideology, and thus a worthy tool of analysis is turned – following up on the dog-cat imagery – into zoology in its entirety.

It may look as though I have already moved from the question, *What is comparable?* to the question, *How compare?* Even so, a number of method-specific issues still lie ahead.

Compare How?

There are many possible ways of conceiving the basic, general strategy of scientific inquiry. My favorite is the one outlined by Smelser. The initial picture of any phenomenon that a social scientist attempts to explain, he writes, ‘is one of a *multiplicity* of conditions, a *compounding* of their influences on what is to be explained (the dependent variable) and an *indeterminacy* regarding the effect of any one condition or several conditions in combination’. In order to reduce the number of conditions, to isolate them,

and to specify their role, the investigator is required (i) to organize the conditions into independent, intervening and dependent variables, and (ii) to treat some causal conditions as *parameters*, parametric constants or givens (as when we invoke the *ceteris paribus* clause) that are assumed not to vary, while treating other conditions as *operative variables* that are instead allowed to vary in order to assess their influence upon the dependent variable(s) (1976: 152–4). To be sure, no variable is inherently independent or dependent, and ‘What is treated as a parameter in one investigation may become the operative variable in another’ (p. 154).¹²

Another general point bears on research designs. By and large, ‘at times comparativists will emphasize similarities, at times differences. They will tend to look for differences in contexts that are roughly similar, or . . . will try to find analogies in contrasting political systems’ (Dogan and Pelassy, 1984: 127). But shifts in emphasis can also become distinct research methodologies. Most comparativists adopt a ‘most similar system’ design; but, as Przeworski and Teune point out, one can also abide by a ‘most different system’ design (1970: 31ff.). In the most similar system strategy, the researcher brings together systems that are as similar as possible in as many features (properties) as possible, thus allowing a large number of variables to be ignored (under the assumption that they are equal). Simply put, a most similar strategy (as with area studies, Anglo-American countries, and the like) assumes that the factors that are common to relatively homogeneous countries are irrelevant in explaining their differences. The recommendation thus is: choose entities that are similar, if possible, in all variables, with the exception of the phenomenon to be investigated. Sure. But the reverse way of attacking the problem is to choose the most different systems, that is, systems that differ as much as possible and yet do not differ on the phenomenon under investigation. In the example of Przeworski and Teune, if rates of suicide are the same among the Zuni, the Swedes, and the Russians (indeed utterly different systems), then systemic factors are irrelevant for the explanation of suicide and can be disregarded (p. 35). Again, fine.¹³

Rules and Exceptions

Entering more troubled waters, an issue that recurs without convincing answers is when and to what extent exceptions kill a rule, that is to say, law-like generalizations endowed with explanatory power. Of course, if we

12. While I quote from Smelser’s more recent writing, one should also look into Smelser (1966 and 1967, *passim*).

13. Whether the most different systems design differs from the most similar one in that the former consists of multilevel analysis and must observe ‘behavior at a level lower than that of systems’ (as its proponents hold, p. 34) is a differentiation open to question. The point remains that seeking contrast and seeking similarity are different approaches.

assume that a law is 'deterministic', just one exception suffices to kill it. But far more often than not we declare our law-like generalizations 'probabilistic', and this appears to let us off the hook. Does it? Note, first, that the argument is not reserved to statistical laws, for which it is impeccable, but extended to any law-like explanation. If so, what does probabilistic mean when no mathematical power is attached to the notion? It can only mean, I believe, that we are dealing with 'tendency laws' with respect to which one or few exceptions do not entail rejection. Even so, exceptions are disconfirming; but it is too easy to leave the point at that.

Assume that our laws are given the *if then* form – a formulation conducive to condition analysis. Assume further, that 'if' does not stand for sufficient but for necessary conditions, the conditions without which a law does not apply. Thus, the spelling out of necessary conditions specifies when a law is, or is not, applicable; and the addition of further necessary conditions restricts the ambit of its applicability. For the issue at hand this means that exceptions are handled (reduced) by reducing the range of application of a rule on the basis of its necessary conditions. For instance, Galileo's law of falling bodies was bound to be experimentally disconfirmed unless the necessary condition 'falling in a vacuum' was entered. There is, however, another way of handling the problem, which is to reformulate a law in such a way as to incorporate its exceptions.¹⁴ And it is only when both strategies have been pursued to their reasonable point of exhaustion that a rule may be retained (if at a low level of confidence) by explaining away its exceptions on *ad hoc* grounds.¹⁵ But it is impermissible, I submit, to declare a law 'deterministic . . . with the exceptions noted' (Riker, 1982: 761). This statement, as stated, compounds two logical errors.

The Case Study

Another unsettled issue bears on how case studies – especially of the 'heuristic' and the 'crucial' variety¹⁶ – relate to the comparative method.

14. Both strategies are discussed and illustrated at some length in Sartori (1986: 48–50 and *passim*). Take the 'rule' that says: 'a plurality system will produce . . . a two-party system . . . under two conditions: first, when the party system is structured, and, second, if the electorate which is refractory to whatever pressure of the electoral system happens to be dispersed in below-plurality proportions throughout the constituencies' (1986: 59). Here the first condition enters a necessary condition, and the second one actually incorporates in the law the exceptions resulting from above-plurality or above quotient distributions of incoercible minorities.

15. My argument is confined to 'rule disconfirmation'. Generally, and in principle, a theory T is falsified, and thus rejected, 'if and only if another theory T^1 has been proposed with the following characteristics: (1) T^1 has excess empirical content over T . . . (2) T^1 explains the previous success of T . . . and (3) some of the excess content of T^1 is corroborated' (Lakatos, 1970: 116).

16. These are the labels employed by Eckstein (1975: 80ff.). Lijphart (1971: 691–3) also discusses the various uses and types of case studies. By combining the wordings of the two

I must insist that as a 'one case' investigation the case study cannot be subsumed under the comparative *method* (though it may have comparative *merit*). On the other hand, comparison and case study can well be mutually reinforcing and complementary undertakings. My sense is that case studies are most valuable – for the comparativist – as hypothesis-generating inquiries. They cannot confirm a generalization (one confirmation adds confidence, but cannot add up to a confirming test), and they can only disconfirm a regularity to a limited degree. But heuristic case studies do provide an ideal – perhaps the best – soil for the conceiving of generalizations. If so, however, case studies are first and foremost part and parcel of theory-building (as Eckstein underscores), not of theory-controlling.¹⁷

Incommensurability

We are ready for the most crucial and, at the same time, most unsettled issue of the lot. Let us go back, in order to confront it squarely, to the knock-down question: Is the comparative enterprise at all possible? There have been all along many ways of formulating this fundamental objection. The more recent one rallies the negators under the banner of a so-called 'incommensurability of concepts'. In my understanding, incommensurable basically conveys that we have no measure, or no common measure, for something. If so, what I have been saying is hardly affected by an incommensurability indictment. However, 'incommensurability' is currently brandished in a strong sense that implies that all our concepts are context-embedded to the point of being inescapably idiosyncratic.¹⁸ This is an overkill – if anything, concepts are generalizations in disguise, mental containers that amalgamate an endless flow of discrete perceptions and conceptions.

But to dismiss incommensurability in its extreme claim is not to dismiss the one-century-old distinction, put forward by Rickert and Dilthey, between idiographic and nomothetic sciences. In their understanding (which

authors one can distinguish among the following five kinds of case study: (1) configurative-idiographic (Eckstein), (2) interpretative (Lijphart), (3) hypothesis-generating (Lijphart), (4) crucial (Eckstein), that is, theory-confirming or disconfirming (Lijphart), (5) deviant (Lijphart). An outstanding instance of the latter is Lipset et al.'s *Union Democracy* (1956), in which the International Typographical Union is systematically studied as a 'deviation' from Michels' iron law of oligarchy.

17. Note that my distinction between case study and comparison does not imply in the least that the latter is a superior form of inquiry. If, as Eckstein (1975: 88) holds, 'The quintessential end of theorizing is to arrive at *statements of regularity*', then the distinctive claim of the comparative method is not the discovery of 'rulefulness' but its testing. There are many paths, not only the comparative one, that lead to discovery of law-like regularities.

18. This extreme view is drawn from Feyerabend (1975), whose epistemological stance is that (i) theory determines concepts, and that (ii) data themselves are a function of theory, so that data described in terms of theory A cannot be 'compared' to data stated in terms of theory B. For a rebuttal, to which I subscribe, see Lane (1987).

preceded the *Annales* school) historians addressed the unique, thus coming in on the side of what we call a configurative, context-embedded focus. Conversely, the natural sciences were nomothetic, sought laws, and thus dissolved singularity into generality. Here, then, we do not have inescapable prisons of closed incommensurables, but an alternative that allows for trade-offs between gains and losses. On balance, case studies sacrifice generality to depth and thickness of understanding, indeed to *Verstehen*: one knows more and better about less (less in extension). Conversely, comparative studies sacrifice understanding-in-context – and of context – to inclusiveness: one knows less about more.

Is there no way of bridging this gap? In theory, that is, methodologically speaking, we do have to choose between alternative strategies of inquiry. In practice, that is, in our actual proceedings, the comparativist is required to draw on the information provided by single-country, configurative studies; and, conversely, the single-country specialist who ignores comparative findings harms his own endeavor.

Take, to illustrate, the topic of corruption. To the contextualist corruption in, say, Egypt is only corruption in Egypt, and is not, furthermore, corruption at all, since ‘paying for a service’ is not perceived as Westerners perceive it, i.e. as an illicit and harmful social practice. Right. The finding of the comparativist will be, instead, that corruption is ‘normal’ (and quasi-universal) in the Middle East, Asia, Africa; ‘endemic’, though deprecated, in Latin America and other parts of the world; and counteracted with some (different) modicum of success in, say, some 20 to 30 Western-type countries. Wrong? No, because his point is to assess the extent to which, across the world, bureaucrats, politicians and eventually judges provide their services on the basis of payments or gifts. The context-ignorant comparativist is likely to be wrong, however, in the interpretation and, in its wake, in the explanation. What he observes – his common denominator – is *a particular class of exchanges*, not corruption as bribery, not the subclass ‘illicit exchanges’. Should we leave this at saying that the contextualist and the comparativist both discover half-truths? Certainly not. But we now need a theoretical framework that accommodates the two halves. In such a framework the general category would be exchange, its subsets ‘economic’ versus ‘extra-economic’ exchanges, and the explanation (leading, in its refinements, to a causal argument) might roughly be that extra-economic exchanges become dysfunctional, illicit and morally wicked when politics reach the stage of structural differentiation that provides – in Max Weber’s terminology – for a ‘rational-legal’ bureaucracy. It is only when a civil service is paid for its services by the State (i.e. by tax revenues), only when judges become civil servants (on a payroll), and when, in turn, politics is no longer conceived as a wealth-making resource, it is only at this point that the citizen comes to expect services in exchange for nothing and that corruption and bribery are perceived as wrongdoings. Note, incidentally, that this

framework not only endows the comparativist with the amount of contextual understanding that he needs; it also suggests that the Egypt-only specialist might be wrong in overstressing Egypt's uniqueness.

Individualizing vs. Generalizing

The methodological point remains, to be sure, that we *are* confronted with an alternative between individualizing and generalizing. Even so, the alternative is not intractable and bridges do exist that help us to switch from generalization to context, and vice versa. In a much quoted passage Sidney Verba makes this convergence appear as a self-defeating sort of vortex, as an entanglement that ends in strangulation (of comparative politics):

To be comparative, we are told, we must look for generalizations or covering laws that apply to all cases of a particular type. . . . But where are the general laws? Generalizations fade when we look at the particular cases. We add intervening variable after intervening variable. Since the cases are few in number, we end with an explanation tailored to each case. The result begins to sound quite idiographic or configurative. . . . As we bring more and more variables back into our analysis in order to arrive at any generalizations that hold up across a series of political systems, we bring back so much that we have a 'unique' case in its configurative whole (1967: 113).

The foregoing may be a truthful account of how we have been messing things up, but should not be taken as a recipe for making headway. 'Where are the general laws?' Well, nowhere – for even if we were capable of formulating them (and we are not: see Sartori, 1986) the cat-dog would kill them. 'Adding intervening variable after intervening variable' certainly is a wrong way of proceeding. My suggestion has long been (Sartori 1970: 1040–5; 1975: 16–19; 1984: 44–6) that an orderly way – indeed, method – of relating universals to particulars is to organize our categories along a *ladder of abstraction* whose basic rule of transformation (upward aggregation and, conversely, downward specification) is that the connotation (intension) and denotation (extension) of concepts are inversely related. Thus, in order to make a concept more general, viz. of increasing its traveling capability, we must reduce its characteristics or properties. Conversely, in order to make a concept more specific (contextually adequate), we must increase its properties or characteristics. As I was saying, the problem is not intractable.¹⁹ But some routes are more difficult to travel than others. The one that I propose admittedly requires painstaking thinking, while it is infinitely easier to behead problems by invoking incommensurability or by letting computers do our work while we relax.

19. Even so, it cannot be handled, I believe, by assuming as Przeworski and Teune (1970: 12) do that 'most problems of uniqueness versus universality can be redefined as problems of measurement'.

Conclusions

Vis-a-vis the high hopes of three decades ago, comparative politics is, to say the least, a disappointment. In the early 1960s the survey of Somit and Tanenhaus (1964: 55–7) indicated that comparative politics was seen as the field in which ‘most significant work was being done’. But only a few years later Verba asked himself: ‘why has there been so much movement and so little movement forward?’ (1967: 113). In part, he replied, ‘the answer is in the toughness of the problem’ (1967: 113). Quite. The other part of the answer is, however, that a discipline without logical, methodological and linguistic *discipline* cannot solve, but only aggravate problems for itself.

In the last forty years or so, we have enjoyed moving from one ‘revolution’ to another: behavioral, paradigmatic, ‘critical’, postpositivist, hermeneutic, and so on. But revolutions (in science) just leave us with a new beginning – they have to be followed up and made to bear fruits. We have, instead, just allowed them to fade away, as ever new beginnings hold ever new promises which remain, in turn, ever unfulfilled. In the process the simple basics that I have been addressing in this essay have gotten lost. David Collier (1991) has provided an assessment of the issues of comparative method debated in the last twenty years. Since Collier’s coverage is excellent, it is highly telling that the control purpose of comparing is nowhere covered. Yes, our sophistication has grown – but at the expense of an increasingly missing core. As is shown by growing numbers of comparativists (in name) that never compare anything, not even ‘implicitly’, thus forsaking standardized labels, common yardsticks and shared parameters. Let us squarely face it: normal science is not doing well. A field defined by its method – comparing – cannot prosper without a core method. My critique does not imply, to be sure, that good, even excellent, comparative work is no longer under way. But even the current good comparative work underachieves on account of our having lost sight of what comparing *is for*, distinctively *for*.

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