THE STATISTICAL TURN IN AMERICAN SOCIAL SCIENCE: COLUMBIA UNIVERSITY, 1890 TO 1915*

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Drawing on recent work in the sociology of science, we propose a sociological approach for understanding the process by which statistical methods were originally incorporated into the social sciences in America. In a departure from past accounts, which have viewed early statistical developments in the United States as part of the history of separate academic disciplines, we analyze interdisciplinary relations *and* local institutional conditions *in turn-of-the-century America to elucidate the adoption and use of statistical methods by James McKeen Cattell in psychology, Franz Boas in anthropology, Franklin H. Giddings in sociology, and Henry L. Moore in economics. We argue that these four thinkers were doing boundary work to legitimize their disciplines in a competitive interdisciplinary field, where they confronted the "newcomer's dilemma" of conformity versus differentiation in relation to other discilplines. All four innovators turned to statistical methods to demonstrate compliance with acceptable scientific models and at the same time carve out a distinctive mode of statistical analysis to differentiate their own discipline from the others. Our analysis also shows that these developments occurred only under certain local institutional conditions. Cattell, Boas, Giddings, and Moore were faculty members at Columbia University at a time when the University had gained a competitive lead in the area of statistics over rival universities. Determined to preserve this institutional advantage, Columbia provided a conducive setting for the interdisciplinary process of the incorporation of statistical methods into the social sciences.*

For more than a half century, statistical methods have been an integral feature of American sociology and social science. This central fact, however, has yet to generate more than scattered interest among sociologists in examining the process by which statistical methodology acquired its importance. This study is an exploratory effort to fill part of this lacuna by examining the incorporation of European statistical methods by thinkers in four social science disciplines at Columbia University at the turn of the century. Extending previous scholarship in the sociology and history of science, we propose a sociological account that focuses on the interdisciplinary and local institutional bases of this development.[1]

THE PROBLEM AND AN ANALYTIC STRATEGY

Two complementary areas of scholarship provide our point of departure: (1) the recent literature in the sociology of science and (2) historical research on the development of statistical methods in the natural and social sciences.

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[1] When referring to a source whose original publication date differs from the date of the edition of the work that is cited, our text first gives both dates, with the original in brackets, and thereafter gives only the original date (in brackets). Page references are to the edition cited.

The hallmark of recent work in the sociology of science has been the "turn to practice" (Latour 1993:489), indicated by the growing attention paid to the work practices of natural scientists and to the specific "tools" (i.e., the specific techniques, procedures, materials, instruments, models, concepts, etc.) they use in these activities. Raising the question "How do scientists pull together and articulate various tools in order to do their work?" sociologists of science have recently contributed a variety of case studies that have demonstrated the significance (for the construction of scientific tools) of an array of social organizational factors, including the infrastructure of local scientific institutions, the configuration of scientific disciplines, and so on (Clarke and Fujimura 1992b:3–7; for examples, see Clarke and Fujimura 1992a; Cozzens and Gieryn 1990; Gooding, Pinch and Schaeffer 1989; Latour 1987; Pickering 1992, 1993). To date, however, these studies have stopped short of addressing the topic we seek to understand; they have examined neither the *activities of social scientists* nor the construction of *statistical tools* of scientific research practice.[2] Moreover, because they focus on "particular practices [in] particular spaces, times, and locations" (Clarke and Fujimura 1992b:5), these case studies eschew—quite rightly, in our view—advancing some general theory under which the development of statistical methods in American social science might simply be subsumed as a specific instance. What the current literature in the sociology of science does offer, however, is a set of flexible concepts pertaining to the social organization of scientific practice that can be, as we will indicate below, selectively adapted to issues that this literature has yet to address.

The second area of scholarship we draw on consists of historical studies of the social and natural sciences where the focus is the development of statistical methods. These studies have taken two main forms. First, several valuable recent works have analyzed the emergence of statistical thinking among natural and social scientists in nineteenth-century Europe. Such works have examined the initial rise and early growth of statistical methods in terms of their internal development (S. Stigler 1986, 1987) and have investigated their link with the broader social, political, ideological, and philosophical conditions of the period (Gigerenzer et al. 1989; Hacking 1987, 1990; Heidelberger and Krüger 1982; Hilts 1981; Krüger, Daston, and Heidelberger 1987; Krüger, Gigerenzer, and Morgan 1987; Porter 1986). These studies furnish the basis, therefore, for our background discussion. Except in passing, they do not deal, however, with how various European statistical tools were subsequently incorporated into the social sciences in America. Focused on statistical innovations that occurred prior to the social reorganization of scientific activity in late nineteenth-century America, such studies likewise offer little guidance for understanding these later statistical developments in the social organizational terms discussed below.

The second form taken by the historical literature on statistical methods has been that of disciplinary histories (i.e., histories of specific scientific fields). At present, these histories are virtually the only works available on the early development of statistical methodology in American social science (for partial exceptions, see Clogg 1992; Duncan 1984; Easthope 1974). The literature on the history of psychology, for example, regularly examines the pioneering statistical work of James McKeen Cattell (Boring 1957, 1961; Jonçich 1968; Murray 1987; Ross 1973; Sokal 1972, 1981, 1987), while studies of the history of economics cover the statistical writings of Henry L. Moore (Christ 1985; Dorfman 1949, 1959; Morgan 1987, 1990; Persons 1925; Schumpeter 1954; Spengler 1961; G. Stigler 1962, 1965, 1968). Similarly, historians of anthropology often discuss the statistical contributions of Franz

[2] Among recent contributors to the sociology of science, Haila (1992) raises but does not pursue the topic of quantitative research practices. Among somewhat older studies, MacKenzie's (1981) work relates statistical developments in nineteenth-century Britain to "society at large," especially to the "interests and experiences of the professional middle class" (pp. 4, 46; see also Norton 1978). This is a seminal study, whose emphasis on interests may be compared with our own concern with thinkers' social-organizational objectives. However, we have not found MacKenzie's macro-level, class-factor model directly useful for understanding the institutional location and disciplinary pattern of development in American social statistics.

Boas (Golbeck 1980; Herskovits 1953; Howells 1959; Hyatt 1990; Stocking 1968, 1974a, 1974b; Tanner 1959; Xie 1988), and historians of sociology focus on Franklin H. Giddings's role in the adoption of statistical methods in sociology (Bannister 1987; Bierstedt 1981; Davids 1968; Hankins 1931, 1968; Hinkle 1980; Laslett 1990, 1991; Northcott [1948] 1967; Oberschall 1972; Odum 1951; Turner 1986, 1991, 1994; Turner and Turner 1990; Wallace 1989). In instances where such histories attempt to identify the sources of these early statistical developments, they generally assign explanatory importance to factors such as the personal and intellectual biographies of Cattell, Boas, Giddings, and Moore; various intellectual and professional issues internal to their particular disciplines; and larger economic, political, and ideological trends in turn-of-the-century American society.

While this informative research is sufficient for many purposes, it proves to be of limited value for addressing questions about the statistical turn in American social science as a more general phenomenon. By attributing explanatory significance to individual biographical circumstances or to factors internal to a particular discipline, this literature provides no adequate explanation for the incorporation of statistical methods by several disciplines at roughly the same time. Conversely, in appealing to trends in American society as a whole, this work also fails to account for the historical localization of these early statistical developments. Such problems are analogous to those Abbott (1988) found in the literature on the professions, when professions are "stud[ied] one at a time" and assumed to "develop independently of each other" (pp. 1, 90). In presenting each discipline as an intellectually separate entity, disciplinary histories overlook the fundamental fact that when Cattell, Boas, Giddings, and Moore pioneered in the statistical area, not only were all four clearly aware of the place of statistical work in other academic fields, all four were directly associated with one another as faculty members at Columbia University. Yet, given the neglect of social organizational issues in disciplinary histories, few scholars have even noted the existence of this Columbia connection (for exceptions, see Bannister 1987:77; Hilts 1981:603–604, 1982, 9–10; Turner 1986:3).

In our view, this historical literature can be complemented by the social organizational analyses found in studies by sociologists of science of the tools of scientific practice. These analyses have, to be sure, been quite heterogeneous. For the specific purposes of this study, however, we examine the turn to statistical methods that Cattell, Boas, Giddings, and Moore each carried out in his own discipline by concentrating on two social organizational factors that were particularly important during the turn-of-the-century period: *interdisciplinary relations* and *local institutional conditions*. We have selected the work of Cattell, Boas, Giddings, and Moore for analysis because the historical literature has made clear that, while these four were by no means the only American social scientists who adopted statistical methods, each was a leading early proponent—indeed, perhaps *the* leading early proponent—of statistical research in his own discipline. Also, each exerted a powerful influence on the *subsequent* spread of statistical thinking in the social sciences: "Cattell's teaching [and] use of statistics [provided] the greatest single factor making for the adoption of statistical methods by American psychologists" (Walker 1929:152; see also Ross 1973:149); Boas's work constituted "one of the strong influences operating to introduce statistical methods into [American] education" (Walker 1929:158); Moore served "as the single most influential economist in the popularization of statistical demand analysis" (G. Stigler 1965: 231; see also Dorfman 1959:205–206); and the students of Giddings (and Moore) "play[ed] a leading role in the introduction of quantitative techniques in sociology" (Oberschall 1972:230; see also Bannister 1987:152–54; Ross 1991:430; Turner 1991, 1994).

Our concern, however, is not with the later spread of statistical methods, but with the process of their initial adoption by American social scientists. To understand this, our analysis moves beyond disciplinary limits to examine *interdisciplinary relations* among different academic fields in the late nineteenth and early twentieth centuries. Emphasis on this factor derives from the work of sociologists of science and others (see Ben-

David and Collins 1966; Clarke 1990; Clarke and Fujimura 1992b; Cravens 1978; Geison 1987; Kimmelman 1992; Knorr-Cetina 1981; Pauly 1984; Ross 1991; Whitley 1984) and is especially relevant for the study of the turn-of-the-century era, which was a period of major reorganization in scientific activity in the United States. It was during this time that the American social sciences first emerged as distinct academic disciplines, as their practitioners used the opportunities created by the expansion of higher education to insert themselves into college and university programs and then to campaign for the establishment of their fields as separate departments. In most cases, though, these objectives entailed hard struggles, because even the largest universities lacked sufficient resources to accommodate all would-be disciplines. This situation engendered intense competition among contending fields, forcing them "to be concerned with their legitimacy or relative standing in the university curriculum" (Geiger 1986:24; also see Geiger 1986:20–26; Veysey 1965: 317–32). Central to these struggles was a substantial amount of what Gieryn (1983) and others in the sociology of science have referred to as "boundary work"—activities aimed at demarcating a given scientific field from both nonscientific fields and neighboring scientific fields in order to separate it from its competitors and enhance its legitimacy (Gieryn, Bevins, and Zehr 1985; see also Clarke 1990; Fisher 1990; cf. Abbott 1988). At the turn of the century, boundary work took many forms, but among fields that were newcomers, particularly in the emerging social sciences, it typically involved two different tasks: (1) demonstrating the newcomer's conformity to acceptable models or standards of scientific practice as they had been worked out by established sciences; and (2) differentiating the newcomer from already established fields, constructing its domain in terms of activities that did not duplicate those found in the other fields (see Camic 1994; Cravens 1978; Ross 1979).

We call these dual tasks of boundary work the *newcomer's dilemma*—using "dilemma" to emphasize the predicament posed by the conflicting requirement of conformity, on the one hand, and of differentiation, on the other. And in the turn-of-the-century context, moves to establish conformity with prevailing models could easily thwart the cause of disciplinary differentiation, just as steps toward differentiation could often call conformity into question—the newcomer's case could be undercut either way. To be sure, this dilemma was not something that occupied every member of every newly-emerging field. Nor was it a problem that allowed only one resolution, let alone some inevitable "right solution"; contemporaries actually proposed diverse strategies that had varying degrees of organizational effectiveness. Our contention, however, is that statistical methods were originally incorporated into the American social sciences in part because they offered—to Cattell in psychology, Boas in anthropology, Giddings in sociology, and Moore in economics—one viable solution to the newcomer's dilemma. The solution was possible because, to some contemporaries at least, statistical tools were at once demonstrably scientific *and* capable of diversification; they thus allowed social scientists to display a general conformity with defensible scientific practices *and* to shape differentiated statistical approaches for their own disciplines.[3]

In this respect, statistical methods may be viewed as an historical example of what Star and Griesemer (1989) have termed scientific "boundary objects": "objects [that] are both plastic enough to adapt to [the specific] needs . . . of the several parties employing them, yet robust enough to maintain a common identity" across social units (p. 393). During the turn-of-the-century period, however, methods did not generally move from discipline to discipline with the same ease as they do today, when boundary objects pass unimpeded from one scientific unit to another as different groups of researchers readily adapt available scientific tools to their own purposes (see Fujimura 1992; cf. Latour 1987:108–44). In the late nineteenth

[3] In the contemporary sociological literature, the processes of conformity and differentiation are emphasized respectively by the institutionalist (DiMaggio and Powell 1983; Meyer and Rowan 1977) and the population-ecology (Hannan and Carroll 1992; Hannan and Freeman 1977) approaches to organizational analysis. In line with certain recent works (e.g., DiMaggio and Anheier 1990), however, our argument accents the interplay of *both* of these processes.

and early twentieth centuries, different methodological tools tended to be strongly associated with particular disciplines. In this context, steps by an academic newcomer simply to borrow and use the methods of an established field—especially a field that laid claim to a similar subject matter—amounted to defective boundary work that cast doubt on the newcomer's distinctiveness.

What was significant about statistical methods, however, was that they afforded—at Columbia University at any rate—the possibility of actually establishing a field's distinctiveness, while also demonstrating its adherence to defensible scientific models, because thinkers from different disciplines were able to successively work out what we refer to as different "modes of statistical analysis." By a *mode of statistical analysis* we mean the application, to a particular object of study, of a subset of the intellectual tools and resources furnished by European statistical thought. One of our aims here is to bring to light the process by which several such modes of statistical analysis took shape in American social science by the early twentieth century. To this end, we advance the argument that each new discipline—or successive academic newcomer—adopted the European statistical approach which, at the time, was *available in relation to its object of study*. If a given statistical approach was already claimed by an established discipline working on a similar object of study, that approach was one *not* available in this sense; and we suggest that, in this circumstance, newcomers branched out to other statistical resources as part of their boundary-work efforts.[4]

To propose this argument is not to put forth the view that a discipline's "object of study" was an entity that was firmly fixed in advance of its method and which, as such, then determined the discipline's statistical approach. While it is true that each discipline's statistical approach was specifically tailored to accommodate its object of study, in a larger sense this was a two-way process of adjustment within the maneuvering space afforded by the interdisciplinary field (for examples of this, Danziger 1987:35; Gigerenzer et al. 1989:xiv; Sokal 1987:36). However, since our concern is the development of statistical approaches rather than the development of disciplinary subject matters, our analysis emphasizes the bearing of a discipline's object of analysis on its methodology more than the reverse.

In the course of this analysis, we interweave our treatment of interdisciplinary relations with an examination of a second organizational factor, *local institutional conditions*. Here too we draw on a major theme of recent studies in the sociology of science (Clarke and Fujimura 1992b; Holmes 1992; Kimmelman 1992; Knorr-Cetina 1981; Latour and Woolgar 1986; cf. Pickering 1984)—a theme that is particularly significant to the turn-of-the-century period. This is because at this time there was substantial variation from college to college and university to university in the configuration of interdisciplinary relations, in the way any particular discipline's object of study was demarcated, *and* in reigning views on the relative value of different methodological tools.

On the whole, these views diverged sharply from those found in our own time, when the need for statistical methods is widely taken for granted by natural and social scientists. Prior to the early twentieth century, statistical approaches were largely absent from work in the natural sciences in American colleges and universities (Fitzpatrick 1956:18–19; Walker 1929:156). Even in the biological sciences—the locus, in Europe, of major statistical innovations—what prevailed in America was a combination of traditional "natural history" and experimental procedures that entailed little statistical analysis (Cravens 1978:15–55; Davenport 1900). Insofar as statistical methodology made academic headway in the United States by the close of the nineteenth century, it did so chiefly through the emerging social sciences, although

[4] In speaking of different modes of statistical analysis, we are not suggesting that different disciplines inhabited entirely separate statistical universes. As will become clear, partisans of what we call the "Continental approach" sometimes employed "British" methods, just as advocates of the "British approach" reported averages, a practice associated with the Continental approach. Our point is, however, that judgments about these common resources varied substantially: The same statistical tools that were regarded as most relevant and appropriate by thinkers in some disciplines were marginalized and even attacked in others.

progress was slow. Many social scientists expressed outright disdain for statistical research (see Sokal 1982:569; Stocking 1992: 150; Thomas and Znaniecki [1918–1920] 1958: 1834), and most higher educational institutions simply left statistical methodology out of their curricula. It is true that, here and there, a psychologist or an economist who had studied abroad might import statistical work into his or her own department; but these were isolated events, tolerated at individual institutions, but not seen by local administrators as offering a scientific model for the younger social sciences to follow (Fitzpatrick 1955; Walker 1929:153–56).

Yet, if the typical institutional response at the turn of the century was simply to omit statistical work or to treat it as a one-person sideline, a dozen or so universities exhibited various signs of greater interest and commitment: most notably, Johns Hopkins, Yale, Pennsylvania, Cornell, Michigan, Harvard, Chicago, Wisconsin, and Clark (Fitzpatrick 1955; S. Stigler 1980, 1986:301). Even at these universities, however, it was rare for statistical methods to spread beyond a single discipline. Here too, in other words, local conditions tended to close to new disciplines the option of using statistical methods to define and legitimate themselves, and this circumstance precluded a take-off of the interdisciplinary process that we describe in this paper. During these same years, though, a markedly different outcome crystallized at Columbia University, where institutional conditions favored the advance of statistical methods from one social science to another.

Conditions at Columbia should not be seen as disconnected, however, from those at other contemporary higher educational institutions. Rather, because turn-of-the-century American universities were all part of a "competitive [national] market for money, students, faculty, and prestige," there existed between them an "intens[e] institutional rivalry" (Veysey 1965:324, 340) that was reinforced by university officials as they struggled to acquire eminence and resources for their own institutions (Geiger 1986:10). These struggles paralleled the boundary work that occurred on the disciplinary front, since the dilemma of conformity versus differentiation had a significant counterpart at the institutional level: To demonstrate their legitimacy as centers for scientific learning, universities sought to replicate the celebrated model of the German research university and to emulate each other's successful modifications of that model; but such conformism endangered the institution's quest for competitive advantages over its rivals, and universities were thus eager for curricular and other innovations they could use to distinguish themselves from one another (Veysey 1965: 11, 330, 340; see also Geiger 1986).

For biological scientists in this milieu, even basic decisions about laboratory research soon became vested with "symbolic social value," as particular organisms, laboratory techniques, instruments, and so on came to be closely "identified" with particular universities (Kimmelman 1992:200; Mitman and Fausto-Sterling 1992:172; see also Clarke and Fujimura 1992b:23–24; Marshall 1987). We argue that at Columbia University much the same thing happened regarding statistical methods in the social sciences—that Columbia developed as a favorable setting for the advance of statistical methods in part because Columbia officials regarded such methods as a way to display their university's adherence to the research practices of a scientific institution, while simultaneously gaining a distinctive competitive edge, an identity-mark that set it apart from its rivals. We propose that it was this orientation that supported and sustained efforts to recruit to Columbia social scientists who were statistical pioneers in their disciplines and that helped to establish quantitative research as the standard of legitimate science for successive newcomers onto the local interdisciplinary field.

In centering our analysis on interdisciplinary and local institutional conditions, we do not discount the significance of the intellectual, biographical, and macrosocial factors identified in previous studies of the development of statistics. Some of these elements are included (at least implicitly) in our discussion, although we give primary emphasis to interdisciplinary and institutional factors, believing that these provide a needed complement to existing accounts. Also, by concentrating on Cattell, Boas, Giddings, and Moore, we offer a highly selective treatment of early statistical work at Columbia, omitting for now other statistically-inclined thinkers at that university and certain aspects of the writings of our four main subjects (including all of their statistical work after the formative period from 1890 to 1915).

A NOTE ON EUROPEAN STATISTICAL RESOURCES

In this study we follow MacKenzie's (1981) definition of statistical methods, or statistics, as "mathematical [tools] for the treatment of numerical data" (p. 2).[5] By the time they were incorporated into American social science at the turn of the century, these tools had already undergone extensive development in Europe. We begin with a brief overview of this process.

Central among the European developments were advances in the study of probability associated with "error theory." Stimulated by problems raised in astronomy and geodesy by discrepant measurements of an object recorded at different times or by different observers, error theory was a body of mathematical work in which discrepant measurements were conceptualized as a "distribution of errors" around a "true value" (Kendall 1968; Porter 1986:93–100). This work went back to the eighteenth century, when it became known that the best estimate for the true value of a quantity was given by the mean of its repeated measurements, since the mean, being the least squares solution, minimized the sum of squared errors. In the early nineteenth century, Gauss mathematically justified the mean as the "most probable" estimate by deriving it from a "normal," or "Gaussian," probability distribution for measurement errors, while Laplace's central limit theorem provided the critical link between measurement errors and the normal distribution (S. Stigler 1986).

These mathematical innovations had an immediate but differential impact on research in the natural and social sciences. For the physical sciences, the growing sophistication in error theory meant an increasing possibility of "precise numerical determinations . . . of 'constants of nature,' [including] specific gravities, boiling and melting points, [and] atomic weights" (Swijtink 1987:262), as the gradual accumulation of repeated measures eliminated measurement errors through the averaging method. For the social sciences, from the 1830s onward, more novel implications began to emerge when the Belgian astronomer, Adolphe Quetelet, proposed extending error theory to the study of humans in society.

Prior to Quetelet's work, social statistics had consisted almost entirely of "practical statistics"—various compilations of numerical information on such topics as births, deaths, and diseases (vital statistics); illegitimacy, crimes, and insanity (moral statistics); or trade, agriculture, and taxation (economic statistics). The gathering of such information had its roots in seventeenth-century English political arithmetic and eighteenth-century German *Staatswissenschaft*, where the term "statistics" (Statistik) was actually originated to describe data relevant to the science of the state (Hilts 1981: 7–19). But with the growth of state census bureaus and private social reform agencies in the early nineteenth century, a sudden "avalanche of printed numbers" appeared on various populations—numbers that were generally presented in raw form, untouched by the application of mathematical tools (aside from simple rules for computing rates and averages) (Hacking 1987, 1990). For Quetelet, however, these same data were grist for the mill of error theory. By viewing them this way, Quetelet inaugurated what we refer to as the "Continental approach" in social statistics, as roughly distinguished from what we term the "British approach." We chose these labels because of the use of the former approach by major Continental figures such as Fechner, Wundt, and Quetelet himself, and of the latter approach by British thinkers like Galton and Pearson. These geographical designations, however, are convenient shorthands only; substantively, the distinction we make between the Continental approach and the British approach broadly corresponds to that drawn by Duncan (1984:224) between statis-

[5] We recognize that this definition differs from the present-day conception of statistics, which emphasizes the quantification of uncertainty in inference or statistical modeling (e.g., Clogg and Dajani 1991). The intellectual-historical developments that contributed to the evolution of statistics in this modern sense are examined by S. Stigler (1986)—though the "statistical turn" treated in the present study was also part of the larger process that made way for the present-day conception.

tics as the "science of averages" and as the "science of variation."[6]

The Continental approach grew from Quetelet's ([1835] 1842) concern to overcome the "seemingly insurmountable" difficulty presented by the fact that "the moral or intellectual faculties" of humans are "influenced by a cause . . . so capricious and so anomalous as the human will" (p. 5). Posing the problem of how to formulate general "laws" of human conduct, given the willfulness and resulting variability of everyday behavior, Quetelet's solution was to borrow the averaging method directly from error theory. In his view, averaging measures over a large population would rid human data of their apparent irregularity and would reveal the constancies, or laws, of a society—thus demonstrating that measurements of human behavior were just as reliable as measurements in the physical sciences.

The effect of this line of reasoning was to impart "enhanced mathematical reality to the mean [of] distributions of the aggregate" (S. Stigler 1989b:159). This outcome followed from Quetelet's observation that, when seen in the aggregate, data for a given group—say, height measurements for a large number of individuals in a particular country—regularly took the form of a normal distribution whose mean indicated "the operation of constant causes, while the variations about the mean were due to . . . 'accidental' causes [that acted] 'fortuitously,'" and, as such, held little scientific interest (Lécuyer 1987:319–20). Likening such deviations from the mean to the errors reported by astronomers in the measurement of fixed stellar positions, Quetelet put forth the view that the statistical average was a "real quantity" that represented the group's essential "type" or "one true value" (Hacking 1990:104–107; Mayr 1982: 47). From this standpoint, there was no need to examine variations from averages because the variations were merely undesirable and unreal aberrations masking the true regularities in social phenomena (for fuller discussion, see Gigerenzer et al. 1989; Hilts 1981, 1982; Lazarsfeld 1961; Porter 1986; S. Stigler 1986).

This Continental approach attracted the attention of many mid-nineteenth-century social thinkers, and in the second half of the century was extended further by the founders of modern experimental psychology, Gustav Fechner and Wilhelm Wundt. In an effort to establish that psychology conformed to the standards of the physical sciences, the Fechner-Wundt tradition used error theory to analyze an individual's varying responses to experimental conditions (Danziger 1987, 1990). Presented with the finding that, over the course of numerous trials, a subject's responses to a constant stimulus generated different measurements, researchers in this tradition interpreted such "variability [as] analogous to observational errors in astronomy" (Gigerenzer 1987:7). For them, as for Quetelet, variability was "*error* around a *true value* . . . in terms of (normal) distributions of intraindividual repeated measures," while the statistical "average" was seen as revealing this true value (Gigerenzer 1987:7)—a quantity that Wundtians interpreted as a "psychic constant" on par with the constants of nature (Wundt [1862] 1961:71–72; see also Boring 1961; Duncan 1984; Porter 1986; S. Stigler 1986).

For the purposes of this paper, we contrast this "Continental approach" with the "British approach," which began to take shape in the late 1870s and produced a shift in intellectual focus from averages to variation. While this change had many antecedents, its immediate origins are usually traced to the writings of Francis Galton, which gradually redirected statistical thought to the analysis of "variation for its own sake" (Porter 1986: 129).

Following an interest in "men who are different from the average" and in the hereditary basis of differences in ability, Galton abandoned the practice of "interpret[ing] deviations from the average in [a] normal distribution . . . as the result of accidental errors" deviating from one true value (Hilts 1973:229). To him, superior and inferior hu-

[6] We cannot overemphasize that the "Continental approach" and "British approach" are simply ideal-type constructs, *not* literal descriptions of the complex intellectual map of Europe. As Porter (personal communication) has commented: "In the 1830s and 1840s, British statisticians were no more skeptical of reliance on mean values than Belgian ones. By the 1870s, there was a huge German literature and a fair French one that criticized Quetelet for his doctrine of the average man and insisted on the heterogeneity of society."

man endowments were not "errors," but outcomes of the workings of nature as genuine as average ability. What therefore demanded attention, in his view, was not some reified average, but the *distribution* of traits in a population and the *properties* of the distribution itself, including its degree of dispersion (a value Galton measured in several ways, some adapted from error theory), its decomposability into subgroups, and (occasionally) its departure from the symmetry of the normal curve. Beyond this, Galton's concern with the transmission of human characteristics from generation to generation led him to investigate the relationship between the distribution of one factor and the distribution of another—a path that carried him, by the late 1880s, to the development of both regression and correlation (for the case of two variables) (Hilts 1973, 1981; MacKenzie 1981; Porter 1986; S. Stigler 1986, 1989a).

Additional statistical tools were devised in rapid succession. By the mid-1890s, Galton's ideas achieved considerable mathematical refinement at the hands of Karl Pearson, Francis Edgeworth, and George Udny Yule, whose work provided algebraic formulas for the computation of correlation coefficients, the least squares approach to multiple linear regression, and the method of higher moments for the analysis of asymmetrical frequency distributions (S. Stigler 1978, 1986).

LOÇAL INSTITUTIONAL CONDITIONS: THE COLUMBIA UNIVERSITY CONTEXT

In the 1870s, when reformers at Johns Hopkins and Harvard launched the move that eventually transformed American higher education from a scattering of teaching colleges into a network of research universities, Columbia College was still "a small old-fashioned . . . school [where] some seven or eight professors [taught] Latin, Greek, [plus a] little natural science" to a tiny undergraduate population (Burgess 1934:161–68). The next quarter-century, however, brought a dramatic change, and Columbia University arguably "succeeded more brilliantly" at academic reform than any comparable institution (Geiger 1986:11).

This change was the work of a small band of local innovators that included Samuel Ruggles, a powerful member of the Columbia trustees, John Burgess, a scholar whom Ruggles brought to Columbia in 1876 as "Professor of Political Science, History, and International Law," and a handful of young social scientists recruited later by Burgess with Ruggles's backing, the most important (for our purposes) being Richmond Mayo-Smith, to whom Burgess allotted the area of political economy (Hoxie 1955; Randall 1957b; Smith 1904). The efforts of this group were supported until 1889 by Columbia's aging president, Frederick Barnard, and subsequently by Seth Low, who was president until 1901, when he was succeeded by Nicholas Murray Butler.

All of these men were alert to the competitive conditions of late nineteenth-century education and willing to undertake the boundary work needed to acquire resources and legitimacy for *their* institution and for *their* academic disciplines as well. Ruggles, Barnard, and Burgess, for example, were determined that Columbia not remain inferior to rival institutions like Harvard, Yale, and Johns Hopkins (Thompson 1946:84–86; Veysey 1965:99). In addition, Burgess wished to overcome the resistance of the few natural scientists at Columbia to the establishment of his own social-scientific field. He later recalled, "with genuine scientific arrogance, they regarded us . . . with contempt [and] opposed us . . . as an expensive luxury. [They] did not consider science anything except natural science and felt that we were spending money . . . which could be better employed" (1934:209–10).

The paucity of natural scientists at Columbia and their limited research accomplishments (see Crampton 1942; Pauly 1984), however, gave these reformers an opening. With Johns Hopkins already ahead in the natural sciences, Columbia would focus on the social sciences, turning to advantage the fact that Americans interested in these areas were still flocking abroad for graduate education (Randall 1957a:110–12). By building on the work of the German-trained scholars on its faculty—Burgess, Mayo-Smith, and Burgess's other proteges—Columbia could display its conformity to the admired model of the German research university, while distinguishing itself from other reforming American universities with regard to the socio-historical studies that were then generally referred to as "the political sciences." This, in any event, was the proposal that Ruggles and Burgess successfully promoted; in 1880, it resulted in the founding at Columbia of a graduate-level School of Political Science (Burgess 1934; Hoxie 1955; Smith 1904; Randall 1957b; Thompson 1946). The School aspired to institutionalize Burgess's enlarged German conception of "science" as any disciplined field of research that advanced the frontiers of knowledge (Burgess [1884] 1934:364, 1934:203); but until the 1890s the School actually grew slowly due to the opposition of natural scientists, old-guard College teachers, and conservative trustees. From the first, however, the reformers took concrete steps to validate the new disciplines that their School represented—steps that included, already in the early 1880s, instruction in "Statistics" and "the method of statistical observations" (from an 1884 document cited in Fitzpatrick 1955:15; see also Burgess 1882:348).

In the context of the time, this was an unusual institutional innovation. In the late nineteenth century, American academic scholarship in the nascent social sciences still consisted mainly of "historical and comparative methods" pursued by going "to the library or to the study or to the archives" (Bulmer 1984:46, 18), while research in the natural sciences generally eschewed statistical analysis. Here and there an isolated scholar might offer a course in vital or moral statistics (Fitzpatrick 1955), and occasional researchers in astronomy or geodesy would contribute to mathematical statistics (S. Stigler 1980). But college administrative support for making statistics a regular part of an academic curriculum was virtually unknown. As a result, most self-styled "statistical" work actually came from outside the academy, generally in the form of "practical statistics" produced by census bureaus, state labor boards, and private reform associations (Bernard and Bernard 1943:783–831; Bulmer, Bales, Sklar 1991; Turner 1991)[7]—although some physicians and physical educators drew upon Quetelet's application of error theory (Hilts 1982; S. Stigler 1986: 218–19; Walker 1928:41).

Columbia broke away from the academic pack to adopt a program where statistics *was* included because local reformers were willing to invest in lines of scientific work on the basis of their prior intellectual experiences. Not that Columbia's reformers were bolder than those elsewhere, or that they were more astute about the future of statistical analysis: It is simply that when its program was undergoing reorganization, Columbia had gathered together in one place men whose different pasts commonly disposed them to include statistics. As a real estate developer, for example, Ruggles had acquired a "reputation as a statistician, [and] had served as American Commissioner to a number of international statistical conferences" (Dorfman 1955:170; see also Thompson 1946); Burgess had studied abroad with a leading German statistician and made "statistics an essential element of [his] image of the social sciences from the start" (Turner 1991:274). Both were thus ready to support Mayo-Smith as he set about—following his exposure to the training given in European statistical bureaus—not only to introduce the first regular coursework in statistics in America (Chaddock 1932), but also to formulate what would become the official Columbia view of statistical methodology.

Mayo-Smith developed the idea that from the use of statistics arises genuine scientific knowledge—knowledge not inferior to that produced by the (prestatistical) natural sciences. Whether in the natural or social sciences, wrote Mayo-Smith (1886a), "Science is systematized knowledge," "general conclusions [reached from] inductions"; and in the social sciences, statistical methods supply a powerful means for inductive research (pp. 114–15). Such methods are not the only tools of the social sciences; working in the School of Political Science alongside scholars (including Burgess) in history and government who relied on comparative and historical methods, Mayo-Smith (1886b) also acknowl-

[7] In concentrating on statistical developments that went beyond the tradition of practical statistics, we do not deny that work of this kind made important contributions to the modern history of quantitative research. We also draw attention to Gordon's (1992) argument that women scholars were especially "active in the infancy of [this kind] of social science quantitative research [which] was mainly developed outside universities" (p. 38).

edged the value of these traditional approaches. But to differentiate the methods used in his own specialties—political economy and later sociology—Mayo-Smith associated these newer fields with the strengths of statistical investigation. As he put it: "Historical and comparative [methods] give us qualitative statements, [whereas] statistics give us quantitative measurements. They give us general descriptions in words; statistics give us *exact descriptions* in figures [and] betray certain fixed relations which have the character of natural laws" (1888: 10–12, emphasis added). As such, statistics provides a "scientific standpoint," a foundation for "objectivity" and "trustworthiness," and a legitimate equivalent to the procedures of the natural sciences—"statistical observation, tabulation, analysis and induction [being for the social scientist what] the dissecting room is for the physician or field work for the geologist" (1888:14, 1895a:485, 1895b:vi).

In building this case, Mayo-Smith generally used "statistics" to mean practical statistics, not mathematical tools for the analysis of numerical data. Although he was familiar with many of the period's leading statistical thinkers (Galton and Edgeworth included) and familiar as well with the literature of statistical theory and with related mathematical concepts,[8] Mayo-Smith's publications tended to "read like an international statistical almanac" (Oberschall 1972:226), in which "statistical method amounted to nothing more than the presentation of tables and charts" (Hilts 1982:8).

At the time, however, this was enough to set things in motion. As one of the first social scientists admitted to the National Academy of Sciences, Mayo-Smith quickly gained the reputation among his contemporaries as "the leading [scholar in] the United States in the development of statistical methods" (letter from F. A. Walker, March 1890, MSP-UC) and as "ranking . . . in the domain of statistics [with] the three or four greatest names on the Continent and in England" (tribute by Giddings, November 1901, MSP-UC; see also Seligman 1901, [1919] 1924). Most to the point: this high reputation immediately redounded to Columbia as an institution, visibly identifying the University with an appealing scientific practice in which it suddenly found itself leading the competition. Once this happened, Columbia administrators purposefully carried out boundary-maintenance activities to preserve this mark of scientific distinction in a period when internationally statistics was becoming increasingly mathematical. Many of their appeals to successive presidents and trustees bring this out: For example, a 1891 hiring request recommended an action in support of "Statistics, in which . . . field especially the instruction at Columbia School of Political Science has achieved an enviable reputation" (Political Science Faculty to Trustees, cited by Wallace 1989:104); and a 1905 request observed that "work in [the] Statistics [Laboratory at Columbia] is acknowledged . . . to be the most complete . . . in any University, American or European, with the single exception of Turin"[9] and proposed an appointment by which "we should be able to offer at Columbia, statistical instruction unequalled elsewhere" (Giddings to Butler, April 1905, Giddings File, CFO-CU).[10]

Nor was the School of Political Science the only site of these developments at Columbia. For his own part, Mayo-Smith associated statistical methods with the particular objects of investigation that were locally seen to constitute the political sciences. He referred to these objects of study by adapting the expression "mass phenomena" from German sources; he defined his subject matter as "social masses," "the action of the

[8] Evidence for these statements comes from the Mayo-Smith Papers in the Department of Special Collections, Joseph Regenstein Library, the University of Chicago (hereafter MSP-UC) and the Mayo-Smith File in the Central Files Office, Low Memorial Library, Columbia University (hereafter CFO-CU preceded by the appropriate file name). The latter archive also contains other materials we have used in this study.

[9] The Statistics Laboratory was an innovation of Mayo-Smith (which Giddings latter carried on) that gave students first-hand exposure to gathering survey information, working with compilations of practical statistics, and so on.

[10] We are not claiming here that statistics was the only area in which Columbia University successfully distinguished itself. In this period, it was also known for its approach to social reform, its program of biological research, its embrace of the scientific "new history" of Robinson and Beard, and more (Bender 1993).

mass," "the mass of men," or "man in the aggregate," distinguishing this focus from the topic of "man, the individual," where man's "physical body" furnished the subject matter of the natural sciences and "mind" fell to the field of psychology (1888:16, 124, 1895a: 480–48, 1895b:2, 6). As to mass phenomena themselves, Mayo-Smith adopted a distinction between economics—seen as the study of "consumption and production, exchange [and] distribution" (1899:12)—and sociology—the science of population (birth rates, death rates, etc.), population divisions (the size and distribution of various national, ethnic, and racial groups), and "social organization" ("fixed relations" among individuals in "associations and institutions") (1888:15–21, 1895a:484, 1895b:1, 5–7). He left other objects of study, however, to Columbia's other academic units, and it was not long before these other units jumped onto the institutional bandwagon that Mayo-Smith and his colleagues in the School of Political Science had successfully mounted.

By the early 1890s graduate education at Columbia was reorganized, as a result, into three divisions or "Faculties": the Faculty of Political Science, made up of a department of "Economics and Social Science" (headed by Mayo-Smith) and a department of "Public Law, History, and Jurisprudence" (chaired by Burgess, who was also Dean of the Faculty); a Faculty of Pure Science, comprising the physical and biological sciences; and a Faculty of Philosophy, a catch-all for the remaining "philosophical, philological, and literary studies" (Butler 1939:140–41, quoting Butler 1889; see Hoxie 1955; Randall 1957b; Smith 1904). The core department in this last division was the philosophy department itself, and even here a powerful push was underway to conform to the standards of science. The department was headed by the Dean of the Philosophy Faculty (and future Columbia president) Nicholas Murray Butler, who sought to "welcome science [into the] humanities" and "to relate the study of philosophy to the results of modern scientific research" (Butler 1895:115, 1901:143; see also Butler 1939:137–45; Randall 1957a: 109–11, 1957b:4–11; Smith 1904:281). Butler, moreover, accepted local understandings of science; he was a close disciple of Burgess and Mayo-Smith and had himself pursued graduate work in Europe. For him, too, "mathematics [was] the indispensable tool of the sciences" (Butler 1895:109), and in staffing his faculty he turned to European-trained researchers who were at the forefront of statistical work in their own disciplines.

INTERDISCIPLINARY RELATIONS AND MODES OF STATISTICAL ANALYSIS

That statistical work had institutional appeal to Columbia administrators did not of itself produce thinkers open to the incorporation of statistical methods in their own disciplines. However, just as turn-of-the-century universities confronted the challenge of distinguishing themselves as institutions while also demonstrating their commitment to common scientific practices, the social science disciplines—as newcomers to the interdisciplinary scene—faced an analogous dilemma of differentiation versus conformity. And, at this level too, responses to the dilemma varied—with statistics holding scarcely any significance for many aspiring social scientists. Columbia's receptivity to statistical research bore fruit, however, because it coincided with those developments at the interdisciplinary level that favored the adoption and use of statistical tools: for, to at least *some* members of the social sciences that were the earliest to emerge, statistical methods did offer a partial solution to the newcomer's dilemma. Columbia was thus able to move beyond Mayo-Smith's practical statistics to become the center of more advanced statistical work, as statistical pioneers in psychology and anthropology were drawn to the University from elsewhere and encouraged to pursue their statistical interests and as their work then became the local standard that stimulated the on-site development of statistical approaches in sociology and economics.

The modes of statistical analysis adopted by the leading thinkers in these disciplines are shown in Figure 1. The diagram's horizontal dimension shows the distinction that we drew earlier between Continental and British statistical resources. The vertical dimension refers to the division—which Mayo-Smith's work articulated most fully—between "mass phenomena," which were associated at Columbia with the Faculty of Po-

		STATISTICAL RESOURCES	
		Continental Approach	British Approach
OBJECT OF STUDY	Individual/Man	Psychology James McKeen Cattell	Anthropology Franz Boas
	Mass Phenomena	Sociology Franklin H. Giddings	Economics Henry L. Moore

Figure 1. Modes of Statistical Analysis

litical Science, and the "individual" mental and physical phenomena, which fell to the Faculties of Philosophy and Pure Science. (It is with apologies that we include the gendered term "man" in this schema, since we are tracking the ideas of turn-of-the-century thinkers.) However, while this individual versus mass distinction was institutionally and intellectually significant to contemporaries, it was sometimes as fuzzy as the distinctions that are currently drawn between the social sciences and the humanities. This was especially so where the topic of *multiple individuals* arose, as it did when students of the "mass" examined the composition of certain populations and when students of the "individual" moved from studying the single subject to a population or group of subjects (on this latter change, see Danziger 1990:73–77). Despite this fuzzy line, thinkers on either side of the individual-mass divide actually used statistical analysis to pursue fairly distinct research topics.

Psychology: James McKeen Cattell

The full name of the Columbia philosophy department to which Butler belonged in 1890 was the department of "Philosophy, Ethics, and Psychology." This combination of topics reflected the department's ancestry in "mental and moral philosophy," but Butler perceived the situation as an opportunity for institutional expansion. In the United States in the 1890s, psychology was just beginning to emerge as an independent academic discipline, and the resistance it faced from the natural sciences would keep it tied to philosophy for years to come (Camfield 1973; Cravens 1978). During his studies abroad, however, Butler had met "with Wundt of Leipzig, who was revolutionizing psychology" and he was aware that research in psychology was also underway at rival institutions like Harvard, Johns Hopkins, and the University of Pennsylvania (Butler 1939: 105). He thus easily persuaded the Columbia administration to recruit in the area of psychology (Butler to Low, February 1890, Butler File, CFO-CU) and he soon proposed the appointment of Wundt's first real American student, James McKeen Cattell (1860–1944), who had recently been named Professor of Psychology at Penn.[11] In 1891, after commuting for a year from Penn to Columbia, Cattell accepted a professorship at Columbia in a separate department of "Experimental Psychology"—though on and off during the following years this department was reabsorbed into the philosophy department (Randall 1957b:19–21; Sokal 1972:456–69).

An erstwhile philosophy major, who had been drawn to natural science research through his own graduate work in Germany and elsewhere, Cattell came to Columbia as a prominent exponent of the Wundtian belief that psychology could discover in "mental processes, [a] great regularity [which is]

[11] The description of Cattell as "Wundt's first real American student" follows Sokal (1972:135). American psychologist G. Stanley Hall had previously worked with Wundt, but (unlike Cattell) he did not complete a doctorate under him. During a hiatus in his graduate work abroad, Cattell studied with Hall at Johns Hopkins, where he also made contact with C. S. Peirce, who was then engaged in pioneering statistical research in psychology (see S. Stigler 1992).

scarcely inferior to that of certain physical observations" (Cattell [1886b] 1947:25). Indeed, his commitment to this belief perhaps exceeded Wundt's own, as Cattell's experiments moved away from Wundtian "introspective reports [by experimental] subjects" (Ross 1973:149). Cattell realized early on, however, that in American colleges and universities the study of psychology still fell short, "traditional psychology [being] vague and inexact, and [not equivalent in] rank . . . with physical science." In his opinion, the field needed "to be weaned by philosophy and to begin an independent growth," but the established natural sciences were strongly opposed to this: "Physics and biology . . . claim that [because] mental phenomena are not subject to measurement, psychology cannot become an exact science" ([1893c] 1947: 26–27, [1893b] 1947:34).

If the established disciplines presented this obstacle to the advancement of psychology, though, they also suggested a solution—at least to Cattell, who had in view the experience of the European physical sciences. The solution was simple: Psychology should apply the methods of the physical sciences to its own object of study—the mind of the individual. "Psychology cannot attain the certainty and exactness of the physical sciences unless it rests on a foundation of experiment and measurement," of "number and statistics" ([1890] 1947:132, [1893a] 1947:37). To Cattell, the great lesson of the history of science was that "it was only after exact methods of analysis and measurement had been introduced that astronomy and chemistry became possible," and this precedent provided "good grounds for hope that methods which have been so fruitful in physics will not prove barren for psychology" ([1888] 1947: 7–8). By extending to the former "subject matter [of] philosophy . . . the methods [of] natural science," psychologists "may hope that we shall some day have as accurate . . . knowledge of mind as of the physical world" ([1898] 1947:109, [1888]:20).

Using terminology from the sociology of science, we see here that for Cattell (as for others in the Fechner-Wundt tradition) quantitative methods functioned as "boundary objects." In Cattell's view, these were the methodological tools of the European physical sciences, but they were tools plastic enough also to accommodate the distinctive subject matter of psychology; they could thus facilitate the boundary work required to ally his fledgling field with acceptable scientific practices, while he simultaneously demarcated it from established scientific disciplines. Indeed, faced with the dilemma of conformity versus differentiation, Cattell was explicit about the attraction of statistics. Hailing the ability of psychologists to make "quantitative determinations [and] obtain . . . statistics of [their] nature," Cattell drew a comparison between his own field and the field of physics which pinpointed the core similarity and difference: "The physicist counts, and he measures time, space and energy [to] describe his world in certain quantitative formulas. [Today's] psychologist counts and he measures time, space and intensity [in] mental magnitudes. [In his studies], mental processes are described in quantitative terms, [which] show that a mental mechanics is more than a possibility" ([1888]:131, 1904:183–84).

Cattell found convincing proof for his assertion in his own statistically-grounded experimental researches. We characterize these as exhibiting a *mode of statistical analysis in which the resources of the Continental approach were applied to the individual as the object of study*. To Cattell, this object of study was understood as "the contents of the individual mind"; he viewed the "individual" as an assemblage of "mental quantities" and their "physical correlates," such as reaction-times and "the time of [other] mental processes, the accuracy of perception and movement, . . . the motor accompaniments of thought [and] memory, the association of ideas, the perception of space, color-vision," and so on ([1893a]:36, 45, 1904:180).

We have proposed that the quantitatively-oriented members of new turn-of-the-century disciplines would adopt the European statistical resources that at the time were available in relation to their discipline's object of study, and Cattell's work offers a clear instance of this. When Cattell began his quantitative studies in the mid-1880s, he was directly situated in the Fechner-Wundt tradition of psychological research. At this date, the key mathematical innovations of the British approach had yet to be worked out, and the only statistical tools generally available were those of the Continental approach. It is true that, after finishing his work with Wundt, Cattell spent an extended period in England, where he became acquainted with Galton and Edgeworth and with their approach to studying variation (Sokal 1972: 249–361). On his return to America, however, Cattell remained strongly committed to the Continental approach, which was still very much "available" for application to the individual as an object of study. The approach had not been appropriated by other new disciplines claiming a similar subject matter, because in the 1880s these other fields remained largely nonexistent (Cravens 1978). In terms of the interdisciplinary context, therefore, Cattell could proceed with his efforts to legitimate psychology by extending to its domain the same statistical approach that had earned scientific legitimacy for the European physical sciences.

Evidence for his use of the Continental approach comes from both earlier and latter stages of Cattell's research career. In his early experiments under Wundt, Cattell's statistics consisted mainly of averages (e.g., averages of the time taken by an individual subject over many trials to react to stimuli such as colors, letters, and words) (see Cattell 1886b; see also Cattell [1885] 1947, [1886a] 1947; Cattell and Bryant [1889] 1947). While recognizing that there was variability around any given average, Cattell maintained that averaging procedures cleared the variability away, producing true measures for "the time parameters of the mind" (Sokal 1972:195). As he explained: "It was my object . . . rather to eliminate [intraindividual and interindividual] sources of variation than to investigate them" ([1886b]:58). This same interest was evident in his famous psychophysical experiments on perception, or discrimination, which he carried out at Penn. Cattell demonstrated that, when measured over repeated trials, a subject's judgments of a magnitude generated a range of values which could be conceptualized—directly following the Continental model of astronomy—as "errors of observation," whose frequencies in positive and negative directions from the magnitude's correct value were distributed in a shape approximating a normal curve, with the average as the true value (Cattell and Fullerton [1892] 1947: 146–51, 197–219; see also Cattell 1893b; Sokal 1972:398–433). In other words, just as "Quetelet [treated] human variability as Nature's errors, [so Cattell regarded] sensory variability as man's error in aiming at perfect discrimination" (Boring 1957:534).

In these experiments Cattell, to be sure, raised certain criticisms of error theory. He did so, however, not to argue for an alternative statistical approach, but simply to show how physical scientists might benefit from the work of psychologists (Cattell and Fullerton [1892]:245–46; Cattell [1893b]). It is true, as well, that much of Cattell's subsequent work was concerned with "individual differences" in mental operations (and the bodily correlates of these operations), not only with the individual "constancy of mental processes" (1904:183, [1890]:132). His shift in focus, which is often attributed to his encounter with Galton, was announced while Cattell was still at Penn (Cattell [1890]), but blossomed after his move to Columbia in his ambitious "mental testing" program which used hundreds of Columbia undergraduates for subjects—and is "usually regarded as [Cattell's] greatest contribution to psychology" (Sokal 1972:550; see Cattell [1895] 1947, [1905] 1947; Cattell and Farrand [1896] 1947). At Columbia, Cattell also carried out research on individual differences in scientific eminence, asking panels of judges from various disciplines to rank-order the members of their field (Cattell [1903] 1947, 1906).

In none of this work at Columbia, however, did Cattell employ a different statistical approach. By the 1890s, the resources of the British approach were increasingly available, but even as he considered the Galtonian topic of individual differences, Cattell did no more than mention these newer methods. In his work on scientific eminence, he continued in the Continental tradition, conceptualizing judges' different rank-orderings as analogous to errors of observation in astronomy (1906: 5) and "accepting the average ranks as furnishing an approximation to a true order of [scientific] merit" (Woodworth [1944] 1947: 5). Similarly, while the Columbia testing data suggested the possibility of using "correlation [to study] the distribution of variations," Cattell again preferred averaging procedures, which reaffirmed the "parallel" between psychology and the physical sciences (Cattell and Farrand [1896]:307, 329; cf. Sokal 1972:340–41). In his view, there was simply no need to tamper with a statistical approach that so well served the cause he pursued for his discipline.

Anthropology: Franz Boas

Among the new social sciences in turn-of-the-century America, anthropology, the generic "science of man," had the most heterogeneous roots. These included: research in comparative human anatomy and physical anthropometry (the measurement of the human body) by biological, medical, and psychological investigators absorbed in debates about the human "races"; studies in natural history on the development or (after Darwin) the evolution of the human species; and various lines of "ethnological" work on the diversity or unity of humankind, produced by comparative psychologists and linguists and by social thinkers interested in the stages of human history (Cravens 1978; Odom 1967; Stocking 1968, 1992:346–52; Sokal 1987).

Prior to the 1880s, these intellectual traditions had no academic base in the United States. The closest anthropology then came to institutionalization was in the ethnological or archaeological bureaus of the museums of a few large urban areas. "Regarded as something marked by exoticism and amateurishness," anthropology was "starved in the academic curriculum" (Herskovits 1953: 22); on the whole, its "marginal status" persisted into the early twentieth century as a result of opposition from biologists, psychologists, and others who had then barely established themselves and were resistant to a newcomer with some of the same interests (Cravens 1978:100–107). Between 1886 and 1892, however, Harvard, Penn, and the new University of Chicago all established professorships in areas of anthropology—and Columbia immediately sought to follow their example (Cattell 1897:165).

As one of its first steps to this end, in 1896 Columbia University appointed Franz Boas (1858–1942), then a special collections assistant at the American Museum of Natural History, as "lecturer in physical anthropology" in the Pure Science Faculty, which had a practice of joint appointments with the Museum (Crampton 1942:12; Hyatt 1990:35). Dean Butler, however, had his own designs on Boas and his field; he regarded Boas as "one of the most competent anthropologists now living" and viewed "Psychology [as the] closest analogue of Anthropology" (Butler to Low, October 1895 and May 1896, Butler File, CFO-CU). As usual, Butler prevailed, and in 1899 Boas was transferred to the Philosophy Faculty as a professor in the new Department of Psychology and Anthropology chaired by Cattell. Two years later, a separate Department of Anthropology was created within the same Faculty, where Anthropology and Psychology long remained organizationally interdependent (Boas 1908; Cattell 1900; S. Moore 1955:150).

Boas's presence brought to Columbia a prominent researcher whose work spanned the whole of anthropology from linguistics and ethnology to physical anthropology and even archeology. In this paper, however, we concentrate on Boas's work in physical anthropology; this is the area in which he made his statistical contribution (Howells 1959; Stocking 1968:161–94; Tanner 1959; Xie 1988). Regardless of his topic, Boas believed it was possible "to treat human data with rigorous scientific method" (Kroeber 1959:vi). He argued, however, that different branches of anthropology employ different analytical tools, with physical anthropology distinguished by its quantitative approach (Boas 1897b:153, [1899d] 1940/1982, 1908; cf. 1895c). It was this aspect of his work, moveover, that was especially salient to the Columbia administration, which hired Boas as a physical anthropologist on the understanding that he would offer courses on his statistical work (Cattell to Low, May 1896, Cattell File, CFO-CU). And a significant factor behind Boas's eventual transfer to the psychology department was Cattell's strong interest in "intimately associat[ing]" psychology with "physical anthropology," so as to enrich the Columbia testing program which involved both physical and mental measurements (Cattell and Farrand [1896]: 648; see also Cattell 1897, [1898]).

Boas's own views on anthropology were essentially in place, though, before he arrived at Columbia, where they were reinforced. A recent immigrant from Germany, where he had been trained in physics, mathematics, statistics, and the Fechner-Wundt tradition of psychological research, Boas had already carried out fieldwork in physical and cultural anthropology and worked briefly teaching anthropology in the psychology department at Clark University. He was thus deeply aware of disciplinary boundaries and of the hard challenge of demarcating the scope and method of anthropology (for biographical information, see Boas [1936] 1940/1982, 1938; Herskovits 1953; Hyatt 1990; Kluckhohn and Prufer 1959; Kroeber 1943, 1959; Lowie 1947; Ross 1972; Stocking 1968). Responding to this challenge, Boas took the position that anthropology's focus was "the history of mankind [as] reconstructed by investigations of bodily form, languages, and customs," with the first of these topics—"the human body and its functions," including the "correlation between somatic and mental" traits—marking out the province of "physical anthropology" (Boas [1889] 1940/1982: 638, 1891a:351, 1904:521). "These subjects are not taken up by any other branch of science, and in developing them anthropology fills a vacant place in the system of sciences" (Boas [1899a] 1940/1982:621). Nevertheless, these subjects were, as Boas knew, still embroiled in territorial disputes, with "the investigation of the physical characteristics of man [already] taken up by anatomists," among others ([1899a]:622). This made it difficult to combat the charge by physical scientists, biologists, and psychologists that the newer disciplines were "inferior in value" to the more established fields (Boas [1887] 1940/1982:640).

Confronted with the dual objection that anthropology was inferior to the existing sciences *and* that it duplicated them, Boas worked to define the boundaries of his discipline in methodological terms that would classify it as similar to the established sciences, while also delineating its differences. In his early years at Columbia, he wrote explicitly about the similarities: "The method of anthropology is an inductive method, and the science must be placed side by side with the other inductive sciences"—with the "natural and mental sciences" which employ the "rigid inductive method" ([1899a]:622, 1904:514). Interpreting this principle in light of his familiarity with the European physical sciences and with the boundary objects that these sciences offered to his own field, Boas held further that in physical anthropology scientific induction entails "the application of rigid *statistical* methods," which require the "accurate measurement" of human physical characteristics and "give . . . us the means of a comprehensive description," expressed "in exact terms" ([1899d]:168–170, emphasis added, 1894b:313). By using statistics, not only would anthropology demonstrate its commitment to inductive science, but it would also actually surpass biological research on human characteristics by "substitut[ing] measurement for description and the exact number for the vague word" ([1899a]: 623).

By the 1890s, however, this kind of appeal to statistics was by no means as straightforward as it had been for Cattell in the less-crowded interdisciplinary environment of the 1880s. As a vehicle for differentiating an academic newcomer, the Continental statistical approach was no longer simply available in relation to the individual as an object of study; for, as Boas knew from his contact with the Fechner-Wundt tradition and the psychology departments at Clark and Columbia, not only were experimental psychologists already using Continental statistics to analyze certain aspects of man's physical and mental constitution, but these methods were also prevalent in contemporary writings on physical anthropometry by students of human anatomy and others (Hilts 1982; Sokal 1987).

Faced with this circumstance, Boas's solution was to branch out to new statistical resources: He attacked the Continental tradition and adopted a *mode of statistical analysis in which the tools of the British approach were applied to the study of man's form*—understanding "man's form" in terms of individual physical (and interrelated mental) characteristics, which Boas conceptualized in a new way. He did so by maintaining that "no two individuals have an identical form" and that human differences are more than accidental "errors" deviating from an "ideal type" as revealed by a statistical average (Boas [1911] 1938:35, 1902:174–75). He held that existing statistical work did not grasp this: not experimental psychology, because it dealt with the single "typical individual"; not anatomical research, because it focused on traits "common to mankind as a whole [as] though no individual differences existed"; not earlier anthropometric studies, because they used comparisons of the average physical measurements between human groups as evidence of hereditary "races" (1927:114, [1911]:35; Stocking 1968:168–69). Boas challenged the Continental statistical ideas underlying these traditions by arguing that: (1) "anthropometric measurements do not, as a rule, follow the laws of chance," since many human attributes are "not distributed symmetrically around an average" (1893:571–72, 1892c:281); (2) when the measures of individual traits appear normally distributed, the normal pattern may still conceal "a number of distinct [individual] types" (1893:573); and (3) even when normal distributions seem to fit observed data, group averages remain inadequate for intergroup comparisons, because each group consists of a "range of variability" which may "overlap" the range of other groups—a consideration that impugns racial classifications by revealing that "the variations inside any single race . . . overlap the variations in another race" (1897b:151, [1894d] 1974:227; see also 1891b, 1892a, [1894a] 1974, 1895a, 1898, [1899c] 1940/1982, 1901, [1911]; Stocking 1968:161–94).

Throughout the 1890s, Boas advanced this critique of statistical research in rival fields as he followed the ideas of Galton and Pearson and utilized the British alternative in his own anthropological work. Scholars have gone so far as to argue that Boas was actually an "independent innovator" in the statistical study of correlation (Stocking 1968: 168) and that he ventured into analysis of variance techniques (Howells 1959; Xie 1988). In his writings, Boas applied these new statistical ideas to major empirical issues in contemporary physical anthropology. At Clark, for example, Boas conducted studies of school children—a topic he chose to address questions about the "growth" of the human form—in which he focused explicitly on the "variation of [bodily] form and function [under] varying conditions" and sought to identify the growth processes that accounted for "the asymmetry of the [distribution] curves" of children's heights and weights (1891a:351, 1892b:257; also 1892a, 1892c, 1895a, 1895b, 1897a; Tanner 1959). In his research on North American Indian tribes, Boas likewise insisted that "a critical study of distributions [of individual measurements] must precede the establishment of the type" (i.e., the physical type-features) used to characterize a given tribe; and he thus emphasized the "distribution of statures" and the "distribution of head forms" among the members of particular tribes, the shape and internal variability of such distributions, and the instances where these required "subdivision" (1893; 574, [1894a]:197, 1894b:321, [1894c] 1940/1982). He directed attention, in addition, to "the varying degrees of correlation" between the distributions of different measurements, examining correlation initially by plotting regression diagrams (1894b), and later, in his Columbia years, by partial correlation and multiple regression analysis (1899b; for further discussion, see Xie 1988:280–81). While at Columbia, Boas applied these same statistical tools to data on the headform variation among European immigrants to explore the mechanisms of heredity in family lines and to demonstrate the degree to which human physical characteristics were open to environmental influences (1903, 1907, 1916a, 1916b; Stocking 1968, 1974a, 1974b).

As he pursued these studies, Boas developed the view that physical anthropology rests, by its very nature, on the "statistical study of [the] local or social varieties . . . of human forms," using "the methods [for] the quantitative study of the varieties of man" formulated by Galton and Pearson ([1899d]:166–67, 1904:521–22; see also 1897b:152). The effect of this conceptualization was to make the British statistical approach integral to the very definition of physical anthropology, and, in establishing this, Boas arrived at a definition of his field that accentuated its scientific foundations *and* clarified how it differed from the sciences that neglected variation and the statistical tools for its analysis. Boas's physical anthropology reverberated, at this point, to his other anthropological work, which marked off the phenomenon of human "variety"—cultural and linguistic, as well as physical—as the raison d'être of the entire discipline of anthropology (1904:522). His interest in human variety can also be traced, to be sure, to Boas's concern with broader ethical and political issues. His thinking on these larger issues was always embedded, however, in a context of interdisciplinary relations in which he sought to "single out" for anthropology a legitimate "domain of knowledge" and where he viewed "the statistical study of variation," whatever its other benefits, as exactly suited to the boundary work that was required (1904:523).

Sociology: Franklin H. Giddings

Compared to anthropology, turn-of-the-century sociology in the United States was in both a better and a worse academic position. The field was better off in a numerical sense—by 1900 some one hundred colleges and universities offered coursework in sociology (Cravens 1978:125). On the down side, however, was the fact that the advance of sociology was closely tied to demands from urban reformers, settlement house workers, and the like, for courses on contemporary social problems, and many higher educational institutions had responded to these demands by appointing part-time teachers of "social science" or "sociology," drawn from the ranks of clergymen or social workers who had no sociological training (Cravens 1978:123–29). Among contemporaries, this produced widespread confusion over the "scope and method" of sociology and relegated the discipline to an "undefined and tentative" place "far behind" the established sciences (Howerth 1894:113, 121), which long viewed the newcomer with "more jeering than admiration" (Small 1916:766; see also Oberschall 1972; Ross 1979).

Traces of all of this were visible at Columbia University. Aware of the demand for courses in social reform, Burgess included "sociology" in his early plans for the School of Political Science. And during the 1880s Mayo-Smith expanded his teaching of practical statistics beyond economic questions to include some of the standard topics of reformist sociology—a move acknowledged by the University in 1890 when he was given charge of a separate department of "Economics and Social Science." Nevertheless, when Mayo-Smith recommended the next year that his new department "be divided into one of Political Economy and one of Sociology" (Mayo-Smith to Low, December 1891, Mayo-Smith File, CFO-CU), the Columbia Trustees rejected the proposal on the grounds that sociology had "not yet reached a basis sufficiently certain to justify" this kind of institutional establishment (Wallace 1989:106; see also Butler 1930:383; Dorfman 1955: 173–76). Concurrently, however, certain rival universities were creating full-time positions in sociology: As the Columbia Political Science Faculty pointed out to the Trustees in 1891, "permanent chairs of Sociology have been created . . . at the University of Pennsylvania and Yale, Chicago and Leland Stanford" (Wallace 1989:104). These examples eventually goaded the Columbia administration to action (Small 1916:762–65), and in 1894 Franklin H. Giddings (1855–1931) was appointed Professor of Sociology, although not in a department of sociology. For years, he was the single sociologist in a department of economists, and only in 1904 did he obtain a semi-autonomous department of "Social Science"; the unit achieved independence in 1924 and was renamed the department of "Sociology" in 1941, a full decade after Giddings's death (Hoxie 1955; Lipset 1955).

At the time of his appointment, Giddings lacked the scholarly record that Cattell and Boas had achieved before they joined the Columbia faculty. Moreover, while they and many of Giddings's other Columbia colleagues had earned German doctorates, Giddings himself was a college dropout who had some training in the natural sciences, but none in sociology (see Wallace 1989:72–99 for biographical information). Rather, sociology was a field that he had "backed into" during his 10-year career as a journalist, writing on economic problems, and his subsequent appointment, in the late 1880s, to the Bryn Mawr Political Science Faculty. When he was named Professor of Sociology at Columbia, Giddings was commuting to the University (from Bryn Mawr), teaching under an ad hoc arrangement, which originated in 1891 when Mayo-Smith initially hired him as a sabbatical replacement for his own social science courses. In the 1890s, Mayo-Smith had few candidates from which to choose, and in a context in which there simply were no statistically-trained American sociologists, Giddings at least seemed on the right track: His sociology course at Bryn

Mawr emphasized Galton and Mayo-Smith's work (Small 1916:762), and his initial writings on sociology showed sympathy for Mayo-Smith's conception of the discipline's subject matter and for statistical methods (Giddings 1890:68, 73).

But Giddings was aware that his appointment as a sociologist was "an experiment" that he needed to justify (1930:384), and he devoted himself to boundary work aimed at securely positioning sociology in relation to more established academic disciplines. Indeed, throughout his long career at Columbia, Giddings insisted that sociology was following the same path previously followed by astronomy, biology, and psychology, for "assuredly, in the course of time, ... our knowledge of society [will be] up to the standards [of] any natural science" (1899:145, 1904: 634). Integral to his contention was an extended argument about the subject matter and method of sociology that pinpointed its similarities and differences with respect to other fields.

In spelling out sociology's subject matter, Giddings pursued a two-fold strategy. He offered, on the one hand, concrete illustrations that laid claim to the Mayo-Smithian domain of mass phenomena by identifying "human aggregation," "human population," various population divisions and groupings, and "social organization" as sociology's objects of study (Giddings 1890:68–69, 1896:39, 1899: 148, 1901a:3, 1904:625, 630). He provided, on the other hand, various formal definitions designed to justify such territorial claims, especially with regard to disciplines like economics and psychology, which were better established locally. Giddings maintained that, unlike economics, sociology is a "general science"; it treats not the "special . . . phenomena of a conscious calculation and production of utilities," but, like psychology, "conscious association" more generally (1894: 403, 1896:24). Yet, unlike psychology, which investigates the association of "the elements of... the individual mind," sociology studies the more "complex phenomena of minds in association with one another" (1896:24–25). Giddings described the consciousness accompanying "minds in association" by introducing sociology's distinctive and defining concept: "consciousness of kind," which designated the state "in which any being... recognizes another conscious being as of like kind with itself" (1896:17, 1897:x). From this concept, he then made his way back to mass phenomena by conceiving human populations, groups, and organizations as "consequences" of consciousness of kind (1896:v).

The methodological tools Giddings applied to this subject matter changed during his years at Columbia. It was always his view that sociology is essentially an "inductive" science, on par with disciplines like astronomy, biology, and psychology (1895: 748; see also 1903:246). In his first major book, however, Giddings presented inductive research mainly in terms of the "comparative and historical methods" that were then being practiced by many of his colleagues in the Faculty of Political Science (1896:64–65). While acknowledging "the statistical method [as the] quantitative form" of these basic methods (1896: 64), the empirical portion of Giddings's own work consisted of a Spencerian comparative-historical analysis of ethnological materials—a form of analysis that promptly drew criticism from America's two leading sociologists, Lester Ward and Albion Small, for remaining "speculative" and lacking "any scientific basis" (Ward and Small as cited in Stern 1932:311, 313). To a newcomer hoping to establish his field's scientific legitimacy, these were serious charges, and they soon led Giddings to rework his methodological position around the idea that it is "the *statistical method* [that] has become and will continue to be the chiefly important method of sociology" (1904:633–34, emphasis added; see also Bannister 1987:74). This shift was evident in his writings by the late 1890s (see 1897:41–45), although he had already been pointed in this direction by conditions at Columbia, where he was hired to take on a course of Mayo-Smith's that "was largely statistical in content" (Giddings as quoted in Small 1916: 762). Subsequently, he was appointed to his department's "Social Science and Statistics" division, assigned to teach (with Mayo-Smith) seminars on "sociology and statistics," and was presented regularly with the example of how other young disciplines had acquired local legitimacy through statistical work (Chaddock 1932; Davids 1968; Wallace 1989).

In this conducive institutional context, Giddings came to view statistical methods as boundary objects that would fortify his defense of sociology. As he now described the situation, statistical methodology constituted the foundation of those sciences that were already established, and it would serve the same purpose in sociology. The moral of the history of psychology and the natural sciences, according to Giddings, was that "the present position of a science can be ascertained [by its] progress in the application of exact methods" and, in the main, "exact method . . . is statistical," an analysis of "quantitative facts" (1899:146, 148, 1900:9). In his view, "physics, chemistry, astronomy, and geology would be helpless without . . . the statistical method," the same method that recent research in Europe (and in America, by those like Mayo-Smith, Cattell, and Boas) had carried into realm of the social sciences (1904:623). This accomplished, it was time for sociology; "as may be judged from the development of [statistical] method, no science . . . is making surer and better progress" (1899:150–51). By this method, thought Giddings, sociology will finally be able to eliminate "the uncertainties of subjective estimate" and to arrive at "laws" not inferior to the discoveries that followed "when exact statistical methods were introduced in biology, in psychology and in economics" (1908: 59, 1909:587).

For Giddings, however, statistical tools were not only a means of demonstrating sociology's association with "the most effectual methods that science has at her command," they were also a way to make good his claim that sociology "differentiates" itself with "methods that are peculiarly its own" (1896:52, 1891:651–52). To this end, he worked out, continually advocated, and then employed in his occasional ventures into empirical research, *a mode of statistical analysis in which the Continental approach was applied to mass phenomena as the object of study.* By the time that Giddings embraced statistical practices (circa 1896–1900), American researchers had applied both Continental and British statistical tools to the study of the mental and physical traits of individuals—as the work of Cattell and Boas made clear to Giddings—but both approaches were still available in relation to mass phenomena. This situation offered Giddings some latitude, and since he did not want sociology to miss out on using the successful British methods, Giddings included them in his list of options for sociologists (1898, 1899:150, 1901a:11, 23, 1904:633, 1910; see also Bannister 1987:76; Turner 1986, 1991, 1994; cf. Oberschall 1972).

Even as he did so, however, he explicitly argued for the "modification" of these methods within sociology (1901a:23; see also 1914:21), because the British approach threatened the fundamental fact at the core of a *sociological* analysis of mass phenomena: namely, the reality of kind, type, or category. Recall that, according to Giddings, sociology's foundational concept is "consciousness of kind." Due to this great force, humans inevitably arrange and classify themselves into "categories . . . of real or supposed resemblance": "color, race, and nationality," "religious belief," "political conviction," and so on (1899:151–52). In Giddings's view, the omnipresence of such categories rendered the data of sociology mainly categorical (or discrete), since statistical analysis in sociology had to begin with "common-sense classifications" (1910:722, 1901a:29). Conversely, British statistical tools, like "the coefficient of correlation," were limited in their usefulness because they "are measures inherent in items of size rather than items of sort . . . or kind [which are prevalent] in sociology" (1914:21). Indeed, as Giddings was aware, British statistical tools were what had enabled Boas to *undermine* many commonsense ideas about racial classification and human types.

The Continental approach did not have the same drawback for sociology, however. To be sure, its assumptions about "normal frequency distributions" were also ill-suited to categorical data (Giddings 1914:21). But setting these assumptions aside, Giddings found in the tradition of Quetelet a statistical approach that gave centrality to concepts of type, and he accepted that the goal of statistical analysis was to clear away measurement error and discover a group's essential "type" (or "true value" on some measure) through averaging procedures (1909:580–81; Davids 1968:70). His primary justification for beginning statistical work with classification, in fact, was that "classification [unites] those things that are truly and essentially alike" (1900:2), and he defended commonsense classifications as devices to minimize observational error, since "the 'probable error' of thousands of individual judgments . . . is extremely small" (1910: 724). Given such classifications, Giddings held that statistical analysis in sociology "consists largely in counting the individuals, qualities, circumstances, or habits in [such] aggregation[s], and in dealing, by various mathematical processes, with the numbers so obtained" to find the "central tendency," "average," or "distinguishing mark for each class" and, where possible, to compare different categories in "more and less" terms (1901a:20–21, 1909:581, 587). As to the nature of these "mathematical processes," Giddings was eclectic in his own research, although what he generally favored was the construction of an "index" as the replacement for an ordinary average in the case of a continuously distributed variable. Through the use of an index, the "central tendency" of a measure for a particular population (e.g., a population's degree of ethnic homogeneity or of kinship) was indicated by a weighted mean, with different numerical values first assigned to the discrete subclasses that comprised a population, and then weighted by the relative proportions of the subclasses (1897:42–43, 1910:740; see also 1901b, 1908).

A striking feature of this work was Giddings's relative lack of concern with variation. In the very period when Boas was redefining anthropology as the analysis of social varieties, Giddings's boundary work went in the opposite direction to reaffirm the reality of consciousness of kind and, hence, the mandate for sociology in relationship to other disciplines. Giddings held that, where consciousness of kind is at work, "the importance of 'kind' itself is apprehended" by members of a social unit, who "manifest a dominant antipathy to . . . variations from [their] type" and seek to "limit [such] variation" (1909:575–76). To Giddings, this meant that marked variation was a denial of the very existence of consciousness of kind—a manifestation of the breakdown of "society," where society is defined as "a type [that] controls the variations from itself" (1909:574). By this reasoning, what followed was not the need for some Boas-style statistical study of variation, but the need for a science that would use the "statistical method [for the analysis of] the phenomenon of type" to examine "the origin, the process, the extent and the results of type control of variation from itself, within a group or population" (1909:580, 578). In Giddings's view, the science so demarcated was sociology (see Northcott [1948]).

Economics: Henry L. Moore

Columbia University's commitment to statistical methods was, as we have seen, an outgrowth of a process launched in the 1880s when Mayo-Smith incorporated "statistics" into the field of economics (political economy). By 1900, however, economics lagged far behind other disciplines, as European statistical tools were successively introduced in psychology, anthropology, and sociology and Mayo-Smith continued to work in the tradition of practical statistics. Columbia was not unique in this respect. By the turn of the century, economics had "achieved recognition as an independent discipline" in many American colleges and universities (Parrish 1967: 1–2)—albeit not by employing up-to-date statistical tools.

It is true that economic *theory* had already attained a high degree of mathematical formalization in Europe and that American economists were familiar with this development. Neither in Europe nor in America, though, was the analysis of economic *data* advanced by the standards of the time. Instead, "economics remained primarily qualitative" (Spengler 1961:139; see also Humphrey 1973; Menard 1987; Morgan 1987). When economists spoke of "statistics," they generally referred, as did Mayo-Smith, to compilations of practical statistics, although "index numbers" (special averages to measure temporal fluctuations of wages, profits, etc.) were also used (Christ 1985; Persons 1925). At the start of the twentieth century, however, "statistical economics hardly existed" (Christ 1985:39), and economics as a discipline continued to experience the same assaults from older sciences that younger academic newcomers faced. Economics was seen as "still occup[ying] the position of an inexact science" and still "in search of a sure foundation upon which to base [its] conclusions" (North 1909:434–35).

When Mayo-Smith died in 1901, Columbia quickly made up for lost time. Finding no suitable senior replacement, the University hired two "adjunct" professors, one interested in the statistical side of economics, the other in the mathematical side (Wallace 1989:175–83, 189). The latter was Henry L. Moore (1869–1958), who alone is our focus here as he eventually became the founder of statistical economics (Christ 1985:42; G. Stigler 1962:1). At the time of his appointment, Moore was a young Johns Hopkins Ph.D., teaching mathematical economics at Smith College and involved in intellectual-historical research (for a biography, see G. Stigler 1962). He had yet to do any statistical work, and "his interest in statistics was not apparent" until after his move to Columbia in 1902 (Christ 1985:42; G. Stigler 1968: 480). However, once he became situated in the pro-statistical setting of Columbia, he worked his way through the European literature on statistical theory, was given teaching assignments in the statistics courses in the Department of Economics and Social Science, and received leaves to study abroad with Pearson. Before his promotion in 1906 from the "adjunct" category to a professorship, Moore stood out at Columbia as "one of the three or four men in America who have followed the most advanced researches and methods of such European statisticians as Francis Galton [and] Karl Pearson" (Giddings to Butler, April 1905, Giddings File, CFO-CU).

In his long career at Columbia, Moore used this knowledge to carry out statistical studies intended to promote "the development of economics as a science" and to end attacks upon the field from other disciplines (Moore 1908:32). He viewed his discipline's situation as follows:

> In the closing quarter of the last century great hopes were entertained by economists with regard to the capacity of economics to be made an 'exact science.'. . . But this expectation has not been realized. On the contrary, faith in the possibility of an adequate 'exact' treatment of the science has progressively diminished, [indicating that] there must have been something fundamentally wrong with the traditional handling of the subject. (1914:84–85)

What this something was, according to Moore, was the "wrong method"; economists had taken the wrong cues from the established sciences when they "identif[ied] the method of physical sciences with experimentation" and then devised substitutes for the latter at the theoretical level, using assumptions such as *ceteris paribus* (1914:85–86, 66). Following standard Columbia practice, Moore held that the existing sciences furnished a more valuable set of boundary objects—that what the work in these disciplines actually revealed was that theory requires an "inductive complement," that induction ideally takes "a quantitative form," and that the inductive method that best facilitates "exact inquiry" and "progress from the data to generalization" (or to "laws") is "the method of statistics" (1908:2, 6, 23, 1914:77, 86; see also 1906:212, 1907b:62, 1911:7).

This argument spotlighted the similarity between economics, as Moore understood it, and the generic activity of science as it had been conceived by his colleagues in other young disciplines at Columbia. For Moore, as for the others, however, effective boundary work involved not only claiming similarity, but also establishing disciplinary differentiation.[12] Moore sought to do this by developing a *mode of statistical analysis in which the resources of the British approach were extended to the treatment of mass phenomena as the object of study.* He thus secured for his discipline the remaining vacancy in the local interdisciplinary field, where Continental and British tools were already being applied to the study of the individual, and where the Continental approach was in use in relation to mass phenomena as

[12] In presenting Moore as engaged in boundary work, we are not retreating from our previous statement that, by the turn of the century, the discipline of economics was already widely established as an independent academic field. During this historical period, boundary work was typically an ongoing activity, not something that stopped at the moment of a discipline's initial institutionalization—for, as American economists discovered, academic establishment did not suddenly silence skeptics outside the discipline who questioned its validity as a science and its right to scarce university resources. So long as such skepticism persisted, so did the challenge of securely legitimating the (relative) newcomer, demonstrating its similarity to and difference from other scientific disciplines. Moore's work represented one possible response to this continuing dilemma.

well. (This statement is a schematic social-organizational summary, not an account of Moore's internal reasonings.)

At the turn of the century, economists were widely divided over their field's proper object of study, which some defined as "individual behavior tendencies" (Spengler 1961: 141–42). From his earliest years at Columbia, however, Moore adopted Mayo-Smith's position that economics and the other social sciences "rest [on] the study of mass-phenomena," "groups," or "aggregates" (Moore 1908:8, 1905:320, 1911:5, 20–21). To him, the "economic facts" of aggregate-level production, consumption, exchange, and distribution thus formed the particular concern of economics, and he worked in his own studies with data on such variables as the annual volume of output in certain industries, the prices of various commodities, and the wages of workers in different locations (Moore 1911, 1914).

In some instances, such data confronted Moore with a serious problem of overabundance, of "observations mount[ing] into hundreds and thousands" (Moore 1911:15), and when this was the case, he tended to adopt the expedient of first "pass[ing] from individual observations to representative facts" by working with group averages (e.g., mean wages in different regions) and then carrying out quantitative analyses focused at the between-group level (1911:14–20)—a practice congruent with his interest in mass phenomena. However, despite this pragmatic use of averages, Moore's research represented a deliberate departure from the Continental approach that he identified with the other social sciences and with some previous work in economics (see 1908:9–12). In his early empirical studies, for instance, Moore vigorously attacked "the common fallacy of inferring from an average or an index number of wages conclusions [about] the general condition of labor" or temporal changes therein (1907b:73). He insisted that rather than focusing on averages, economists needed to take account of the "curves for different industries" and other "appropriate sub-groups," and to "consider forms of deviation from the average . . . rather than the average itself alone" (1907b:62–63, 1907a:579; see also 1911:79, 175). In his later work, he generalized this in words that convey the gravity that then attached to choices between different statistical tools: "The most marked development of science in the latter half of the nineteenth century took its departure from the study of deviations from the average rather than of the average itself, and economists will . . . adjust their theories in the light of this, [for] one of the invaluable services [of] the newer statistical methods [is] to liberate speculation from *the bondage of the average*" (1911:183, emphasis added).

By "the newer statistical methods," Moore meant the tools of the British approach, which he thought that contemporary economics should import from other disciplines, particularly from "recent biometric and anthropometric work," as he wrote referring to Pearson and Galton (Moore 1907b:62, 1911: 4–5, 183) and perhaps to Boas as well (Dorfman 1955:186). Not that these boundary objects were directly transferrable; Galton, Pearson, and Boas had used correlation and regression to study the relation between measurements of different organs in the same or related individuals, while economic analysis utilized aggregate data from units that were often distinct in place and time (Persons 1925, 187, 1927: 167). But Moore perceived the underlying similarity between the two situations and applied the British approach to investigate the distributions and patterns of variations of a variety of economic variables. In his first major book, for example, he used simple correlation, partial correlation, and regression to specify "the law of variation [and] the degree of association" between wages and several other economic factors (subsistence costs, living standards, etc.) (1911:19). Likewise, in his later work on economic cycles, he analyzed the "relation between the variations in the supply of [products] and the resulting variations in their respective prices" by means of multiple correlation and regression, which he strongly advocated as a way to control for confounding factors in natural settings and thereby to eliminate some of the assumptions that had defeated the efforts of earlier economists. To Moore, multiple regression "inquires directly, what is the relation between [two economic variables], not *caeteris paribus*, but other things changing according to their natural order" (1914:62, 67; see also Christ 1985; Morgan 1990; G. Stigler 1962).

In all of this, Moore's views often sound closer than those of Columbia's other statistical pioneers to the ideas of present-day social scientists. His writings describe "statistical economics" as a way to "test . . . the hypotheses and theorems of [theoretical] economics," to discover "facts and empirical laws for the elaboration" of new theories, and to furnish empirical estimates of the parameters in theoretical models specifying the causal relationship among economic factors (1911:23; see also Moore 1908; G. Stigler 1962). Even as this line of thinking carried him forward to early developments in econometrics (Morgan 1990), however, Moore's commitment to extending statistical methods into economics remained rooted in the circumstance that, in his time and place, statistical methods had become a legitimate defense for the scientific boundaries of his discipline.

CONCLUSION

Recent studies by sociologists of science of the tools of scientific practice improve our understanding of the historical process by which European statistical methods were incorporated into the American social sciences. The adoption of statistical methods occurred in a period when statistics were virtually absent from the natural sciences in America, and most previous literature has treated the incorporation of these methods as part of the history of individual social science disciplines viewed in isolation from one another.

This study suggests that these disciplinary histories can be complemented by the social-organizational perspective found in the case studies of sociologists of science, especially by analyses of interdisciplinary relations and local institutional conditions. Our exploratory research shows that the move in American social science toward statistical research was not simply a moment in the development of individual disciplines of psychology, anthropology, sociology, and economics, but it was also part of the boundary work pursued by each discipline with respect to other academic disciplines in the turn-of-the-century interdisciplinary field. As the emerging social sciences sought to establish themselves and to resolve the newcomer's dilemma of conformity versus differentiation, Cattell, Boas, Giddings, and Moore used statistical tools as boundary objects. By turning to statistical methods, each of the four thinkers attempted to demonstrate his discipline's adherence to accepted models of scientific practice and to construct a distinctive mode of statistical analysis that used available European statistical resources to investigate his field's own object of study. This process was localized, however: It advanced from one discipline to the next and then to the next primarily at Columbia University, which acquired an early lead over rival universities in the area of statistics and came to be institutionally identified with statistical methodology. Determined to cultivate this reputable scientific identity, University administrators recruited quantitative researchers in psychology and anthropology, supported their statistical work, and thus effectively established statistical methods as the model of legitimate scientific research for later thinkers in sociology and economics.

These turn-of-the-century historical developments were, of course, only the beginning. Between 1915 and 1930, the American social sciences made a much broader move toward quantitative research—a move that involved more thinkers and more disciplines than we have considered here (see Bulmer 1981, 1984; Ross 1979:126–30). In this later period, Columbia University gradually relinquished its early preeminence in statistics, and the different modes of statistical analysis that had been differentiated between 1890 and 1915 began to intermingle as a new generation of researchers drew upon the intellectual tools that had become available. This blurring of lines was already evident in the work of Cattell's student, Edward Thorndike of Columbia Teacher's College, who brought to the growing specialty of educational psychology a "composite model of research," which made use of both the Continental and British statistical resources (Danziger 1987). Yet later statistical thinking, as the Thorndike example would show, was also firmly embedded in an interdisciplinary and local institutional context, a context that differed significantly from its turn-of-the-century counterpart, but that was no less relevant for understanding methodological developments. Even as these subsequent developments unfolded, moreover, they built upon the legacy of the Columbia University era. The sweeping movement to quantification in the 1915 to 1930 period rested on the premise that statistical work was the touchstone of science—the premise that had been established through the social process in which Cattell, Boas, Giddings, and Moore took part.

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