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# Perspectives

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## The Wonderful World(s) of Ideal Types

*Uta Gerhardt, Lehrstuhl für Soziologie II, University of Heidelberg, Germany*

When I was a child, I used to find that my parents would be friendly with our neighbors across the garden fence but talk about “those ardent Nazis” in the privacy of our living-room. Maybe it was such observations of context-related behaviors which I witnessed as a child, which eventually made me into a sociologist. Being a child in the Germany of yesteryear, of course, I would not be considered a person listening in as my parents exchanged their feelings of contempt for the neighbors. Unknowingly, I was engaging in participant observation anticipating the attitude of the sociologist I was to be. When I overheard with keen attention what my parents were saying to each other in their nightly exchanges of opinions on relatives, neighbors, or strangers, I must have been puzzled about how much situational contexts mattered for taken-for-granted everyday life. Maybe these and other memories of my childhood were the background for my interest in ideal types, the one and only methodological program for modern sociology. From the first day that I came across the works of Georg Simmel and Alfred Schütz, I could not help thinking that they tackled the same problem as did Max Weber. All three analyze, I am convinced, the social as well as sociological world(s) densely populated with ideal types.

*See IDEAL TYPES on page 2*

## Another Voice on What Happened to Scientific Sociology

*Bernard Phillips (formerly with the University of North Carolina, the University of Illinois, and Boston University)*

I'm most encouraged by the debate between Jonathan Turner (January 2002) and David Willer (April 2002). I believe that there is no issue facing our discipline at this time—and not just the theory section—more important and more urgent than the question of how to further develop our approach to the scientific method so that we can fulfill the dreams of our founders as well as our own dreams: for a sociology that is rapidly cumulative and is the foundation for highly effective problem-solving procedures. Several years ago I proposed to the ASA—a proposal which ultimately failed—that every Section should be given an additional session at the annual meetings if it organized papers on this very topic.

*See VOICE on page 5*

### IDEAL TYPES *from page 1*

The view I offer is one of three wonderful worlds of ideal types. These three worlds I was fortunate enough to explore, in the course of some thirty years of writing sociological theory and doing qualitative research. From the early 1970s until the beginning of the new millennium, the three worlds of ideal types came to take shape one after the other.

The first of these concerned social roles. When first reading Ralf Dahrendorf's *Homo Sociological*, I wondered whether he made adequate use of the third antinomy in Immanuel Kant's *Critique of Pure Reason*. The outcome was a study that conceptualized social roles as the realization of ideal types. The argument was that social roles are societal forms functioning as idealized typification schemes. These schemes, as I documented using ample empirical evidence, are the backbones of orientation in multiple contexts of status, position, and situation, simultaneously activated when they are subject to continuous interpretation in reciprocal interaction (Gerhardt 1980).

Whereas this first attempt at understanding ideal types focused mainly on the works of Georg Simmel and Alfred Schütz, the second concerned Weber. The idea was to transcend his methodological caveats into a method of systematic interpretive, qualitative research. In this vein, over some twenty years, case-structure-analysis evolved which uses Weberian ideal-type methodology for a method of data interpretation (Gerhardt 1994). A major empirical study documented an unprecedented richness of data and their use

for sociological explanation, provided that ideal types are used to analyze social structures as they influence biographical dynamics (Gerhardt 1999).

The third realm of ideal types, involving the social as well as sociological world(s), is the history of modern social theory. The intellectual history of Weber's idea of ideal types, no doubt, began (or ended, for a considerable period of time) with a false assurance. One year after Weber's untimely death, in 1921, philosopher Heinrich Rickert claimed authority over Weber's idea of ideal type. In a footnote inserted into the third edition of his seminal work, first published in 1902, *The Limits of Natural-Science Concept Formation*, Rickert claimed that he, Rickert, had been the author of the idea which Weber elaborated in 1904. Rickert stated, using the plural for his own person: "Not only Weber's methodological studies which follow my book but also his substantive dealing with social life show why only through the way which we go can knowledge be gained about logical structure in the... empirical sciences." (Rickert 1929: 263). Rickert's claim that neo-Kantianism spurred Weber's ideal-type conception was not alone. In 1922, Rickert's disciple and younger colleague at Heidelberg, Alexander von Schelting, devoted a 130-page article to the presumably Neo-Kantian roots of Weberian methodology. To be sure, both philosophers constructed a connection which had never existed in Weber's lifetime. The upshot was that Weber had even written a criticism of Rickert's philosophy of values, in an unfinished text recently published by H. H. Bruun (2001).

This text which Weber wrote in 1903 in the Italian town of Nervi praised Georg Simmel. Simmel's *The Problems of the Philosophy of History* (1892), to be sure, established the principles of perspectivity (relativity) of knowledge interest and selectivity of analytical aspects in concept formation. Simmel rejected the philosophies of history of Karl Marx and Herbert Spencer, replacing their positivism with methodological concern for concept formation – which Weber was to call "Wertfreiheit." Weber, a decade after Simmel's groundbreaking quest for methodology, introduced the ideal type as the heuristic construction assuring "objectivity" in the social sciences.

One context where Weber was taken seriously (and understood correctly) was the work published in 1932, Alfred Schütz' *Phenomenology of the Social World (Der sinnhafte Aufbau der sozialen Welt)*. The book was a masterful completion of Weber's idea of ideal types, extending Weber's methodological concern for concept formation into a principle of *Verstehen* that permeated the everyday social world. (To be sure, Schütz warned that the two realms of ideal type formation, the mundane social world and the social sciences, be kept apart.)

So there is an untold story of the ideal type. My recent study *Ideal Type: The Methodological Foundation of Modern Sociology* (2001) reconstructs the intellectual history of this conceptual program in the twentieth century. The story told in this book turned out to be not only much longer than many seem to have assumed so far but also a lot more fascinating. In a way, considering how little is known of the realm and use of ideal types Weber appears to have been tragically undervalued.

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*Perspectives* is the newsletter of the Theory Section of the American Sociological Association. It is published quarterly in January, April, July, and October. The deadline for all submissions is the fifth day of the month before publication. We welcome news and commentary as well as announcements about conferences, journal information, calls for papers, position openings, and any other information of interest to section members.

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## Theoretical Maturity: Ideal Type Models as well as Turner's Dream?

*J. I. (Hans) Bakker, University of Guelph, Canada*

In his editor's report Jonathan H. Turner (2001) laments the fact that many of the submissions to *Sociological Theory* are not "actual efforts to theorize about a property or process in the world." Therefore, I would like to begin by indicating that most of my work *has* been about processes like "class relations" (Bakker 1992). But, ironically, work about the "empirical world" is not likely to be regarded as a contribution to theory. So, the dilemma for those of us who are interested in applying theory to the study of processes "in the world" is that even after we "theorize" in that way, we nevertheless have to make some arguments about "methodologies of theorizing" in order to be heard by other theorists! There is a bit of a catch-22 (double-bind) involved in not following the more commonly engaged in theoretical activities like the "history of ideas" (Bakker 1999) or "programmatically statements" (Bakker 1995).

In a subsequent comment, Turner (2002) indicates six trends in sociology of the last thirty years: (1) hyper-differentiation, (2) anti-science, (3) political correctness, (4) worship of the masters, (5) bad intro texts, and (6) stale debates. He stresses the need to have insightful theories about "generic social processes" (GSP's) and "generic forces" (GF's). Much of his argument is correct. His analysis of what ails sociology, particularly sociological theory, is cogent, but his solution can be regarded as a bit too narrow. I would like to suggest that part of the difficulty with Turner's solution to the six problems he identifies is encapsulated by his statement: "If my dream were pursued, sociology would be a much smaller discipline, but it would be a natural science..." Since most of my work has not been oriented toward constructing sociology as a natural science I would like to defend an *equally valid* strategy: sociology as an empirically-based, comparative and historical discipline which uses Ideal Type Models and *Verstehen* to study limited ranges of times (t-n) and clearly-designated "spaces" (s-n).

It is probably true that if sociology were pursued principally from a natural science methodological approach, it would be a much smaller discipline. I would like to briefly argue for a different strategy. The basic idea is to make sociology "theoretically mature" by recognizing that very little of what we are most interested in as sociologists actually lends itself to a natural science approach.

I say this despite the contention by Willer (2002) that "there is a movement for the development of sociology as a science." I have no argument with Willer's excellent attempt to go beyond "systematic empiricism" (Willer and Willer 1973). Nor would I deny that it is possible to use the "experimental" approach of natural science (i.e., the Hypothetico-Deductive Method) to study social structures. I wish all those involved in the Group Process meetings success! But what I do want to say, loudly and clearly, is that research "theory" is not *just* the "method" of the natural science approach. A good "science" approach will require "theory" rather than just "systematic" or "abstracted" empiricism. But, it is also true that sociology does not have to be limited to a physical or natural science approach to be heuristic. Hence, my position is that both Turner and Willer over-emphasize the natural science approach, to the detriment of an equally valuable approach: Ideal Type Models and *Verstehen*.

Most sociologists, in fact, do not study "human nature" by "ignoring the distracting complexities of a society's 'content' and 'uniqueness'..." (Turner, Beeghly and Powers 2002: 345).<sup>1</sup> A theoretically mature sociology could also include and emphasize the importance of Ideal Type Models of Comparative and Historical Contexts. It could begin to view a "modern hermeneutic" type of *Verstehen* (Dilthey 1996) as central rather than peripheral to many methods and research results (Freund 1978: 149-52, 164-86). The study of complexities of uniqueness in time and space using historical and classificatory Ideal Type Models (Turner et al 2002: 195-99) could be viewed as just as important as efforts at transcending time and space through natural scientific laws. To fully grasp the methodological importance of Ideal Type Models (ITM's) it is necessary to consider "real types" and Real Type Models (RTM's).

What is a "real type"? The question is not often asked. In science a real type is not necessarily something concrete and particular, like: "This specific chair you are sitting in right now." Instead, the "reality" of the type is due to the existence of that type, at least hypothetically, for all Time and all Space. It is truly "generic." Take the elements in the Periodic Table as examples. Such elements as Hydrogen and Uranium are considered "real types" because they are considered to exist for all Time (T-u) and all Space (S-u) in the "universe." We have few "real types" in sociology and many of our variables (as used in correlation-based techniques) are much more "fuzzy" than is usually acknowledged (Ragin 2000:3-17, 165-71, 189-98).

That runs against the grain because it is commonly assumed that the IT (and ITM) is associated primarily or even exclusively with Max Weber. If we re-evaluate the methodological status of Max Weber's IT's and ITM's we can regard Weber's key ideas concerning "ideal" versus "real" in terms of a significant alteration in our

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## BOOK ANNOUNCEMENT

**Sociology and the Real World**

By Stephen Lyng and David Franks  
(Rowman and Littlefield, 2002)

*Sociology and the Real World* by Stephen Lyng and David Franks was published in February 2002 by Rowman and Littlefield. The volume draws attention to how seldom sociological hypotheses are tested and refined by real world application. However difficult this may be, the methodological consequences of this omission for valid theorizing is generally ignored. The book begins by arguing that this situation is in part fostered by discursive practices in sociology discouraging the use of words like reality, objectivity, truth and reason. Guided by a transactional epistemology that refuses to separate the enlightenment dualisms into oppositional extremes, the critical tension is maintained between the verbal and the nonverbal. This allows for a more coherent notion of objectivity, truth and embodied reason. It also allows a middle ground between foundational theorizing and the unfettered relativity fostered by aspects of the recent "linguistic turn". Although talking is action, the reality often implied by the term "deed" must be seen as more than talk alone. Significant symbols are not forged merely through verbal agreements, but also arise out of responses to manipulative actions on impartial aspects of the social and physical world. These uniformities, however interpretable by various symbols, place limits on extreme social constructionism wherein reality is merely what we agree it is. Parallels are drawn between Lakoff and Johnson's nonreductionist neuroscience and Chicago transactional pragmatism. The resulting framework argues for sensorimotor action and emotion as generically and genetically primary for reasoning. It questions the sufficiency of a social psychology that is limited to the verbal and more generally is wary of thinking unrelated to "deeds". To demonstrate the utility of a transactional approach, a social theory of embodiment is proposed, Habermas' theory of communicative action is revised and extended in scope, and a framework for social problems research is developed linking theory and praxis.

Please attend the **Theory Mini-Conference** on Saturday, August 17 at 8:30, 10:30, and 12:30 on Sociological Theory and Empirical Research. On Sunday at 10:30 a special session honoring Philip Rieff will also be held.

The Theory Section Reception will be combined with our business meeting at 3:30-4:15 on Saturday.

Our award announcements will be made at the business meeting/reception. Please come to honor our colleagues.

VOICE *from page 1*

I share both Turner's and Willer's dream of a scientific sociology, granting that the nature of that dream varies for the three of us. With respect to Turner, I completely agree that the "hyperdifferentiation of the field" is a fundamental problem in our quest for a scientific sociology, given our forty-plus Sections with little communication among them. I also agree with his critique of "the anti-science movement," illustrated by much of critical theory and postmodernism, granting that those trends are also useful in constraining us to question our present approach to the scientific method. On the other hand, I do not share his complaint about an overemphasis on "race, ethnicity, gender, class, and sexuality" so long as there remains room for the rest of our topics. That emphasis is not merely an instance of "political correctness": it alerts us to how little we have achieved in developing the solid platform of knowledge required to solve such fundamental problems.

Neither do I share his argument with "the continued worship of the masters." Marx, Weber, Durkheim and Mead — and I'd add Simmel to his list—all pointed away from the "hyperdifferentiation" we see today, and they were also genuinely concerned about fundamental problems in modern society. I believe that by invoking their works—certainly not "worshipping" them—we remind ourselves of the importance of both breadth and basic problems, reminders that are sorely needed. As someone who has written two introductory texts and two texts on research methods (one in three editions), I grant with Turner the publishing pressures for achieving visual appeal at the expense of serious content. Yet a number of intro texts have made large yet unsung contributions to the discipline, given their efforts to pull together what specialists have kept apart. Finally, I disagree with Turner's critique of "the perpetuation of certain long-standing debates." I believe that it is crucial for debates such as this one to continue so long

as they remain unresolved. I also believe that we should use the knowledge gained from these debates as a basis for actively constructing the kind of scientific method and sociology we've dreamed of.

As for David Willer's "So What Did Happen to Scientific Sociology?" I agree with his orientation to a systematic and logical approach to the scientific method. I also admire his own deep commitment to the discipline, as expressed in his many

**We invite all those interested in this debate or in our own efforts to join us during the evenings scheduled for our "shadow conference" in Chicago.**

publications. Further, I believe his work toward "a movement for the development of sociology as a science" by enlisting the support of other sociologists is not only useful but essential. I was most impressed by his article with Murray Webster, Jr., "Theoretical Concepts and Observables," which appeared in ASR many years ago (August 1970:748-57), where he attacked Merton's emphasis on "middle-range theory" as departing from the abstract concepts or theoretical breadth that a science requires. Yet when he claims that "there is a movement for the development of sociology as a science and it can be dated quite precisely to August 1988"—referring to his own efforts along with those in the theory-construction movement while ruling out the efforts of the rest of us—I find myself in sharp disagreement. For here he contradicts his own 1970 article invoking abstract theoretical concepts which can serve to integrate the diverse efforts within the entire discipline. And he also contradicts Turner's sharp critique of those within the theory-construction movement as having "a mechanical view of how to build theory, converting what is a creative process into something akin to the theoretical equivalent of the SSPS manual." My own critique of Willer's approach is by no means a blast at formal theory, network theory, small-group experiments

or exchange theory, all of which are useful, but rather is a plea for following the spirit of Willer's 1970 article calling for general theory.

I hope that this debate in these pages spreads to other Section newsletters, to Footnotes and to our journals, and I plan to help make that happen. I also hope that we sociologists go beyond such debates to act on what we believe to be a more effective approach to the scientific

method, one that will fulfill what Mills called "the promise of sociology." For my own part, the just-published *Beyond Sociology's Tower of Babel: Reconstructing the Scientific Method* (Aldine, 2001) is my own effort to move beyond debate to sketch and illustrate an approach that promises to build bridges connecting our specialized areas, by contrast with what Turner called "hyperdifferentiation." I call for abstract sociological concepts within a systematic approach, following Willer's 1970 article, that come to grips with social organization, culture, history, the individual and the situation, all within the same study. As for Willer's emphasis on work by a number of sociologists, a group of us have joined forces in a volume, *Toward A Sociological Imagination: Bridging Specialized Fields* (University Press of America), that should be available for the Chicago ASA meetings. It is edited by myself, Harold Kincaid (a philosopher of social science) and Thomas Scheff, with substantive contributions ranging over many fields by the editors as well as by Howard Becker, David Britt, Chanoch Jacobsen, James Kimberly, Richard Lachmann, David Maines and Suzanne Retzinger. The volume grew out of the first conference—at the ASA 2000 meetings in D.C.—of the "Sociological Imagination Group," an informal group

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thinking about science. Although I agree with Turner that “worship” of the founders as if they were saints is counter-productive, it is nevertheless sometimes important to carefully re-examine our “exegetical motifs” (Kugel 1997). Recent work on Weber (e.g. Ringer 1997, Drysdale 1996) can be helpful, for example, in over-coming some subtle biases in interpretation of Weber’s approach.

The “smaller discipline” that Turner dreams about would involve the formulation of true natural science laws which are empirically valid for all Time and all Space for the relevant “universe” (i.e., for all of human history on earth for the last 100,000 years or so, or at the very least for all of the known literate societies on earth). Anything else would involve specification of historical and spatial context. An Ideal Type generalization might be valid for a broad range of historical time (t-n) and a wide variety of geographical settings (s-n). But that does not necessarily make it fully “generic.” For example, it is quite possible that complex, industrialized societies in Western Europe (s-n) have exhibited many of the features of suicidal behavior that Durkheim discusses, at least for the last few hundred years (t-n). But there is nothing in Durkheim’s famous text that clearly specifies that the same Generic Social Processes (GSPs) also occur in China in the 10<sup>th</sup> century or India in the 2<sup>nd</sup> century. Durkheim’s “science” is not based on natural science “laws” in any direct and obvious manner.

None of this denies the important idea that “social facts” must be studied as “things,” in the sense that such social facts are external to the individual. But it is impossible for Durkheim’s formulation of the contrast between Roman Catholic and Protestant suicides to immediately cover the situation in other complex societies (e.g. Indonesia) at other times in history (e.g. the eighth century). A Hindu-Buddhist civilization (Majapahit) is not similar enough to nineteenth century France to warrant a covering “law” of anomic

or egoistic suicide.

However, when we reconceptualize Durkheim’s *Suicide* as Methodologically parallel to Weber’s (2002) *Protestant Ethic* then a clearer comprehension of the generalizations found in Durkheim’s study emerges. Just as the term “Protestant Ethic” refers first and foremost to one time and place (essentially seventeenth century Northwestern Europe), the term “anomic suicide” also refers first and foremost to one time and place (nineteenth century France; see Durkheim 1951: Appendices I, II, IV, V and VI; Appendix III refers to Central Europe).

One possible conclusion is that sociological theory should not be limited to the discovery of “generic” aspects of being human in societies. The discovery of generic, trans-historical and trans-spatial (cross-cultural) “laws” would be very useful. But at the same time we can also take the notion of Cross-Cultural, Comparative and Historical Sociology (CHS) more seriously. Ideal Types and ITM’s which are limited to empirically-based generalizations about a range of times and places (e.g., seventeenth century England) are not generic and do not conform to the “natural science” approach. But it is not strictly just a question of the science versus the non-science approach. We can still build causal models. But we will not have true laws, applicable to “real types.” Nothing similar to the Laws of the Periodic Table or the Laws of Thermodynamics is *likely* to emerge directly from a study of a dependent variable in terms of longitudinal data for twenty industrialized countries (s-n) over a one hundred year period (t-n). The “law” would require much broader empirical investigation to be established as a true scientific law.

Although it is commonly represented that way, the Ideal Type is not necessarily just one of many “tools” in the tool chest of qualitative research methods, along with “content analysis” and “participant observation,” etc. Instead, it is reasonable to consider the Ideal Type (and ITM) as a central methodological insight applicable

to sophisticated quantitative as well as qualitative research. Generalizations based on IT’s and ITM’s cannot be “universal” (for all T-u and S-u) but they can be empirically valid and reliable (for a range of t-n and s-n). They cannot be the basis for the kind of natural science based on “laws” concerning “real types,” but that does not mean they are not “social scientific” (*wissenschaftlich*).

If the argument presented here is fundamentally flawed then it can be falsified. For example, if natural science laws are a significant part of sociology today (or logically in the future) then it should be possible to indicate such laws. Perhaps the Group Process approach will pan out in that manner. But my dream of the future of sociology is not a much smaller discipline, based exclusively or even mainly on such laws. I can see the importance of that dream (and I suspect that perhaps some clues may be found in evolutionary psychology and cognitive science). The truly “generic” aspects of

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## Responses to “Blaming Postmodernism”

What implications, if any, do the September 11 attacks have on the past, present, and future of postmodern thought? Does being a “postmodernist” or “cultural relativist” mean that you can’t condemn terrorism with any moral authority? These are among the many questions that Richard Rorty, Benjamin Barber, David Novak, Cass Sunstein, William Galston, and others address in the next issue of *The Responsive Community* (Summer 2002) when they respond to Stanley Fish’s essay, “Blaming Postmodernism.” You can order *The Responsive Community* by visiting <http://www.gwu.edu/~ccps/rcq/index.html>, calling 1.800.245.7460, or emailing [comnet@gwu.edu](mailto:comnet@gwu.edu).

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human behavior would be shared by all members of the species trans-historically and trans-culturally, regardless of social organizational structures. But such natural science type laws about *homo sapiens sapiens* would probably be more social psychological rather than “holistic” in terms of the structures of “social systems.”

The discipline of sociology would probably be better off by adopting a greater awareness of the importance of historical and contextual understanding. Rather than natural science “laws” (for all T-u and S-u) we could concentrate more of our efforts on constructing explicit ITM’s (empirically valid for sets of t-n and s-n). For example, the critique by James S. Coleman (1994:6-10) of the logic of “methodological individualism” in Weber’s *Protestant Ethic* indicates that Weber’s argument may lack complete clarity with regard to moving from macro to micro level generalizations. But Coleman’s criticism does not imply that the structure that theory should have in sociology should be based entirely on trans-historical and trans-civilizational laws. That is evident, for example, from his critical discussion of Weber’s ITM of “modern bureaucracy” as opposed to “patrimonial-prebendal” or “patrimonial-feudal” bureaucracy (Coleman 1994:169-72). The “pure” or “generic” concept of “bureaucracy” does not seem to be foremost in Coleman’s analysis of various game scenarios.

Sometimes it is necessary to re-think our foundation myths. A candidate for “founder” of sociology *as it is actually practiced* is not the stereotypical version of Comte but Wilhelm Dilthey (Bakker 1999). Dilthey first formulated the notion of *Verstehen* as a historical and contextual understanding. Building on Schleiermacher’s (1998) insight that hermeneutic understanding comes from a knowledge of historical context, Dilthey argued that a “social science” (*Geisteswissenschaft*) cannot simply use the logic of the methods of natural science. Weber utilized Dilthey’s insight about *Verstehen* to modify

the idea of a “science” that would deal with social actors and social structures. So his mature contribution is an interpretive “understanding sociology” (*verstehende Soziologie*), combining adequacy at the level of “meaning” (*Verstehen*) AND adequacy at the level of “causal relations” (the French *sociologie*).

Turner explicitly privileges a type of “scientia” which is concerned with “laws.” I am arguing here that while the search for laws which are valid for all times (T-u) and places (S-u) for a particular “universe” (i.e., this earth) is eminently worthwhile, the search for such laws is not really what most of sociology has been about. If we broaden “scientia” to its original Latin meaning we can see that an “understanding sociology” (*verstehende Soziologie*) requires us to not limit our theorizing to the construction of generic laws (GSPs and GFs). Any attempt to use *Verstehen* to interpret things as if they were generic is likely to fail. For example, Goffman’s insights concerning the presentation of self are heuristic for the ITM of “the Self” in North America post World War II (analogous to Riesman’s “other directed” personality) but may be much less valid when applied to other times and places (e.g., a twelfth century Tibetan Buddhist monk).

To Turner’s credit, he is one of the few textbook authors who, along with his co-authors (Turner et al 2002 [1998]), explicitly mentions Dilthey’s Methodology and “Weber’s Response to Dilthey’s Work.” The Ideal Types (and ITMs) help us to assess the importance of meaning in context. Concepts like “the Reformation” and “modern bureaucracy” become ITMs that help us to understand the context of individual meanings and social actions. (Which, in part, helps to answer Coleman’s strictures.) However, after a very clear presentation, Turner nevertheless attempts to utilize Weber’s work to “formulate a set of timelessly valid propositions.” He and his co-authors bemoan the fact that ‘very few such statements can be gleaned from his works’ ([1998] p. 182). But they do not

ALSO allow for the possibility that since Weber was not primarily interested in “timelessly valid propositions” his approach is useful in and of itself. Going on to do something different is o.k., but it should be made clear to students that the initial work is not therefore to be regarded as lacking or incomplete.

There is nothing wrong with extracting “law-like” propositions from Weber’s work. But, the ITM’s themselves should also be viewed as valuable. Hence, for example, the term economic “class” essentially refers to “modern” and legally regulated, capitalist money economies. To use the term “class” generically is to move beyond any historical context that could be used for testing social psychological generalizations. The ITM as a context for *Verstehen* is a central Methodological insight about the problematic nature of interpretation based on generic, strictly universal (T-u & S-u) laws. It is a bit surprising that this way of viewing Dilthey-Weber “understanding sociology” is so de-emphasized in Turner’s dream.

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**Endnotes**

1. They are discussing Durkheim's *Rules* in this passage. But there is no explicit mention that Durkheim's rules concerning the construction of "general types" or "classificatory types" might have any Methodological relationship with Weber's ideas concerning "Ideal Types." Weber, too, sought to ignore certain kinds of complexities. But he did not do so in order to avoid acknowledging the "uniqueness" of a historical context. Weber did not attempt to develop a typology of stages of evolutionary development of "societies" (in a generic sense) because he did not believe it was possible.

## Book Announcement

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University of California Press  
July 2002

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VOICE *from page 5*

organized the previous year. We've also met at the Anaheim ASA meetings and will meet in Chicago and at future ASA meetings. We invite all those interested in this debate or in our own efforts to join us during the evenings scheduled for our "shadow conference" in Chicago. I also invite interested readers to correspond with me at bernieflps@aol.com As founder and Co-Editor of Aldine's "Sociological Imagination and Structural Change" series, I also invite contributions.

I believe that the problem of understanding human behavior is far more complex than biophysical phenomena. It is nothing less than our bureaucratic and stratified social structure and worldview—and not merely our sociological orientations to the scientific method—which have diverted us from opening up to that complexity. I also believe that we sociologists are in a far better position than any other group—whether politicians, journalists or academicians—to make headway on the exceedingly complex problems within modern society. In my view, we all have an urgent responsibility to find ways to achieve the rapid cumulative development of our knowledge.

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