CHAPTER 4

FOUR WAYS TO MEASURE CULTURE: SOCIAL SCIENCE, HERMENEUTICS, AND THE CULTURAL TURN

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I would like this quote (first 7 lines) to be set off more clearly as an epigraph. Can you suggest appropriate stylistic convention? (At least a line of separation?, indent? etc.?) (Thanks)

Introduction

"The business of pinning numbers on things—which is what we mean by measurement—has become a pandemic activity in modern science and human affairs. The attitude seems to be: if it exists, measure it. Impelled by this spirit, we have taken the measure of many things formally considered to lie beyond the bounds of quantification. In the process we have scandalized the conservatives, created occasional chaos, and stirred a ferment that holds rich promise for the better ordering of knowledge" (Stevens 1959).[3]

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insert "have been applied to create formal models of culture in the social sciences. We make use of"

In this essay, we review some of the ways in which ~~formal~~ measurement practices ~~been used to study culture. We focus exclusively on formal~~ models that are built from empirial data and, in this context, we make two ~~further~~ distinctions. We talk about the kinds of modeling projects that happened before as compared to those that happened after the sweep of cultural turns that have moved through the social sciences over the last few decades. Second, we talk about types of formal modeling projects that have explicitly hermeneutic goals in comparison to those that do not. Practitioners of the former sort want to use measurement tools to make interpretations to unlock useful readings of texts. Those of the latter persuasion seek robust measures of cultural forms that can be fitted onto other explanatory frames.

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We use the term model in a highly restricted sense. For us, a model must include the study of empirical (collected) data, analyzed with quantitative or other formal pattern analysis procedures so as to obtain a structure-preserving reduction into a simpler, more easily describable set of features or characteristics.[4] We use culture, on the other hand, in an unrestricted way. For the purpose of this essay, we include as cultural forms whatever the modeler presumes them to be.[5] We begin with a discussion of the nature of formal measurement models in the social sciences. We describe these as the products of a dually ordered system of practice that are articulated into discursive formations that constitute distinctive styles of gathering, conceptualizing, and analyzing data. We compare this mode of scholarship to more hermeneutic styles of research, which leads us to comment briefly on recent debates over method in the social sciences. The chapter then shifts focus as we turn to a detailed discussion of four different types of formal (measurement) models that have been especially important to the cultural sciences over the last century.[6] We conclude by revisiting the problem of how to theorize scientific hermeneutics by comparing our theorization of the practice of data analysis to Ricoeur's theorization of the practice of text analysis.

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Formal Models of Culture—A Definition and Four Functions

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What is a formal model? In our usage, a formal model is the product of *data analysis*. The first term here, *data,* refers to a social object that we (as practitioners) construct to stand in for the object of our investigation; in this case, for some aspect of the social world. To gather data, we invoke measurement conventions about features of the social world or situation, conventions about what ought to be noticed and recorded. Data collection itself is an institutionalized social technology, organized through professional projects, made up of ensembles of activities that are defined, enacted, differentiated in function, ordered in series, elaborated, and extended, even as they are also dually articulated into corresponding theoretical systems concerning the nature of the social—its character and its secrets. What results is a socially produced ensemble of craft skills that can be more or less stable over time.[7] The act of data gathering relies on conventions regarding the recording of: (1) a counting of things in the world (which implies the Kantian problem of noticing things in the world); (2) a distinguishing of definable features of things in the world; (3) a mapping of relations linking things (or features of things) in the world to one another; and so on.[8] Data, then, are the organized collection of this information, derived from a series of acts of measurement, stored in a retrievable fashion. Data collection is thus dependent on both particular theories of measurement and select repertoires of measuring practices.

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Much the same could be said about the *analysis of data*.[9] One engages in specific acts (or sets of acts) of formal analysis (drawn from collectively shared repertoires of statistical, mathematical, or logical data reduction practices) in order to produce a formal model (which we define now more specifically as a reduced form representation of a larger, more complexly ordered system of information organized as a "dataset"). An *analysis* depends on some specifiable process for reducing this larger complexity (contained in the data) by a structure preserving mapping (a homomorphism) achieved by an agreed on set of analytic practices (hence, a transformation toward greater simplicity based on an elaborated theory of pattern reduction that may or may not be already absent from the mind as background consciousness).[10] To make a model, one first takes the data as measured (and thus always already pre-interpreted), stakes a claim on a conventionally acceptable structure preserving transformation of the dataset, and offers a narrative account of why this is a valuable simplification (what it helps us to understand, why understanding this is interesting, etc.). All this is presented in an effective rhetorical form (e.g., McCloskey 1986; Bazerman 1988; Dear 1991). Thus, a data analysis involves claims and interpretations (some of which are explicit, most of which are not) about how and why the designated model maps onto a theory about the social world as represented by the specific (as available) set of data (Breiger 2000, 2002). In a given social field, the stronger the linkages and the deeper the resonances, the more analytically elegant, intellectually "sound," aesthetically pleasing, helpful to a valued theory, inspiring to others, aligned with vested interests, etc., the greater the "value" of the analysis (and the model).[11]

In other words, science is a human institution like any other, defined by its own peculiar rules, moralities, systems of social organization, styles of practice, forms of knowing, types of religiosities, and all the rest.[12] As is true of other academic knowledge production programs (e.g., analytic philosophy, critical theory, literary criticism, and so on), formalist social science has its own distinctive institutional characteristics as well as its own unique advantages and disadvantages. In this essay, we focus on what we see as the advantages of formalism as they apply to the study of culture. In this respect, we feel, like Bourdieu, that scientific work has tangible benefits. Bourdieu writes, "The 'art' of the scientist is indeed separated from the 'art' of the artist by two major differences: on the one hand, the importance of the formalized knowledge which is mastered in the practical state, owing in particular to formalization and formularization, and, on the other hand, the role of the instruments, which, as Bachelard put it, is formalized knowledge turned into things. In other words, a twenty-year-old mathematician can have twenty centuries of mathematics in his mind because formalization makes it possible to acquire accumulated products of non-automatic inventions, in the form of logical automatisms that have become practical automatisms" (Bourdieu 2004, p. 40).[13]

We begin by proposing four kinds of intellectual (knowledge) functions that formal modeling can contribute to an analysis of culture. These move upward from less to more highly ordered types of knowledge interventions:

(1) **Representation Function**: Much of human perception and cognition occurs through informational systems that are geared toward condensing complexity to simplicity and for providing ordered representations of more complex wholes. Data analysis performs a similar type of condensing function: it compresses the vast complexity of a social situation into a measurable simplicity of specified variables. On the downside, the process demands a necessary brutalization of reality since most available information must be discarded. Indeed there is actually a *double distortion* taking place here since both the *data process* and the *analysis process* take us (by design) from complexity to simplicity, from more to less information. On the upside, one has the ability (within certain bracketed ranges of convention) to forcefully influence how these representational processes occur and thus to influence the logic of the discovery process.[14]

(2) **Heuristic Function**: Formal models perform a heuristic function in the sense that they gather our thoughts for us and ground them in an understanding. They collapse disparate things and relations into meaningful bundles; they gather bits of information into larger aggregate formations—narrative forms, action sequences, classificatory systems, and the like. They hold complicated relational systems steady and still in our mind, and, thus, they anchor our cognitions about things in the world, providing unifying gestalts that are often iconic or narrative in form.[15]

(3) **Power Function**: Data analysis (as we have defined it here) also has a power function in the sense that formal models operate as representational embodiments, extensions of our thoughts into material space. In this sense, data analysis extends our cognitive capacities in much the same way as any tool (or technology) allows us to achieve greater material impact by amplifying our natural physical capacities.[16] To borrow a concept from Paul Ricoeur, one can say that the objectification of data analysis is in some ways parallel to the process that occurs when discourse is objectified as a text. Specifically, data analysis creates what, following Ricoeur (1971), we think of as a second order externalization (and thus, as with Ricouer, we see this as a kind of materialization function) by taking our theories about the world, giving them material form (as data, however hobbled), interacting with them, (however roughly, through analysis), and, then interpreting them, (however heroically through a reading of signs within a field of meanings and actions, as a part of the lived habitus of model building). Data analysis filters our thoughts through the material world. By watching how those waves come back to us again, in what ways they are bent or changed, we learn.[17]

(4) **Sociality Function**: Finally, data analysis both facilitates and is critically dependent upon the social organization of knowledge production. Data projects derive from (and are produced by) social technologies that are built around craft communities of professional academics located in scientific fields. Data analysis allows for a particular style of collaboration because externalized data (and the methods that produce them) facilitates a distribution of work across many researchers and research sites.[18] In the natural and physical sciences (especially) these linkage systems (articulation structures connecting material objects, styles of action,

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systems of discourse, and deeper institutional logics together) have enabled the production of especially fine-grained divisions of labor, allowing for large multiplier effects and the concomitant production of highly complex (industrial scale) systems of knowledge production that often generate powerful bursts of technological (and intellectual) innovation.

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Methods Debates—Before and after the Cultural Turn

But what if these knowledge functions are of no use when it comes to studying human institutions because the basic methods of scientific investigation are poorly suited to the kinds of knowledge-gathering activities necessary for an adequate study of the social? This issue has been the source of multiple, long-standing philosophical disagreements in the humanities and social sciences. Our only comment on this complex matter is to point out that in recent years this debate has fundamentally shifted ground. What used to be an argument (in the 1970s) about the nature of human culture and its implications for knowledge practices in the social sciences has now moved (and has been moving for a while) to a series of new debates concerning the proper way to interpret the meaning of cultural texts. In other words, the discussion has jumped from the *theory* of cultural analysis to the *method* of cultural analysis. Like the last debate, the current argument is often played out across divides of methodological practice (e.g., the tensions are often at their highest between quantitative and qualitative scholars, and these divides serve as frequent fault-lines in departmental, disciplinary, and interdisciplinary politics), but the issues are very different today than they were thirty years ago.[19]

A key issue back then was whether human actions and institutions are fundamentally different from other kinds of phenomena (of the sort studied by natural scientists) because humans are critically oriented toward meaning. What goes on in human life is broadly oriented towards the ways that humans understand their life-worlds. Scholars of the social world need to understand what is being understood. But the regularities governing human meanings are fundamentally different than the kinds of laws that are the objects of traditional (natural) scientific modes of explanation. Trial and error, hypothesis testing, all of this may be well and good for explaining the natural world, but these procedures will not carry us through the work that we must do if we are to interpret human action that is fluid, malleable, self-produced, and not anchored in invariant principles or rules.

An edited volume by Rabinow and Sullivan (1979), published thirty years ago as a self-proclaimed manifesto of "the hermeneutic turn," made this argument. The volume's introductory essay explains what was at stake. "(A)s long as there has been a social science, the expectation has been that it would turn from its humanistic

infancy to the maturity of hard science, thereby leaving behind its dependence on value, judgment, and individual insight" (p. 1). Citing Kuhn's description of what must happen for a field of science to move from a chaotic "pre-paradigm" state into a fully developed scientific system, Rabinow and Sullivan confidently proclaim, "Now the time seems ripe, even overdue, to announce that there is not going to be an age of paradigm in the social sciences. We contend that the failure to achieve paradigm takeoff is not merely the result of methodological immaturity, but reflects something fundamental about the human world" (p. 4). The volume reprinted classic essays by Thomas Kuhn, Clifford Geertz, Charles Taylor, Paul Ricouer, and H. G. Gadamer (among others), all of them "exemplary of the interpretive or hermeneutic approach to the study of human society" (p. 1).[20]

Many of these essays continue to serve as powerful foundational statements for the human and social sciences, but the debate that had so inflamed the passions of the day and framed the logic of the book is now largely moot. The key difference is the cultural turn itself. Thirty years ago the issue of culture was still in dispute; today it is not. Somewhere between then and now nearly all practicing social scientists, even the most quantitatively inclined, experienced some version of a "cultural turn." Thus it is now a veritable tenet of modern social sciences that the world *is* socially constructed, which is to say that knowledge of this state of affairs is now included as part of the collectively shared intellectual background, or, to use Holton's (1988) imagery, the underlying thematic of this set of interlocking intellectual fields.[21]

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But just what this means is complex. For us, a key difference is the shift from assuming that culture is constituted by a logic deriving from other kinds of things—the economic base, the material infrastructure, the tyranny of numbers, the demands of biology, the mandates of function, the transformations of structure, the rhythms of the super-organic—or whether culture is instead seen as being the ever-present and inseparable other side of the material, the meaningful complement to embodied practices, and thus treated as something which in fact makes up (constitutes) other kinds of things. This further presumes, or so we would argue, a mutual constitution of the cultural and the non-cultural, a duality of meanings and things, as culture is in turn shaped by what it constitutes (e.g., Giddens 1979; 1984; Friedland and Alford 1991; Sewell 1992; Orlikowski 1992; Breiger 2000; Friedland 2009).

Although this division is quite similar to Kauffman's (2004) distinction between the kind of cultural research that emphasizes a connection to *exogenous* factors (hence the goal is to make linkages between cultural and social processes) and more recent styles of cultural analysis concerned instead with *endogenous* cultural processes (in which the goal is to see how culture itself operates according to a more specific internal logic), in fact, we mean something different. Both before and after the cultural turn, there have been some cultural scholars who emphasized the linking of cultural and social processes together in exogenous analyses and others who focused on the identification and explication of internal mechanisms defined (endogenously) within the cultural system itself. In other words, the tension between endogenous and exogenous approaches to cultural analysis that Kauffman has

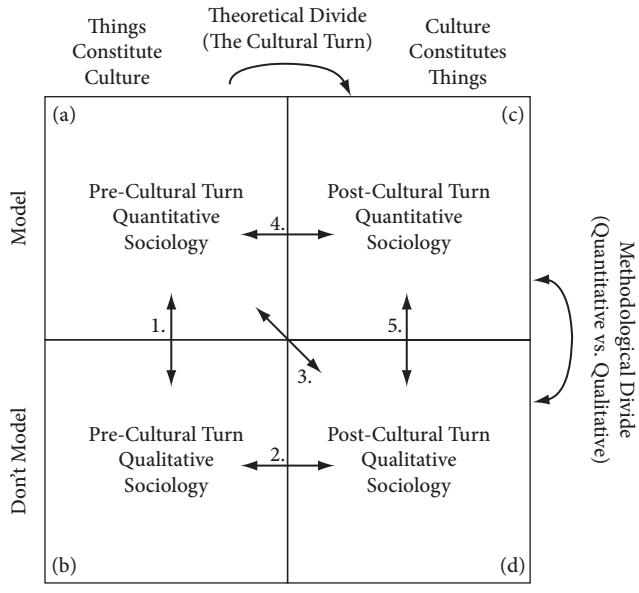
identified cycle through the discipline (in fractal style, according to Abbott 2001). What is different for us and what is designated by our use of the notion of a cultural turn (which suggests a movement from one gestalt system to another) is the sense of the social world as being broadly constituted through systems of shared meanings (in a Wittgenstein language-game sense of the term). And so, what is perhaps most distinctive about post-cultural turn scholarship is the far more nuanced appreciation of how the meaningfulness of social life creates the foundation for that which is the material, the practical, the structural, the social, and even perhaps the biological sides of social life. It is thus the sense of how culture constitutes the social world of things (rather than how a logic of things defines the rules of culture) that most precisely delineates post- from pre-cultural turn scholarship.

Of course the particular way in which the cultural turn has been experienced, understood, internalized, theorized, or resisted varies broadly. For many American social scientists, the "social construction" of the social world exists just as Berger and Luckman (1967) explained it (or perhaps it was Kuhn 1962), and they do not worry much about the matter or reflect on what it means for what they do when they go to work everyday. For others, the analysis of how culture constitutes the social is the very focus of their work. The theoretical basis for understanding culture is, of course, quite complex, Culture is seen as part of a whole, as elements of a dialectic, as a dually structured order linking, for example, material and cultural domains.[22] Others, see it as fragmented, idiosyncratic, and de-centered. But, in general it is, we think, true enough, that the accepted theoretical foundations of most social scientific fields today (at least in the American case) include some version of social constructionism as a core element defining the theoretical background.[23]

To give just a few quick examples, consider the sociology of organizations (the field we know best). The new institutionalists, who have been strongly constructionist from their beginnings in the late 1970s, have arguably emerged as the dominant perspective in the field today. More tellingly, even the organizational ecologists (the other huge success story in this arena) have made their own cultural turn over the years. In the 1970s, their theories were based on the logic of biological ecosystems. By the 1990s, their work was grounded in arguments about legitimation and the socially constructed character of value. In the last decade, the core of the program has turned to emphasize the study of the communication of cultural codes as a new method for analyzing the ecological space of organizations.[24] This framework is made explicit in the latest book by Michael Hannan and his colleagues:

> We attempt to develop a fresh perspective on forms and populations. This approach…emphasizes the social construction of categories, forms, and populations. In this respect, it follows the institutionalist tradition started by Max Weber and reinvigorated by Philip Selznick, which strongly suggested that normative matters needed to be incorporated in any attempt to understand organizations. That is, organizations should not just be analyzed objectively in terms of their patterned activities, functions, and external ties—they must also be considered in terms of their social meanings and interpretation given to them by contemporaneous actors (Hannan, Pólos, and Carroll 2007, p. 31).

Table 4.1 Debates over Method: Up to, Including, and Beyond the Cultural Turn



Another example is social network analysis. Thirty years ago, many within this intellectual community were self-consciously anti-cultural positivists. During the 1990s (as we will explain presently), a number of the leading figures in this community began their own version of a cultural turn, and since that time many others have followed.

This is not to say that the debate over method has come to an end. On the contrary, in some ways, it has intensified as it has shifted shape. Table 4.1 can help to visualize this transformation. Here, we follow John Hall's (2004) procedure for differentiating culture scholars along two binary dimensions, and, like him, we divide individuals according to "whether they conceptualize phenomena in *meaningful* versus *non-meaningful* terms" (Hall 2004, p. 117). We operationalize this idea differently, however, by placing culture scholars, who have what we call a pre-cultural turn sensibility in the left column, and post-cultural turn intellectual projects on the right. As our column headers suggest, what matters for us in this distinction is not just meaningfulness per se (because cultural modelers have long had an interest in analyzing meaning), but also the way in which meaning operates within an explanatory narrative. As we noted earlier, pre-cultural turn scholarship tended to emphasize how various kinds of things (that exist outside of culture) drive, organize, or constitute the logic of cultural forms. In contrast, post-cultural turn scholarship tends to highlight the ways in which culture (and meaning) is itself constitutive of the other things that make up the social world.[25] Pre-cultural turn theorizing tends to isolate culture from other aspects of the social. Post-cultural turn scholarship seeks to systematically unpack the co-constitution of the cultural and the noncultural.

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As the table suggests, the central dispute of the cultural turn era (as reflected, for example, in the Rabinow and Sullivan text) was an argument that stretched across both a theoretical and methodological divide, separating post-cultural turn qualitative sociologists and pre-cultural turn quantitative sociologists (arrow #3 in Table 4.1). The crossing of two dimensions of differentiation results from the uneven development of the field, with qualitative and interpretivist scholars making an earlier and in some ways more forceful cultural turn than their formalist colleagues who often came along later and with less enthusiasm. Today, the debate is more likely to erupt between post-cultural qualitative scholars and post-cultural turn quantitative analysts (hence, arrow #5).[26] But to understand the character of those arguments, we need to more fully distinguish between the different approaches to modeling culture, especially the differences between what we will call hermeneutic and non-hermeneutic styles of formal analysis.

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Four Types of Culture Models

The divide between quantitative and qualitative methodologies may be less of a binary distinction than a disjoint gradient. At least when we reflect on the variety of formal approaches that have been used for modeling culture, it is clear that some are far more interpretively intentioned than are others. In this section of the chapter, we will highlight that distinction as we describe four types of modeling strategies that have (classically) been used to analyze culture. To be clear, we are now focusing on just the top row of Table 4.1 (analyses using quantitative measurement models), but we are going to split this group again horizontally along a new fissure, distinguishing between those modeling projects that are hermeneutic and those that are not (see Table 4.2).

Here, we must clarify from the start that we use the term "hermeneutic" in a particular sense. We focus on the definition of hermeneutic that has been suggested by Paul Ricoeur who we cited earlier and whose work has been central to the field. According to Ricoeur (1971), "The word 'hermeneutics' concerns the rules required for the interpretation of the written documents of our culture" (1971, p. 197). Although, it is true that Ricoeur sees the model of the text as an exemplar for other types of interpretation, as Ricoeur's own work makes clear, spoken language differs from written language in a number of important ways (and, as we will explain later, one must carefully distinguish the two when considering the problem of interpretation). Recognizing that this solution leaves open a whole variety of other theoretical and methodological conundrums (about agency, subjectivity, speech activity, intersubjectivity, and the like), we nonetheless will follow Ricoeur's conception of hermeneutics as a way to begin this discussion. In each cell, we have named one scholar (whose work we will describe in some detail) because they provide ideal typical examples of the four types of cultural modeling styles of interest to us here.

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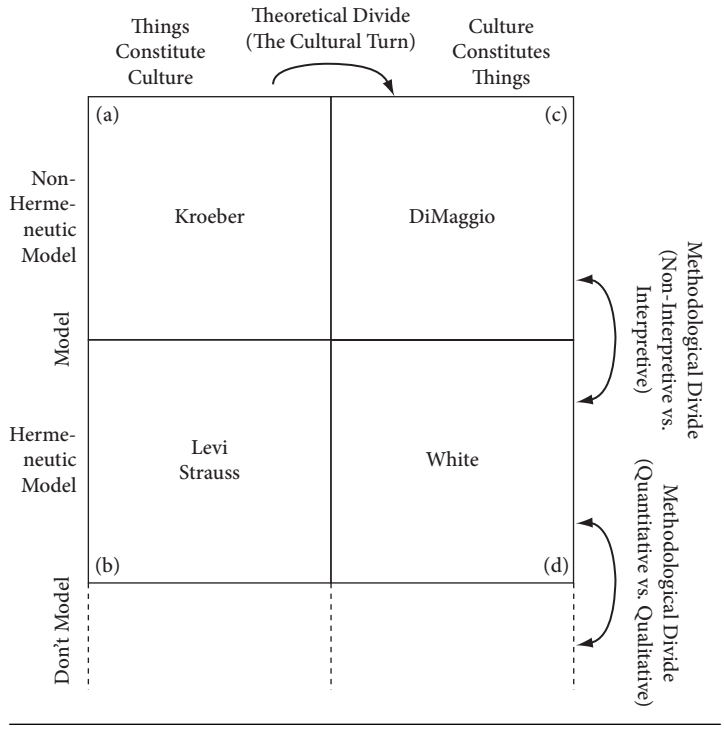
Table 4.2 Four Types of Culture Models: Interpretive vs. Non-Interpretive, and Pre- vs. Post-Cultural Turn



Cell A. Pre-Cultural Turn/Non-Hermeneutic: Alfred Kroeber

Probably because their work was inherently comparative, anthropologists were the original masters of the culture concept. So it is no surprise that Alfred Kroeber, a leading anthropologist, was one of the first American social scientists to develop a modern approach to modeling culture. Kroeber's interest was similar to Durkheim's in that Kroeber wanted to demonstrate that culture existed, that it had its own character or logic to it, and that it operated at some remove, outside of, and above the heads of particular individuals who otherwise participate in it. He wanted to prove this with scientific rigor, and so he took up the study of women's evening dresses as a way to address the problem (Kroeber 1919).

Kroeber begins his paper by describing the rising and falling of various cultural trends, trends that have a kind of sweep and force that reflect something bigger and broader than any particular individual experience. Even a genius like Shakespeare must be seen in historical context because, as Kroeber explains, there is a clear pattern that underlies "the story of the Elizabethan drama from its stiffly archaic inceptions through the awakening in Greene and Marlowe, the Shakespearian glory, the slackening to the level of Fletcher…to the close of playhouses by the civil war, the picture of an even-sided curve rises in the mind." The curve might strike us as tracing something like the ebb and flow of a specific literary genre within an institutional

field, but Kroeber sees something different. His culture concept is framed by his location within the emerging intellectual field of American anthropology as it existed a century ago. He thus leans more toward a functional aesthetics where the quality of cultural forms vary absolutely, not just as cultural traits defined by the presence or absence of specific practices and conventions, but rather, also, as expressions of cultural excellence.[27] Arrayed on a timeline, Shakespeare's "masterpieces…fall in the first decade of the seventeenth century. His more prolix and less intense tragedies and comedies, and the plays of contemporaries nearest him in achievement, precede and follow by a few years. Each quinquennium more distant from the culmination is marked by greater crudity in recession, more extended laxity in progression of time…" (p. 235). Thus for Kroeber, the quality of cultural expression goes from low to high and then back down again in a pulse of authentic creativity that quickly burns itself out. And this pulse, it turns out, is not that of any person or individual. It is the rhythm of culture itself that Kroeber is following.

Kroeber explains, "If such a surge stood unique, it would be meaningless. But it is so often repeated in the history of aesthetics that something of a generic principle must be involved. The classic French drama…the Dutch and Flemish schools of painting…and, we might add, philosophy—each of these isolable movements has been traced through similar course of origin, growth, climax, decline, and either death or petrification, analogous to the life stories of organisms" (p. 236). Here then is a second expression of Kroeber's historicity. When he thinks about patterns in cultural forms, he sees them as expressions of a larger force of nature that is operating on social processes, perhaps some deep underlying logic that governs all of human culture, and, according to which, every cultural form must respond.[28] This suggests a force of culture that is more than just what humans experience (or, at least, more than what they are knowledgeable of). Culture is driven by abstract causal forces, factors lying outside the parameters of individual human experience, and in that sense, although this would clearly count as an example of an endogenous approach to culture (Kauffman 2004), it shows how culture that is decoupled from an adequate sense of the social leads to a different kind of exogenity, to a logic that is both immanent to and also outside of culture itself. This is why we classify Kroeber as an ideal example of what we are calling a pre-cultural turn analytic sensibility. For although he was completely focused on analyzing culture, and in spite of his other impressive contributions to the field, Kroeber was weighed down by theoretical tools that were thoroughly marked by his historical, situational, and professional period, such that, ultimately, for Kroeber, things (which, in this case, are largely abstractions) constitute the logic of culture (rather than the other way around).

We have also placed Kroeber in the top row of Table 4.2 because he employs a non-hermeneutic style of the formal model. In this regard, Kroeber stands at the beginning of a long line of formal modelers in the field of culture. The problem, at least as Kroeber saw the matter, is that "the variability of the phenomena is qualitative whereas a workable law or deterministic principle must be quantitative in nature." And thus he confronted the classic question, how does one move from quality to quantity?[29] He considers, for example, the possibility of using a set of

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informants to make numerical judgments about the qualities of particular cultural objects, but he rejects this strategy because "it would rest on a series of composite photographs of verdicts as to qualities, and not on verifiable measurements" (p. 236). However, Kroeber was also involved in archeological research so he had an expectation that material artifacts could be used to deduce the properties of cultural forms and, indeed, "manufactured objects offer an approach which no other class of civilizational data present: they can be accurately and easily measured." Still the quantity of found artifacts are "often insufficiently large, or from interrupted periods, or of uncertain date" (p. 238). And, more than this, many objects (especially if they are more utilitarian in nature) are less responsive to cultural trends.

This explains why Kroeber turns to the study of women's evening dresses. On the one hand, dresses are good to model with because they are especially cultural in nature. They are "material objects whose chief end is ornament." They are impractical, specialized, highly aestheticized, and, as Kroeber's explains, "the variations are therefore purely stylistic" (p. 239) and thus more likely to exhibit long term cultural trends undistorted by the periodic shifts of utilitarian demands and inventions. Moreover, in contrast to the vagaries accompanying the analysis of archeological artifacts, Kroeber turns to study women's evening wear precisely because the data can be gathered. The styles were preserved in texts, as images published in fashion magazines. "Such journals have existed for over a century; they are exactly dated; and they bring together in each volume a considerable number of examples to which rule or calipers can be applied without hindrance" (p. 238). Kroeber sampled annually starting from the year he was doing this research, 1918, to as back as far as 1844.[30]

Kroeber looked for a set of simple metrics that he could use to gauge stylistic changes over time. He describes how, as a graduate student at Columbia (around 1900) he had first tried to tackle the problem by measuring all relevant features of the dresses, but he was frustrated by his efforts. "One might measure collars or sleeves or ruffles for some years, and then collars and sleeves and ruffles disappeared.... If one took as a base the total length of the figure, coiffures fell and rose by inches from time to time, or were entirely concealed by hats or nets. I abandoned the plan as infeasible" (p. 239). But he returned to the project nearly twenty years later with a new set of intuitions. The second time around he limited himself to eight simple measurements (four lengths and four widths) that could be applied across the entire range of his data. He began by measuring: "1. Total length of figure from the center of the mouth to the tip of the toe. If the shoe was covered, the lowest point of the skirt edge was chosen. The selection of the mouth obviated all difficulties arising from alteration of hairdress" (1919, p. 239). Thus, Kroeber starts with a very practical concern, how to measure features of cultural forms that would apply across the range of variation of all objects within the field. He then proceeds to record the length of skirt (calculated by measuring from the floor to the bottom of the skirt and subtracting from the first measurement), the waist (minimum width), the "length of décolletage ... [d]iameter of the skirt at its hem ..." as well as the width of the shoulders "or more accurately, the width of the décolletage across the

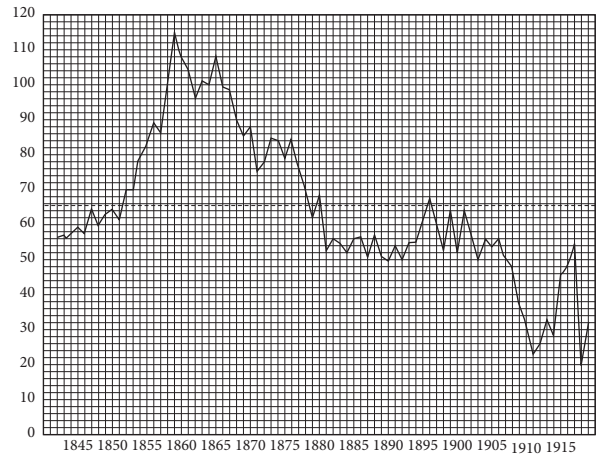

Figure 4.1 Kroeber's Model of Cyclic Patterns in Women's Dress Fashions

Source: Copied from Kroeber 1919, p. 247.

shoulder" (p. 240). He measured ten dress images for each year, "the first ten suitable for measurement being taken from each volume so as to insure random instead of subjective selection…" (p. 240). He calculated ratios for all of the measures and plotted the values on a set of three graphs with a horizontal axis marking off each year in his study, and a vertical axis marking off the ratio values on a percentage scale (see Figure 4.1).[31]

Figure 4.1 illustrates Kroeber's model of stylistic trends in the widths of women's skirts. It is a simple graph. It tracks what might appear to be a relatively unexciting wave or trend line, rising up, falling down again (although the down slope seems to have some interruptions on its way to the bottom). Presumably, it will rise up again and fall down again, or, at least, so Kroeber wants us to believe. Kroeber's interest in this work is to illustrate, with scientific formality and repeatable results that a logic of culture exists, that it is bigger than, and thus external from, any individual self. As he surveys his results, he matches the data analysis step by step to things that he knows about the cultural forms he is investigating. He also gently works in a set of theoretical assumptions about what may be going on in the graph. In the process, he narrates his model into life. Here is how he describes this graph:

> When our record opens in 1844, it finds evening toilettes of moderate skirt width, 57 per cent. of the body length. (*) For several years the proportion fluctuates mildly, gradually rising.
>
> In 1851, having attained a percentage of 61, the width of skirt suddenly begins to mount rapidly and continuously, until the plotted curve skyrockets to the extreme maximum of 116 in 1859. This is the apex of the crinoline hoop skirt fashion, when the flare of the skirt exceeds the height of the person. In eight years the skirt diameter has nearly doubled.
>
> From 1859 on, the history of the skirt may be summarily described as a fifty years' progressive constriction.

> The narrowing after 1859 is not as rapid as the widening immediately preceding; but within three years the proportion has fallen from 116 to 96. At this point a new sub-factor enters: the train. The skirt as a whole continues to lose fulness (sic), but the attached train more than compensates for the shrinkage of diameter at its base. The plot therefore shows a checking of the descent, a new rise, and a secondary maximum of 108 in 1865. {* Mouth to toe, or to lowest point of skirt if the toe is covered} (pp. 247–248)

His narration follows the graph from its beginning to its end, and he repeats the process with each of the other graphs (which track the other measured qualities of the dress). What he finds are some surprisingly coherent wave trends reflecting the character of stylistic changes. In Kroeber's terms,

> We have, I think, now found reasonable evidence of an underlying pulsation in the width of civilized women's skirts, which is symmetrical and extends in its up-and-down beat over a full century; of an analogous rhythm in skirt length, but with a period of only about a third the duration; some indication that the position of the waist line may completely alter, also following a "normal" curve, in a seventy-year period; and a possibility that the width of shoulder exposure varies in the same manner, but with the longest rhythm of all, since the continuity of tendency in one direction for seventy years establishes a periodicity of about a century and a half, if the change in this feature of dress follows a symmetrically recurrent plan (pp. 257–258).

And so, with a simple measurement protocol, one that is easy to understand, and replicate Kroeber provides a model of what appears to be systemic cycles (he calls them waves) of incremental change in the styles of women's dress fashions.[32] Kroeber's piece is a classic in the history of formal models of culture, not only because of what he was able to show, but also because the work provides an excellent example of a very particular approach to modeling culture.

In this work, Kroeber illustrates a style of cultural modeling that reflects a pre-cultural turn analytic sensibility because for Kroeber culture is ultimately an artifact decoupled from the broader institutional systems that make up the social order as well as from the set of cultural understandings that constitute these systems. Culture has its own logic (and the study looks to unpack an endogenous quality of the cultural forms), but the motor for that logic is outside of the lived experiences and meanings of the cultural world and even in a sense (perhaps) outside of the social itself. Kroeber is imprecise about this. At one point he notes, "There is something in these phenomena, for all their reputed arbitrariness, that resembles what we call law: a scheme, an order on a scale not without a certain grandeur. Not that fashion of a future date can be written now. Every style is a component of far too many elements, and in part uniquely entering elements, to make true prediction possible" (p. 258). But, in the end, there is something, ineffable, perhaps, that explains these cultural forms. "The super-organic or superpsychic or super-individual that we call civilization appears to have an existence, an order, and a causality as objective and as determinable as those of the subpsychic or inorganic" (p. 263).[33]

substitute "individuals in that"

Kroeber's project also reflects a non-hermeneutic style of cultural modeling because the data are *not* used to gain understanding about what people think about; what they think that they are doing; or even what the underlying parameters of the cultural meaning systems are that enable certain types of thinking to occur. Instead, culture, for Kroeber, is a particular kind of social object. It is embodied in material artifacts that have variable qualities and manifestations whose shape and character can be modeled. In Kroeber's essay, culture is everywhere and nowhere. The subject of research is style, and yet there are no stylish people in this paper. There are no knowing agents or reference to talk about the meanings or experiences of fashion. What is being studied instead are the after-effects of the actions of culturally knowledgeable people. We are left with material artifacts, things that were made, and our task is to carefully examine and compare these objects, one to the other, as a way to begin to understand them. It is truly as if we are archaeologists trying to learn something about the culture of an ancient people, and all we have available to observe are the potsherds left behind in the dirt. As an archeologist, one has limited access to the lived experience of the people one wishes to understand, and so one must do what one can, gather up whatever evidence can be gathered, study it very carefully, pay attention to features of design, construction, style, and then one can infer what it was that may have been going on. But, of course, Kroeber did not need to study women's evening dresses in this manner. He was, after all, an accomplished ethnographer, renowned for his fieldwork among native American tribes (it was, Kroeber who worked with, and famously wrote about, the Ishi, the last of the Yahi Indians). Thus, he could have chosen to do a study in which he talked with women who purchased dresses. He could have interviewed magazine editors, seamstresses, and the men who escorted these women to their evening events. But in this stream of research, he made a different choice. He chose, first of all, to study texts, not people, and to use a data analysis technique that led to him to gather very small bits of information across a large expanse of time, a strategy with which he hoped to identify characteristics of stylistic change that would not be otherwise observable using the methodology of conventional ethnography.

Cell B. Pre-Cultural Turn/Hermeneutic: Lévi-Strauss

This then raises the question of what would a hermeneutically focused formal model of culture look like? The key distinction, by our accounting, is whether the researcher has the goal of developing a formal measurement model which is intended to help explicate, summarize, or to otherwise cast light *directly on the meanings* inscribed in a cultural text. Claude Lévi-Strauss, another leading anthropologist of his day, can help illustrate this type of modeling project. A key figure in the history of structuralism, Lévi-Strauss made significant contributions both to the theory of kinship and also to the analysis of cultural myths. He did this, most especially, by developing ways of applying the lessons he had learned from structuralist linguistics to the study of other types of human institutions. It is particularly

1	2		4			7	8
	2	3	4		6		8
1			4	5		7	8
1	2			5		7	
		3	4	5	6		8

Figure 4.2 Levi-Strauss's Model of the Structural Logic of a Myth

Source: Copied from Levi-Strauss 1963, p. 213.

with respect to the study of myths that Lévi-Strauss pioneered the use of formal models of culture that advanced an explicitly hermeneutic goal. Although he did not (generally speaking) use statistical forms of data analysis, he nonetheless measured features of the meanings within the myths and employed a form of Boolean logic to collapse complex structures into simpler forms.[34] He did this to produce formal models (structure preserving homomorphic reductions) of mythical stories. In generating these models, his goal was very consciously interpretive; he sought to make their meanings clear and to reveal the deeper underlying pattern of relations that would capture the symbolic code. In this sense Lévi-Strauss was very much a pioneer in the development of hermeneutically oriented formal measurement models of culture.

One of the earliest and still most recognizable examples of Lévi-Strauss' efforts can be found in his essay on "The Structural Study of Myth." Figure 4.2 reproduces his model of the structures of meaning in a mythical narrative, the kind of model that Lévi-Strauss applies to provide a reading of the Oedipus myth. In this essay, he argues that the goal of an interpretation is different when one works at the level of myth than when one works at the level of a linguistic statement. Although myths are a part of language and must (in general) be modeled in an analogous fashion (in terms of the relations that link them together as a system), Lévi-Strauss also makes clear that myths operate in a different discursive register. Rather than looking to find the patterns of relations that link particular phonemes, morphemes or sememes together into coherent sounds, concepts, and sentences, Lévi-Strauss argues that it is necessary to look for the relevant "mythemes" that make up the "gross constituent units" of the myth. This involves the task of linking sentences together in patterns of relations and, more than this, an investigation of the various "bundles of relations" that constitute the core elements of the myth.

Lévi-Strauss' technique depends on breaking the myth into simple sentences (or "*statements*") and then looking for commonalities in the semantic functions that each sentence performs. In the case of the Oedipus myth, the statements are semantically (and thus structurally) divided into common bundles of relations. Figure 4.2 shows the layout of such an analysis with the sentences arrayed across each row in the order of their telling, but they are also grouped into columns and assigned numbers (1–8) that correspond to the bundle of relations that each mytheme belongs to. "All the relations belonging to the same column exhibit one common feature which it is our task to discover" (p. 215). The first such bundle

from the Oedipus myth includes a series of statements in which blood relations are overemphasized, "Cadmos seeks his sister Europa, ravished by Zeus," "Oedipus marries his mother, Jocasta," "Antigone buries her brother, Polynices, despite prohibition." The second bundle of relations have to do with the underrating of blood relations—"The Spartoi kill one another," "Oedipus kills his father, Laios," "Eteocles kills his brother, Polynices." The third bundle has to do with monsters that are being slain, while the fourth bundle has to do with difficulties in walking straight and standing upright.

Lévi-Strauss explains that if the goal is "to *tell* the myth, we would disregard the columns and read the rows from left to right and from top to bottom. But if we want to *understand* the myth, then we will have to disregard one half of the diachronic dimension (top to bottom) and read from left to right, column after column, each one being considered as a unit" (p. 214). In the process, Lévi-Strauss provides a simplified understanding of the deeper system of relations that ties the myth together as a set of complementary relational systems. His vision is very structuralist at this level—he writes, "It follows that column four is to column three as column one is to column two. The inability to connect two kinds of relationships is overcome (or rather replaced by) the assertion that contradictory relationships are identical inasmuch as they are both self-contradictory in a similar way" (p. 216).

By applying these methods, Lévi-Strauss strips the myth down to its bare elemental form, thereby revealing a kind of structural map with which the *meaning* of the myth can then be more easily understood. Thus Lévi-Strauss's interpretation of Oedipus: "The myth has to do with the inability, for a culture which holds the belief that mankind is autochthonous [born from the earth], to find a satisfactory transition between this theory and the knowledge that human beings are actually born from the union of man and woman. Although the problem cannot be solved, the Oedipus myth provides a kind of logical tool which relates the original problem—born from one or born from two?—to the derivative problem: born from different or born from the same? By a correlation of this type, the overrating of blood relations is to the underrating of blood relations as the attempt to escape autochthony is to the impossibility to succeed in it" (p. 216).[35]

substitute "might"

In spite of his commitment to developing hermeneutically grounded formal models, Lévi-Strauss was nonetheless locked into a pre-cultural turn theoretical sensibility. Like Kroeber, Lévi-Strauss was very much focused on what Kauffman would describe as an endogenous analysis of culture, and yet, also like Kroeber, there was a strong sense of something else outside of the cultural artifacts themselves, some other more fundamental logic that constituted culture's dynamic essence. For Lévi-Strauss, the essential force behind the logic of culture (and this includes other social institutions which were also understood to be structured like a language) derived from "the universal laws which make up the unconscious activity of the mind" (1963, p. 65). Here Lévi-Strauss talks of constructing "a sort of periodic table of linguistic structures that would be comparable to the table of elements which Mendeleieff introduced into modern chemistry" (1963, p. 58), and he praises Kroeber's work on women's fashion noting, "a remarkable analogy between

substitute "much"

these researches and those of a contemporary biologist, G. Teissier, on the growth of the organs of certain crustaceans" (1963, p. 59). In other words, for Lévi-Strauss, just like Kroeber, the analysis of culture was driven by the pursuit of an essential core that served as an anchor and an origin (an archê in Foucault's sense) for cultural expression.[36] This meant that the goal of Lévi-Strauss's analysis was not ultimately directed to analyzing the ways that material practices, social structures, and other *objective* social processes can be reconsidered as culturally constituted and (dually) constituting activities, rather, Lévi-Strauss hoped to understand how some things (exogenous factors, outside and beyond the lived experience of culture) ultimately explain the logic of cultural forms.

substitute "might be understood"

substitute "interested in"

Cell C. Post-Cultural Turn/Non-Hermeneutic: DiMaggio

During the 1960s, with the exception of qualitative researchers who were mostly working at a more micro-level focus, the study of culture largely went out of fashion in American sociology.[37] The concept had been devalued, thanks in part to its close association with Talcott Parsons' theories, but also, in this period of middle-range sociology, research concerns had shifted toward more micro- and meso- levels of social life whereas culture had conventionally been treated as something describing society as a whole. In practice, a new appreciation for the interpretation of culture was being developed by emerging communities of qualitative sociologists during this era, but that work was not getting picked up by quantitative scholars who were focused on their own middle-ranged theoretical projects. Also during this period the use of attitude measures (and other types of subjective measurement regimes) were coming to be used less frequently as many sociological subfields embraced more "objectivist" approaches to data modeling. This means that qualitative and quantitative scholars were tending to go their separate ways, and culture (with a few notable exceptions such as political sociology which was still focused on the role of opinions in generating outcomes) largely ceased to be an object for quantitative analysis.[38]

substitute "techniques"

substitute "interpretive"

insert "modern"

Richard Peterson was one of the first American sociologists to put culture, understood in a more meso-level (and hence in a more institutional sense), back on the agenda for those who used measurement models to study the social world. Peterson had been trained as an organizational sociologist, had worked with Alvin Gouldner, and also had a deep and abiding interest in music. He brought the two interests together in his ~~early~~ work on cycles in symbol production (Peterson and Berger 1975). Like Kroeber, Peterson's efforts were non-hermeneutic: they were not concerned with the interpretation of the meanings of cultural forms. Rather his goal was to find a solid and defensible metric for measuring variability in cultural forms which could then be explained with respect to the conditions within a particular social domain, in this case, in an industry—the popular song industry. But Peterson went considerably further than Kroeber in that he also grounded his model for explaining the variation in cultural forms not in abstract notions (of the

del

super-organic) but instead, in a sophisticated and elaborate theory and analysis of the social organization of the culture industry.

An important next step was taken by Paul DiMaggio who followed Peterson's lead in grounding his study of culture in a detailed analysis of the social structural terrain in which the cultural forms were embedded. Borrowing from the most advanced methodological traditions of American stratification research, DiMaggio also developed a sophisticated and nuanced way of measuring variations in the cultural form itself. In his work on cultural capital, for example, he used a factor analysis to find patterns in the ways in which high school students are oriented with respect to their understanding of, appreciation of, and practical experience with elite cultural forms. Following Bourdieu, DiMaggio theorized that students would have acquired cultural sensibilities and orientations from the lived experiences and cultural milieu (e.g., the habitus) of their household of origin, and that those students who had mastered the skills and knowledge associated with elite forms of culture would be in possession of a high volume of what Bourdieu called cultural capital.

DiMaggio's (1982) studies showed that one could use technical methodologies (from the American social stratification tradition) to analyze data about an individual's relationship to culture and then use that metric as an effective predictor of students' success (as measured by grades achieved in high school). Figure 4.3 shows an example of one of DiMaggio's models of culture. The table summarizes the results from a large number of regression models. The first row shows results from trying to predict the grades of the daughters of men who did not complete high school. The first column has the contribution to explained variance of the students' scores on measures of intelligence. The beta coefficient (0.2930***) shows that effect in a standardized metric that can be easily compared to the contributions of variance from other predictor variables (such as column three which has the measured effect of cultural capital (0.1636***). DiMaggio also reports the overall variance explained ($R^2 = 0.1453$) and the amount of extra variation explained once the cultural measures are included in the models (0.0310). The largest effects for high levels of cultural capital occur among the daughters of college educated men, where the overall contributions to explained variance in grades are quite high. In models such as this, DiMaggio helped bring culture back to the statistical mainstream as a measured quality of social life shown to be on par with other, more presumably objective features of social organization.[39]

DiMaggio's models are non-hermeneutic. Without interpreting anyone's understandings, (e.g., using measures of more subjective phenomena) as in the more traditional survey approaches to culture (conceptualized as clusters of attitudes, values, norms, and beliefs), DiMaggio showed that survey technology could usefully be employed to measure significant cultural phenomena. With this he was able to demonstrate the importance of culture as a causal variable while still relying on more "objective" types of measures—visits to museums, piano lessons, familiarity with high culture, iconic figures, that were easy for mainstream quantitative sociologists to accept as having reasonable face validity.[40]

Dependent Variable		1	2	3	4	R^2	Increase in R^2 with Vars. 2–4
	Females with Non-High School Graduate Fathers					N = 342	
Grades in All Subjects	B	.5006	.3136	2.2030	.7400	.1453	.0310
	s.e.	.0660	.3966	.5010	.4966		
	beta	.2930***	.0331	.1636***	.0614		
Grades in English	B	.0440	.0906	.3312	−.0195	.1148	.0452
	s.e.	.0086	.0504	.0651	.0633		
	beta	.2093***	.0788	.2036***	−.0133		
Grades in History	B	.0654	.0958	.2638	.0213	.1358	.0272
	s.e.	.0093	.0546	.0705	.0685		
	beta	.2840	.0761	.1479***	.0145		
Grades in Manthematics	B	.0450	.0693	−.0005	.1535	.0442	.0075
	s.e.	.0098	.0577	.0745	.0724		
	beta	.1943***	.0548	−.0003	.0951*		
	Females with High School Graduate Fathers					N = 342	
Grades in All Subjects	B	.6216	−.8526	2.7058	.3466	.1776	0.494
	s.e.	.0980	.5542	. 6034	.6064		
	beta	.3266***	−.0844	.2238***	.0301		
Grades in English	B	.0629	.0831	.3641	−.0114	.1755	.0659
	s.e.	.0115	.0657	.0715	.0722		
	beta	.2906***	−.0720	.2608***	.0086		
Grades in History	B	.0634	−.0008	.4029	.0278	.1640	.0611
	s.e.	.0132	.0749	.0816	.0824		
	beta	.2585***	−.0006	.2546***	.0185		
Grades in Mathematics	B	.0870	−.1431	.2245	.0647	.1384	.0230
	s.e.	.0141	.0804	.0875	.0884		
	beta	.3356***	−.1034	.1343*	.0406		
	Females withcollege Graduate Fathers					N = 113	
Grades in All Subjects	B	.7317	−.6325	4.1952	−.2707	.2034	.0910
	s.e.	.1738	1.0617	1. 1567	1.2902		
	beta	.3714***	−.0530	.2968***	−.0174		
Grades in English	B	.0913	−.0148	.4244	−.0566	.2093	.0729
	s.e.	.0214	.1264	.1386	.1561		
	beta	.3894***	−.0108	.2649**	−.0317		
Grades in History	B	.0673	−.0764	.6174	.0944	.1644	.1160
	s.e.	.0247	.1459	.1600	.1801		
	beta	.2557**	−.0496	.23431***	.0470		
Grades in Mathematics	B	.0702	−.0924	.3971	.0661	.1216	.0578
	s.e.	.0235	.1387	.1521	.1713		
	beta	.2877**	−.0647	.2380*	.0355		

* $p \leq .05$, two-tailed.
** $p \leq .01$, two-tailed.
*** $p \leq .001$, two-tailed.

Figure 4.3 DiMaggio's Model of the Predictive Power of Cultural Capital

Source: Table 5, DiMaggio 1982, p. 197.

DiMaggio's models are, on the other hand, very much a reflection of a post-cultural turn sensibility. The main innovation here was borrowed from Pierre Bourdieu, the notion of treating culture as a resource that was concrete, fungible, measurable, that operated in tangible ways, and could be used in models to explain respectable amounts of variance in data sets just like other types of status attainment factors studied in the Wisconsin tradition of data analysis. By featuring cultural capital prominently as an independent variable, highlighting a case where culture clearly explains things, this essay was marked as one of the early successes of the new (American) cultural sociology project, an early expression of what we now call "the cultural turn."[41] But, all of this work is still in a sense very much within the frame of Kroeber's approach to modeling culture in the sense that the core meanings of these cultural phenomena are not the object of investigation. Not meanings, understandings, the content of talk, nor the way of knowing what is known are

being modeled in these projects. Instead, culture is modeled as objects that are thought to be reflections, effects, or markers of that which is known, understood, or experienced. Thus the explanatory project is concerned with the effects of culture, not its meaning. In the sense we have outlined here, these are non-hermeneutic styles of modeling culture. This does not mean, by the way, that DiMaggio is unconcerned or uninterested in these matters. On the contrary, his work has always highlighted the power of culture (understood as systems of meanings) to influence the social. The question instead has to do with a strategy for analysis—what is the best way to model culture and its effects?[42]

Cell D. Post-Cultural Turn/Hermeneutic: White

More recently a different group of formal modelers have come on the scene who also begin from a post-cultural turn theoretical sensibility but who differ in that they focus very specifically on modeling meaning, which is to say, they are working to develop tools (e.g., styles of formal data analysis) that can gain a better purchase on the interpretative qualities of discourse systems. Their goal, at some level and in some fashion, is to use data analysis to offer a kind of reading of a cultural text or, if not that, than at least to devise models that can serve as effective tools to reveal hitherto unseen features of discourse systems that can usefully contribute to a more effective reading of a cultural text. In other words, like Lévi-Strauss, these scholars focus on using formal models to enhance their capacity to read and interpret systems of cultural meanings. Also like Lévi-Strauss, many of the scholars in this group are interested in the development of formalist styles of relational structuralism as a way into the problem of measuring and interpreting culture. They are, to borrow a phrase from Alexander and Smith (2001), *structural hermeneuticists* because they apply structuralist methodologies in the pursuit of hermeneutic goals. In contrast to Lévi-Strauss, however, this new generation is not bound to the same kind of pre-cultural turn sensibilities (such as the emphasis on functional theories of cognition) that had plagued Lévi-Strauss. A more salient precursor here might be another anthropologist, Clifford Geertz (1973), who called for a semiotic theory of culture, by which he meant an approach to cultural analysis where meanings are foundational to experiences, are themselves structured like a language, and are articulated into systems of lived experience and practice.[43]

Harrison White is our exemplar figure for this cell. In the main this is because White's current intellectual project, starting most clearly with the publication of the first edition of *Identity and Control* (1992), has been dedicated to a systematic rethinking of the sociology of agency, culture, and institutional analysis from a perspective that highlights the use of social network theories, methodologies, and sensibilities. To this task, White brings a long and distinguished career of scholarly work concerned with the development of different variants of structuralist theorizing. Indeed, by the time the first edition of *Identity and Control* was published, White had already been (for some thirty years) one of the leading figures in the social scientific program of social network analysis.[44]

substitute "several decades"

In the early 1960s, after reading Lévi-Strauss and becoming fascinated by the social and mathematical problems raised in structuralist analysis, White began developing and extending the theory and method for applying relational mathematics to the study of the social organization of groups. This was a style of formal analysis that emphasized not persons (or objects) but relations (or ties); not the essential features of things, but the configurational patterns of relations that constitute the core logic of social structures. And while DiMaggio (forthcoming) argues that network analysis is, in many ways, the perfect style of data analysis for the formal study of culture, in point of fact, most network analysts were, at least at the beginning, largely disinclined to pursue questions of culture, especially a hermeneutically oriented analysis of culture.

Traditionally known as one of the most "geeky" specialties in sociology (because it was one of the places where mathematical theory and sociology melded in productive ways), network analysis was also, for many years, a bastion for a certain kind of materialist fundamentalism. This perspective treated the measurement of objective relations (network ties) as a more effective analytic strategy than anything that could be gleaned from listening to what was said in subjective accounts of individuals who were living inside highly constraining social network structures. Such data was regarded with suspicion because, when asked, it was presumed that these individuals would willingly offer, *retrospectively*, rational accounts of actions that may well have been undertaken for largely tacit, implicit, or otherwise unknown reasons (or so went one account). In other words, up until the time that White himself led what can be described as a cultural turn in the social scientific subfield of network analysis, the tradition had all too often been associated with an attitude that saw cultural analysis as being more or less antithetical to the development of useful models of social life.[45] White broke sharply with his more objectivist colleagues when he began to develop his broader theory of action and control (1992, 2008). The epistemic character of this break shows all the more reason why White's own turn toward culture was significant (and why we see him as the exemplar figure for this cell). Not only did White put matters of interpretation on the agenda for social network analysis more broadly, he also, over the years, encouraged his students (who have themselves gone on to be leaders in both of these sub-fields) to pursue projects that grappled in fundamental ways with theoretical questions that often focused on matters of culture.[46]

In his new project, White has been working to develop something like a generalized phenomenology of network life that emphasizes how the experience of being an agent seeking forms of control in a network world is constituted through a series of other types of relational systems, systems of talk organized into conversations, groups of regularly interacting others who share common systems of discourse, common discourses that materialize into institutional systems of rhetoric, and so on.[47] Moreover, White has made his intentions in this regard quite clear. Culture and the localized sense of meaningfulness (and within that, the relational systems of meanings that make culture and meanings operate) are profoundly foundational (indeed constitutive) of the lived experience of social agents as they maneuver their

way across interlocking network spaces. Thus the more subjective side of network life—the meanings, narratives, conversations, styles of talk, genres of understanding, and logics of cultural fields more broadly—are themselves the proper object of formal modeling, or, as White puts it, "Interpretative approaches are central to achieving a next level of adequacy in social data…" (White 1997, pp. 57–58).

Following White's lead, a number of social network scholars took up the interpretative study of culture and started modeling meanings. This group of "hermeneutic structuralists" have deployed two key principles. First, following in the path of semiotics and discourse theory, cultural meanings are understood to consist of relational systems within which sign elements are linked together in networks of similarity and difference (Mohr 1998). So, for example, Gibson (2003) models conversations as social networks; Bearman and Stovell (2000) use network models to analyze the narrative structures buried in the life stories of Germans living inside Nazi Germany; while Smith (2007) uses similar procedures to compare and contrast models of the same historical narratives seen from two different perspectives, two ethnic communities living alongside the Yugoslovian/Italian border. Ruef (1999) used text data to map the discursive logic of the health care industry, and Rawlings and Bourgeois (2004) do the same for higher education (to name but a few examples).

The second principle describes the notion of an ordered duality, according to which two discrete institutional subdomains are shown to be connected in such a way that they can be modeled as uniquely ordered structural logics that are linked through an articulation of co-constitution. Mohr and Duquenne (1997) use Galois' lattices to interpret the dual institutional logic of Progressive Era poverty categories and their corresponding relief practices. Mische and Pattison (2000; Mische 2007) use lattices to model the dualities that link political ideologies with the organizational histories of Brazilian youth activists. Reviews of popular music are analyzed by van Venrooij (2009a, 2009b) who uses correspondence analysis to model the co-constitution of musical genre categories (defined with reference to the embeddedness of each album within the field of aesthetic discourse) with the social organizational logic of the music industry itself (major or independent producers, white or black artists, men or women). Breiger (2000) generalizes about these types of projects, fashioning what he calls a methodological toolkit for practice theory, which he then applies to data in order to model the dually ordered logic of power and precedent within U.S. Supreme Court decisions. Mohr and White (2008) theorize these types of dually ordered relations in the context of a general logic of institutions that seeks to link "together different orders and realms of social life, notably the agentic with the structural, the symbolic with the material, and the micro with the meso and the macro structures of social organization" (p. 485).

To give an example of the kind of culture model that is being developed under this analytic framework, we have reproduced a figure from one of our own articles, "Modeling Foucault: Dualities of Power in Institutional Fields" by Mohr and Neely (2009) (see Figure 4.4.). This image was created from a block model analysis

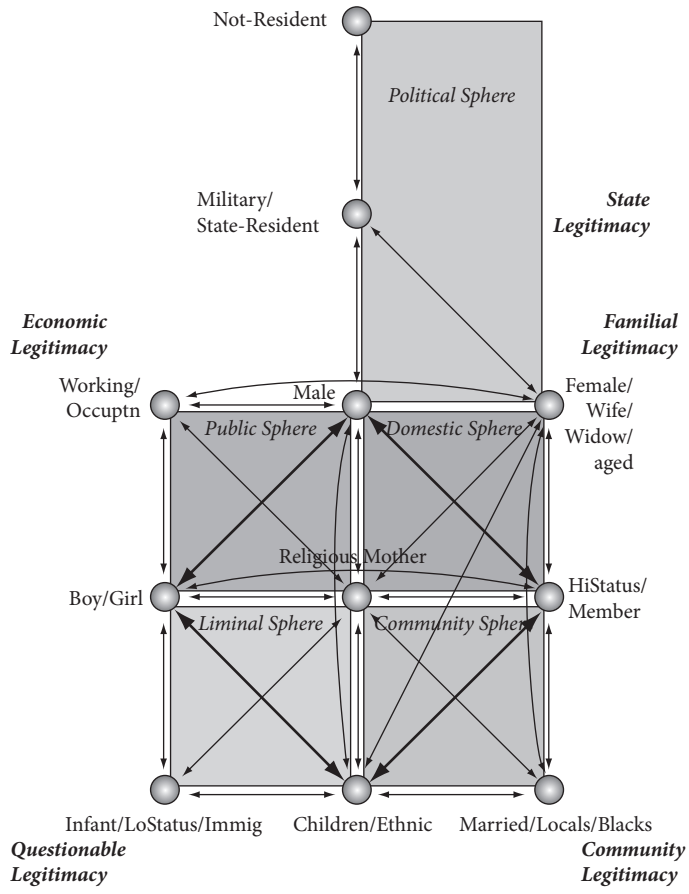

Figure 4.4 Mohr and Neely's Model of the Hermeneutic Structures of Poverty Discourse

Source: Copied from Figure 4, Mohr and Neely 2009, p. 229.

of relational ties within a textual database collected from the 1888 edition of the New York City Charity Directory.[48] Information was recorded from the 168 identifiably custodial institutions listed in that year's directory (this included all the orphanages, lunatic asylums, men's and women's prisons, reform colonies, poor houses, industrial schools, homes for fallen girls, and so on, that were located geographically within the Borough of Manhattan in New York City). We collected the full texts of all statements that were made regarding the identities of people that were (or at least claimed to be) included for treatment (as was reported by each custodial institution in its self-description for the Directory). In the paper, models are used to identify four different types of classification systems, each grounded in a different logic of power as suggested by Foucault's writings. In Figure 4.4 our focus was on systems of power as subjection that were deployed in the meta-institutional domain of discourse (in contrast to systems of power that involve practices and technologies and those that operate at the level of the institutional field).[49]

insert "were"

Block models were originally developed by Harrison White and his students in the 1970s as a data reduction technique that was intended to identify and map role structures in social networks. The procedure collects together those individuals in a network that are "structurally equivalent" to one another by virtue of their standing in similar patterns of relations to all others in the network (White, Boorman, and Breiger 1976; Boorman and White 1976). In Figure 4.4, this method of analysis was applied to a dataset consisting of texts—statements taken from the discourse system regarding identity classifications that were in use (and intelligible) in this institutional field. Specifically we looked to discover which identity terms were structurally equivalent to which others with respect to their patterns of use. So, for example, the category of being *female*, a *wife, widowed*, and *elderly* are ~~shown to be~~ structurally equivalent to one another, which is to say, they occupy the same role position with respect to the deeper system of institutional (discursive) meanings operating in this segment of the field. Said differently, according to the assumptions of this model, we can say that these identity terms are (institutionally speaking) synonymous—they *mean* the same thing.

The second virtue of a blockmodel analysis is that it provides a mapping of the overall relational logic of the identity categories because the models show which terms overlap with which others with respect to their usage profiles (in Figure 4.4 this is represented by the links connecting points). Following on the basic assumptions of semiotic theory, we ~~can presume that the meaning~~ (at the level of discourse) of any term (any role cluster of terms in this case) is defined relationally by its location vis-à-vis the other significant terms in the relevant code system. Thus, from this higher-order structural mapping we can perhaps obtain some understanding of the "sense" of the system and of how ideas and meanings within the field of custodial organizations go together in a culturally (or institutionally) intuitive way.[50] From Figure 4.4 we find the traces of a fairly coherent interlocking structure of discursive spheres, a core structure made up of four sets of complementary systems of legitimacy—economic, familial, community, and a sphere that we label as questionable (or liminal) legitimacy. Above this is a fifth sphere of state-level legitimation. Each of these spheres captures a specific region of institutional meaning that provides sets of common understandings about the kinds of categories of people who are appropriate for the very different forms and styles of incarceration that were in operation in New York City at that time.

To complicate matters further, we find superimposed on this structure what looks to be the vestiges of another more traditional cultural logic. This subgraph is defined by four vertices—Male, Children, Boy/Girl, and HiStatus/Member (which is marked in Figure 4.4 with **bold** lines). In the paper, we argued that this discursive substructure closely resembles the kinds of structural patterns that characterize the logic of kinship systems (concerned with the exchange of marriage partners) that Lévi-Strauss (1949) and other anthropologists have described. This structure revolves around the mother (at the hub), and involves a linking of males to

high-status others (who nonetheless share membership in an identity community), which is in turn linked to children, and then, to boys and girls, who are again linked back to the masculine head of household. Perhaps, at the meta-institutional level at least, the institutional logic (or discursive formation) that anchors the discourse ~~system on identity~~ within the custodial system is defined by a combination of different rhetorics of legitimation (and illegitimation) that are wound around one another in historically specific tangles.

substitute "identity system"

del

substitute "field"

Our claim for a model such as this is that it helps make sense of what is going on in the text because it provides a vision of a deeper structural logic underlying and giving meaning to the collection of individual statements. This parallels what Foucault (1972) claimed to be the function of a discursive formation, which he defined as the "historically produced, loosely structured combinations of concerns, concepts, themes, and types of statements" which serve to give meaning to the basic units or elements of discourse (which were the statements or "*énoncé*"). Any given statement, according to Foucault, necessarily presumes a great deal that is above and beyond any purely linguistic meanings, and, thus, it is only through the grounding of a particular discursive formation that a bundle of statements can be made intelligible. In Figure 4.4, the statements (in Foucault's sense) are the various organizational claims about the identities of people that are institutionalized within each organization. These linguistic meanings themselves are never completely lost to the analysis. Rather, the blockmodel works by reducing the complexity of the relational system within the text to something that is easier to read in two specific ways. First, the terms are distributed (through the analysis) into discrete groups of structurally equivalent terms. These groupings can thus provide a sense of what part of the meaning of a given concept (understood now in the discursive sense) is possibly most salient for proper reading of the institutional system. Second, the graph itself helps to provide an interpretation of the discursive formation by showing the arrangement of parts, and, thus, which segments of the discourse structure align with which other segments, thereby providing clues as to what kinds of factors could be in play or what dimensions would be most salient if we are now faced with the task of making sense of the text represented as a formal model.

Thus, in this last style of work, formal models of culture are used to highlight the ways in which cultural systems are organized as patterns that interlink alternative relational bundles of meaning. These models are intended to aid our ability to interpret the meaningful character of cultural systems and to assist in what Ricoeur calls "the genius of guessing" (1971, p. 212). But, unlike Lévi-Strauss, these investigations are not geared toward the ultimate identification of essential properties of things (or generalized universal rules) that constitute (by an arché or a telos) the true nature of cultural forms. Instead these models are focused on understanding some of the various ways in which cultural forms co-constitute domains of the social (and material) world in an ongoing and complex process of social construction, destruction, and re-construction.

substitute "essential"

The Formal Model of a Text: Meaningful Action Considered as a Dataset

Earlier we invoked the work of Paul Ricoeur as a way to define the concept of hermeneutic interpretation—"the rules required for the interpretation of the written documents of our culture" (1971, p. 197). In this last section of the chapter, we want to look more closely at the connection between Ricoeur's (1971) ideas about the use of hermeneutic methodologies in the social sciences and our own ideas about the use of formal measurement models for analyzing culture.

First, for Ricouer it is important to distinguish the study of discourse from the study of language. Discourse, that which is said, is an action in the world. Language, in contrast, is an abstraction. "(I)t is as discourse that language is either spoken or written" (p. 197). Ricoeur proceeds to identify four critical distinctions that flow from this. (1) Temporality: "Discourse is always realized temporally and in the present whereas the language system is virtual and outside of time"; (2) Subjectivity: "Whereas language lacks a subject in the sense that the question 'who is speaking?' does not apply at its level, discourse refers back to its speaker by means of a complex set of indicators such as personal pronouns"; (3) Symbolisation: "Whereas the signs in language only refer to other signs within the same system, and whereas language therefore lacks a world just as it lacks temporality and subjectivity, discourse is always about something. It refers to a world which it claims to describe, to express, or to represent. It is in discourse that the symbolic function of language is actualized"; and (4) Otherness: "Whereas language is only the condition for communication for which it provides the codes, it is in discourse that all messages are exchanged. In this sense, discourse alone has not only a world but another, another person, an interlocutor to whom it is addressed" (p. 202).

But it is not only a focus on discourse that is important. In particular Ricoeur wants to pay attention to discourse that is written, that is, to texts themselves. There are several reasons for this. As Ricoeur explains, texts contain a complex layering of meanings that spoken discourse lacks. To highlight this, Ricoeur compares written text to spoken speech in the same way that he contrasts language and discourse. (1) Temporally, speech is "a fleeting event" (p. 198). A written text, on the other hand, *fixes* the event by inscribing it in material form.[51] In the process, the meaning of the speech event expands beyond its original intent, or, as Ricoeur puts it, "What we inscribe is the *noema* of the speaking. It is the meaning of the speech event not the event as event" (p. 199). (2) Regarding subjectivity, with spoken language "the subjective intention of the speaking subject and the meaning of the discourse overlap each other in such a way that it is the same thing to understand what the speaker means and what his discourse means. . . . With written discourse, the author's intention and the meaning of the text cease to coincide" (p. 200). And thus, ". . . the text's career escapes the finite horizon lived by the author. What the text says now matters more than what the author meant to say, and every exegesis unfolds its procedures within the circumference of a meaning that has broken its moorings to the

psychology of its author" (p. 201).[52] Similarly with respect to (3), the symbolic function of speech, "in spoken discourse this means that what the dialogue ultimately refers to is the situation and to the interlocutors. This situation in a way surrounds the dialogue, and its landmarks can all be shown by a gesture, or by pointing a finger, or designated in an ostensive manner by the discourse itself through the oblique reference of those other indicators which are the demonstratives, the adverbs of time and place, and the tense of the verb." This is in contrast to the written text that is freed from the ostensive limits of any particular situation and in fact opens up for us symbolic worlds that transcend the immediacy of the present. It is writing "which frees us from the visibility and limitation of situations by opening up the world for us, that is, new dimensions of our being-in-the-world . . . only writing, in freeing itself, not only from its author, but from the narrowness of the dialogical situation, reveals this destination of discourse as projecting a world" (p. 202).[53] And finally, as Ricoeur says, (4), "It is perhaps with the fourth trait that the accomplishment of discourse and writing is most exemplary. Only discourse, not language, is addressed to someone. This is the foundation of communication. But it is one thing for discourse to be addressed to an interlocutor equally present to the discourse situation, and another to be addressed, as is the case in virtually every piece of writing, to whoever knows how to read. The narrowness of the dialogical relation explodes" (p. 202).[54]

Pointing to these differences Ricoeur explains that these qualities make the text (and the reading of texts) an especially appropriate model for the social sciences more generally. Indeed, he proposes to replace Weber's description of the object of the human sciences as the study of "meaningfully oriented behavior" with the study instead of the "readability-characters" that define behavior. "My claim is that action itself, action as meaningful, may become an object of science without losing its character of meaningfulness, through a kind of objectification similar to the fixation which occurs in writing" (p. 203). Thus, Ricoeur suggests that this fixation of meaning that characterizes written texts (the escape from the immediacy of the moment, the de-coupling from the mental intention of the author, the movement beyond the ostensive situation, and the change to a universal form of address) leads to a kind of shift in levels. The text defines an essential externalization of the speech act, a transformation into a form of discourse that depends upon an objectified embodiment of meaning in the material world. "This objectification is made possible by some inner traits of the action which are similar to the structure of the speech-act and which make doing a kind of utterance" (p. 204).

Ricoeur proceeds to explain how speech acts provide a model for human action more generally, and he finishes by showing how the methodologies that are appropriate for analyzing texts would be equally appropriate to the task of analyzing other forms of social action. This implies a specifically hermeneutic method where the goal is not to know an essence or an originary, but rather to gain an understanding of the text in its various shared embodiments and intentionalities and to recognize how it is that this very layering of understandings in fact constitutes the text as a meaningful entity.

According to Ricoeur, one important reason to use the model of hemerneutic interpretation is that it "provides a solution for the methodological paradox of the human sciences"(p. 209) by highlighting "the dialectical character of the relation between *erklären* and *verstehen* as it is displayed in reading." In other words, precisely because the text is caught somewhere between the author and the reader—"(t)o understand a text is not to rejoin the author"(p. 210)—that an interpretation of a text demands both forms of understanding be brought to bear, both explanation and comprehension. "There is a problem of interpretation not so much because of the incommunicability of the psychic experience of the author but because of the very nature of the verbal intention of the text. This intention is something other than the sum of the individual meanings of the individual sentences.... This plurivocity is typical of the text considered as a whole, open to several readings and to several constructions" (p. 212). And yet in spite of this plurivocity (and indeed, precisely because of it), texts are amenable to a style of formal analysis, "and the validation of an interpretation applied to it may be said, with complete legitimacy, to give a scientific knowledge of the text" (p. 212). Moreover, when considering Lévi-Strauss' contribution to the modeling of myths, Ricoeur asks whether it is not after all that the "function of structural analysis is to lead from a surface semantics, that of the narrated myth, to a depth semantics, that of the boundary situations which constitute the ultimate 'referent' of the myth?" (p. 217). Thus, Ricoeur concludes that structural analysis (of the sort described in the examples provided here) is "a stage—and a necessary one—between a naïve interpretation and a depth interpretation ... then it would be possible to locate explanation and understanding at two different stages of a unique *hermeneutical arc*" (p. 218). For Ricoeur, the two modes of knowing in the social sciences are necessarily linked. "Guess and validation are in a sense circularly related as subjective and objective approaches to the text" (pp. 212–213).

This argument by Ricoeur for substituting the model of the text for more traditional theories of social science,inspired us to wonder if we could extend Ricoeur's arguments to also address the questions we have raised here regarding the use of formal models for interpreting texts. Just as the text, in contrast to spoken speech, produces a discursive action that operates at a different level of temporality, subjectivity, symbolization, and otherness, so too we think that an analysis of formal measurements of the text, in contrast to the the text itself, produces a style of knowing that operates at a different level of temporality, subjectivity, symbolization, and otherness and that understanding this difference may help us to better appreciate the problems associated with using formal measurement models to analyze culture.[55] Thus, with respect to (1) temporalization, a formal model of a text allows for the focused deconstruction and reconstruction of temporal ordering in a manner that is fundamentally different than a traditional textual analysis because textuality (in both the telling and the hearing) is invariably linked to specific genres of narrativity (think again of Lévi-Strauss's distinction between the reading and understanding of a myth). In contrast, data analysis may operate with equal facility both synchronically and diachronically (Bearman and Stovell 2000), both forward and backward,

across very small or extremely large expanses of time (Moretti 2005). Thus when viewed through the lens of a dataset, the temporality of a text can itself can be incorporated into the analysis in such a way that it is theorizable, measurable, and, subject to a kind of material externalization that gives us access to other ways of understanding.

In the case of (2) subjectivity, if the written text is distinctive from spoken discourse because it breaks the link between the author's intent and the meaning of the text, a similar sort of rupture occurs in the analysis of the text as data, a break opens up between the intention of the modeler and the meaning of the data.[56] Like textual discourse that goes out from the mind of the speaker and assumes material form, so too, a dataset, once it has been collected, has its own embodiment, its own dead letter of information, and the modeler is forever committed to living with its otherness.[57] This is not to say that a modeler has to take the first attempt of an analysis as final, or, for that matter, that one need hold on to one's original plans, hypotheses, or presumptions. But, once one commits oneself to a measurement and a dataset, then one can contextualize one's findings, enhance the imagery, and locate the findings with respect to a literature, but one cannot change the information contained in the data to suit one's preferences or expectations. Thus in a fashion that is in some ways analogous to the externalization (indeed, the alienation) experienced by the author of a text, a text modeler is both the master of and mastered by one's data.[58]

Another parallel occurs at the level of (3) symbolization, which in the case of the written text claims a freedom from the "ostensive limits of any particular situation" or a particular speech act and thus becomes capable of expressing a broader symbolic world. Data analysis implies a similar sort of leveling-up to a broader social world in that it at least potentially contains information from a higher level of sociality, a sample of texts, a distribution of authors, and a collection of writings from across a field. Thus it allows for a kind of aggregation of acts of meaning in the same way that institutionalists from Durkheim forward have been seeking to understand social order. Said differently, a formal model of culture has the capacity to open up a world that is institutional in character: It can take in data from across a broad social space as information.[59] This is not to say that unless you use a formal model you cannot make comparisons between texts, for what else is a hermeneutic analysis if it is not about an opening up to a multiplicity of texts? Rather our point is that perhaps in this moment, the level that separates the reading of the text from the reading of the text as a dataset begin to approach one another at a kind of articulation point whose structure we are seeking to explicate.

Finally, at the level of (4) otherness, recall that the written text is distinct from spoken speech because only in the text is the audience made general, as a product of the text itself. The same could be said of a data analysis which is not only about an opening up to a generalized discursive other. It is also about making the project of knowing that what is shared by and co-constructed by a community of guild members, craftspeople, and professional scholars, who, in the case of a data analysis, share not only in a conversation but also in an awareness, and critical appreciation, of all the other materialities (as described above) that characterize the analysis of

the text as a dataset. In short, we agree with Ricoeur's contenton that the model of the text is a good way conceptualize the dilemmas of the human sciences more generally, and, as we have suggested here, it is also a good framework for thinking about the problem of how to formally model a text.

Conclusion

In this essay, we have discussed some ways that formal measurement models have been used to study culture. We began by defining these models as the product of data analysis, which we described as a social technology, hence a set of theories and practices, methods of knowing and observing, that are enacted by communities of practitioners who construct regimes of analytic practice through their participation in them. We suggested four knowledge functions that formal models can usefully perform, and we argued that the old debates over method had grown stale at least in part because the terrain had shifted, as both quantitative and qualitative practitioners have increasingly found themselves working the same space, looking for meanings that are locked into texts. This has led to a series of new debates about the nature of the hermeneutic method itself.[60] We then described four types of formal modeling strategies that have been applied in the study of culture, before and after the cultural turn, with hermeneutic goals or not. This gave us the four cells in which to situate the work of Kroeber, Lévi-Strauss, DiMaggio, and White as exemplars of different styles of culture modeling. We finished with a comparison of Ricoeur's ideas about written (as supposed to spoken) discourse to our own ideas about the use of formal models in measuring cultural texts.

substitute "opposed"

There are certain obvious limitations to what we have proposed. Probably the most important of these is that by confining ourselves to the study of written texts as both a practice and an exemplar for exegesis, we have foreclosed on the possibility of learning about the process by which meaning is actively produced, the way that speaking is located in a lived, dialogic process of agents and interactions and ways of making sense. We are thus responsible for promoting what Calhoun and Sennett (2007, p. 5) would describe as "a study of paintings not painting, of values not valuing." In response, we would argue that an overvaluing of the text (a transgression of which we are surely guilty) ultimately says more about the incompleteness of the present project than it does about the fundamental nature of the problem itself (e.g., the relation between formal measurement and interpretation). There are any number of formal modelers studying culture who are far more concerned with analyzing the agentic moment than we have given voice to here. Indeed, Harrison White himself is currently much more interested in agency and action and far less focused on textuality and institution than our account of his work would suggest (e.g., White 2008; Godart and White 2010). For that matter, our own focus on the institutional moment (here and elsewhere) has always reflected more of a strategy for developing

substitute "(as opposed to qualitative) styles of understanding"

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a useful approach to data analysis than it is a commitment to a static vision of the social. This then is precisely the type of disadvantage one incurs in this style of work—an example of the type of reductionism that one must sometimes embrace—but one does so in the hope that the costs are outweighed by the benefits (and that one's analytic advance will lead to further improvements and less reductionism).

The problem is not just under-development, however. There are good strategic reasons to study structure. If, we see the social world as fundamentally ordered along the duality of agency and structure, then this will tend to produce a corresponding duality of methods in the human sciences. Giddens (1979) theorizes this quite usefully; he suggests that we distinguish the sociology of "strategic conduct" (painting) from the sociology of "institutional analysis" (paintings), and he proposes that each of these projects necessarily depends on a kind of a bracketing (he says a methodological epoché) of the other. While it is true enough that it would be wrong to understand culture only from the perspective of written text (rather than spoken speech), it would be equally wrong to do the reverse. This is not only because the dialectic between speech and language is an important part of the whole that is lived experience, but, in equal measure, we would say (along with Ricoeur) that a scientific approach is as an important part of the whole that makes up the methodology of the human sciences.

We do not, however, mean to suggest, that a formalized mode of text analysis can or should displace traditional hermeneutics. Though it has not been the focus of our chapter, we think that the various modes of knowing each have their own unique costs and benefits.[61] Nor do we presume that hermeneutic scholars are not themselves similarly beholden to their own styles of obligatory practices, their own sets of craft skills, guild knowledge, caches of collective wisdom, and the like. But we do think that what is perhaps most distinctive about interpretively intentioned measurement models of culture is how a particular kind of social technology can be built, shaped, and wielded to good effect. When this happens, the nature of the text (its discursive register) and the style of interpretative practice that can help us read the meaning of that text is transformed in a fashion that (as we have argued here) is in some ways rather like the difference between spoken and written discourse that Ricouer has described. Recall that ultimately Ricoeur's ambition was to offer "a fresh approach to the question of the relation between *erklären* (explanation) and *verstehen* (understanding, comprehension) in the human sciences" (Ricouer 1971, p. 209). This has been our goal as well.

substitute "so on"

NOTES

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We ~~would like to~~ thank Jeff Alexander for encouraging us to "do something daring and original" and for working supportively with us throughout the various stages of this process. Thanks also to Ron Breiger, Clayton Childress, Paul DiMaggio, Elvin Hatch and Corinne Kirchner for giving one of our (several) drafts a close reading and for offering

valuable and substantive suggestions (not all of which we have been able to incorporate or address). Special thanks also to John Hall whose advice and thoughtful commentary on early versions of the paper were critical to the development of our argument. We also thank (especially) Roger Friedland, Josep A. Rodriguez, Kess van Rees, and Marc Ventresca for many enlightening conversations on the topics covered in this essay.

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3. From an essay that Stevens contributed to a volume of papers initially presented at a 1956 symposium on "Measurement" sponsored by the American Association for the Advancement of Science (1959, p. 18). Stevens was the Harvard psychologist who invented the canonical measurement scales (nominal, ordinal, interval, ratio). He was one of a cohort of practitioner/theorists who developed many of the conventions for formal analysis in the modern social sciences. He was also, like many in this cohort, very reflexive about the process. As he wrote some years earlier, "The stature of a science is commonly measured by the degree to which it makes use of mathematics. Yet mathematics is not itself a science, in any empirical sense, but a formal, logical, symbolic system—a game of signs and rules" (Stevens, 1951, p. 1). We say more about these issues below.

4. This definition actually allows for a broad range of methodological endeavors, but it does restrict us to talking about projects where a set of data is analyzed for patterns and reduced by some formal means. This would include some more formalist styles of what has traditionally been called qualitative work and it would also exclude some types of quantitative scholarship that is more purely metaphoric or imaginary in style (see, e.g., Macy and Willer, 2002). This is an admittedly unusual specification since we are insisting on the practice of data collection as a criterion for inclusion (whereas, even among quantitative scholars, common usage would, on the contrary, think of models as abstractions from the real). Indeed, as Elvin Hatch (personal communication) reminds us, there many formal modeling traditions in the social sciences that do not rely at all on the kinds of quantitative measurement practices that we are emphasizing here. But we employ this definition advisedly, as a way to delimit the range of claims we want to defend in the more philosophical sections of this essay.

insert "are"

substitute "specify"

5. This does not mean we think the matter is simple or unproblematic. On the contrary, we see a theory of culture as engaging a far more complex debate than what we are prepared to deal with here. Our interest is more limited, by focusing our taxonomy on variations in method, we are in fact hoping to learn something new about the way that culture has come to take on theoretical meaning in the social sciences.

6. Let us be clear, our goal is not to review the literature on measuring cultural meaning (which would be a very large task indeed) but rather to set out in an admittedly idiosyncratic fashion a set of ideal-types which are intended to illustrate some of the broader processes that are in play.

7. Science studies scholars have helped us see scientific worlds (including social scientific worlds) as normal places where people interact in normal ways, even as they work at constructing extraordinary communities, technologies, and insights. These studies have also sensitized us to the ways that science is also a distinctive endeavor located in a specific social space; it is constituted in structures of articulation connecting things, actions, and logics (e.g., Latour and Woolgar 1979; Latour 1987; Knorr-Cetina 1999).

8. There are, of course, different ways to count. There has been a flood of important work on the institutional construction of data and statistics over the last few decades, works like Hacking (1975), Porter (1986), Ventresca (1995), and Platt (1996).

9. We draw here on the work of a number of scholars who have eloquently helped to theorize the character of data analysis in the social sciences as a particular kind of socially constructed collective endeavor. See, for example, Cicourel (1964), Duncan (1984), Breiger (2000), and Bourdieu (2004). These should be compared to the writing on these topics that occurred in the decade or so after the Second World War. This was the time when, as a result of a number of technological developments (some the result of increased hybridization of scientific practices that occurred around the war effort), new conventions and techniques for collecting data and formalizing its analysis were being tinkered with and tried out by a cohort of theorist/practitioners from across the humanistic disciplines. Over the course of a decade or so, these scholars virtually invented much of what is now taken for granted (background assumptions) for the modeling practices of the modern social sciences. Lazarsfeld, for example, was central to the process of constructing a theory of data analysis (including grappling with key constructs such as "the variable") that still undergirds much of mainstream American sociology. Kurt Lewin is another who helped provide the foundations for the development of modern social network analysis. S. S. Stevens (whose quote we use at the beginning of this chapter) is another example ~~from psychology.~~

10. Most obviously in American social sciences, pattern reduction practices have largely relied on the corpus of traditional statistical models, especially those associated with linear modeling traditions that emerged in American social science departments most forcefully after the Second World War as the dominant measurement paradigm. See Abbott (1988) for a classic assessment and critique.

11. Bourdieu describes the nature of scientific fields thus, "The structure of the power relation that constitutes the field is defined by the structure of the distribution of the two kinds of capital…scientific capital: a capital of strictly scientific authority, and a capital of power over the scientific world which can be accumulated through channels that are not purely scientific (in particular, through the institutions it contains) and which is the bureaucratic principal of temporal powers over the scientific field such as those of ministers and ministries, deans and vice-chancellors or scientific administrators…" (2004, p. 57).

12. Friedland (2009), who argues for taking religion as the model for institutional analysis, provides a powerful theoretical grounding for such a conception.

13. Ernst Cassirer provides some of the earliest and most effective arguments concerning the virtues of formal analysis in the natural and social sciences (Mohr 2010). Cassirer writes, with formalization "the world of sensible things…is not so much reproduced as transformed and supplanted by an order of another sort" (1953, p. 14).

14. Qualitative methods do this same thing in different but similar ways. Our point is that these are modes of practice.

15. Hesse (2000) describes how models work in science more generally. Tufte (1983) is a classic example of how visualization of data allows the researcher to see and theorize differently.

16. Technology is a species of power and we can ask questions about how and on what object any power is exercised" (Jonas 2004, p. 24). Bourdieu has a useful description of what we think of as the material benefits of a formal analysis. He says, "The specificity of the scientific field is partly due to the fact that the quantity of accumulated history is especially great, owing in particular to the 'conservation' of its achievements in a particularly economical form, with for example organization into principles and formulae or in the form of a slowly accumulated stock of calibrated actions and routinized skills" (Bourdieu 2004, p. 35).

17. We develop this argument in more depth at the end of the chapter.

substitute decades immediately following"
Insert "Paul"
substitute "social sciences"
insert "social scientific"
del
insert "The psychologist"
substitute "a third"
substitute to begin the

18. Once again, knowledge production is also (obviously) distributed in other nonscientific academic fields but again the distribution mechanisms are different. So what we are describing here is something like a mode of knowledge production.

19. Abbott (2001) develops a theory of binary splitting (he describes in terms of fractals) to account for changes of this sort in academic fields.

20. The volume actually grew out of a NEH seminar directed by Robert Bellah at the University of California Berkeley in 1976–1977.

21. Here again, we have touched on a huge topic. Elsewhere, we have written on developments in the case of American sociology, see Friedland and Mohr (2004).

22. On the distinction between structural and systemic styles of theorizing culture, see John Hall (2004, p. 117).

23. Naturally, there are also exceptions—scholars who still carry a torch for an older intellectual paradigm (though in truth, much of this is generational, and, as in this case, change happens as older cohorts pass from the scene). On the other hand, the rise of new types of biologically determinant explanations (as in some contemporary versions of evolutionary psychology for example) count as counter-examples to this broader trend; they are anomalies from this perspective, or in the spirit of Abbott (2001) and Collins (1998), they are the (inevitable?) counterbalancing contenders for the intellectual attention space.

24. The work of scholars such as Podolny (1993) and Zuckerman (1999) were also critical here. Mohr and Guerra-Pearson (2010) provide a more detailed commentary on these developments.

25. Of course, there were also strong culturalist projects (emphasizing endogenous studies of culture) before the modern cultural turn. Most obviously, in anthropology, Boas and his students (e.g., Margaret Mead, Ruth Benedict, Edward Sapir, and Alfred Kroeber, who we discuss below) held cultural-centric perspectives. So did Lévi-Strauss (who we also discuss below). But, these were more often exceptions to the larger intellectual trends of the time and, as we show in the case of Kroeber and Lévi-Strauss, there was usually a tendency even in these works to link culture back to exogenous forces of one sort or another (see Hatch 1973). What is missing here is the deeper sense of how culture constitutes the social (and vice versa).

26. See, for example, the recent exchange between Richard Biernacki and John Evans published in Reed and Alexander (2009).

27. This is reminiscent of the cultural idealism of Matthew Arnold; see Williams (1983).

28. In 1919 Kroeber was pushing back against Spencerian theories of the "superorganic," but just what would take its place was still quite unclear. As he says in the introduction to his career summarizing text, *The Nature of Culture* (1952), "The risk in a high degree of consciousness of a separate order is that of going on to reify its organization and phenomena into an autonomous sort of substance with its own inner forces—life, mind, society, or culture. I have probably at times in the past skirted such lapsing and have at any rate been charged with mysticism. However, mysticism is by no means a necessary ingredient of level recognition. The value of the recognition is largely methodological. It is only by a *de facto* cultural approach to cultural phenomena that some of their most fundamental properties can be ascertained.…However, if one is going to be broadly theoretical or philosophic about culture, it seems to me that its acceptance as a distinctive order of phenomena in nature cannot be evaded" (p. 4).

29. Espeland and Stevens (1998) provide the classic discussion of the problem.

30. He first uses *Petit Courrier des Dames*, beginning in 1844 "for the reason that that was the first volume of a fashion journal which I happened to know to be accessible in

New York City" (p. 243). Because of a break in the availability of the data series, Kroeber switches to *Harper's Bazar* as of 1868.

31. In spite of these efforts, Kroeber still has a very hard time collecting his data. "Insufficiency of material or oversight has resulted in a few years being represented by only nine sets of measurements. Unfortunately also, there is a scarcely a year for which ten illustrations could be found in each of which all eight measurements were recordable. A gown may be shown very completely in full face except for one corner of the skirt, which is hidden behind the chair of a seated companion. The basal skirt width can often be pretty well guessed in such cases, and an estimate was generally made; but only actual measurements have been included in the averages discussed.... The consequence of all these little circumstances is that the majority of the eight features observed are represented, year by year, by less than 10 measurements, sometimes only by four or five" (p. 241).

32. ~~Interestingly,~~ Kroeber returns to this project 20 years later and publishes a second paper with Jane Richardson as the first author. They have added data, extending the series from 1787 to 1936 and significantly increased the mathematical sophistication.

33. At other moments, Kroeber is far less metaphysical about all of this and he leans toward explanations that emphasize mechanisms of social attention, suggesting that to be noticed as different in matters of style, you can't go back (in dress length, e,g.), you have to go forward. This "ratchet effect" in the sequencing of style behavior is picked up and developed more fully in Lieberson's (2000) efforts to model cultural forms by studying changes in first names. Lieberson moves the explanation away from culture as an abstraction and anchors it in an understanding about social cognitions.

34. Post cultural turn sociologists have been especially keen to develop new kinds of Boolean methods of data analysis that are extensions of this style of thinking (see Ragin 1987, 2000; Abbott 2004; Hannan, Pólos, and Carroll 2007).

35. Ricoeur argues that there is, in fact, a dialectical relation linking the two forms of understanding in the social sciences, explanation (*erklären*) is dialectically tied to interpretation (*verstehen*). To illustrate his contention, he points to this same analysis by Lévi-Strauss in order to make the argument that scientific understanding demands interpretation as its precondition. "First, even in the most formalised presentation of myths by Lévi-Strauss, the units which he calls 'mythemes' are still expressed as sentences which bear meaning and reference. Can anyone say that their meaning as such is neutralised when they enter into the 'bundle of relations' which alone is taken into account by the 'logic' of the myth? Even this bundle of relations, in its turn, must be written in the form of a sentence. Finally, the kind of language-game which the whole system of oppositions and combinations embodies, would lack any kind of significance if the oppositions themselves, which, according to Lévi-Strauss, the myth tends to mediate, were not meaningful oppositions concerning birth and death, blindness and lucidity, sexuality and truth. Besides these existential conflicts there would be no contradictions to overcome, no logical function of the myth as an attempt to solve these contradictions" (1971, p. 217).

36. In his writing on Nietzsche, Foucault describes this as "an attempt to capture the exact essence of things, their purest possibilities, and their carefully protected identities, because this search assumes the existence of immobile forms that precede the external world of accident and succession" (1977, p. 142).

37. In the following sections we focus exclusively on the American field of sociology. During this era other (national) intellectual communities worked through parallel disputes but a commentary on those histories is beyond the scope of this essay.

substitute "died"

insert "certainly"

substitute "has emphasized"

38. This is a very incomplete treatment of a very complex topic. Mohr and Rawlings (2010) provide a more detailed history of these developments. It must also be said that subjective measurement strategies (for example, those using survey data on the various types of socio-cognitive modeling projects that measure meaning in quasi-laboratory conditions) never did die out and, in fact, have only grown more refined and powerful over time. Our (admittedly narrow) focus in this essay is on a totally different, though in many ways parallel, theory of meaning and measurement.

39. In later work, DiMaggio and Mohr (1985) extend this work to show that this measure of cultural capital can also be used in models to predict students' overall educational attainment as well as the educational attainment of their spouse. Mohr and DiMaggio (1995) apply models to consider inter-generational transfers of cultural capital.

40. DiMaggio used the Project Talent dataset (from the early 1960s) that had a broad array of culture measures.

41. DiMaggio is also an important figure in the new institutional approach to organizational analysis that we described earlier. There are close parallels to the theorization and analyses of culture in both projects (not so surprising since he was a key theorist in both projects). In the new institutional school, the meaningfulness of cultural understandings is seen as primary. As in Peterson's work, institutionalists developed sophisticated models to study the social organization of industries and organizational fields. And like DiMaggio's cultural capital studies, institutionalists from the beginning employed formal modeling techniques to take the measure of cultural forms. It is another post-cultural turn project because new institutionalists conceive of their task as the mapping out of the causal effects of cultural processes in organizational fields. Originally, their approach to modeling was to use non-interpretative (explanatory) models of culture to illustrate, for example, the increasing homogenization of organizational forms within an organizational field. More recent work has shifted toward more hermeneutic styles of modeling meaning (see essays and commentary collected in Powell and DiMaggio 1991 and, for more recent work, Greenwood et al. 2008). Ventresca and Mohr (2004) provide a more full account.

42. Although originally a pioneer of non-hermeneutic cultural modeling, DiMaggio has always pushed the field forward and he has since gone on (especially in some more recent work) to develop new approaches to modeling hermeneutic processes (thus moving down to cell D). See, for example, DiMaggio, Goldberg, and Shepherd (2008).

43. Of course, Geertz (1973) was highly critical of Lévi-Strauss whose work comes in for specific critique in several chapters of this text. But notice that the critique does not address or even consider Lévi-Strauss's hermeneutic project, Geertz's rebuke focuses on Lévi-Strauss's insistence on a pre-cultural turn construct of culture as something that takes shape and form from something that is outside of it, and thus ontologically prior to discourse, which (again) in Lévi-Strauss's case was the primacy of a certain style of cognitive determinism.

44. Mullins (1973) identifies White as the leader of the social network analysis tradition and it is useful to know that White had personally mentored a large proportion of the leading figures in that field

45. This is an overstatement when one considers the long tradition of using formal models by anthropologists (D'Andrade, etc.) and others (psychologists such as Osgood, e.g.) to develop interpretative approaches to culture. There were also a number of important hermeneutically inclined precursors in the field of social network analysis itself. See Mohr (1998) for a more extensive review.

46. See Mullins (1973), Aazrian (2005), Fuhse (2009), Kirchner and Mohr (2010), and Mische (forthcoming) for more detailed histories.

delete "s"

47. The key source is still the first and now (in some ways, very different) second editions of *Identity and Control* (1991, 2008).

48. The original database includes information on all the organizations in the directory at different intervals of time. In this model, we only included information from one small part of that larger dataset.

49. Figure 4.4 only considers the subset of identity terms that are derived from a meta-institutional level, but in the paper, another set of institutional level identity terms is also modeled. So, for example, in this analysis it is general status designations (gender, race, class, age, etc. that are being modeled), whereas the institutional level identity categories include categories that are owned by professional communities within a given institutional field, in this case it includes categories such as the homeless, the neglected, the nervous, the unruly, the drunk, the fallen, and so on. In the paper, both identity matrices are analyzed separately and then again together.

insert "effectively"

50. It is important to emphasize that a fully informed understanding of the history and context of these events is a necessary prerequisite to conducting this style of research. Both for generating the dataset and for analyzing the dataset, the methods described here will not automatically produce new knowledge so much as they will help one to build on one's knowledgable understandings by allowing one to see and interpret a broader, more fully detailed mapping of these social institutions.

51. Ricoeur links this to Plato's Phaedo, "Writing was given to men to 'come to the rescue' of the 'weakness of discourse', a weakness which was that of the event. The gift of the *grammata*—of that 'external' thing, of those 'external marks', of that materialising alienation—was just that of a 'remedy' brought to our memory.…It replaced true reminiscence by material conservation, and real wisdom by the semblance of knowing" (1971, p. 199).

52. "Henceforth, only the meaning 'rescues' the meaning, without the contribution of the physical and psychological presence of the author. But to say that the meaning rescues the meaning is to say that only interpretation is the 'remedy' for the weakness of discourse which its author can no longer 'save'" (p. 201).

53. "For us, the world is the ensemble of references opened up by the texts" (p. 202).

54. "Instead of being addressed just to you, the second person, what is written is addressed to the audience that it creates itself. This, again, marks the spirituality of writing, the counterpart of its materiality and of the alienation which it imposes upon discourse" (p. 203).

55. Here again, Ricouer's ideas apply, "The hierarchy of the levels of language includes something more than a series of articulated systems: phonological, lexical, and syntactic. We actually change levels when we pass from the units of a language to the new unit constituted by the sentence or the utterance. This is no longer the unit of a language, but of speech or discourse. By changing the unit, one also changes the function, or rather, one passes from structure to function" (Ricoeur 1974, p. 86).

56. Breiger (2002) provides a number of compelling examples of this point.

57. Unless, of course, one concludes that the data are so problematic that they need to be modified in some allowably legitimized corrective fashion—removing outliers, weighting the sample, going back for supplementary data, etc. We are, after all, resilient in our acts as agents in a field.

58. Of course, news about ethical violations and the occasional outright fraud does occur in science, but it is nonetheless really more the exception that defines the rule. Scientific practitioners, as a lot, are deeply committed to the rule of data, to a genuine system of externalization, and thus to a style of life that is organized around a sense of professional ethics and craft skills.

59. Of course, there are other uses for data analysis beyond the study of the institution, but the analogy we think still holds because what is at issue is how a data analysis can be used to symbolize a broader logic that embodies a theory of the social world.

60. Mohr and Rawlings (2010) develop this argument in greater detail.

61. As Ricoeur says with respect to the dialectic between language and speech, "The triumph of the structural point of view is at the same time a triumph of the scientific enterprise. By constituting the linguistic object as an autonomous object, linguistics constitutes itself as a science. But at what cost? Each of the axioms we have listed is both a gain and a loss" (Ricoeur 1974, p. 83).

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