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IN DEFENSE OF JARGON *

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The speakers of Jargon are coming. Beware ! King Jargon of Akkademe, a sociologist, leads them. If we are to believe a recent reviewer in the *Times Literary Supplement,* they come laden with heavy equipment. One of the jargonists, says he,

> armed with a battery of compasses, field glasses, range-finders, altimeters, so loaded down with apparatus that his prose limps heavily from one demonstration to the next, eventually ends up more or less where other, less prudent, more haphazard travellers have preceded him, after crashing impetuously through the bush, or simply following their eyes and noses.

The anonymous reviewer's peeves strikingly resemble those of the British historian of France, Richard Cobb, who a few weeks earlier had warned an American audience against the poisonous spread of "faceless" history, of sociological history.

The witty *TLS* review had made me reflect again on the alleged compulsion of sociologists who tread on historical terrain to bring with them incomprehensible vocabularies and useless apparatus. I have come away from the reflection thinking there is good reason for historians to form that impression of some sociological work, but even better reason for historical sociologists to continue the habitual practices which sometimes give rise to such an unhappy impression.

During the Second World War, American schoolboys and soldiers carried around little cards displaying the profiles of the chief varieties of German and Japanese aircraft, and were exhorted to memorize those fateful outlines. Know your enemy ! How should historians recognize the invading sociologists ? Not by their profiles, but by their actions.

Since people began bothering to distinguish them from historians, sociologists have returned to the lands of their forefathers by three different routes. Sometimes they have dipped into the past for cases to test hypothetical uniformities with no particular historical content, much as anthropologists occasionally attempt to determine which features of kinship systems depend on each other by examining their covariation over a large sample of societies. Swanson's analysis of the conditions under which various kinds of theologies appear, for example, considers the Romans, Egyptians, Aztecs and Israelites along with the Bemba, Iroquois, Lepchas and Nyakyusa.

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Sometimes the sociologists have sought to identify uniformities in history, or even general laws of change. In doing so, they have shuttled between amusing and infuriating the historians, occasionally making such colossal claims or blunders as to encourage Karl Popper's conclusion that sociology, as a discipline devoted to extracting a general law from a single case — the whole of human history — is impossible. When Crane Brinton, prefacing *The Anatomy of Revolution,* announces he is about to commit a social science, he has a modest form of this search for universals in his mind.

Instead of treating history as a sample bin or a huge block to be carved into identical segments, however, the sociologists have sometimes taken on tasks from the historian's own normal workload. One of the disconcerting things about Neil Smelser's study of *Social Change in the Industrial Revolution* is that (whatever else it does) it makes a series of assertions about what was going on in the relation between family and economic enterprise in nineteenth-century Britain which matter for the historical interpretation of the period itself. Smelser's painstaking review of the evidence on child labor in the cotton textile industry has forced even those students of the early industrial revolution who have no faith or interest whatsoever in broad statements about social change to ask themselves whether the real strain and protest did not arise when children began to work independently of their parents (under improving conditions of wages, hours and comfort) rather than when the children were first drafted to work long and hard in factories. Nor was this something *any* slogger would have found, since Frances Collier's doggedly detailed analysis of the same subject for the same period quite missed the point.

As it happens, I have serious reservations about the general scheme which brought Smelser to reexamine changes in the cotton textile industry, and am far from prepared to count his discoveries as corroboration of the scheme. Yet I am sure that thinking schematically — thinking sociologically — encouraged him to frame stimulating new questions and search for valuable new evidence. Even Edward Thompson, than whom few historians could be more unsympathetic to Smelser's portrayals of British workers' movements as responses to "strain" and "role-conflict", concedes that Smelser's method opened up serious and neglected questions to investigation.

This is the moment to discard the metaphor of invasion and battle which has served us so far. Instead of juvenile gangs of historians and sociologists, one armed with zip guns, the other with clubs, contesting well-marked turfs, we might better think of the descendants of two peoples once distinct though drawn from the same general stock, long settled together, still displaying some average differences in accent or hair color, recalling in moments of stress and on ritual occasions (such as this one) their traditional rancors and rivalries. No doubt that is why my colleague

S. D. Clark says that nothing but the "biases and prejudices inherited from the past" set off sociology from history, and why E. H. Carr declares that "the more sociological history becomes, and the more historical sociology becomes, the better for both".

Yet the nuances of accent linger and matter. The ritual accusations hurled by historians at sociologists have some foundation. They caricature practices which in fact appear more frequently in the work of men calling themselves sociologists than in the work of men calling themselves historians. Sometimes the sociologists, overzealous for useful lessons overlearned, caricature themselves.

Three practices make the biggest differences. They are the insistence on explicit conceptualization, the use of systematic comparison, and the attempt at objective verification. In caricature, verification comes out apparatus, comparison comes out unhistorical analogy, and conceptualization comes out jargon. The three practices hang together, since the concepts set the terms of the comparison and the verification ordinarily pushes the comparison into finer detail.

Why concepts ? Wrong question. No historical work proceeds without concepts, but they are usually implicit — notions like revolution, or power, or solidarity, playing a large but unseen part in the reconstruction of the past, and appealing to an unstated common understanding between writer and reader. Sir Lewis Namier's work depends heavily on agreed meanings for the words "friends", "interest" and "transaction". Historians seem to hear the call to define and lay out concepts deliberately when the subject is technical and unfamiliar or when criticisms of previous efforts have centered on their vocabularies. These days, for example, when an author brings the vocabulary of class into play he ordinarily takes pains to attach his treatment to one or another of the standard ways of identifying classes and dividing them up.

The sociologists, having often been burnt in the attempt to let common agreement on the content of such everyday terms as "class", "development", "urban" and even "family" do their conceptual work for them, now habitually take such pains. At times the pains make outsiders suspect masochism or exhibitionism; why five twisted pages to define "role" ? At times staggering files of definitions parade — in circles — as coherent theories. Yet the conceptual efforts have paid handsomely in such specialties as demography, where only by keeping a tight hold on the distinctions among longevity, mortality, life expectancy and median age were the practitioners able to work out how a declining death rate, with people living longer, commonly and unexpectedly produces a younger population. Here, jargon wins while common sense stumbles off the track.

So far, few historians would disagree in principle, whatever their feelings about the awkwardness with which the sociologists perform their definitional dances. But jargon, as we have been discussing it here, also predisposes its user to one side of an argument which sets historians to clawing at one another; reliving the event through empathy with its principal actors versus reconstructing it from the outside.

The imaginative reconstruction of past states of mind focuses attention on individual actors — or collections of people treated as if they formed a single actor — and often relies on hidden postulates of universal motives like greed, fear and anxiety. A dozen years ago, as I began to study counter-revolutionary activity in western France during the 1790s, the feature of the varying interpretations and controversies clinging to the counter-revolution which struck me most was their enormous emphasis on the state of mind of "the peasants" considered as a bloc. Not to mention their utter disagreement on that state of mind. It occurred to me, with a sociologist's usual predilections, that it might be useful to ask who the peasants were, how they were organized, in what respects they *could* be considered a single actor.

Asking such questions did not charm their answers from the documents. It did help me to search for information on the organization of villages before the Revolution, to realize that the peasants so often characterized as one were not only decidedly various, but also hid an important and agitated contingent of domestic textile workers, to locate potent local divisions within a rural population which historians had treated as homogeneous, to get an idea of the connection between those local divisions and the outbreak of violent counter-revolution, to recognize the futility and irrelevance of the unending debates over such questions as whether (on the day the great insurrection of 1793 began) the peasants went *en masse* to beseech nobles to lead them.

Some people will say my piecing together of the counter-revolution's social context entailed much more imaginative reconstruction — not to say outright fantasy — than I admit. Perhaps. My point is simply that in much conventional history the imaginative reconstruction of the states of mind of a limited number of crucial actors is the very eye of the analysis, while in another, more sociological, sort of history, a different sort of question and a somewhat different range of evidence comes into play.

This second sort of history gives more value to information from outside the immediate historical setting; the self-conscious decision to deal with villages as units calls to mind what is already known (or believed) more generally about the character and complexity of villages, and thereby muffles the impulse to assign a single mentality to all the villagers. When Eric Wolf writes his sparkling history of rural Central America and

Richard Morse his lucid history of urban South America, their knowledge of how the institutions of villages and cities vary, their skill in sorting out the unique from the common, their sense of social organization in many segments of the world outside of contemporary Central or South America command respect.

The systematic comparison we have been discussing has two facets. The first is the explanation of something which happened in a certain segment of a society — only one region rebels, only one occupational group emigrates — by methodical examination of the ways in which that segment differs from the rest of society, or is like it. (Not that such comparison is uniquely sociological. Thucydides, after all, made quite a fuss over the contrast between Athens and Sparta some time before Comte coined the Greco-Latin "sociology". The study of systematic covariation over multiple units, however, beginning with the unit in which the crucial phenomenon occurs, then moving on to other units in which something else, or nothing at all, occurs, shows up only irregularly in historical work, while underlying almost everything sociologists do.)

The second facet is more debatable; it consists of the search for analogies in other places and times for the purpose of illuminating the case at hand. It often includes the importation of a vocabulary not actually used by the historical actors in question, of distinctions they did not consciously make. For reasons which would themselves be worth exploring, we tend to accept without too much discomfort the transfer to the past of economic notions like national income or liquidity and of demographic notions like life expectancy while being irritated by the use of class distinctions the people of an era did not themselves employ. In fact, this is precisely the channel by which — for better or for worse — ideas about what is true of contemporary societies and what is true of past societies flow together.

Here the caricatures begin. By uncritical use of analogies, sociologists and historians alike have at times called forth the fearsome adjective UNHISTORICAL. There is, for example, a common kind of analysis we might call Unnatural History. It often permeates discussions of revolution or of social movements; a case in point is a recurrent article in the *American Journal of Sociology*, published with insignificant variations in title, authorship and vocabulary, which asserts by means of a handful of examples that there is a single underlying Process of Revolution. At first it looks like natural history, in the sense that it portrays the standard setting and life cycle of a distinct species of event. Closer inspection usually shows that the unnatural historian has begged the question by assuming that his identification of common sequences within the events singled out *ipso facto* confirms that they belong to the same species... as if declaring that a man's life has a beginning, a middle and an end, then that a paper before a learned society has a beginning, a middle and (thank goodness) an end established that men's lives and papers before learned societies came from the same species.

When we do have grounds for assigning several beasts to the same species, on the other hand, natural history and judicious analogy make good sense. For all their vagaries, cities form a distinct type of community. When Colin Clark tells us that contemporary cities display a strikingly regular pattern of decline in density from center to periphery and that fast-growing western cities go through a cycle of spurting increase in density followed by long, steady decrease, we have the right to wonder whether the same might be true of Augustan Rome or Capetian Paris. The yes-no-or-maybe answer would tell us something interesting both about ancient Rome and about contemporary cities.

By considering how one would find out whether the similarities are great or small, we move over into the problem of verification. Observers of American academia have all noticed the swing sociology made after 1920 from incredible abstraction to unbearable concreteness, whence its reputation for verifying the obvious and/or trivial. One of our graduate students, steeling himself for a grim general examination, recently composed a more diverting counter-examination containing the following questions :

> It was said twenty years ago that a sociologist is someone who spends $40,000 to find out the address of the local whorehouse.
>
> In view of the exigencies of modern research, how much money would you apply for now for this project?

Polling techniques, statistical analyses, and, of course, electronic computers all encourage this impression of apparatus uncoupled from common sense.

Some of what passes for Method in sociology is nothing but self-deception, window-dressing or ritualized anxiety-reduction. A good many of the procedures which frighten or offend historians, however, follow directly and reasonably from a concern with explicit conceptualization, systematic comparison and objective verification.

One quintessential, everyday sociological operation sums up the way these traits come together. That is coding. The codebook is a compilation of instructions for transforming varied observations of social phenomena into standard categories. A visitor almost never sees a codebook in a historian's workshop, and hardly ever leaves a sociologist's shop without having seen one. In survey research, for example, a questionnaire often has numbered blocks running down its side, so that the coder can quickly tally males as 1, females as 2, or Yes, No, Maybe and Don't Know as 1, 2, 3, and 4, and so on; it is pre-coded. More complex codes guide the translation of general expressions of opinion, or the plots of children's stories, or the sequences of events in international conflicts.

There's the rub. Coding makes possible the sort of systematic comparison and verification I have asked you to admire. But it also forces the units into a comparable form which can be spurious, as when hastily-identified "neighborhoods" in a big city range from organized entities to occidental conglomerations. Even more crucial, the coded data become the reality. The investigator manipulates, compares, tallies, correlates, factor-analyses the coded data, not the recorded observations... and certainly not the phenomena originally observed. If you want to understand the world view a sociologist is really imposing on his work, forget his research proposal, ignore his report of findings and concentrate on two things : the data sheet and the codebook.

For years the effective world of sociologists was shaped by the phenomenology of the Hollerith card : a collection of comparable units each represented by a card, each unit bearing a set of more or less independent traits represented by the columns on the card, each trait classifiable into nine or ten mutually exclusive alternatives represented by the punches in the column. Thus varying opinions of individuals can be correlated with one another, varying problems of communities can be related to their size, varying strike patterns of industries can be accounted for by the characteristics of their workforces. New techniques relying on the computer's capacity to store, relate and transmit complex information are fast demolishing the old limits set by the Hollerith card, but many of us are finding it hurts to push our well-tamed minds past those limits.

Both the merits and the drawbacks of coding, as well as of the mode of analysis coding epitomizes, have come home to me in the course of studying changes in the character of violent collective conflicts in France since the Revolution. From the perspective of France's general history, or of political change in western Europe, or of what we all too loosely call modernization in general, the changes in the pattern of protest in France deserve close scrutiny. At present, we have some passing enumerations of conflicts in the wakes of general political histories, some monographic studies of particular upheavals deemed especially significant, some glancing treatments of strike activity, plus valuable but fragmentary suggestions (from such writers as Labrousse, Rudé and Duveau) of a shift toward a modern, industrial, urban form of collective conflict around the middle of the nineteenth century. That is not enough.

Some features of France's evolution from Louis Philippe's accession to power in 1830 to de Gaulle's accession to power in 1958 leap out at anyone who knows a modicum of French history. Urban street fighting swells and then ebbs, the village bread riot fades away to be followed by long years of rural quiescence and then to be replaced by a much more organized farmers' protest, workers go from sporadic attacks on local masters to insurrectionary strikes, to disciplined and massive demonstrations. And so on. Someone with sensitivity, a talent for apt examples and a great deal of endurance could write a very good unsociological account of the transformation of French political upheaval over that span. But to capture the regional and temporal patterns, to include the significant periods and places in which nothing happened, to get a grip on disturbances which had no obvious political impact, to test alternative explanations of these shifts, all require something very much like coding.

A group of us at Harvard (and, soon, at Toronto) are undertaking that coding. Using voluminous material from French archives, political yearbooks, newspapers and conventional secondary sources, we are attempting to enumerate, describe and code some 50,000 strikes and perhaps 4,000 violent collective conflicts occurring in France from 1830 to 1960. When this slippery mountain of data is in place, we shall try to climb and claim it by systematically examining when, where and with what participants to various forms of conflicts occurred — and, for that matter, did not occur.

Although there are great chunks of information still to be put in place, in one sense the analysis is largely completed. The selection of communes, departments, industries and individual conflicts as major units for analysis clears the way but commits us to certain comparisons while making others very difficult. The definitions and distinctions built into our ponderous codebooks mold the forms the ultimate findings can take. Our continual wrangling over the details of coding, I have realized, is not the pettifogging it seems to be; it is a debate over the articulation of theory, concept and fact; it is the means by which our understanding, or misunderstanding, of French social organization enters the analysis.

This advance commitment is risky business. Its merit is to bring the risks out into the open. For in any historical study of France's political evolution they would lurk nearby. The facts of collective conflict are so abundant and various they lend themselves to a dozen different plausible interpretations — Marxist or anti-Marxist, economic or demographic, Louis Chevalier's or Ernest Labrousse's. The facts surpass common sense. They cry for systematic verification.

The truth is that sociologists developed their obsession with verification only after stumbling repeatedly over the errors of common sense : the common sense which supposed that divorces were more frequent among the wealthy than the poor, the common sense which said that since delinquents live in bad housing the construction of good housing would end delinquency, or the common sense which imagined that when population increased rapidly the birth rate must be going up. In each case, when men got around to applying careful measures to appropriate comparisons, they found it wasn't so.

"Measures" do not necessarily mean numbers. William Whyte's remarkable *Street Corner Society* presented precious few numbers, but it established through meticulous observation that a big-city area labeled by outsiders as a disorganized slum actually lived by a complex, coherent internal organization. What matters is to establish tests which are relevant, public and repeatable. To be sure, quantifying often helps by identifying silly assumptions, pointing up unsuspected relationships and establishing whether purported differences are big enough to make a difference. But "statistical history" and "computerized history" do not necessarily stand closest to sociology. Few historical efforts could be more alien to sociological work as I have described it than François Simiand's worried search for the One True Curve to account for the oscillations of the French economy.

This portrayal of sociological method may strike you as rather imperialistic. By these definitions, some historians have been practicing sociology without a license, without any desire to obtain a license, for a long time. Indeed, I *would* be content if my discipline could take credit for Lawrence Stone's tracing of the fortunes of the British aristocracy, Albert Soboul's dissection of the Parisian working class, or Rudolph Braun's account of the disintegration of Zurich's domestic industry. Unhappily, none of the three has shown any particular eagerness to be mistaken for a sociologist.

My argument therefore comes down to saying that a certain very useful mode of analysis appears quite regularly in sociological work and rather rarely in historical work. The mode combines explicit conceptualization and identification of the units under analysis, systematic comparison of those units, and deliberate measurement of the variations among them. Done badly or misunderstood, the measurement becomes mere apparatus, the comparison becomes unhistorical analogy, and the conceptualization becomes pitiful jargon. Therefore understand, and teach us by earnest criticism to do these things well. Sociological jargon may yet find a place as one of the historical languages.