

*STIFTUNG VOLKSWAGENWERK AND FORD FOUNDATION*

OR

*STATA CORPORATION AND SPSS, INC.:*

**WHO RULES SOCIOLOGY?**

*A Supply-Side Driven Critique of the Discipline's Segmentation*



Paper Presented at the ASA Annual Meeting 1999

*August 6-10, 1999*

Available Online: [http://www.gwdg.de/~uspw/tkoenig/papers/structure\\_of\\_sociology.pdf](http://www.gwdg.de/~uspw/tkoenig/papers/structure_of_sociology.pdf)

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## **SUMMARY**

Genesis and consequences of sociology's segmented differentiation are discussed. It is argued that sociology's current differentiation is not a result of theoretical considerations, but instead has been largely determined by social developments. In particular, social movements have increased their grip on sociological theory, as many activist scholars have a greater allegiance to their movement than to the academy. The commercialization of sociological literature has put some additional extra-scientific pressures on the discipline. As a consequence, sociology has become organizationally proliferated along lines that have little to do with intra-disciplinary developments. This dysfunctional segmentation has led to a weakening of sociology towards other extra-disciplinary influences. Namely, particularly in so-called "quantitative sociology," a growing dependency on commercial enterprises in the fields of data collection and data analysis can be observed.

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## 1 THE THESES

“List the major subfields of sociology. Then try to arrange them in some pattern that has more intellectual bite than alphabetization. Hard, isn’t it?”

(Davis 1994: 180)

That the social sciences industry depends on its owners and consumers, that is, those persons or institutions that fund these disciplines and — to some limit — “consume” their product is an old hat. That corporations or governments might influence through their funding policies the content of research, has been widely debated. Much less attention has been paid to the fact that — just like in any other industry — not only the demand side, but also suppliers (organizations and persons that offer data or data processing material) influence the final product, i.e., in this case sociological theories.<sup>1</sup> I will argue that this neglect of the influence of supply-side organizations constitutes a caveat of current research in the sociology of knowledge.

In my contention, this gap presents a problem, since sociological research — particularly its so-called “quantitative” variety — over the last century has become more and more driven by the ease with which certain data and data tools are made available. The reason for this development in my view lies in the organizational outlook of the sociological profession. It is the segmentation rather than functional differentiation of the profession that guarantees a critical information advantage data and data tool suppliers enjoy over their consumers, i.e., the community of sociologists. Let me now explicate these contentions:

## 2 THE ARGUMENT

My main theoretical propositions are:

*Conjecture 1:* The current differentiation of the sociological profession is one of segmented style.

*Conjecture 2:* The origins of this segmentation lie in an excessive orientation of sociology at extra-scientific concerns, namely social problems as they have been constructed in the public sphere.

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<sup>1</sup> Of course, I am not the first one to focus on this relationship. Although I am sure even earlier criticisms do exist, Adorno & Horkheimer’s ([1947] 1975) critique of social research à la Lazarsfeld comes to my mind first. More recently, though, supply-side oriented explanations of social-scientific work have been either historically oriented (e.g., Kern 1982) criticizing what I see as the excessive supply-side dependency or they have even considered elevating “pragmatism” from a sociological account of science to the level of epistemology (Fuchs 1993: 26ff).

*Conjecture 2.1:* The two major collective actors that determine the research program in sociology are the polity and social movements, with additional substantial input from corporations.

*Conjecture 2.1.1:* Polity and corporations influence the research program through funding policies, that is, through material resources.

*Conjecture 2.1.2:* Social movements influence the research program through the cognitive influence of sociologists and through the entrance of social movement activists *as activists* into the academe, that is, through cultural capital.

*Conjecture 2.2:* Over the last half-century, the influence of social movements on the sociological research program has increased. At the same time, the strength of all other influences has oscillated around a constant level.

*Conjecture 2.2.1:* This increasing influence of social movements has been partly due to an increased incorporation of activists *as activists*.

*Conjecture 2.2.2:* Another major pacemaker of the growing influence of social movements has been the commodification of sociology, which selectively promotes sociologists concerned with social movements' topics that sell well.

*Corollary 2.2.3:* The extra-sociological influence on the sociological research program has increased.

*Conjecture 3:* The segmented organization of sociology leads to a neglect of sociological epistemology, theory, and methodology, that is, the ideational aspect of the discipline.

*Conjecture 4:* The weakness of sociological input has made sociological theory vulnerable to supply-side influence. That is particularly true for so-called quantitative sociology that relies almost exclusively on outside providers for data and research tools.

In sum, we arrive at

*Corollary 5:* A demand-side driven weakness makes sociology susceptible to a further supply-side driven weakening.<sup>2</sup>

This theoretical skeleton points the finger at an empirical problem (the weakness of sociology) and contains a couple of non-trivial hypotheses in the form of "unintended consequences:" For one, most social movement activists turn to sociology with a focus on the solution to problems. It is precisely this focus that makes it difficult to obtain those solutions from sociology. Second, market forces foster the institutionalization of collective actors that might very well once turn against the market. It even contains some possible policy implications: for instance, motivating social movement activists

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<sup>2</sup> Systems theorists might want to frame this as a strong environmental influence on the autopoietic capacities of the social science system. Rational choice disciples may speak of a sellers' market at the interface between sociology and its data (processing) suppliers and a buyers' market at the interface between social sciences and its consumers.

not to look for solutions, if they want to find some<sup>3</sup>. Better yet, it creates a theoretical headway, as it modifies the following conventional sociological wisdom:

“Among the most well documented conclusions from studies by sociologists and historians of science is the conclusion that scientists constantly engage in activities to create, defend, and reinforce their intellectual, social, and political turf.” (Moore 1996: 1592)

At least for sociology and, by implication, for all policy-driven science, this conjecture is only partially tenable. Sociologists might very well attempt to defend their “intellectual turf.” However, they fail to do so, as they concede intellectual territory to outsiders, when expanding their “social turf.” My model thus seems promising, if true. But is it theoretically reasonable? Let me explicate.

## 2.1 The Segmentation of Sociology

Currently, sociology is unified through a mechanical, rather than organic solidarity in Durkheim’s ([1893] 1984) terminology. Over the last century, the discipline has become differentiated in the form of *segmentation*. Some might want to call the process that has led to this type of organization “professionalization,” but it is more aptly termed “proliferation.” (Turner & Turner 1990: 147ff). At present, sociology’s major organizational subgroups encompass not only such traditional subfields as sociology of religion, sociology of knowledge, political sociology, but also more recently evolved narrower specializations, such as Latino/a studies, studies of nationalism, etc. In contrast, functional subgroups specializing in certain methodological, theoretical or epistemological problems are much less frequent. For instance, of the 39 *American Sociological Association* (ASA) sections that currently maintain a website, thirty are concerned with “substantive” issues, while only three<sup>4</sup> — *comparative and historical sociology*, *mathematical sociology* (as subfields of sociological methodology) and *rational choice* (as one of theory) — are decidedly covering portions of sociology carved out along functional lines. Two further sections are concerned with methodology and theory at large. Not a single section is primarily concerned with epistemological issues.

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<sup>3</sup> Actually, this is a typical *Tragedy of the Commons* (Hardin 1968), thus somehow — either through institutional structures or a culture of professionalism — the *free rider problem* has to be overcome. There are other possible policy implications. For instance, Marxists might have noticed that the overthrow of the capitalist economy would also help (Conjectures 2.1.1 and 2.2.1).

<sup>4</sup> It could be argued that the section on *Marxist Sociology* also represents a functionalist differentiation.

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 Robert E. Park & Ernest W. Burgess (1921):  
*Introduction Into the Science of Sociology*

 Neil J. Smelser ([1967] 1994):  
*Sociology*, 5<sup>th</sup> ed.
 

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*Common Subdivisions*

 Collective Behavior  
 Social Control

 Collective Behavior and Social Movements  
 Deviance and Social Control

*Subdivisions Driven Primarily By Theoretical Developments*

 Isolation  
 Social Contacts  
 Social Forces  
 Competition  
 Conflict  
 Accommodation  
 Assimilation  
 Progress

 Socialization  
 Social and Cultural Change  
 Organizations  
 The Economic System  
 The Political System

*Subdivisions Driven Primarily by Societal Events*

 Inequality, Stratification, and Class  
 Community and Urban Life  
 Ethnic and Racial Inequality  
 Sex Roles and Inequality  
 Age and Inequality  
 The Family  
 Education  
 Religion  
 The Dynamics of Populations
 

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**Table1** Theoretical Subdivisions of Sociology According to Major Textbooks

Some might want to argue that the division of labor I am calling for already exists, and there is some truth to this point of view. There are sociologists who have successfully specialized in theory, and there are others, who — earning much less distinction (Davis 1994: 187, footnote 10) and mainly in quantitative methods — specialize in methodology. However, the segmented differentiation is far more pervasive. For example, in 1989 specializations in *Marriage and the Family*, *Sociology of Sex and Gender*, *Medical Sociology*, *Race / Ethnicity / Minority Relations*, all sociological subfields that revolve around social, not theoretical or methodological issues, were all among the top seven specialties ASA members identified as their foci of research (Ennis 1992: 261). Worse, hierarchical cluster analysis of joint specialty patterns of the ASA members (*ibid.*) reveals that *Sociology of Sex and Gender* is closer related to *Medical Sociology* than to *Race / Ethnicity*, with which it shares the strong theoretical concern with identity. *Military Sociology*, seemingly a natural ally of *Social Organizations (Formal and Complex)*, is as closely related to *Sociology of Art and Leisure* as it is to the former.

## 2.2 A Demand-Side Explanation

A great deal of this differentiation can be explained by the current economic pressure to frequently publish in order to maintain or achieve professional status. As it has been put in a rare organizational analysis of the profession,

“[t]he organization; differentiation of the ASA reflects, more than it has caused, the proliferation of subfields and specialties. [...] There is, in a very real sense, a specialty for everyone in American sociology, particularly when it is recognized that there are multiple subspecialties for each of [...] the] 50 or so ‘main’ areas of specialization.” (Turner & Turner 1990: 157, similarly: Starr 1983)

But the economic demand argument carries us only thus far; it mainly explains that a proliferation of sociological specialties has occurred, but is essentially silent on the question, how this differentiation is structured in practice.

### *The Institutionalization of Social Movements*

The segmented differentiation is in no way a “natural” outcome of theoretical immaturity and organizational proliferation of the discipline. Table 1 shows the identification of subfields according to two prominent textbooks of sociology, one is from the first, the other from the second half of this century. Reflecting the enormous changes the discipline has undergone over the course of the century, there is hardly any nominal overlap. Naturally, even less substantive commonalities would be found, if one were to look at the actual content of the books. For present purposes it is more important, though, that it appears that the subdivisions of the field have become less oriented to theoretical developments within the discipline and, instead, have become tied to societal events. While Park and Burgess’ (1921) subdivisions such as “accommodation” and “isolation” still display a high level of abstraction, Smelser’s ([1967] 1994) divisions reflect either already an acute awareness of “culturally resonant” (Berger 1971; Gamson 1992) frames of social problems (e.g., “ethnic inequalities”) or at least are located at a much lower level of abstraction (e.g., “the political system”). In sum, while previously the subdivisions of the field seem to have flown from the theoretical state of the art, more recently they have “been dictated by real and perceived social trends in the larger society” (Smelser 1990: 53).

It is instructive to look at the content of the subfields that have developed in practice. There are now, e.g., African American studies departments<sup>5</sup> at a multitude of universities (“obviously,” that is,

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<sup>5</sup> I am aware that African American studies is — strictly speaking — not a subdiscipline of sociology, but encompasses many humanities and social sciences. Nevertheless, there are sociologists who specialize in African American studies. Thus, this distinction is irrelevant for the argument put forth here.

following the logic of genesis of subfields, only in North America) and there exists an Institute for Research on Poverty at the University of Wisconsin, but one will be hard pressed to find any equivalent for “white trash studies.” One finds an ASA section for “alcohol and drugs,” but searches in vain for one on “poultry and horse meat,”<sup>6</sup> although, after all, many people do eat poultry. The point here is to illustrate — admittedly bluntly —, that it is no accident that “collective behavior and social movements” is one of the two specializations that has survived the major restructuring of sociology, as social movements have become incorporated into the academy. African American studies have been made possible by the civil rights movement and although the temperance movement was ill timed for an incorporation in the academy, movement organizations such as *Mothers Against Drunk Driving* were certainly instrumental in the construction of an “alcohol and drugs” subfield. Thus, at least since the 1950s, the history of sociology and the history of social movements have become intimately intertwined. In the beginning it was what Blumer (1951) has called general social movements that inspired sociologists in their selection of subfields. Later, “the trend of social movements towards professionalization” (McCarthy & Zald 1973) also led to their institutionalization as subspecialties within the social sciences and the humanities. The focus shifted from theory to practice, as many social movement activists entered the academe as activists, whereas before they would enter the academy as scholars, who considered their activism an extra-professional activity (Lipset 1994). Either way, “the sociology of peace and war is part of the peace movement.” (Gamson 1990: 88)<sup>7</sup> In short, since some decades ago

“students with practical or reformist interests [have become] the basic audience and resource [of academic sociology], and ‘scientific’ sociology [has] survived only as a small component of a larger discipline that met different needs of its principle constituents.” (Turner & Turner 1990: 181)

Thus far, there hardly arises any scientific problem, though. At least since Weber ([1904] 1988: 158; [1904] 1949: 61) it has become basically uncontestedly (e.g., Dahrendorf [1962] 1989: 146; Keuth 1989: 20f; Little 1993: 376; Popper 1963: 46) clear that the questions asked in the scientific endeavor depend on the social and political standpoint of the involved scientists. Depending on one’s own political standpoint, one might then criticize the selection of research projects on political

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<sup>6</sup> The sale of horse meat has become outlawed in the state of California as of November 5, 1998, thus mirroring the legality issue of the drug topic.

<sup>7</sup> There are numerous treatments of the relationship between social movements and sociology. For instance, Seidman (1993) gives an exemplary overview of the development of gay and lesbian studies through the efforts of gay liberationists, and later lesbian feminists and queer nation. On the other hand, Taylor and Raeburn (1995) discuss some of the problems some activist scholars might face.



or moral grounds, which in this case has been done from both the “right” (e.g., D’Souza 1992) and the “left” (e.g. Gitlin 1997a; Gitlin 1997b; Kauffman 1990; Paglia 1991).

But although I do agree with most of the latter criticism on a moral level, the argument I am making here is of a different kind, namely epistemological in nature. It hinges on the fact that the current differentiation of the discipline comes at the expense of a functional differentiation, as “[n]either Theoretical Sociology nor the Methodology section [of the ASA are] central to the structure of the discipline” (Cappell & Guterbrock 1992: 271).<sup>8</sup> Hence, the proliferation of the discipline along socio-politically generated lines is an obstacle to its theoretical maturation, as a mature science is characterized by cleavages that arise out of its ideational history (Krauze 1972), which, of course, is centered around theoretical and methodological questions. “There is a lack of standards that accompanies the lack of coherence,” (Wolfe 1992: 770), *inter alia* because extra-scientific actors have gained influence on intra-scientific decisions.<sup>9</sup> Before I develop the “theoretical mechanics” of this conjecture, let me briefly consider the ideational justification for the segmented proliferation of sociology, though.

#### *Ideational Justification of the Segmentation*

The segmented and event-driven mode of sociology’s differentiation is not only a result of “sociology in action.” It is also firmly grounded in some hidden ideological premises that many, if not most sociologists treasure.

There are at least two conscious ways to justify some or all of the current segmentation of the discipline. One could claim that some segments are actually theoretically guided subfields of the discipline, or one could argue on the epistemological plane that segmentation of sociology is, in fact, functional for the “progress” of sociology. My argument, though, is that neither of these two venues is typical for the actual reasoning of sociologists. Instead, a tacit misunderstanding of epistemology renders the problem of segmented differentiation invisible. Let me briefly outline each of these three possibilities.

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<sup>8</sup> Inexplicably, the authors still go on to conclude that “much sociological joint-specialization can be explained by ideational forces.” (*ibid.*, p. 272)

<sup>9</sup> I deliberately use here a terminology that some might brand — wrongfully, in my opinion — “positivistic.” For one, I follow Knorr-Cetina (1981: 136ff) in the observation that the distinction between social and natural sciences is largely based on a misunderstanding of the actual epistemology of the latter and, thus, is tenuous at best. Secondly, indeed, for sociologists who have followed Feyerabend (1975) in their epistemology there is little value, indeed, in the argument put forth here. However, for reasons that cannot be discussed here due to space limitations, the epistemology adopted here follows in large parts Popper ([1934] 1966).

### Theoretical Foundation

Alain Touraine has taken the theoretical road to justify and, indeed, foster one specific subfield of sociology, that is, social movements. Touraine (1985; 1988: xxiv) suggests that social movements should become one of the new major building blocs of a theory of society. Yet, at the same time he

“defends the idea not to fuse societal and sociological categories, but on the contrary to increasingly decouple the two systems.”  
(Touraine 1981: 76, translation mine, emphasis in the original)

Touraine thus does not argue in principle against a functional differentiation of sociology, but, on the contrary, seeks to end a segmented differentiation. In reality, he simply redefines one, and only one, specific existing cleavage in sociology as functional. Moreover, — for good reasons<sup>10</sup> — Touraine’s approach has not become dominant in the field of social movements, much less in overall sociology. In no way Touraine’s suggestions can therefore serve as a theoretical justification for the current segmental organization.

### Epistemological Foundation

Slightly more common and theoretically less ambitious are epistemological arguments that suggest that sociology should not be preoccupied with functional differentiation, as it makes no theoretical headway. This is the position Stephen Cole takes. Cole acknowledges the fact that

“[i]nstead of sociologists selecting their research problems to address pressing issues, most sociologists do descriptive work that is motivated by their personal interests and sometimes experience.” (Cole 1994: 148)

Cole is not the least bothered by this situation, though, as what he calls general theory is anyway doomed to remain either infeasible or tautological, as “i n t e r e s t i n g sociological phenomena tend to be more nearly idiographic than nomothetic” (*ibid.*, p. 141, emphasis mine). As far as Cole is concerned,

“[p]hysicists study phenomena that as far as we know never change. The structure of an atom is the same as and the same as it was a million years ago and the same as it will be a million years from today. Further, the structure does not change from one country or from one material substance to another.” (*ibid.*, p. 138)

By the same token, Isaac Newton could have answered:

“Sociologists are in really good shape. The apple on which I studied the gravity principle yesterday, is today of completely different structure and substance; I ate it.

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<sup>10</sup> For instance, there is little evidence that supports Touraine’s axiom that every society contains a central conflict (Brand 1996: 53), his theoretical concepts also frequently remain ambiguous (Cohen 1985: 701, 707; Rucht 1991: 370), the adequacy of his methods is questionable (Japp 1984: 327; Rucht 1991: 396ff), and his receptivity of competing theories is underdeveloped (Gamson 1983: 814).

What is more, I walked up the hill and repeated my experiment with a cannon ball, which resulted in completely different values for the gravity constant.<sup>11</sup> The state I am living in, on the other hand, has still the same laws, still the same ruler.”

Which is where I rest my case. The problems sociological theory has run into really are not a matter of the inviability of sociological theory in principle, but originate in the fact that the “interesting” social problems are imported in an atheoretical and positivist fashion as points of departure in sociological theory. Not because “interesting” problems cannot be explained through general theory, the interest in general theory has faded, but because the interest in general theory has faded, general theory cannot illuminate “interesting” problems, as a strong problem orientation hampers theoretical development.<sup>12</sup>

#### A Tale of the Unity of Theory and Method

It thus seems difficult to justify the current segmentation on either theoretical or epistemological grounds. This is probably the reason why in actuality neither theoretical nor epistemological arguments play an important role in the development towards segmentation of the discipline. Instead, the current situation is probably best described as an *ex post* rationalization of the segmentation. The basic idea that upholds the current organizational form hinges on the notion that the choice of

“[m]ethods does and should not depend on a methodological ideal, but on the object of study and the theoretical approach (*der Sache*).” (Adorno [1962] 1989: 130, translation mine)

This is certainly a point well taken. However, I contend that this epistemological insight frequently masks a situation where neither theory nor ensuing methodological approach are carefully selected. It is the object of study takes precedence over both. As a consequence, sociological segmentation arises.

At the same time, functional differentiation is sacrificed on the altar of the unity of theory and method. From the proclamation of the unity of theory and method in sociological research, sociologists have jumped to a necessity of an integration of both theoretical and methodological, even epistemological developments within the same research project, which is more often than not conducted by a single researcher or a researcher employing several “apprentices.”

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<sup>11</sup> Note, that according to the current dominant opinion in physics — even without going into the problems Heisenberg’s uncertainty principle poses or whatever else modern theoretical physics might have in store — the Newtonian gravity constant is not a parameter in the strict sense, but in effect depends on both the mass of the object and distance from the earth.

<sup>12</sup> This is simply the reversal of van den Daele, Krohn & Weingart’s (1979: 45) insight that theory orientation of a science is incompatible with external problem orientation.

But while it is certainly true that theory and methods in sociology, an organizational and thus more often than not personal integration of the two strands does not seem to be warranted. In fact, in most natural sciences the development of a theory or its translation into a specific methodological approach and the actual application of the method are customarily performed within the same organizational framework, the laboratory, where there are different persons executing the different tasks. But we do not have to venture as far as physics or biology to see that a division of labor along functional lines would not be detrimental to the value of sociological theory. After all, the most successful theories have been developed under these conditions. From Coleman to Elster, Foucault to Lyotard, or Habermas to Luhmann, the most appraised sociological theories of the late 20th century have been constructed by scholars that relied only cursorily, if at all, on empirical research in their most important theoretical works.<sup>13</sup>

This does not mean that empirical validity is unimportant. To the contrary, I do believe that empirical falsification attempts are essential to test the quality of a theory. That, however, is an altogether different question from the question of how to test a theory. Indeed, to avoid the pitfalls of positivism, it is not only possible but even desirable to organizationally separate theoretical and empirical endeavors. Not only such separation relieves the theorist from continual concerns with methodological feasibility, but it also introduces a safeguard against inductive theorizing, which supplants my efficiency argument with an epistemological underpinning.

### **2.3 The Weakness of Sociology**

Thus, I contend, the ideal of the sociological “allrounder,” who integrates epistemological, methodological and theoretical sophistication in his or her research via a somewhat hazy understanding of the unity of theory and method, is cherished by many sociologists. But the tale of the allrounder all too often does not fit reality. The evolvement of “issue sociology,” far from integrating epistemology, theory, and methodology instead neglects all three. Let me consider each of them in turn.

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<sup>13</sup> Obviously, there do exist exceptions, most prominently probably in the figure of Pierre Bourdieu, who managed to combine massive empirical fieldwork with grand theorizing in *Distinctions*.

*Epistemology: The (Unintentionally) Nationalist Sociologist*

The apparent neglect of epistemological issues, in Kuhnian fashion<sup>14</sup> relegated into a subfield of sociology of knowledge, can be used to illustrate some of the problems that have resulted from the segmentation of sociological research.

A case in point is the incapacity of much of recent sociological literature to seriously apply the basically uncontested<sup>15</sup> epistemological view that a sociologist must engage in a “second-order observation” (Luhmann 1990: 15ff). What is meant by this is that sociologists, as observers of social action, which is frequently communicative action, need to “break” with everyday language categories in constructing a theory (Bourdieu, Chamboredon & Passeron [1973] 1991), as these are unmediated data that should be used only in attempts to falsify a theory. You might want to call this process of “breaking” a “deconstruction” of everyday categories. Yet, despite the current fad of what is called “postmodernism,” deconstruction of categories — constantly reiterated in theoretical writings —, rarely takes place in the actual research practice or theory construction, nor does it affect the organization of the profession.<sup>16</sup>

I am certainly not the first sociologist who observes that one of the most common cases of the violations of this epistemological insight is the confusion of the key category of the discipline — society — with the nation state.

Recall Stephen Cole’s explicit justification of issue sociology partially hinged on the observation that social phenomena “vary from one country to another.” From there, it is only a small step to assault general sociological theory with the following example:

“Consider [...], why in a single society — the United -States — there have been fewer women in a high-reward field like medicine.” (Cole 1994: 139)

But, of course, such conceptualization stems from a

“prejudice which blocks conceptual development [and that] consists in the presumption of a territorial multiplicity of societies. China is one, Brazil another, Paraguay is one and so too then is Uruguay. All efforts of accurate delimitation have failed, whether they rely on state organization or language, culture, tradition. Of

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<sup>14</sup> Although Kuhn’s ([1962] 1976) *Structure of Scientific Revolutions* is strictly speaking only a sociological, at times even merely historical account, of the actual research process, it can easily be transformed into an epistemological standpoint, if one considers the current research practice as normatively valuable. The latter is my reading of Kuhn’s theory.

<sup>15</sup> There are exceptions, Kreutz (1993) relying on Durkheim’s ([1915] 1965: 486) notion that collective representations cannot be wholly inadequate for explaining social reality, advises to merge categories of practice and categories of knowledge.

<sup>16</sup> Billig (1995) forcefully shows that ironically Richard Rorty is among those scholars that commit this mistake consistently.

course there are evident differences between living conditions in these territories but such differences have to be explained as differences within society and not presumed as differences between societies. Or does sociology want to let geography solve its central problem?" (Luhmann 1992: 68)

Sure, Cole's *faux pas* is a c o m m o n one. Even "in the case of Max Weber there is evidence that his support for German nationalism directly influenced his conception of 'society'" (Billig 1995: 53) and, indeed,

"[ m ] a n y twentieth century sociologists, when speaking of 'society', no longer have in mind (as did their predecessors) a 'bourgeois society' or a 'human society' beyond the state, but increasingly the somewhat diluted ideal image of the nation-state." (Elias [1976] 1991: XXXVII, emphasis mine)

If even the very concept that defines the discipline is frequently epistemologically marred, there seems to exist some chance that epistemological insights would not penetrate sociological theories as much as they could. Still, result-oriented scholars might want to argue that as long as sociological theory works well, we should not quibble about some epistemological objections that primarily are of aesthetic value. So how does sociological theory fare?

*Grand Theory: The Relationship Between Storks and Infants Revisited*

"A comparative study of nationalism [Greenfeld 1992], for example, may require fluency in five or more languages, surely a sign of rigor by anyone's standards."

Wolfe 1992: 777

When on the pages of the discipline's theoretical flagship journal an author's abundant linguistic skills are defined as a theoretical stringency,<sup>17</sup> one begins to wonder about the discipline's theoretical aspirations. And, in fact, it has been shown that for all practical purposes most of the discipline's theoretical headway has little, if any, impact on the bulk of the "empirical" research.

"That does not mean that general theorists are not cited by sociologists who do empirical research; but these citations usually appear at the beginning of the article as a ceremonial citation and have little influence on the actual conduct of the work." (Cole 1994: 140)

While this is probably an overstatement of the problem, I do think that in particular studies that utilize statistical methods at least sometimes throw in "the usual suspects" (race/ethnicity, gender and

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<sup>17</sup> Ironically, among the numerous strengths of Greenfield's study one probably cannot count analytical coherence, as "[r]ather than being integrated into a coherent comparative analysis, the five case studies [...] mostly stand alone." (Hechter 1993: 504). "Greenfeld does not clearly channel a wealth of documentary detail. In much of the book vast amounts of historical data are piled up without indication of real relevance to the argument." (Pryke 1994: 314).

income as a proxy for class or status) as control variables without having spent a fair amount of time on their theoretical justification.

As an illustration, let me take an example from my own field of specialization, that is, religious and social movements. The example is not intended to be in any way representative of sociological work in general, but, instead, should simply illustrate a kind of research which is possible within the discipline. Whether the research procedure that has marred the work in question is a simple outlier, part of a sizable minority of work done by the profession, or if it is common practice, presents an empirical question that is yet to be investigated.

The piece (Bibby 1997) that should illustrate my point is concerned with religious affiliation and disaffiliation patterns. Its central hypothesis is that one's geographical mobility no longer predicts the volatility of one's religious affiliation. As is customary, the study starts out to name a few variables that have been put forth by theorists<sup>18</sup> as possible explanatory variables. The identified variables include "secularization, [...] individualism, [...] higher education, [...] growing diversity and relativism, [...] socialization and,] institutional factors," (Bibby 1997: 293) but notably neither "gender" nor "race." Yet, oddly enough, in the actual empirical analysis these two variables do turn up as "social characteristics," presumably because of their potential explanatory power (*ibid.*, p. 300). The author's reference list does not contain any hints about why gender or race might be important variables with respect to religious affiliation patterns. Nor do we get a clue about what kind of theoretical concept of either race or gender the study advocates. Thus, the author is either unaware of the voluminous debates that surround these two concepts, or he regards them as irrelevant. Either way, the selection of the variables does not seem to be theoretically driven, but instead seems to have rested on different considerations.

My contention is that the selection of variables in this and many other cases instead rested on the simple fact that the variables in question were available.<sup>19</sup> By "available" I do not mean that these were the only indicators available to measure a theoretical concept, which would present the problem discussed here as a pragmatic one. I contend, instead, that the author used the variables simply because they were "there." Gender and race (however defined!) simply were some of the variables that were contained in the analyzed data set. Guided by the principally valuable desire to avoid spurious correlations, the author has replaced the Satan with the Beelzebub.

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<sup>18</sup> In line with Cole's conjecture, there are such theoretical heavyweights as Talcott Parsons and Robert Bellah among the theorists named.

<sup>19</sup> Hacking (1990) makes a similar argument regarding the use of census data.

Almost anyone who has ever taken a class in statistical methods has heard the tale of the significant positive correlation between the number of children born in a region and the number of storks that live in the same region. Usually, this tale is told to sensitize students to the possibility of spurious correlation, and to introduce them to methods (currently usually multivariate regression) which can detect spuriousness. While this might be an effective device (it worked for me) for making students internalize the importance of control variables, it often remains unexplored that only a hard core positivist would have computed the correlation between storks and infants in the first place.

As scholars on both sides of the so-called positivism dispute have pointed out, such positivist endeavors hinder the theoretical development of sociology (Adorno [1957] 1989: 82; Albert [1965] 1989: 270; Dahrendorf [1962] 1989: 148; Habermas [1963] 1989: 159; Popper [1962] 1989: 107). Even without epistemological quibbling, Bibby's proceeding is highly questionable, though. Imagine, for the moment, that Bibby had found a significant relationship between race and religious affiliation volatility,<sup>20</sup> but, in reality, this relationship would have appeared due to chance. This is not an entirely unlikely situation: Bibby excluded 13 variables on the grounds of too low Pearson's  $r$ 's. Let's assume, for the sake of the argument, that indeed these relationships are zero in reality. Assuming further a significance level of .05, there would have been roughly a 50/50-chance<sup>21</sup> that one of the coefficients would have turned out significant in a random sample. It is conceivable that Bibby could, thus, have "invented" another relationship between storks and infants.<sup>22</sup>

We need not despair, though. It is no accident, in my view, that Bibby's article was published in *Review of Religious Research* and not in, say, *Rationality and Society* or *Leviathan*, that is, in journals centered around a methodological program or theoretical issues instead of social ones. My hypothesis then becomes that those scholars who pay little attention to methodological, theoretical and epistemological problems publish mainly in "issue" journals, because their reviewers frequently have an extra-scientific stake in the selected topics, and, are, subsequently, more likely to overlook

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<sup>20</sup> Actually, we have no way of knowing, if not indeed the relationship was significant, as Bibby does not report either standard errors or significance levels, but instead recurs to an arbitrary cut-off value of  $r=.15$  for the decision, if or not to include variables in an ensuing path analysis. There is a ton of other methodological problems in the article, but that is a story to be told below.

<sup>21</sup>  $p=1-(.95)^{13} \gg 49$ .

<sup>22</sup> A real-world example for such a stork-infant-relationship is relative deprivation theory that came about through tinkering with attitude data (Lazarsfeld 1963: 766). The concept was hugely successful until the early seventies (Marx & Wood 1975), but ultimately became one of the few sociological ideas "that ever turned out to be demonstrably wrong." (Davis 1994: 181)



methodological shortcomings. Before I continue with some more specific hypotheses, let me have a brief look at problems with methodology.

*Methodology: The Temptations Posed by Mice*

One would think that a discipline, in which theory and epistemology frequently take a backseat is preoccupied with methodology. Alas, this is not necessarily the case. Let me focus on quantitative methodology, where in my view most deficits lie.

Let me submit three cases that show that the choices of quantitative methodology — unless written by specialists in methodology or coming from high-profile quantitative departments — like variable selection in the theoretical part of the endeavor also not infrequently depends primarily on availability, that is, `what is included in the standard statistical packages`.

In the question of structural equation models *versus* logistic regression, Davis (1994: 190f) has put forth numerous reasons, why the former for most purposes is preferable. Why are then still “the vast majority of studies [published in the leading journals] documented by columns of logistic regression coefficients” (*ibid.*, p. 190)? Davis himself does not give a hint, but it seems suspicious that *SPSS* for years has carried a routine on logistic regression, but not on structural equation models.

Considering latent class models *versus* various kinds of cluster analyses is a similarly clear-cut case. Unlike cluster analyses, latent class analysis offers the possibility of significance testing and has the virtue of explicitly modeling the non-observability or “latency” of most variables in sociology. In combination with the fact that it presupposes only nominally scaled variables, it should even be able to frequently beat out factor analysis. The possibilities latent class analysis offers, in particular with respect to the testing of typologies, have already been demonstrated (Hagenaars & Halman 1989). Yet, there seems to exist a shortage of actual empirical applications: The June 1998 *Sociological Abstracts* database contains altogether 44 records that contain the keywords “latent class analysis.” Of those, 28 are concerned primarily with methodological issues, while 16 can be considered mainly applications of the method, five of which are written by the pioneers of the method in sociology (Jacques Hagenaars and Alan McCutcheon). Compare that with 386 records containing “cluster analysis,” itself not a popular method. *SPSS* does contain a routine for cluster, but not for latent class analysis.

My third example concerns the failure of most statistically supported studies to incorporate the *Bayesian Information Criterion* (BIC) as goodness of fit statistic. In an article published in

*Sociological Methodology* (which is certainly not an esoteric journal that caters only to a tiny minority of scholars) Raftery (1995) argues at great length, why BIC for most purposes is better suited than  $\mathcal{C}^2$  to assess model fit: It puts a premium on the parsimony of a model (pp. 112, 117), it accounts for sample size and even allows comparisons across non-nested models (p. 134). What is more, it is dazzlingly simple to compute.<sup>23</sup> One would think BIC would have replaced or at least supplanted  $\mathcal{C}^2$  in most studies conducted after the publication of the article. Alas,  $\mathcal{C}^2$  is still the most commonly used measure and I have yet to find a regression analysis that reports BIC. Again,  $\mathcal{C}^2$  is the standard statistic *SPSS* reports, while BIC is not even available.

In sum, it seems likely that many decisions in quantitative methodology have less to do with a meticulous evaluation of methodological alternatives, but on what is available by the mouse click.

### 3 CONCLUSION

Does all sociological research fail to live up to epistemological, theoretical, and empirical standards? Of course not, much scholarship stands the scrutiny. Most research, including the present one,<sup>24</sup> could still be improved through an ideological and institutional reorganization of the discipline. As my argument has shown, a functional, rather than topical organization of the profession would be an

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<sup>23</sup> A good approximation is:  $BIC = L_k^2 - df_k \ln N$  (Raftery 1995: 135).

<sup>24</sup> There is, actually, a biographical reason for why I choose to suggest this project. While finishing my dissertation, I felt that the thesis was based on a selection of only a fraction of the available theories and methods, and that my knowledge of methodological approaches depended more on coincidences than on systematic selections, not the least on tinkering with *SPSS*.

The relatively unsystematic acquisition of knowledge is also one of the reasons why criticism of current research appears so effortlessly. Consider the following thought experiment: Assume all knowledge of a science is contained in one million conjectures and there are two researchers, Calvin and Garfield. Calvin knows one tenth of all conjectures, Garfield knows one thousandth; both have acquired their knowledge at random. Now, assume Calvin investigates a phenomenon, whose full explanation would require a thousand conjectures. Then, with about 60% probability Garfield will know one of those relevant conjectures Calvin does not know, and thus can effectively criticize the latter. Now imagine, both have specialized in one subfield that comprises one tenth of all conjectures, that is, they know proportionately twice as many conjectures in this field, i.e., 20% and 2%, respectively. Then, the probability that Garfield knows at least one relevant conjecture Calvin does not know falls below 15%. Hence, under these conditions, existing theories would require more knowledgeable persons for effective criticism, which would prohibit premature refutations of a theory.

(Probabilities have been approximated using the below formula, which cannot be explicated here for space restrictions.

$$p(n, c, g) = \sum_{i=0}^{n-1} \sum_{k=1}^{n-i} \binom{n}{i} c^i (1-c)^{n-i} \binom{n}{k} g^k (1-g)^{n-k},$$

whereby  $n$  denotes the number of relevant conjectures,  $c$  the proportion of relevant conjectures Calvin knows, and  $g$  the number of conjectures Garfield knows.

important step toward a more efficient sociology. To achieve such mode of differentiation the external influence on the sociological system needs to be reduced.

My main criticism is that supply side influence is conservative, and thus opposed to the scientific endeavor that strives for creativity, read: innovation. Regarding manufacturers of data processing tools, it is to be expected that software programs will simply become more comfortable to handle, rather than fostering a spread of little known, yet possibly superior techniques. The reason for this conservatism is that the creation of new demands is expensive, since a product which is entirely unknown to the prospective consumers needs the back-up of serious marketing efforts. The costs of a marketing campaign alone might not be worth the investment in a market as small as the social science community. Worse, most social scientists cannot be reached by traditional marketing methods, as they have an ethos of scientists, not business entrepreneurs. Thus it does not pay for a company to implement innovative solutions, unless the demand side, i.e., researchers, pull for their development.<sup>25</sup>

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<sup>25</sup> A real world example is the sample simulation program SAMP, whose most recent update “has not become more ambitious, just more attractive” (Davis 1997).

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