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1   The lost vision

Auguste Comte (Comte, 1830-1842) proposed what is now considered a naive and unsophisticated argument: sociology could develop abstract laws about the operative dynamics of the universe; it could test these laws using a variety of methods to gather empirical data; and it could use the knowledge thereby accumulated to help reconstruct society in more humane ways. As cruel as our retrospective view of Comte is these days, this vision is still a very good criterion by which to assess the current state of sociology as a science. Does sociology have accepted laws? Are theories systematically tested? And, is sociology used in making policy decisions? If our answer to these questions is 'no', or only a qualified 'yes,' then the state of our science is less than it should be. I am afraid that only the first question can be answered with some level of affirmation, for sociology does have some basic laws, but unfortunately there is little consensus over which ones are best. And there are many—perhaps a majority within theory circles—who question the very idea of developing scientific laws. The second question is clearly negative: theories are rarely subjected to empirical tests. Indeed, there is a large gap between theory and research in sociology. And the final question is only partially affirmative, because policy is dominated by atheoretical research in which sociologists serve primarily as social demographers for those who make decisions in terms of their ideological biases. For neither theoretically-informed research nor empirically-confirmed theory exerts great influence on policy and societal reconstruction.

Thus, the state of the science is not what Comte had hoped for, and certainly not what I advocate (Turner, 1979, 1985a, 1990c). In a world rife with problems of organization, this is indeed a great tragedy. How, then, did we lose Comte's vision and come to this sorry state of affairs? Let me offer some observations in the pages that follow.

2   The theory-research gap

Comte's advocacy emphasized that knowledge is accumulated when theories are directed to basic and fundamental properties of the social universe and when data are gathered for their theoretical relevance. In this way, the collection of data contributes to the development of theories about what is basic, universal, trans-situational, and fundamental to human social organization rather than to what is peripheral, unique, ad hoc, time-bound, and episodic. Conversely, clear and well articulated theories focus research, keeping it from becoming the slave of the latest client with money.

Yet, this connection between theory and research has not been consistently maintained. There is now an enormous gap between theory and research in sociology. Much of the blame must be heaped upon theorists who seem reluctant to theorize. What, then, do they do, if they are not theorizing?

One prominent activity among those who label themselves theorists is history of ideas—a perfectly legitimate and reasonable activity which, however, belongs in history. The moods of the time, the biographies of great masters, the lineages of ideas, the institutional context of ideas, and many other interesting topics are explored by those doing history of ideas. There is only one problem: such activity does not explain how the social world operates which, after all, is what theory is supposed to do.

Another prominent activity is metatheorizing where the theories themselves are subject to analysis. Such activity should assume that a well developed body of theory exists as a resource for metatheorists, but the lack of theory does not appear to daunt metatheorists. Any idea, person, concept, school, paradigm, perspective, and so on is a candidate for analysis. George Ritzer (1987, 1989, 1991a, 1991b), the leading guru of metatheory, has recently made a useful distinction among three types of metatheorizing (Ritzer, 1991b): there is, first of all, metatheory designed to create a general theoretical orientation (M_o); there is, secondly, metatheory designed to understand how and why a theory was produced (M_u); and finally there is metatheory intended to produce new and better theories (M_p). It is M_o and M_u that I criticize here, because these do not result in explanations of processes in the real world. My own work in developing theory (for representative illustrations, see: Turner 1975a, 1975b, 1979, 1984, 1985a, 1985b, 1987, 1988, 1990a, 1990b, 1990c, 1991, 1992a, 1992b) represents an effort to perform M_p because it has sought to analyze theories with an eye to producing a more comprehensive theory; but as is clear, there are few theorists working in my tradition, nor others in other M_p traditions (see later discussion for my views on who is doing creative M_p). If more theorists did M_p, we would be much better off as a scientific discipline, but among those performing metatheory, the vast majority engages in some version of M_o and M_u—activities not likely, I believe, to increase our theoretical understanding of the operative dynamics of the social universe.

Yet another kind of activity among theorists is, for want of a better label, philosophizing. Here the old epistemological issues are raised; and typically, it is concluded that knowledge equivalent to that in the 'hard' sciences cannot be developed. Moreover, other old issues are once again examined: idealism vs. materialism, dialectics vs. evolutionism, micro vs. macro, and so on. In all of this debate, or 'dialogue', positions are staked out and then insults lobbed back and forth to those in the other camp. Such debates become ends in themselves; and soon, concern with explaining anything about the workings of the actual world is abandoned.

Still another kind of activity conducted by those who consider themselves theorists is, again for want of a better term, moral preaching. Here the evils of society—capitalism, modernism, technological invasion of lifeworlds, or whatever—are seen as somehow violating humans' basic needs or in some way oppressing classes of humans. Proposals for changing this situation are offered, although these tend to get buried in philosophical discussions so as to be not only obtuse but impractical. Indeed, a reading of critical theory in the 1960s and 1970s or of more recent post-modernists can offer a feel for this kind of activity: a portrayal of the world in terms of some ideological bias is offered, but this portrayal is so distorted by ideology and by philosophical wanderings that the preachings about 'what's wrong' with society become obscure, taking on a kind of unreality. Thus, discourse remains confined to academia and, at times, the broader intellectual community, but it rarely influences either scientists or policy-makers. The result is that the operative dynamics of the social universe are neither accurately portrayed nor explained.

A final activity that is often considered theory is elevating the lowly empirical generalization to the status of theory. Thus, much description is made to look like theory by couching these descriptions in more general terms. Many 'theories of the middle range' (Merton, 1957) reveal this feature: generalizations extracted from data in some empirical phenomenon—family, organizations, social movements, urbanization, social mobility, inequality, and the like-are couched in less situation-specific terms. Such transformations often hide the time- and context-bound nature of the generalizations, although at times the statements do represent a movement to a more generic level. But most of these 'theories of' (name your empirical topic) are not the kind of explanatory theories we should seek; at best they represent more clearly stated empirical generalizations that require a theory to explain them. The problem is that this fact is often not recognized, with the result that sociology gets a distorted view of what theory should look like. Yet, if these middle range theories are recognized for what they are—inducted empirical generalizations—they have the potential for closing the theory data gap, because they array empirical results in a way that facilitates their explanation with a more abstract theory.

Thus, much of what is considered theory in sociology is something else—history of ideas, metatheory, philosophical discourse, moral preaching, and empirical generalizing. As a consequence, theorists often fail to explain anything, and in so doing, their work does not suggest hypotheses to guide researchers in the collection of data or provide tools for interpreting existing data sets. And as theorists fail to stimulate researchers, the practice of research takes on an atheoretical life of its own, responding to grants and other sources of funds more than desires to explain the universe; and conversely, as data gathering appears so atheoretical, theorists retreat into their own world of activity which, increasingly, is about itself rather than about the basic properties of the universe.

I have certainly overstated my case, but the trend is clear. The theory-research gap is widening; indeed, it almost appears as an untraversable schism. We should thus ask: How did this situation come about? How did Comte's elegant vision come to seem like an idealization, incapable of ever being realized?

3   The detachment of theory from reality

Much of the detachment of theory from research occurred, ironically, as a result of efforts to specify how theories are to be tested. In a story which has not been adequately told, the Vienna Circle transformed Comte's positivism into 'logical positivism' which invoked the physics ideal of deductive axiomatic theory (Turner, 1992c). If the deductions from abstract laws could be precise, and conducted in terms of a rigorous calculus, then higher order propositions and empirical hypotheses could be connected; and because empirical hypotheses would be 'logically' derived from higher order propositions, an empirical test of a hypothesis would assess the plausibility of a theory (Hempel, 1965). This all sounds very good until it is recognized that no science, except portions of physics, can produce such theory (Freese, 1980). Use of formal calculus in deductions is usually not possible, nor even desirable; excluding exogenous processes from intervening in those forces specified in laws is impossible in sciences like sociology which cannot construct controlled experiments and which must, thereby, work in natural systems. Thus, in actual fact, most sciences make 'folk deductions' from abstract laws; that is, if an empirical regularity appears to contain variables that are indicators of more generic processes in the theory, it is considered a 'reasonable' test for assessing the plausibility of a more abstract proposition.

These kinds of considerations got lost in the 1960s and early 1970s when a 'theory construction' movement swept sociology, especially American sociology. Within a very short period of time, a spate of books was published on how to 'build' and 'construct' theory (e.g., Blalock, 1969; Dubin, 1969; Reynolds, 1971; Hage, 1972; and Gibbs, 1972). These books were, I suspect, viewed as the equivalent in theory for their counterparts in methodology. They constituted rather ponderous instructions on 'how to' construct theory, step by step in a

kind of cook-book fashion. The tenets of logical positivism were uncritically accepted in these books, and coupled with a kind of American 'can do' mentality, theorizing was reduced to technique or treated like a data set on which some statistical package could be imposed.

The result of this movement was for efforts to build theory in this way to become incredibly pretentious, excessively formal and, at the same time, uninteresting and narrow. Researchers often could not understand these efforts, or care about them. Other theorists began to think that if this is what theory must be, perhaps sociology cannot be a natural science. As general disillusionment with logical positivism set in, theorists turned on themselves, attacking the prospects for theoretical science, while researchers simply tuned theorists out.

Suddenly, the theorists themselves found excuses for not being able to be scientific. Among the most popular were these:

1. Humans have the capacity for agency which means that they can remake the very nature of the social universe, thereby obviating timeless and universal laws.
2. Social arrangements are constructed in a context, defined by its history and particulars which cannot be analyzed by abstract, context-free theories.
3. Positivism extracts its laws from what exists, and as a result, the laws of social science reaffirm the status quo, thereby making those who practice science apologists for the current state of affairs. And so it went, with the result that theorists began to do just about anything except explain how the world works. Theory thus became even more detached from science and research, often moving into the humanities.

The current situation is one where theorists do not theorize, and researchers pursue data for its own sake. And positivism, after the fall of logical positivism, became identified with atheoretical quantitative research. Comte would indeed turn over in his grave at this turn of events. Yet, at the same time, some very creative theoretical work is being done, albeit along many diverse and poorly integrated fronts. And so, despite the sour mood against formal theorizing, some of the very best theorizing since the work of the great masters was being performed—a fact that gives me pause for optimism.

4   Promising, though disjointed, theorizing in sociology

A great deal of debate has occurred in recent years over the micro-macro gap—that is, between large-scale, long-term patterns of organization of populations, on the one hand, and small-scale, shorter-term patterns of interpersonal behavior, on the other. Some have even added a 'meso' level to fill in this gap (Collins, 1988). Indeed, true to sociology's metatheoretical and philosophical turn, there has perhaps been more discussion of the problem of linkage than actual theorizing about macro, micro, meso level processes. My view is that there has been too much agonizing over such issues; instead, we should recognize that theorizing about interpersonal processes yields different knowledge than theorizing about population-level processes and that these two types of knowledge may not be easily reconciled theoretically. With this said, let me now mention areas of theorizing at the micro and macro levels that I see as promising.

Micro Level Theorizing

The greatest success of sociological theorizing is at the micro level; we now know a great deal about this domain of our universe. Indeed, we should not expect any startling breakthroughs; rather, we should seek to codify our theories, as I sought to do in A Theory of Social Interaction (Turner, 1988). The great breakthroughs in micro processes were made by Émile Durkheim (1912 (1954]), George Herbert Mead (1934), Alfred Schutz (1932 [1967]), and perhaps Sigmund Freud (1900 [1953]). The work of symbolic interactionists, along with role theory, has filled in the Meadian legacy. Especially noteworthy, in my view, have been the works of Ralph Turner (1962, 1968, 1974, 1978, 1979) and Sheldon Stryker (1980) because they have developed a series of propositions on the relationship among social structure, self and identities, and behavior. The Schutzian phenomenological legacy has exerted the greatest impact through ethnomethodology (Garfinkel, 1967; Sachs, Schegloff, and Jefferson, 1974; Heritage, 1984) which has developed theoretical constructs pertaining to a domain of inquiry that was ignored by symbolic interactionism. Similarly, the Durkheimian legacy filled in gaps in interactionism with the work of Goffman (1959, 1967, 1974) on ritual, talk, and face and, later, with Collins' (1975) blending of Durkheim, Goffman, Weber, and ethnomethodology. And the Freudian legacy is evident in the sociology of emotions, a neglected topic in all of micro sociology (e.g., Scheff, 1988, 1990). Not all perspectives on the sociology of emotions stress Freud's variables—anxiety, repression, and subconscious action; some emphasize dramaturgy, others power, and still others psychology, but all owe their inspiration to Freud for the basic insight that emotions, especially repressed emotions, are a powerful force in human interaction and social organization (Scheff and Retzinger, 1991).

One of the most intriguing revivals at the micro level, but also extending to the macro as well, is utilitarian theorizing. Exchange theory has for many years been one of the bright spots of sociological theorizing, often bridging the micro-macro schism (e.g., Homans, 1961, 1974; Emerson, 1972, 1986; Cook, 1987; Willer, 1986), but the revival of a rational choice perspective by Coleman (1986, 1990), Hechter (1987), Heckathorn (1988, 1990), and others reintroduces game theory and other concepts of utilitarian theorizing into mainstream

sociological theory. This is an event that has considerable potential, as I have argued in a number of places (Turner, e.g., 1991, 1992b).

A final sign of vigor in micro theorizing, and at times macro theorizing (e.g., Maryanski and Turner, 1992), is the willingness to consider the effects of biological forces on human interaction. For many decades, the biological foundations of human interaction and social organization have been ignored, but the barriers to this kind of work are coming down; and new journals such as **Social Ecology** have emerged to galvanize this interest. For human behavior and interaction must be, to some unknown degree, influenced by our genetic legacy, as this emerged in the 60 million years of primate evolution (Maryanski and Turner, 1992).

Macro Level Theorizing

It is at the more macro level, where our accumulation of knowledge is not as advanced as at the micro level, that some of the most exciting scholarship is now being produced. One line of work is the revival of functionalism as neo-functionalism (Alexander, 1985; Alexander and Colomy, 1985, 1990; Colomy, 1990). Here the early concerns of Spencer and Durkheim with the dynamics of differentiation and integration have been retained without an undue emphasis on functional needs and requisites. And even scholars such as R. Münch, who do retain this emphasis, have learned to couch their conclusions in propositional form without concern with system needs.

Another line of work, which also owes its early inspiration to Spencer and Durkheim, is human ecology. Here, research and theory on urban ecology (e.g., Kasarda, 1972; Frisbie, 1980; Frisbie and Kasarda, 1988) and organizational ecology (e.g., Hannan and Freeman, 1977, 1989) have been a source of inspiration for more general ecological theories (Hawley, 1986) that retain the emphasis on competition and selection but connect this emphasis to mainstream macrostructural forces like power, stratification, conflict, and production.

A much newer line of inquiry, which shares with human ecology the use of concepts from bio-ecology, is coevolution (e.g., Durham, 1991; Boyd and Richerson, 1985). While this perspective is more prominent in anthropology, it offers considerable potential in sociology because it adopts precise concepts from the synthetic theory of Darwinian evolution and applies them to the transformation and transmission of systems of cultural symbols. And even somewhat independently of these highly formal theories, more discursive use of coevolutionary arguments has begun to appear in cultural sociology (e.g., Wuthnow, 1987).

Perhaps the most significant work of the last decade has been the emergence of a more theoretically informed historical sociology (e.g., Mann, 1986; Skocpol, 1979; Tilly, 1978; Goldstone, 1990; Braudel, 1977; Collins, 1986). Here earlier, more descriptive work (e.g., Moore, 1966) has been replaced by the use of history to suggest or illustrate more general social processes; and while many of this newer generation of historical sociology have reservations about positivistic theory, they nonetheless are willing to abstract above historical details.

The decline of Marxist scholarship has been a healthy development, because of its extreme ideological assumptions. But the movement of Marxian scholarship to world system processes (e.g., Wallerstein, 1974) did represent a useful adaptation of Marxian ideas. Along with this shift in Marxian scholarship has come a revival of Weber whose ideas have been extended into more sophisticated theoretical work. Collins is clearly the leader in making Weber more theoretical; and his numerous works, but especially his **Conflict Sociology** (1975) and **Weberian Sociology** (1986), document the staying power of Weber's ideas in sociological theory.

5   Conclusion

These remarks lead to a paradoxical conclusion: At the very time that sociology and social theory as an enterprise have lost Comte's vision and retreated away from theorizing about the operative dynamics of the social universe, some of the most creative theorizing in the history of the discipline is being developed. How can this be so?

My answer (Turner and Turner, 1990) is that the research-theory gap, the failure of most theorists to theorize about the operation of the actual world, and the partitioning of sociology (particularly American sociology) into so many specialties all make the theoretical unification of sociology difficult. Add to this mix the warring theoretical camps—Marxism, functionalism, symbolic interactionism, ethnomethodology, exchange theory, structuralism, structuration theory, and so on—theorizing is further partitioned. The result is that research and theory go their separate ways, specialists become myopic and narrow in their concerns, and theorists throw insults at each other. Sociology thus remains a house divided — indeed hyper-differentiated — and becomes incapable of seeing and consolidating the creative theoretical work being done.

Structurally and culturally, I see no easy way out of this problem. Sociology is structurally splintered, and cultural symbols-anti—science, relativism, eclecticism, postmodernism and the like-legitimate this situation. Perhaps I am justifying my own work, but I see the only hope as this: entrepreneurial efforts to clarify and consolidate existing theories and to make them more appealing and amenable to empirical tests by researchers. only by demonstrating the viability of positivistic theory can Comte's lost vision be recaptured. Without this vision, sociology runs the risk of becoming a trivial discipline in a world whose problems desperately call for a natural science of society.

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