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MEANS AND ENDS OF COMPARISON IN MACROSOCIOLOGY

Charles Tilly

ABSTRACT

John Stuart Mill's own warnings rule out the application of his experimental methods to social processes. Although previously popular in the social sciences, big case comparisons are properly disappearing. Social scientists should shift to the search for general causal mechanisms in multiple, never repeated, structures and processes.

Variation *in vitro* differs significantly from variation in natural history, a *fortiori* from variation in social history and macrosociology. After laying out his famous Methods of Agreement and of Differences, as well as his often-ignored Methods of Residues and of Concomitant Variation, John Stuart Mill reminded readers that his Methods applied exclusively to experimental procedures. Mill confined them, furthermore, to relatively simple phenomena entailing little interaction among causes, which meant they would not much advance understanding of living organisms. He therefore issued a stern warning:

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If so little can be done by the experimental method to determine the conditions of an effect of many combined causes, in the case of medical science; still less is this method applicable to a class of phenomena more complicated than even those of physiology, the phenomena of politics and history. There, Plurality of Causes exists in almost boundless excess, and effects are, for the most part, inextricably interwoven with one another. To add to the embarrassment, most of the inquiries in political science relate to the production of effects of a most comprehensive description, such as the public wealth, public security, public morality, and the like: results likely to be affected directly or indirectly either in *plus* or in *minus* by nearly every fact which exists, or event which occurs, in human society. The vulgar notion, that the safe methods on political subjects are those of Baconian induction—that the true guide is not general reasoning, but specific experience—will one day be quoted as among the most unequivocal marks of a low state of the speculative faculties in any age in which it is accredited. Nothing can be more ludicrous than the sort of paradoxes on experimental reasoning which one is accustomed to meet with, not in popular discussion only, but in grave treatises, when the affairs of nations are the theme. "How," it is asked, "can an institution be bad, when the country has prospered under it?" "How can such or such causes have contributed to the prosperity of one country, when another has prospered without them?" Whoever makes use of an argument of this kind, not intending to deceive, should be sent back to learn the elements of some one of the more easy physical sciences (Mill 1887, p. 324).

Later, Mill identified the chief difficulties in applying his experimental methods to human affairs: not only the complex interaction of causes, but also the fact that his methods required *a priori* a finite, specified set of hypothetical causes. Aimed at social processes, Mill's Methods remained always, fatally vulnerable to the allegation that a hitherto-unsuspected cause was operating.

No one has much improved on Mill's own initial statement of objections to application of his four experimental methods in the explanation of social processes. Yet, as John Goldthorpe complains, twentieth century social scientists have often invoked the Method of Agreement and the Method of Differences as justifications for big case comparisons—hereafter BCC. In this invited response to Goldthorpe's analysis, I will neither recapitulate the independent critique of BCC I have presented *ad nauseam* elsewhere nor describe in any detail the alternatives to BCC I have advocated and practiced incessantly for many years; this discussion focuses on what Goldthorpe says about BCC.

Goldthorpe rightly claims that switching from "variables" to "cases" does not mitigate the problem of coherent comparison; in fact, it makes Mill's own strictures all the more applicable. If they had listened to Mill, social scientists would never have adopted BCC. Goldthorpe misses the crucial next step. Small *Ns*, Galton's diffusion processes, and appeal to black-box causation do bedevil many applications of BCC, but all constitute soluble secondary difficulties. Here is the primary difficulty: BCC provides a fine heuristic but a logically and ontologically flawed basis for serious explanation of social processes. Although they might not have adopted the Comtean evolutionist approach that Mill himself advocated, from the start attentive readers of John Stuart

Mill should also have rejected the program Edward Tylor styled the Comparative Method in 1889. No less a figure than Francis Galton, after all, identified the program's crippling weaknesses at its very unveiling (Hammel 1980). Yet only now, more than a century after Tylor's explication of the Method, is the program collapsing. Its charms long led social scientists to ignore its fatal vices.

As a program for investigating, writing, teaching, communicating, and job-creating, comparative-historical analysis in the BCC mode has seen very good days. Those days will soon pass. Vital, vibrant work on big structures, large processes, and huge comparisons in space-time will continue in sociology and other social sciences. Historical inquiry will thrive, but not in the mode that has come to define the field during the last scholarly generation: BCC. The lining up of civilizations, societies, cultures, wars, revolutions, and other great chunks of social experience for arguments about causes and meanings will persist as the heuristic and literary trope it has been for hundreds of years, but will shrivel as a method of systematic analysis. BCC will shrivel for several reasons: because its faulty ontological premises are finally outweighing its undoubted contributions as a means of disciplining inquiry; because the system of distinct, bounded sovereign states that long served as its implicit warrant is rapidly disintegrating; because the rise of relational, historicist, and institutional thinking in the social sciences is raising insuperable challenges to all portrayals of social life as the work of neatly-bounded, self-motivated, rule-following actors, individual or collective.

Comparison of large social chunks in search of invariant laws has marked the social sciences since their emergence as self-regarding disciplines—certainly since 1889. In different styles, Max Weber, Oswald Spengler, and Pitirim Sorokin exemplified and justified sociologists' investment in vast comparative enterprises. During the 1940s, big comparative-historical inquiries lost much of their lustre in sociology—in 1959, the American Sociological Association-sponsored volume *Sociology Today* surveyed the whole field, but offered no sustained discussion of historical or comparative analysis—only to revive handsomely with S. N. Eisenstadt, Reinhard Bendix, Stein Rokkan, Barrington Moore, Jr., and others from the late 1950s onward. That second wave is now subsiding. The sea will survive, but its chief currents already run in other directions.

In their time, historical-comparative inquiries provided splendid antidotes for unhistorical and antihistorical maladies in social science. However one disagreed with them on other grounds, such masters as Bendix, Rokkan, and Moore made evident how greatly where, when, and in what order some social process occurred mattered to *how* it occurred. They exposed the bankruptcy of the quasi-evolutionary pseudo-history in which searchers for the secrets of development lined up whole societies, generally identified by the existence of a durable state, along a single continuum from least to most advanced, then

inferred the standard developmental path from that continuum—or, worse yet, from currently-observable characteristics of its most advanced members. They validated concerns about power, freedom, and human agency bequeathed to social science by Karl Marx, Max Weber, and other ancestors. They thereby motivated rich, ambitious historical and comparative examinations of human struggles.

From early on, nevertheless, postwar historical-comparative analysis followed multiple paths in addition to the comparison of civilizations, societies, cultures, and momentous events. Inspired partly by a populist hope to reconstruct history from below and partly by collaboration with historians who were trying to renew their own craft through self-conscious adoption of social-scientific procedures, students of family structure, population processes, communities, political struggle, and economic change dug deeply into historical materials without concentrating on massive case-by-case comparisons (Abbot 1994; Monkonen 1994). Despite strident epistemological challenges from postmodern critics, such studies still thrive today (see, e.g., Hanagan 1994).

Yet the emblem of comparative-historical analysis, Big Case Comparison, is now fading. BCC is fading because of (1) ontological inadequacy, (2) disintegration of state systems, and (3) relational, historicist, and institutional thinking.

Ontological inadequacy? The presumption that distinctive, autonomous, coherent, self-sustaining civilizations, societies, cultures, and/or great events not only exist but possess their own logics *sui generis* undergirds the BCC program. Where empirically-identifiable states, organizations, networks, or connected sequences of action actually constitute the objects of study, to be sure, social scientists have ample reasons to formulate ideas concerning their regularities and to undertake systematic comparisons among them. But presuming their intelligible existence *a priori*, inferring the coherence of societies from the presence of states, or taking historically-constructed memories of events—wars, revolutions, social movements, transitions, or others—as grounds for their comparative study founds analysis on the fallacy of misplaced concreteness. Half-aware of the difficulty, many of BCC's most ardent practitioners are abandoning it for historically-grounded studies of social processes (Lloyd 1993; Smith 1991).

Disintegration of the state system? Implicitly or explicitly, the BCC program has always relied on presumptions about the division of the world into coherent nations and states, presumptions that only became prevalent with the consolidation of the European state system and its rapid seizure of world power during the nineteenth century (Thomson 1995). Whether consolidated states as the world has known them for two centuries are now losing their grip or merely adapting as the world-system changes remains hotly debated (Tilly et al. 1995). Massive flows of capital, labor, commodities, information, and technology across national boundaries and increasing prominence of such

transnational structures as the European Community and GATT are surely both reducing the autonomy of most states and undermining their capacity to regulate activities within their territories. Meanwhile the expansion of communal-ethnic struggles over political power within existing states (Gurr 1994) discredits any easy equation of society or culture with state. Continuation of these trends is already attracting the attention of macroanalysts to non-national webs of social relations; it will eventually destroy the plausibility and interest of comparisons among state-defined societies (Puchala 1995; Ruggie 1993; Wendt 1994; Wendt and Barnett 1993).

Relational, historicist, and institutional thinking? As approaches in contemporary social science, we might distinguish *systems theories*, with collectivities (including that great collectivity called Society) following autonomous and compelling logics; *methodological individualism*, with its reduction of social reality to the self-motivated actions of individual actors; *phenomenological individualism*, with its parallel reduction of social reality to the consciousness of actors, individual or collective; and *relational realism*, with transactions, interactions, or social ties serving as starting-points of social analysis. The first three have run their course, while the fourth is gaining strength. In a wide variety of fields, furthermore, the idea of incessant human improvisation that lays down subsequent constraints on behavior in the form of memory, culture, institutions, and social ties contradicts any possibility of chopping social life into neatly-bounded, self-motivated, rule-following actors, individual or collective (Friedman 1995; Nelson 1995; Resnick 1996; White 1992). Macroanalysis will benefit enormously from these new ideas about social process, but not through a continuation of Big Case Comparison. In that sense, the once-dominant program of comparative-historical social science is now *writing fims*.

John Goldthorpe has in fact recently been writing anticipatory obituaries for BCC (e.g., Goldthorpe 1991). He has, however, emphasized secondary traits of our moribund friend. The situation is both worse and better than Goldthorpe claims. Worse, because social scientists including Goldthorpe have wasted a great deal of time fretting about the logic of comparing whole countries to account for similarities and differences among those countries, when for most purposes they should simply have eschewed such comparisons. Better, because social scientists have always had more effective explanatory logics available than BCC. For effective social science, like effective science of any other kind, does not concern cases or variables, but valid causal mechanisms, wherever and at whatever scale they occur.

In a limiting case—where behavior of a state or of state-circumscribed institutions is itself at issue—the state-defined country may indeed turn out to be the appropriate unit of comparison. But even there the crucial causal mechanisms will commonly operate at several different scales, and be verifiable for precisely that reason. Despite the limited scope for experiment in their

inquiries, $N = 1$ has not kept geophysicists, cosmologists, paleontologists, or ecologists from doing valuable scientific work. For practical purposes, N has equaled the number of independent observations they could make of processes in action or their outcomes. Historical students of large-scale social processes similarly take advantage of multiple purchases on crucial causal mechanisms, each intervention into the historical record constituting another opportunity to be proven wrong.

On what grounds, for example, do most students of state formation believe that (a) under a wide, roughly specifiable set of historical circumstances successful warfare creates states, and (b) in those circumstances different organizations of warfare produce systematically different state structures (Porter 1994; Rasler and Thompson 1990; Starr 1994)? They believe those propositions not because of large- N statistical analyses or neat John Stuart Millian comparisons of cases but because for a large range of times, places, and situations they can construct relevant, verifiable causal stories resting on differing chains of cause-effect relations whose efficacy can be demonstrated independently of those stories. They also believe the propositions because they look robust over many kinds and scales of evidence, from statistical analyses of wars to close reconstructions of particular historical sequences.

That scholars will eventually supersede such gross, imprecise propositions with more refined, more adequate, and partly contradictory analyses does not gainsay the superiority of the search for widely applicable cause-effect relations over BCC and related searches for invariant sequences or structures. If Goldhorpe rightly stresses the impossibility of identifying such causal mechanisms by means of pure induction from case studies, he somehow fails to recognize the possibility of deducing relevant hypotheses from historically-grounded theories of the middle range (Merton 1957, p. 9).

Relevant causal situations far exceed the domain of neatly-bounded, mutually-exclusive, substantial states. States have been forming in various parts of the world for roughly sixty centuries. In most of those times and places, warmaking has dominated state formation. In a nice dialectic, the massive creation of military forces during the last two centuries has actually attenuated the impact of military activity on state structure both (a) through promoting the creation of civilian organizational infrastructure having its own autonomous weight and (b) through reliance on implicit bargains with major political actors that thereby have gained the power to steer the state toward their own interests.

Cause-effect relations linking state structure to military activity include the generalization of concentrated coercive means to non-military compulsion, the creation of centralized administrations as a by-product of extracting means for war, and bargaining with civilian populations over those means. Like the causal mechanisms to which geologists and ecologists appeal, such causal mechanisms appear in different combinations and sequences, with different

weights, in concrete historical situations (Stinchcombe 1978a). No more than any geologist imagines all mountains to form as minor variants on the same model does an intelligent analyst of state structure confine the military-state relation to a single invariant pattern; like a wise geologist, she shows how widely-applicable causes concatenate into substantially different outcomes depending on initial conditions, subsequent sequences, and adjacent processes. Although all analysts can—and frequently do—aggregate these causes to a national scale, in fact they operate at many scales, from encounters between households and tax collectors to the settlements through international intervention of national rebellions and civil wars. Hence the possibility of verifying the efficacy of ostensible causes at one scale, then aggregating or disaggregating them to trace their analogs at other scales.

Do the causal mechanisms involved reduce ultimately to the rational actions of motivated individuals? Some do, most don't. More of them correspond to the complex, contingent, collective effects of social interaction dealt with by evolutionary economists, transaction-cost organization theorists, and network analysts (Baron 1984, Bowles and Gintis 1993; Granovetter 1988; Merton 1936; Nelson 1995; North 1991; Portes and Sensenbrenner 1993; Simon 1991; Stinchcombe 1978b). Warfare generates centralized administrations, for example, in part because through no one's intention the seizure of means for military action—men, horses, food, clothing, weapons, information, and money—disrupts non-military routines, creates new social connections among both rulers and ruled, alters the physical environment, produces perverse effects, and stimulates concerted popular resistance. Each of these effects calls forth remedial action on the part of authorities. Repeated, with their own unanticipated consequences and indirect effects, those remedial efforts constitute central administrations. Such a causal web certainly includes intentional action, but much of it consists of errors, unanticipated consequences, indirect effects, alterations of social networks, and influences mediated by the non-human environment.

In these regards, each state has its own distinctive concatenation of causes; the generation of central administration by land warfare operated differently (and less pervasively) in Holland than in neighboring Prussia. It does not follow, however, that the causes operated chiefly, much less exclusively, at the scale of states. Social scientists have often slipped into the fallacious assumption that if two comparable social units differ with respect to some attribute the difference between them must result from differences in other attributes of the same social units; they have relied on monad individualism writ large, a generalization to social aggregates of the idea that the cause of any individual's behavior must be some propensity, trait, or decision of that same individual (Bhargava 1992). In fact, differences among social units commonly result from locations in social networks, from environmental effects, from localized events that cumulatively affect the unit as a whole. An eternity of correlating and

comparing aggregate characteristics of the units will never identify the crucial effects.

Do we need other examples? We could draw them from the historical study of citizenship, where lawful but variably-conjoined causal mechanisms at other scales than the nation clearly contributed to what we now see as entrenched national differences (Cerutti, Descimon, and Praak 1995; Cohen and Hanagan 1995; Somers 1993). We could examine gender inequality in employment, where effects of state policy and educational systems certainly appear, but the great bulk of variation depends on different concatenations of causal mechanisms—notably the fine segregation of jobs—that appear widely across the world (Bielby and Baron 1986; Blau and Kahn 1992; Charles 1992; Petersen and Morgan 1995). We could turn to genocide, infant mortality, aging, nationalism, democratization, revolution, income inequality, or racism: measurable and existentially significant international differences in all these regards exist. They result in part from events and policies at a national scale. Yet as normally practiced Big Case Comparison can do no more than discipline our thinking about these complex phenomena in preparation for genuine explanatory efforts.

It makes little difference whether we choose large-*N* multivariate analyses or small-*N* case studies. If we are to arrive at explanations, we will have to construct relevant, verifiable causal stories resting on differing chains of cause-effect relations, relations whose efficacy can be demonstrated independently of those stories. Those stories will feature strong contingency and path-dependency. Their validity will ultimately depend not on Millian experimental logic, not on deductions from covering laws, not on precise multivariate analyses, but on the demonstrated presence and robustness of the causal mechanisms they entchain.

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I have incorporated in this paper most of my "Macrosociology Past and Future," *Newsletter of the Comparative & Historical Section*, American Sociological Association, 8 (1995), pp. 1, 3, 4, and have built on ideas laid out more extensively in "The Bourgeoisie Gentilshommes of Revolutionary Theory," *Contention* 2 (1993), 153-158, "To Explain Political Processes," *American Journal of Sociology* 100 (1995), 1594-1610, "Invisible Elbow," Working Paper 221, 1995, Center for Studies of Social Change, New School for Social Research, "Durable Inequality," CSSC Working Paper 224, 1995, and "Citizenship, Identity and Social History," *International Review of Social History* 40, supplement 3 (1995), 1-17. I claim to have practiced what I preach, among other places, in *The Contentious French* (Cambridge: Harvard University Press, 1986), *Coercion, Capital and European States, 990-1990* (Oxford: Blackwell, 1990), *European Revolutions, 1492-1992* (Oxford: Blackwell, 1993), and *Popular Conception in Great Britain, 1758-1834* (Cambridge: Harvard University Press, 1995). *Big Structures, Large*

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Processes, Huge Comparisons (New York: Russell Sage Foundation) made the essential points in 1985, but subsequent theorists and practitioners did not hear them; perhaps the more strident tone of this essay will catch their attention.

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