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TWO STYLES OF RESEARCH IN CURRENT SOCIAL STUDIES

C. WRIGHT MILLS

When in the course of our work we are uncertain, we sometimes become more concerned with our methods than with the content of our problems. We then try to clarify our conceptions and tighten our procedures. And as we re-examine studies that we feel have turned out well, we create conscious models of inquiry with which we try to guide our own work-in-progress.

It is in terms of these models that we sometimes gain that sense of craftsmanship that is one subjective yield of work well done.

Modern men have generally been happier in their sense of craftsmanship when they have felt that they were at least approximating the generalized model of the laboratory. "Every step in science," Charles Peirce wrote, "has been a lesson in logic." In our search for a general model of inquiry, we have usually seized upon the supposed Method of Physical Science, and we have often fetichized it.

In the sociological disciplines, this grateful acceptance of "Science" is often more formal than operative and always more ambiguous than clear-cut. As a going concern, in the social studies, scientific empiricism means many things, and there is no one accepted version, much less any systematic use of any one model of science. The same work, admired by some as "great," is disparaged by others as "journalism." Professional expectations about method are quite confused, and our sense of craftsmanship may be realized in terms of quite different modes of inquiry.

There are, in fact, at least two working models of inquiry now available in current social studies, and accordingly two senses of craftsmanship in terms of which work is judged, and on the basis of which controversies over method occur.

ONE

The first of these two research-ways might be called the macroscopic. It has a venerable history, reaching notable heights, for example, in the work of Weber and Ostrogorski, Marx and Bryce, Michels, Simmel and Mannheim. These men like to deal with total social structures in a comparative way; their scope is that of the world historian; they attempt to generalize types of historical phenomena, and in a systematic way, to connect the various institutional spheres of a society, and then relate them to prevailing types of men and women. How did the Crusades come about? Are Protestantism and the rise of capitalism related? If so, how? Why is there no socialist movement in the U. S.?

The other way of sociological research might be called the molecular. It is, at first glance, characterized by its usually small-scale problems and by its generally statistical models of verification. Why are 40 per cent more of the women who give marketing advice to their neighbors during a given week on a lower income level than those who gave it during another week? Molecular work has no illustrious antecedents, but, by virtue of historical accident and the unfor-

fortunate facts of research finance, has been developed a great deal from studies of marketing and problems connected with media of mass communication. Shying away from social philosophy, it often appears as technique and little else.

Everyone involved in the social studies will recognize these two styles, and by now, a good many will readily agree that "we ought to get the two together." Sometimes this program is put in terms of the statement that the sociologist's ideal task during the next decades is to unite the larger problems and theoretical work of the 19th century, especially that of the Germans, with the research techniques predominant in the 20th century, especially that of the Americans. Within this great dialectic, it is felt, signal and continuous advances in masterful conception and rigorous procedure will be made.

If we inquire more closely into just how the two research-ways differ, we find that there is sometimes a confusion of differences that are non-logical with those that are logical in character. This is revealed, for example, in statements of the difference between the two styles as a political and intellectual dilemma: the more socially or politically significant our problems and work (the more macroscopic), the less rigorous is our solution and the less certain our knowledge (the less molecular).

There is much social truth in such statements; as they have so far been used these two styles of thought do differ in their characteristic value-relevance and political orientation. But this does not mean that any political orientation is inherent in the logic of either style of thought. The evaluative choice of problems characteristic of each of the two methods has not been *necessarily* due to logical capabilities or limitations of either. Molecular work of great political relevance is logically possible; and macroscopic work is not necessarily of broad significance, as a glance at many "political science" monographs proves all too well. No, many of the differences between the two styles are not logical, but social:

From the standpoint of the individual researcher, the choice of problems in either style of work may be due to academic timidity, political disinterest, or even cowardice; but above all it is due to the institutional facts of the financial life of molecular research. Molecular work requires an organization of technicians and administrators, of equipment and money, and, as yet, of promoters. It can not proceed until agencies of research are sufficiently developed to provide detailed materials. It has arisen in definite institutional centers: in business, since the twenties among marketing agencies, and since the thirties, in the polling agencies; in academic life at two or three research bureaux; and in research branches of government. Since World War Two the pattern has spread, but these are still the centers.

This institutionalization of the molecular style has involved the applied focus, which has typically been upon specific problems, presented so as to make clear alternatives of practical—which is to say, pecuniary and administrative—action. It is *not* true that only as general principles are discovered can social science offer "sound practical guidance"; often the administrator needs to know certain detailed facts and relations, and that is all he needs to know.

The sociologist in the applied focus no longer addresses "the public;" more usually he has specific clients with particular interests and perplexities. This shift, from public to client, clearly destroys the idea of objectivity as aloofness, which perhaps meant responsiveness to vague, unfocused pressures, and thus rested more on the individual interests of the researcher. In applied research of the molecular style, the client's social operations and economic interests have often supplied the sometimes tacit but always present moral meaning and use to the problem and to its solution. This has meant that most molecular work of any scale has been socially guided by the concerns and worries set by practical government and business interests and has been responsible to them. Accordingly, there is little doubt that the applied focus has tended to lower the intellectual initiative and to heighten the opportunism of the researcher. However technically free he may be, his initiative and interest are in fact usually subordinate to those of the client, whether it be the selling of pulp magazines or the administration of an army's morale.

Very little except his own individual limitations has stood between the individual worker and macroscopic work of the highest order. But the rise of the molecular style means that the unattached man cannot pursue such research on any scale, for such work is dependent upon organization and money. If we would "solve" the problem raised by the coexistence of these two styles we must pay attention to the design of work that is possible for the unattached men who still comprise the bulk of those claiming membership in the sociological community.

The rise of applied molecular work, as it is now being organized, makes questions of moral and political policy of the social studies all the more urgent. As a bureaucratization of reflection, the molecular style is quite in line with dominant trends of modern social structure and its characteristic types of thought. I do not wish to consider these problems here except to say that they should not be confused with any differences of a logical character between the two styles of inquiry.

TWO

There are at least three relative differences of a logical sort between the macroscopic and the molecular styles of work as they are now practiced: the molecular is more objective; it is more open to cumulative development; and it is more open to statistical quantification.

Objectivity means that the work is so done and so presented that any other qualified person can repeat it, thus coming to the same results or showing that the results were mistaken. Subjectivity means the reverse, and thus that there is usually a persistent individual variation of procedure—and of solution. Under this difference lies the fact that when work is objective the procedures used are systematized or even codified and hence are available to any qualified operator; whereas in subjective work the procedures are often not systematized, much less standardized or codified.

This in turn means that in objective work there is a more distinct possibility of cumulation—or at least replication!—both in terms of empirical solutions

and in terms of the procedures used. In the more subjective macroscopic work the sensitivity and talent of the individual worker weigh more heavily and although there may be those who "take up where he left off," this is usually a continuity of subject-matter, general ideas, and approach rather than an accumulation of procedure. It is possible within a few years to train competent persons to repeat a Sandusky job;¹ it is not so possible to train them to repeat a Middletown study. Another sample of soldiers in another war can be located on a morale scale and comparisons built up; Max Weber's analytic and historical essay on bureaucracy has not been repeated or checked in the same way, however much it has been criticized and "used." Macroscopic work has not experienced the sort of cumulative development that molecular work during the current generation of sociologists has.

It is descriptively true that the molecular style has been heavily statistical, whereas the macroscopic has not. This, again, is an aspect of the greater codification and the lower level of abstraction that molecular work entails. And it can be confidently supposed that as macroscopic work is made more systematic it will become more quantitative—at least as a general form of thought. For example, Darwin's *Origin* as well as many of Freud's theories are quantitative models of reflection.

Each of these three points is underpinned by the fact that molecular procedures can be, and have been, more explicitly codified than those of the macroscopic style; and by the fact that molecular terms are typically on a lower level of abstraction than most macroscopic conceptions.

Insofar as the logical differences between the two styles concern *procedures*, they are differences in the degree of systematic codification. Insofar as they involve *conceptions*, they are differences in level of abstraction.

THREE

When we say that molecular terms are on *lower* levels of abstraction we mean that they isolate from larger contexts a few precisely observed elements; in this sense they are of course quite abstract. When we say that macroscopic concepts are on *higher* levels of abstraction, we mean that they are more generalized, that the number of single variables which they cover are more numerous. The molecular term is narrow in scope, and specific in reference: it deals with a few discrete variables; the macroscopic researcher gains his broader scope by using concepts that cover, usually less specifically, a much larger number of variables.

There is no one clear-cut variable, the presence or absence of which allows application of the concept, "capitalism": under such concepts there is likely to be a pattern of interrelated variables. Thus, such concepts are not only high-level but their index structure is an elaborately compounded affair. Put technically, most big macroscopic concepts already have under them rather elaborate, and often unsystematic, cross-tabulations of several variables; most molecular terms stand for single variables useful for the stubs of such tables.

¹ Paul F. Lazarsfeld, et al., *The People's Choice* (New York: Duell, Sloan and Pearce, 1944).

We can consider a term in its relation to some empirical item(s)—that is, its semantic dimension; and we can consider a term in its relation to other terms—that is, its syntactical dimension, or if you like, its conceptual implications.² It is characteristic of molecular terms that their semantic dimensions are pronounced, although syntactical relations may also be there. It is characteristic of macroscopic terms that their syntactical dimensions are pronounced, although semantical relations may also be available.

The higher macroscopic levels are more syntactically elaborate; semantically they involve a hierarchy of compounded indices pointing to whole *gestalts* of attributes. Macroscopic concepts are often sponge-like and unclarified in their semantic dimensions. Sometimes, in fact, they do not have any index structure that enables us to touch empirically observable facts or relations.³ They have under them only a vague kind of many-dimensional indicator rather than an index. Yet, with all this, it may be that whether a statement is macroscopic or molecular is a matter of degree—a question of at what level we introduce our syntactical elaboration.

FOUR

Our choice of level of abstraction occurs, if I may simplify the matter, in at least two distinct junctions of our research act: The character and scope of the unit that we take as problematic, the *what-is-to-be-explained*;⁴ and the model of explanation—the concepts we use in the solution of the problem.⁵

² We can also consider it in relation to its users—the pragmatic dimension—which I am not here considering. These are the three dimensions of meaning which Charles M. Morris has systematized in his “Foundations of the Theory of Signs,” *International Encyclopedia of Unified Science*, Volume I: Number 2. University of Chicago Press, 1938.

³ To sort out the dimensions of a macroscopic concept requires us to elaborate it syntactically, while keeping our eyes open for semantic indices for each implication so elaborated. To translate each of these points into molecular terms requires us to trace the hierarchy of inference down to single, clear-cut variables. In assertions using macroscopic concepts, we must watch for whether or not the assertion (1) states a proposition, or (2) unlocks an implication. The guide-rule is whether the statement involves one empirical factor or at least two. If it involves only one factor, then it simply “spells out” or specifies one of the conceptual implications of that one factor; its meaning is syntactical. If the assertion involves two factors, it may be a proposition, a statement of a relation which can be true or false; its meaning is semantical.

⁴ In either style, one may of course start with a simple declaration of descriptive intent, finding more precisely-put problems as one goes along. In either style, too, the assembly of stray facts without any general significance or interconnection may be found; the new (molecular) ideography is no different in this respect from the older macroscopic kind. Both are composed of details not connected with any problem and entailing no evident syntactical implications.

⁵ The difference here is not a difference in the general logic of explanation: in both styles of work a third factor (or fourth or fifth factor) is appealed to in the explanation of some relation observed.

The explanatory intent of the macroscopic style is to *locate* the behavior to be explained within a structural totality or a cultural milieu; it finds its explanation in this “meaningful location”—which means that it seeks to interpret in the terms of a highly intricate, inter-related complex of variables.

The explanatory intent of the molecular student is to break down the behavior of the

The grand tradition in social studies has been to state both problem and explanation in more or less macroscopic terms. In contrast, the *pure* molecular student goes through the whole research act on the molecular level. In the simplest scheme of observation and explanation there are four possibilities:

EXPLANATIONS	OBSERVATIONS TO BE EXPLAINED:	
	Macroscopic	Molecular
Macroscopic	I	II
Molecular	III	IV

I. Both what is to be explained and its explanation can be on the macroscopic level. E.g.: Why do many people follow Hitler? Answer: Because in the bureaucratization of modern society, life-plans are taken over by centralized bureaucracies in such a way that when crises occur, people are disoriented and feel that they need guidance. Bureaucracy has thus resulted in a trained incapacity of people to steer themselves. In crises the bureaucratic routine that trained them is gone: they therefore follow Hitler. Etc.

II. When the problematic observations are molecular, but the explanation macroscopic, the question is thought to be too general and figures on the vote, pro-Hitler sentiment, and urban residence, for example, are taken as what is to be explained. Then they are explained macroscopically, although usually in a more modest way because of the molecular problem-setting. E.g.: The urban people were more disoriented and thus in need of the image of a Father who would promise to plan their lives and take care of them. They therefore voted pro-Hitler. Etc.

III. The problematic observations may be macroscopic and the explanation molecular. Why do some people follow Hitler? Answer: We know that only 5 per cent of the population went to college: this is a fact pointing to social ignorance, which is further confirmed by the correlation of education and political information, revealed in all our polls. Ignorance, thus established, goes far to explain why some people follow Hitler. Etc.

IV. In this type of procedure, both phases are held to the molecular level. E.g.: The question is too general to be appropriately answered, it must be re-phrased: 30 per cent of the adult population voted for Hitler in a given election. Why? Answer: When we take into account the rural-urban distribution, the religious, and the income level of the population, we find that 80 per cent of the rural, Protestant, high income level voted pro-Hitler, only 15 per cent of the urban, Catholic, low income. These three factors in the combination indicated seem to explain something about why certain people voted pro-Hitler and others did not. Etc.⁶

Notice the following characteristics of these four models of thought:
The inadequacies of the purely macroscopic and the purely molecular (I & IV) are tied in with the fact that in both cases there is no shuttle between levels of abstraction. Since rigorous proof only exists empirically on the molecular levels, in the pure macroscopic there is no proved connection between problematic ob-

individuals involved into component parts and to find the explanation in the association of further simplified attributes of these individuals.

⁶ All illustrative facts and figures in this paper are products of the imagination.

servation and explanation; when you are persuaded by such work, it is only because "it makes so much sense," it is syntactically convincing. On the purely molecular level there is a connection proved between problematic observation and explanatory observation, yet here the larger implications and meaning of that association are neither explored nor explained. When you are unsatisfied with such work it is because, although it is "neat" and "ingenious," you feel "there is more to it all."

In procedures II & III there is a shuttle between the macroscopic and molecular levels but it does *not* occur in the same phase of the total research act: we do not move from macroscopic to molecular inside the problematic phase, and we do not do so inside the explanatory phase. This means that the problematic observation and the explanation are not logically connected.

When the problem is molecular and the explanation macroscopic (II), there is an error of *falsely concretizing a concept*: in explaining some molecular observation by appealing, *ad hoc*, to a macroscopic concept, that concept tends to be handled in discussion as if it were a definite variable statistically related to the molecular observation.

When the problem is macroscopic and the solution molecular (III), the error might be called *unduly stretching an index*: in explaining some macroscopic observation by appealing to a molecular variable, that variable is unduly generalized and handled in discussion as if it were a carefully built index. The molecular explanation is *imputed* to explain the macroscopic observation, not connected.⁷

What all this (II and III) amounts to is the use of statistics to illustrate general points, and the use of general points to illustrate statistics. The general points are not tested, nor necessarily enlarged; they are conveniently adapted to the figures, as the arrangement of figures are cleverly adapted to them. The general points and explanations can be used with other figures too; so can the figures be used with other points.

Perhaps there is nothing especially wrong in all this; it is almost respectable procedure in some circles. But it does fall short of what is coming to be our vision of what social inquiry might be.

FIVE

I have discussed these research-ways at length in order to be able to set forth an "ideal" procedure, which we can use as a sort of lordly measuring rod for any piece of work in current social studies. The inadequacies indicated above may be summarized in one positive statement: If our work is to be clarified, we must be able to shuttle between levels of abstraction *inside each phase* of our simplified two-step act of research. This, of course, is simply another way of referring to the

⁷ In some research shops, the term "bright" is frequently applied when molecular facts or relations are cogently explained by macroscopic suppositions (II).

When further molecular variables, whose meaning is generalized very far—i.e., stretched—are brought in to explain, and they work, the result may be referred to as a "cute" table (III).

I mention this only to indicate that there is slowly emerging a shop language to cover the procedures I am trying to assert.

problem of indices and their place in the research process. Examine this simplified chart:

	PROBLEMATIC	EXPLANATORY
Macroscopic	1	2
Molecular	3	4

Only by moving grandly on the macroscopic level can we satisfy our intellectual and human curiosities. But only by moving minutely on the molecular level can our observations and explanations be adequately connected. So, if we would have our cake and eat it too, we must shuttle between macroscopic and molecular levels in instituting the problem *and* in explaining it—developing the molecular index structure of general concepts and the general conceptual implications of molecular variables. We move from macroscopic to molecular in both problem and in solution phase (1 to 3 and 2 to 4); then we relate the two on the molecular level (3 and 4); then we go back to the macroscopic (3 to 1 and 4 to 2). After that we can speak cautiously (i.e., bearing in mind the shuttles made), of relations on the macroscopic level (1 and 2).

To illustrate these shuttles, we may now design one ideal way of asking and answering a general question: Why *do* some people follow Hitler?

First, we accept the question macroscopically, and without losing any of its intended meaning, break it into more manageable (molecular) parts: “following Hitler” means: Expressing pro-Hitler sentiments to an interviewer, consistently voting for him, going out on the street to demonstrate when he or his agents request it, urging others to follow Hitler. Etc.

Each individual in a cross-section of the population may be classified in terms of a table composed of such items, and the tables reduced to a scale of types. Thus we build an index for “following Hitler;” our observation of what-is-to-be-explained is molecularly translated: transparent and specific indices are available.

We also accept, as a rather complicated hypothesis, the macroscopic statements (A) that people follow Hitler because of an inability to plan their own life-ways, (B) that this inability has been trained into them by work and life in bureaucratic structures, (C) that it was the crises and collapse of these bureaucracies that precipitated their allegiance to Hitler, whom, (D) they see as the big planner of their little lives.

Now this is somewhat tangled, although ordering it into these four assertions helps some. We have set ourselves quite some work, in translating and interpreting molecularly each of the four parts of the hypothesis. To short-cut it: for (A) we develop an index for “inability to plan life-ways.” Perhaps we ask each individual about details of his daily routine and his weekly and yearly cycle, scoring each detail as to its indication of ability or inability to plan. We also ask directly about the images or lack of them that they have about the future and their future, etc. Then we carefully relate these scores, and come out again with a scale of types: at one end are those most able to plan their life-ways, at the other end those least able.

Then we go to segment (B) of the hypothesis, building indices to work and leisure within bureaucracies. And so on, with (C) and (D).

Finally, we interrelate our molecular indices to all four features of our hypothesis, reduce them, and emerge with a master scale: at its top are people who seem unable to plan their own lives, have been duly exposed to and “trained” by bureaucracies,⁸ who began to be pro-Hitler in the major crises in Weimar society, and who have an image of Hitler as an omnipotent regulator and giver of satisfactory life-plans.

Given the crude state of our empirical technique and the clumsiness of our index building, we would probably finish with five cases in our extreme types, but that in itself has no logical meaning: what we are doing is translating an elaborate macroscopic explanation into molecular terms, and this must be done if we are serious about relating problematic observation to explanation. If we have other macroscopic explanations we must handle them in the same way; in our design we must think through their index structure.

Now we run our observations to be explained against our explanation, and this is what we obtain:

OBSERVATION OF HITLER SENTIMENT	PREDISPOSITION ACCORDING TO BUREAUCRATIC HYPOTHESIS		
	High	Intermediate	Low
Pro-Hitler	80%	20%	5%
Intermediate	15	60	15
Anti-Hitler	5	20	80
Total	100%	100%	100%

Maybe. But if so—

After controlling all the possible other variables we can think of, the reader might agree that we have earned the right to discuss, on the macroscopic level, bureaucracy, dictatorship and the character traits of modern mass-man. That is, to shuttle between macroscopic observations and macroscopic explanations.⁹

SIX

Even this brief discussion of this sketchy model suggests general rules of procedure for interpenetrating more neatly molecular terms and macroscopic concepts. We must build up molecular terms; we must break down macroscopic conceptions. For, as matters now stand, the propositional meaning of many macroscopic statements is ambiguous and unclear; the conceptual meaning of many molecular statements is often barren.

Any macroscopic statement that makes sense *can be* reduced to a set of molecular assertions—by untangling its dimensions and clarifying the index structure

⁸ For simplicity of presentation, I skip here the causal links between, e.g., B and A implied in the hypothesis.

⁹ Of course, by the time we had gone through the three steps outlined, surely Hitler would have us in his clutches; but that is an irrelevant incident, and of no concern or consequence to the *designer* and methodologist of research, however inconvenient it might be to the research worker.

of each of them. Any molecular statement can presumably be built up to macroscopic levels of abstraction—by combining it with other molecular indices and elaborating it syntactically—although many of them are probably not worth it, except as a formal exercise in ingenuity.

Every macroscopic study runs the risk of being confused by the wealth of materials that come into its scope. In order to decrease the chance of ambiguity in the semantic dimension of macroscopic conceptions, we must strain towards a clarification of their index structure and, while making them as clear as possible, we must work towards an increased codification of how we are using them.

Every molecular study involves a series of guesses about the important variables that may characterize and explain a phenomena. In order to increase the chance that our focus will be upon key variables, we must strain towards possible levels of macroscopic concepts in our molecular work, but not stretch indices of explanatory variables, or at least do so only with an awareness of our speculative posture.

The sociological enterprise requires macroscopic researchers to imagineer more technically, as well as with scope and insight; it requires technicians to go about their work with more imaginative concern for macroscopic meaning, as well as with technical ingenuity. Perhaps we cannot hope, except in rare instances, to have combined in one man all the skills and capacities required. We must proceed by means of a division of labor that is self-guided, in each of its divisions, by an understanding of and a working agreement upon a grand model. When as individuals we specialize in one or the other phases of this model, we must do so with a clear consciousness of the place of that phase within the model, and thus perform our specialist role in a manner most likely to aid another specialist in the architectonic endeavor. The development of such clear consciousness, in fact, is the complete and healthy significance of discussions of the method of the social studies.

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