Symbolic

The development of methodological and philosophical discussion of sociology and anthropology paralleled and was closely connected to the creation and self-definition of these fields as academic disciplines. Among the basic strategies were American statistical sociology, which appealed to Karl Pearson’s positivism, Durkheimianism, which appealed to “collective” concepts, and neo-Kantian German systematic sociology. The disputes over Weber in particular were formative. Commentary by apriorists, ranging from Alfred Schutz, von Mises, Kelsen, and Winch, as well as appropriation by Popper and to the synthetic thinker Talcott Parsons, defined such key issues as normativity, the rationality of action, objectivity, and relativism.

Keywords: Max Weber, Emile Durkheim, Karl Pearson, Georg Simmel, normativity, Alfred Schutz

The beginning of the 20th century coincides with the establishment of the modern disciplines of the social sciences, chiefly in the United States but on a smaller scale in Western Europe as well. These disciplinary structures, which varied from country to country, provide the organizing principle of this handbook. The early 20th century history of methodological, and more broadly, philosophical, writing in these areas is inseparable from this discipline-building process. Parts of the rationale for the distribution of topics among these new “disciplines” had to do with methodological issues, notably the emergence in the most powerful of the new disciplines, history and economics, of methodological and theoretical orthodoxies which had the effect of excluding topics, on methodological or metatheoretical grounds, which had previously been important to them, and of inducing the scholars who had been concerned with these topics to seek alternative disciplinary homes and at the same time to construct methodological defenses and accounts of their own activities in the framework of these new homes. In this chapter my aim will be to identify and explicate the major alternative approaches to the problems of disciplinary identity and “method” broadly construed, and to indicate how each of them produced, and responded to, philosophical issues. I will confine the discussion to the period before 1945 for the most part. I will also, in line with the aims of this Handbook series, largely ignore the mid-19th century background to the disciplinary projects of this period, though Auguste Comte, J. S. Mill, and Herbert Spencer did provide, and formed part of the consciousness of the thinkers who seized the disciplinary moment. I will also leave the parallel story of anthropology, which is closely bound up with the “culture concept,” to others.

The immediate context of the disciplinarization of sociology was the transformation of two fields, statistics and history, which shed large chunks of content as they took their current shape. The principal body of thought that was excluded from the discipline of history, for example, was philosophy of history. The philosophy of history had been provided with an influential exposition in Robert Flint’s *Historical Philosophy in France and French Belgium and Switzerland* (1894), which a few years later would have been called social theory and have been part of sociology. Several founding figures of American sociology were from history, such as Albion Small, the central figure in the creation of the influential department of sociology at the University of Chicago and the editor of the leading sociology journal, *The American Journal of Sociology*, who had been trained as an historian at Johns Hopkins, the first American university to embrace the German model of graduate education. The history done at Hopkins during that period included work that was very much like what later came to be understood as sociology: a major project of the time was a study of cases of cooperative ventures. The principal product of Johns Hopkins’ social science during this period was a multivolume series of studies on cooperative production and profit sharing, which were important responses to the so-called “social question” of the period, the problem of the rapidly expanding working class. Franklin H. Giddings, the founding figure at Columbia, was an economist, who broke in with a study of profit-sharing and, as a journalist, had written on indexing inflation. The American literature on labor, labor statistics, and the problems that produced them, profit-sharing and cooperatives, had analogues elsewhere, and these were important to the circumstances in which the discipline of sociology was institutionalized. In the period of the “first globalization” of 1880-1905 parallel institutional structures developed, notably bureaus of labor statistics, which shared methods, and, along with censuses, shared computational techniques, notably the technique which led to modern data processing technology in the form of digital punchcards, Hollerith cards, whose first successful application was in competition with the colorchip mechanized counting technologies employed by the pioneering Massachusetts’s bureau of labor statistics (which published Giddings’s first study, under its director Carroll Wright).

Bureaus of labor statistics were official bodies in the states of the United States, eventually in the national government, and in England and Europe. They operated as a social analogy to the geological surveys that had been established in the same places in the previous half-century and, like these surveys, had the ability to publish bulletins. The bulletins contained not only the reports of statistical studies of labor but qualitative and opinion material on the cooperative movement. As in the geological surveys, much of the circulation took the form of exchanges with other survey bodies, so the bulletins provided a means of international communication. This body of official statisticians incorporated a methodological tradition, derived from the statistics movement of the 19th century that had been organized around international congresses, which in part was concerned with “the social question” but developed technically particularly in relation to the problem of suicide (Porter 1986).

Statistics in the older sense of these world congresses, and indeed of this tradition, was a substantive discipline rather than a branch of mathematics, and ordinarily the product was commentary on descriptive statistical reports, with occasional gestures toward the idea of underlying laws (cf. Mayo-Smith [1895]1910). Nevertheless, there was a strain in this tradition that was more ambitious, usually taking the form of assertions that statistical studies which
produced tables, typically of rates kind, led to, or pointed to, underlying laws, but failing to explain how they did so. Adolphe Quetelet, who had been one of the central figures in the congress movement of the 1840s and 50s, had developed a complex analogy between the idea of the *homme moyen*, the hypothetical individual who was the embodiment of the statistical mean, and the center of gravity of planets, whose perturbations he likened to the statistical variations in rates over time. And at the end of the 19th century Gabriel Tarde proposed a theory of imitation to account for the way in which statistical patterns changed by spreading from geographical points. Yet as a discipline this older form of statistics failed to make the transition to disciplinary status, either in Europe or in the United States. Although it continued longer in Germany in association with labor statistics, with Catholic social reform inquiry, and to some extent on its own as a social or labor adjunct to economic statistics in the German university, it was nevertheless kept apart from the new pure science of sociology as it was established in Germany which, as we shall see, had a different philosophical rationale, focused typically on a philosophical problem posed by Georg Simmel at the turn of the century: what is society?

When sociology as a topic was reassembled out of the bits that were not taken by economics and history, it incorporated a great deal of the social statistics tradition as well as the theory of history, which was rebaptized as a social theory, and in both instances, the incorporation involved methodological and metatheoretical regrounding, which, in the case of social theory in sociology, involved the construction of a new genealogy in which Auguste Comte, the originator of the term sociology, played a large role. The methodological ideas of the social statistics movement, which had been opposed to Comte and which Comte opposed, were also incorporated. This new disciplinary construction thus involved conflicting elements, and much of the methodological writing of the time, including the methodological writing of Émile Durkheim and Max Weber, as well as the less well-known sources of mainstream American sociology and its critics, was concerned with reconciling these conflicts. Both Durkheim and Weber, in different ways, were concerned both with statistics and with claims that amounted to a surrogate for universal history (cf. for Weber, Mommsen 1977: 1-21), as were virtually all the figures discussed in this chapter: Georg Simmel, Franklin H. Giddings, Charles Ellwood, and Talcott Parsons. The range of issues of course went far beyond these, especially the other issues of disciplinary relations, with biology, psychology, the normative, ordinary language, ethics, legal philosophy, liberalism and anti-liberalism, and economic theory to name but a few. But the fundamental conflict between science-like fact and the problem of large scale historical truth inherited by “social theory” reappears in many guises.

2. Durkheim and the Statistics Tradition

Durkheim provided the earliest and most coherent reconciliation of this fundamental tension, and it is striking how similar the lists of intellectual sources and problems actually were between these principal solutions to the common problem of constructing a “science” out of the materials and topics in question. Durkheim took over one of the best developed topics of the older social statistics tradition when he decided to deal with the problem of suicide in terms of his new methodological conception of sociology. This conception incorporated and critiqued elements of Quetelet, J. S. Mill, Comte, and neo-Kantianism in its French form (deriving particularly from Charles Renouvier and his notion of representation), and represented not only a continuation of but also an attempt at the realization of the ideal of the social statistics movement
to eventually go from statistical observations of rates and stabilities of rates to the underlying explanatory laws.

Under the French system of academic patronage, Durkheim, once he secured his position in Paris at *L'École Normale Supérieure*, had the advantage of being able to assemble a body of talented but dependent protégés who could be set to the task of writing in a Durkheimian fashion about particular topics of interest from a Durkheimian standpoint, to write reviews of books in sociology and related disciplines which were rivals or which could potentially be incorporated, and to establish claims upon topics for the new discipline. Durkheim’s vehicle for doing this was *L’Année Sociologique*, which provided an outlet for publication for his protégés, but, under his strong editorial hand, assured the consistent application of his methodological ideas and his (imperialistic) idea of what sociology was.

Durkheim begins his methodological text, *The Rules of Sociological Method* ([1895]1982) with a central philosophical issue. If sociology is to be a science, what are its facts? He borrowed from his own philosophical teachers (notably Émile Boutroux) the notion that every science has its own distinct class of not only laws but facts, and borrowed from Quetelet the specific model of facts, namely that rates of sufficient stability were to be treated as facts. Curiously, the concept of the stability of rates was essentially dead among statisticians at the time Durkheim made it his own. Advances in the understanding of the combinatorial mathematics that produced the joint distributions of the kind of rates, such as age specific suicide rates, summarized in standard (Halley) tables were shown by those methods not to have any special, unexpected stability. Durkheim, in contrast, used these rates as a model for understanding non-statistical social facts, such as stable and repeated rituals, folk sayings, and even laws. But what Durkheim did with these facts was to reconceive them as indices, but imperfect ones, of underlying realities that are not directly accessible but are the true subjects of the laws of social science and the true determinants of social phenomena.

The way of formulating the problem is reminiscent of the tabular statistics tradition, which routinely gestured at the notion that there would be payoffs from the collection of statistics in the form of future knowledge of underlying laws. But with the exception of Quetelet himself, who, as mentioned, had developed a complex analogy between laws about stable rates and the orbit of the planets, it had been only a gesture. Durkheim faced the same problem: there was no obvious way to get from the kinds of stable rates which statisticians in the 19th century collected to “laws” in any familiar sense. Durkheim himself attempted this task only once, and then used the attempt as a kind of model and exemplification of his conception of sociology rather than as a beginning of a large scale program of analyzing statistical rates. The text is *Suicide* ([1897]1951), which, despite its status as a classic of social science, was for his successors, and even his students (cf. Halbwachs *The Causes of Suicide* [1930]1978) not a paradigmatic work in the sense of an exemplar that would be used as a model, and appears now as a cleverly contrived empirical demonstration of the plausibility of what is best understood as an ontological thesis about the existence of social reality beyond the level of the psychological and the individual. (cf. Turner 1996).
The reasoning in this text depends on and exemplifies Durkheim’s conception of what for him was the paradigmatic social fact: obligations. Obligations were at once psychic and external. Psychic in the sense that they were experiences in the form of promptings of the conscience; which were psychologically similar, as causes, to the promptings of individual desire, but from a different source. They were external in the sense that they were experienced as something apart from the desires, beyond the will of the individual to change, and, Durkheim argued, derived from collective life. Thus they were psychic and collective, a “conscience collectif.” The collective character of these promptings or constraints was something of a mystery, because the psychic mechanism was problematic. They were nevertheless collective in a banal factual sense: they were not universal, but were specific to particular collectivities, such as the nation or “domestic society,” the family, and their psychic force varied with and was dependent on the strength, frequency, and character of the social relations in question. Rituals, such as participation in religious life, strengthened them. Suicide was the result of imbalances between obligations and individual desires in two dimensions, with respect to integration and regulation, later neatly restated by Mary Douglas as “group” and “grid” (1982). Where the obligations were minimal, there was under-regulation, or anomie, or in Douglas’s terms, low in grid. The result of lack of regulation, for example of easy divorce, is that wants become unbounded, producing unhappiness (Durkheim [1897]1951: 253). Where social contact or integration was diminished, for example among Protestants, whose religious life fails to produce strong collective states of mind with as great a consistency as that of Catholics (Durkheim [1897]1951: 170), the psychic force was weak. In Douglas’s terms they were low in group. Where the group was omnipresent and blotted out individual psychic life, as in the military, the balance was also lost. Each imbalance conformed to an identifiable statistical pattern of differences in suicide rates.

Durkheim’s analytic strategy in this text relies on the preexisting methodological tradition about cause and statistics that was established in Mill’s System of Logic ([1843]1974), but rejected Mill’s own understanding of the role of probability in favor of the method of concomitant variation, which Mill had thought was inapplicable in social science. Durkheim wanted to argue that statistical relations which took the form of perfect parallelisms with respect to increases or decreases in rates between two variables, such as suicide and some measure of social integration, the seasons, temperature, or the rate of mental illness, could be treated as genuine laws, as examples of concomitant variations, and those relationships which fail to exhibit perfect parallelism could be dismissed as failing to establish the existence of law-like relations. The text itself consists of tests of numerous hypotheses using these criteria, in which the best-known relationships in 19th century social statistics, such as those between climate and suicide, were shown to be irregular and thus not properly nomic.

By the time of the writing of Suicide and The Rules of Sociological Method ([1895]1951), the statistical core of this ambitious argument was the claim that psychological explanations of social facts were invalid, because the relevant facts consisted of variations between people or groups who were “psychologically” the same. For example, there were regular and significant differences between countries and regions with respect to suicide rates in relation to such variables as religion, and there were significant differences with respect to such things as marital status. Official statistics, however, classified suicides in terms of individual “causes” that had no connection with these differences. Neurasthenia, for example, which was commonly cited as a cause, did not vary in a way that corresponded to variations in suicide. Thus the explanation of suicide in terms of psychological traits, such as neurasthenia, were understood to be incomplete or false— incomplete if there was no account of why suicide-producing neurasthenia varied...
socially, false if they amounted to essentially circular characterizations of the suicide victims’ psychology, in which the fact of suicide was taken as sign of neurasthenia. This neatly confirmed Durkheim’s basic idea, that the psychic forces that account for the patterns are not individual but collective.

The only relationships that do prove to be nomic are a series of relationships that Durkheim himself provided, and even these, as it happens, are perfect parallelisms only in a very peculiar sense which Durkheim somewhat inconsistently applied in a way that favored his own laws. He allowed for the correction of laws in the parallelisms that result from interfering factors in his own cases but makes no effort to save, for example, the climatic hypothesis by allowing its exceptional cases to be explained away. The apparent inconsistency in treatment is perhaps justified, because it is apparent that for Durkheim himself the important consideration in connection with the laws is not Millian but Baconian. (1998). Which is to say that Durkheim wished to use these successful identifications of parallelisms not to assert that they directly represent causal relations but rather to argue that they are what Bacon called luminous instances, or places where the operation of underlying causal reality shines through the mass of interfering and obscuring variations that are characteristic of social phenomena.

Durkheim’s argument recalls the passages in Mill’s System of Logic in which Mill concedes that the application of the method of causal inference in the social sciences is ordinarily confounded by the sheer complexity of social phenomena and their causal relations. Durkheim in effect accepts this reasoning, but argues that in some instances the underlying social realities which are governed by genuine laws shine through. Durkheim had already constructed a critique of Mill’s account of causal analysis which was realist in character, and argued that the true task of casual analysis was the identification of real underlying causal structures, something that Mill’s methods, which worked to reveal underlying causal structure if and only if the categories selected for analysis happened to correspond to the genuinely causal category, failed to assure. It is this realism that differentiates Durkheim from Mill. But the meaning of Durkheim’s social realism has been in dispute ever since he wrote.

2.1 What was “real” for Durkheim?

Although much about Durkheim’s methodology and philosophy remains contested, especially with respect to his ontology or realism (Jones 1999), his “functionalism” or teleology, the implications of his use of the term “representations” in a fashion derived from Renouvier (Stedman-Jones 2001), and his discussion of categories, current historical scholarship on Durkheim gives us a Durkheim who is dramatically different from the caricature of a functionalist and positivist that was endlessly attacked, especially in British sociology, in the dispute over “positivism” of the sixties and seventies. The issue of teleology is the most straightforward. Durkheim was regarded by many of his readers as a “functionalist,” and he does employ the term “function” repeatedly. This need not mean that he accepts the idea of a pattern of “functional” explanation or analysis that is distinct from causal analysis. In a footnote to The Rules of Sociological Method he says this:

we note that, if more closely studied, this reciprocity of cause and effect could provide a means of reconciling scientific mechanism with the teleology implied by the existence and, above all, the persistence of life. ([1895]1982: 144 n4)
This suggests that he rejected or anticipated the rejection of teleological explanation, and that he supposed that teleological explanations, if adequate, were incomplete causal explanations which, if fully developed, could be analyzed into causal mechanisms involving reciprocity between causes (an example of which would be feedback mechanisms, in which the changes in outputs produce changes in the external world which change inputs and then in turn change outputs). These explanations appear to be “teleological” only if the causal reciprocities are not specified.

Durkheim’s “social realism” is a more confusing domain. The arguments of his major texts, notably his dissertation (The Division of Labor in Society), Suicide, The Rules of Sociological Method, and The Elementary Forms of Religious Life, established the “social” character of many phenomena, but did so in accordance with a theory that was deeply problematic, and which Durkheim himself altered as his views developed. We may begin with the “social realism” of his earlier writings. These asserted the autonomy, in the sense of explanatory irreducibility, of a certain class of facts, “social facts,” the orderliness of this domain of fact, and the consequent reality of the collective theoretical entities that the explanation of these facts required. This reasoning was connected to a philosophical argument, found in Durkheim’s mentor Boutroux, but also prefigured in Comte, to the effect that each science in the hierarchy of sciences had its own laws. Thus, according to Durkheim’s argument in The Rules, the laws of sociology were laws governing the contents and operations of the collective conscience or consciousness.

These contents were understood to be simultaneously casual and representational—indeed to consist of representations whose combinations were governed by psychological-like laws. Durkheim avoided the problem of explaining precisely how this would work. Because both collective and individual consciousness had the same type of content and shared the mind, which was thus duplex, partly social and partly individual, the problem of being simultaneously causal and representational was shared with psychology. Durkheim’s manner of describing the laws in question indicated that he meant to conceive of the connections between representations as similar to logical connections, i.e., concerned with the combination of representations, consistent with the neo-Kantianism of Renouvier (cf. Stedman-Jones 2001). The idea of the substrate of society, the level of causal reality in which causal social processes subsisted, which in The Division of Labor in Society ([1893]1964) he had understood as the way individuals are grouped, also came to be more explicitly identified with the workings of the “collective consciousness” or “collective conscience” (cf. Nemedi 1995).

By his later claim that “society is made of representations” he meant that the categories that were constitutive of cognitive experience in individual thought were themselves also constitutive of the distinctive social orders in which they appeared and were the basis of sociality. Durkheim then focused on the problems of the diversity and origins of distinctive categories, particularly those categories involving obligations and collective rights. This line of argument about categories could of course be turned onto science and social science itself, and indeed the implications of this argument begin to be drawn out in Durkheim’s essay with Marcel Mauss on primitive classification ([1903]1963) in which Durkheim suggests that the categories of the natural world, including causality, are derivative of the constitutive categories in the social world generally.

The significance of this way of understanding the problem of society and social diversity for 20th century sociology and anthropology was enormous, but its influence has mostly been indirect, except in France itself, where there is a strong interaction between sociological and philosophical ideas about constitutivity, and the neo-Kantian tradition was extended by reference
to Durkheimian ideas as in such texts as Henri Bergson’s *The Two Sources of Morality and Religion* (1935) and Célestin Bouglè’s *The Evolution of Values* ([1926]1969). The affinity of this approach to Alexandre Koyré and Jean Piaget, its transformation into “structuralism” by Piaget and Claude Levi-Strauss, and its subsequent development by Michel Foucault and Georges Canguilhem is obvious, and these ideas have appeared in various forms in American-Anglo philosophy of science, notably in Ian Hacking where they have been applied to social science and indeed the history of social science itself (1990). The applications have characteristically served to expose the relative or historical character of constitutive assumption, as in the case of Gaston Bachelard, in a way that served to undermine any sense of the validity or trans-historical merit of any existing scheme of intellectual categories.

Nevertheless, much of the history of the Durkheimian movement itself consisted of the abandonment of the specifically methodological claims, and of the psychology and ontology of the collective conscience in its original form, though this was done in an inexplicit and incomplete manner. By the 1930s, even Durkheim’s own students, such as Maurice Halbwachs writing on suicide, had retreated to the core argument, which he understood in terms of the notion of “social” influences on suicide, for which there was a strong and unchallenged body of correlational fact in the form of systematic differences in suicide rates of persons in different “social” categories ([1930]1978). This retreat left open the question of what the contents of “the social” were, while simultaneously establishing a strong presumption that there was something irreducible and ontologically distinctive that corresponded to the notion of “the social” or “society.” Durkheim’s own students, including Halbwachs who also wrote on such topics as “collective memory” (1992), typically reasoned that this something amounted to collective ideation of some kind. This was a view shared with German thinkers, who amalgamated the theory of “objective mind” to the category of the social, thus making the basic fact of society into a fact about its shared cultural content (cf. Freyer 1998). Marcel Mauss, Durkheim’s most distinguished student, turned to anthropological material, and substituted the concept of practice for the problematic machinery of “collective consciousness,” a substitution which allowed him to describe cultural diversity in terms of the distribution of practices and beliefs in social groups. Mauss’s notion of practice, exemplified by his notion of techniques of the body, reappears in the writings of Pierre Bourdieu, which characterize a practice as an ordering structure of dispositions ([1972]1977). However, in Bourdieu it is given a teleological interpretation in terms of the reproduction or maintenance of social domination by a group, and in this form appears in cultural studies and cultural sociology.

There is, however, a connection between Durkheim and the later literature on collective intentionality which does not reflect this pattern of dissociation from the idea of collective consciousness. In 1926 the American philosopher Roy Wood Sellars wrote an introduction to his wife’s translation of Celestine Bouglè’s book *The Evolution of Values* ([1926]1969). To understand the connection between this text and later “Sellarsian” ideas about normativity requires a discussion of Durkheim’s ethics, which is at once naturalistic and normative. Durkheim was a “relativist” about norms, but his was not an individual relativism, in which individuals “choose” their values, since the ordinary individual was of necessity born into a society made of representations and constituted himself out of these representations or categories and experienced them as real constraints or as a real categorized or represented reality. These were real and real as obligations, but were misunderstood by ethical thinkers and religious thinkers, and indeed ordinary people, with respect to their character as “social facts.” For Durkheim this meant that
any sort of positive ethical theory was delusive and that the only feasible replacement for it was a kind of science of morality (1920). Intellectual intervention in the reform of morals was possible only in times of moral change in which the categories were themselves becoming reconstituted in a period of collective effervescence of the flux of moral ideas and through a new social fusion. But this could be accomplished only indirectly, so to speak, by contributing to the formation of new collective categories with moral force, a casual process that philosophical ethical theory was at best incidental to. This is the thesis that appears in Bouglé ([1926]1969) and is explicated by the elder Sellars.

Sellars formulated this in terms of the slogan “values are collective because imperative and imperative because collective” ([1926]1969: xxxv). Sellars called this Durkheim’s “social naturalism,” and reiterated Durkheim’s objection to monistic transcendental idealism, namely that it cannot account for the diversity of morals ([1926]1969: xxxi). The “naturalistic” solution solves the problem of objectivity by locating value not in the things valued but in the social medium. Consistent with his conception of social reality as genuinely causal but cloaked and obscured by false conceptions, Durkheim argued that primitive religion, which consisted of solidarity-producing collective rituals, was the embodiment of a kind of systematic error. The primitive concept of God served as a surrogate for the misunderstood and misrecognized reality of our dependence on society, so that, in effect, the solidaristic rituals of the Australian aborigines were about society itself, not only because of the solidarity inducing consequences of the rituals, but because society is God-like in its relation of superiority and causal priority to us as individuals. This was an argument which had a close relation to Comte, who also argued that the great lesson of sociology was the fact of our dependence. Durkheim took this for granted, and reasoned backward to account for religion as a primitive partial apprehension of this truth. This account also allowed Durkheim to explain why representations of the “superior reality,” which we experience as morality or “hyper-spiritual” forces, enable them to be experienced as external and constraining (Bouglé [1926]1969: 143-4). They do so because they represent, imperfectly, the real constraint of our dependence on society and of society itself.

We may question whether Durkheim’s ethical argument can be salvaged without his notion of collective consciousness, which Bouglé continues to employ. One issue is the question of why, in Roy Wood Sellars’s slogan, “values are imperative because collective,” which is to ask why collectiveness implies imperativeness. Durkheim, who defines social facts in terms of “constraint,” answers this by definition. But the definition is part of the package containing the notion of collective consciousness as a real force, the psychological model of homo duplex, and thus an ontological commitment to something like group mind. Here a connection with the younger Sellars becomes clear. Wilfred Sellars, in his “Imperative, Intentions, and the Logic of Ought” (1963) attempts to answer this question about the imperativeness of the collective without appealing to group mind by analyzing statements of the form “we disapprove of women smoking, but I do not” as non-contradictory statements distinguishing the “collective intention” of disapproval and the individual intention of approval, treating both as intentions that are self-binding. But this also raises the question of whether the “collective intention” is merely a descriptive fact which is not binding in and of itself.

3. “Mainstream” American Sociology

The Durkheimian school was embroiled in controversies which other French sociological movements, notably those associated with labor (LePlay) and official statistics (Tarde), and lost
key members in World War I, but dominated the university system through its central position in Paris, and its institutional and intellectual domination of anthropological inquiry, a position of power prolonged by American financial support. Where it lacked power, it also lacked influence, especially in German and American sociology, which operated under quite different institutional conditions and with different disciplinary rivalries and divisions of labor, particularly with philosophy.

American quantitative sociology, as a distinctive enterprise with something resembling a “paradigm,” began to take a distinctive form in the 1890s and came to dominate the field in the first three decades of the 20th century, under the leadership of Franklin H. Giddings, the first professor of sociology at Columbia who was appointed in 1896. Giddings can be credited with inventing the account of the relationship between theory and statistical data which has been dominant in U. S. sociology. Like Mill and Quetelet, whose works he criticized, he put the actualization of any full “resolution” of the problem in a fully empirical theoretical sociology off into the distant future. He was, nevertheless, eager to apply the methodological lessons being retailed by contemporary scientists (or by contemporaries in the name of science) to the question of what empirical inquiry can warrant in the way of theory, and in the 1890s this meant Ernst Mach and Karl Pearson. From Mach, Giddings took the idea that mechanics is nothing more than description and concluded that “no other branch of knowledge can claim to be more” (1901: 45). This prepared him for Pearson’s similar, but even more extreme, message that not only was developed science no more than description, the laws of physics themselves were not, properly speaking, descriptions but were themselves idealizations. Pearson himself regarded the ideas of “cause” and “explanation” as animism. Thus, Giddings’s philosophical point of departure was unusually stringent: the idea of theory itself had only a tenuous hold in the conceptions of science on which he relied.

Giddings’s embrace of these doctrines created obvious difficulties for him as a theorist, which he resolved in a Pearsonian fashion. The logical status of sociological theory, as Giddings explained it, is defined by its place in what he called the three “normal” stages of the scientific method: guesswork, deduction, and verification. The three stages are a modification of Pearson’s stages of ideological, observational, and metrical, which Giddings quoted in his lectures, and which itself was a modification of the Comtean stages, taught to Pearson by a Cambridge librarian who served as his mentor. Giddings gave the following formulation:

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\text{Science cannot, as a distinguished scientific thinker said the other day, even get on without guessing, and one of its most useful functions is to displace bad and fruitless guessing by the good guessing that ultimately leads to the demonstration of new truth. Strictly speaking, all true induction is guessing: it is a swift intuitive glance at a mass of facts to see if they mean anything, while exact scientific demonstration is a complex process of deducing conclusions by the observations of more facts. (1920: xvi-xvii, emphasis in the original)}
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His own work, he hoped, would enable his readers “to see that much sociology is as yet nothing more than careful and suggestive guesswork; that some of it is deductive, and that a little of it, enough to encourage us to continue our researches, is verified knowledge” (1920: xvii). The solution to the puzzle of the relation between statistical sociology and social theory this suggests is that “speculative” social theory is a source from which the sociologist may take basic concepts
to see if statistical data “mean anything,” then deduce conclusions from strict formulations of these guesses and test them on “more facts,” thus gradually adding to the stock of “verified knowledge,” which thus consists of the accumulation of statistical results that serve, if not to answer theoretical questions, to replace theoretical “answers” with metrical descriptions of relations.

The novel assumptions that emerged in the writings of Giddings and his students are separable into two areas. The first contains what we might call a theory of statistical explanation, which held that the processes governing the properties of interest to sociology were “causal” and could be approached by statistical methods which were the result of an amalgam of Pearson and G. U. Yule which could serve as a substitute for experiment. Before becoming more or less standardized or conventional, and indeed largely tacit, this assumption went through a number of formulations; its various conceptualizations were designed to explicate the statistical practices of correlation and partial correlation, and to support the claims that partialling was a practical equivalent of experiment and that correlations were “generalizations.” The second contains what might be called a theory of measurement, which held that the properties of interest to sociology were subject to significant measurement error, that they could ordinarily be measured in a variety of ways which imperfectly correlated with one another, and that their status as “measurables” could be established by common consent to the substitution of numbers for words.

3.1 Cause and correlation

In the 1911 edition of the Grammar of Science Pearson argued that the supposed difference between cause and correlation is merely a matter of degree: the difference between the laws of physics and the relations between, for example, parental and adult children’s stature, is the quantitative fact of degree of variation; but even observations in physics show some variation. It is hopeless, he thought, to claim that the quantitative degree of variation found in various relationships represents a qualitative difference (cf. Turner 1986: 219-24), and, accordingly, he urged the abandonment of the distinction between cause and correlation. The price of this reasoning is high, because almost everything is, as Pearson himself insisted, correlated with everything else. Giddings and his students struggled, alongside their biometer colleagues, with the question of the proper middle ground between accepting the radical collapse of cause into correlation and adhering to the traditional notions of cause and law, and in searching for an adequate mode of formulation for the compromise. Giddings himself made several striking contributions to what was to be the ultimate resolution of the problem in sociology. In his Inductive Sociology (1901) the discussion takes the form of correcting Mill (whose discussion of complexity and the problem of disentangling causes had the effect of denying the possibility of significant causal knowledge in the social sciences) by redefining the task of social science. He remarks that although “it is not always possible perfectly to isolate our phenomena, as, for example, in Mill’s familiar example of the effect of a protective tariff, we may nevertheless be certain that we have found the only sufficient antecedent if we know we have found the only one commensurate with the results” (1901: 17). The paradigmatic means of establishing this is “systematic observations of the resemblances and differences of occurrence in a series, and of magnitude” (1901: 16), meaning correlational analysis. The terminology of resemblances is Mill’s and John Venn’s, the idea that the mathematical expression of a comparison between “classes or figures,” or a correlation coefficient is “always equivalent to a generalization or law” is taken from Pearson, but also exemplified in anthropologist Edward Tylor’s “much simpler”
diagrammatic method, as used in his studies of the relation between matrilineality and matrifocal residence patterns (Giddings 1901: 283).

If we begin with Giddings’s 1901 comment on Mill, we can understand the particular dilemma he was addressing. To the extent that we want to retain the possibility of isolating “sufficient antecedents” or major causes, it is not sufficient to point to correlations, however high. In 1921 Sewall Wright, USDA scientist and animal geneticist, described the problem in his classic paper on path analysis.

Birth weight and gain after birth are highly correlated. Here neither variable can be spoken of as the cause of variation in the other, and the relation is not mathematical. They are evidently influenced by common causes, among which heredity, size of litter, and conditions which affect the health of the dam up to the time of birth at once come to mind. (1921: 560)

Thus correlation, however high, does not assure cause. Sometimes prior knowledge of experimental evidence suffices to warrant the claim that a “causal” but imperfect relation exists, but this evidence does not eliminate the possibility that some or all of the observed correlation is a result of “common causes.” In practice, then, interpreters of natural experiments are left with a negative definition of cause— a causal relation exists where there is no common cause and where there are some rough grounds for supposing a causal relationship exists.

Lacking experiment, sociology was left with this negative definition: a cause is a statistical association which is not spurious. Of course this definition has the problem that defining spuriousness itself requires an appeal to the notion of causality, whether spuriousness is the result of common causes or confounding. Giddings’s positive version of the notion of causation, as presented in his methodology textbook of 1924, was this: “If, in a large number of cases, we find a high correlation of the occurrence frequencies of the result attributed to it, the presumption of causal nexus is strong” (1924: 179). Giddings conceded that there was no rule which distinguished spurious correlations from causal ones, but he identified a key symptom: a high correlation “points to the major causal nexus,” when it persists “while other factors and correlations come and go.” A correlation can be presumed not to be a mere “arithmetical accident” under this circumstance when the hypothesis of a common cause can be excluded and where there are no highly correlated causes which may be confounded with the putative cause (1924: 180).

One suspects that Giddings may have been following the widely used methodological text of his youth, W. S. Jevons’s *Principles of Science* (1874), in accepting that this was as much as could be usefully said on the subject. Jevons himself had said that “no rule can be given for discriminating between coincidences which are causal and those which are the effects of law” (1874: 262). Yet, as a philosophical conception of cause, the formulation of cause as non-spurious correlated sequence is not very satisfactory. As noted, one obvious difficulty is in the definition of spuriousness as the existence of common causes or confounding—the concept of cause which is to be defined also appears in the presumed definition. Worse, the definition produces a regress in making judgments about the existence of common causes because claims about their existence or nonexistence always depend on claims about the non-spuriousness of the relations assumed to be common causes, i.e., the nonexistence of common causes that would relegate the alleged common causes themselves to the category of “spurious.”
By mid-century, apologetic writings on behalf of sociological research tended to evade these questions by assimilating natural experiment to psychological experimentation, a process to which Giddings contributed by calling correlational results “uncontrolled experiments” and to which Giddings’s student F. S. Chapin contributed. By 1945, we find Paul Lazarsfeld, who picked up many of his methodological ideas from Samuel Stouffer, W. F. Ogburn and thus, like Chapin’s student George Lundberg, a member of the third generation from Giddings, arguing against “the futile controversies as to whether or not a correlation is a causal relationship.” According to Lazarsfeld, “the meaningful way to put this question is: To what degree is a given correlation equivalent to a controlled experiment?” (1945: ix). The flaw in this way of putting the problem, which was also Yule’s way in the paper generally regarded as the locus classicus of the concept of spuriousness (1895, 1896), is this: there is no way to know whether a given correlation is “equivalent to a controlled experiment.” If the apparent relation is spurious, the concept of experiment would not be valid. But the claim that a relation is spurious itself depends on the analogy to experiment, leading to a regress that cannot be ended “empirically.”

Giddings understood this defect of the analogy to experiment, and thus combined the use of the analogy with the informal or prudential idea that a “major causal nexus” will persist while other correlations come and go. He did not attempt to say more than this, perhaps out of scruples derived from Pearson and Mach, specifically the idea that causality was a feature of science that was useful at the stage of guessing and observation but would ultimately vanish. This combination of scruples and distinctions is still evident in his student Ogburn’s writings in the thirties. By 1934, Ogburn would formulate these claims by saying that statistics had “limitations” as a method, especially when compared to experiment, the use of genuine controls. Statistics, particularly multiple regression and correlation, is sometimes a “substitute for the laboratory,” but one of quite limited application in social science, primarily because of the number of relevant factors and the difficulty of measuring and obtaining data on them, also because of difficulties in assumptions, such as the assumption of linearity in the relationships (1934: 16). Ogburn is quite Pearsonian in his separation of description and explanation:

Statistical tables are only a framework in which the data may be examined, and a coefficient or curve is merely an abbreviation of the table. As to what the arrangement of figures means depends on what the author or reader brings to them in the way of association—very much as one gets the meaning of a political cartoon in a newspaper.

(1934: 17)

The terms must be “explained” and the relation must be “interpreted,” but this must be done on grounds extrinsic to the table. The grounds must be “scientific” for the interpretation to be “scientific.” How this is to be done is not made clear in this paper, apart from Ogburn’s repeating without citation Giddings’s “three steps” from hunch to hypothesis to verification. He reasons that statistical evidence provides only a partial check on hypothesis, or to put it differently, a check on the part which is contained in the statistical tables themselves, not on the “interpretation” as such (1934: 14-15).

By the time of the great post-1945 expansion of sociology, these subtle philosophical distinctions were simply forgotten. The “ex post facto design” (a term invented by Giddings’s student Chapin in 1937, cf. Chapin [1947]1974: 95) stood on its own as a communicable practice or tradition (cf. Campbell and Stanley 1966), and the problem of spuriousness became a problem.
the prudent analyst learns to avoid by appropriate design, especially by controlling for “nuisance variables” such as prior distributions of demographic traits in sample populations that produce illusory relationships. Correlational analysis itself diminished in importance. The standard technology of card sorters lent itself to contingency table analysis, and it was not until the introduction of computers in the sixties that correlational analysis regained its position as the dominant technique. It was not until then that the issues with correlation became philosophically salient (especially with Meehl 1970), though many of the issues were apparent from a series of papers by Herbert Simon in the fifties (cf. 1954), including one with Rescher (1966).

3.2 Measurement

The measurement reasoning of the Giddings’s circle also marked a significant break with “statistics” as practiced by their predecessors. The Science of Statistics ([1895]1910), written by his colleague at Columbia, Mayo-Smith, is, with respect to measurement, a part of the 19th century moral statistics tradition. The statistical material studied is exclusively the sort collected by the state, such as vital statistics, which Mayo-Smith comments on with a “sociological purpose” ([1895]1910). In contrast, Giddings’s first methodology book, Inductive Sociology (1901), attempts something quite different: the measurement of “magnitudes” which derive from theories of sociology as well as from terms in common use, such as “labor unrest.” The kinds of data he proposes to use are not very different. But they are used, and conceived, in a distinctly new fashion.

The theoretical problem which concerned Giddings for most of his life was the problem of forms of political association and the interaction between them, social relations, and the formation of human personality types. It was a tenet of Spencerism absorbed by early sociology that each of these “evolved.” Spencer was also concerned with problems of interference with evolution, and American sociologists of the period, such as William Graham Sumner, who remained within the broad confines of the idea of social evolution through competition, concerned themselves primarily with the problem of the brakes and limitations on social evolution. Sumner’s Folkways (1906), for example, was an examination of the most fundamental and unchanging morals and customs of society, the mores, which constrained social evolution by limiting change to those developments consistent with the mores. Giddings, who came to sociology from a chair of political science at Bryn Mawr, concerned himself with the limitations of evolution that forms of political association created and with the constraints on political evolution which result from conflicts with primordial ties, such as kinship, and from the inadequate psychological or characterological evolution of personalities. In particular, he was concerned with such questions as whether Italian immigrants would undermine American democracy as a consequence of their psychological traits and their primordial loyalties.

One might investigate this impressionistically, by deciding if some group has some characteristic, such as “forcefulness,” (one of Giddings’s four basic psychological types), which is pertinent to the theoretical question of their capacity for sustaining particular forms of political association. But only systematic quantitative evidence in the form of magnitudes can enable the precise determination of correlations. One constructs a magnitude in this fashion:

Suppose that we desire to know whether the men of Montana represent a type of character that might be described as forceful, but that we find no testimony, no record of personal observations, directly bearing upon our inquiry. We know, however, that by the general
consent of mankind, men who follow adventurous and daring occupations are described as forceful. Turning then to the census, we learn that a majority of men in Montana follow adventurous and daring occupations.

Accordingly, by substitution, we affirm that a majority of the men of Montana are of the forceful type of character. (Giddings 1901: 27)

One might say that the 19th century statisticians, and Mayo-Smith, reasoned informally “by substitution” in their commentaries on facts of vital statistics, and one can also find plausible instances of the idea of measurement “by substitution” warranted by “the general consent of mankind” (or “face validity” in later parlance), but the combination of the two in an explicit model of hypothesis testing was not present.

Perhaps a genealogy of this concept of measurement, thin to the point of transparency, is unnecessary. Nevertheless, because the problem of measurement subsequently became so important to quantitative sociology, some background and some explanation of its absence from competing traditions, such as the Durkheimian, is relevant. The primary source of Giddings’s innovation here was the matter of race, one of the defining issues for Columbia at the time. Giddings’s student, Frank H. Hankins, makes the point in An Introduction to the Study of Society (1928) that “all the customary indices of racial difference, viz. stature, cephalic index, hair color, eye color, skin color. Nasal index, hair form, alveolar index, etc. in fact extensively overlap one another” (1928: 95).

Norwegians are obviously taller on an average than Japanese, but some Japanese are taller than many Norwegians. White and Negro cannot be distinguished by stature; nor by cephalic index; even as regards skin color and hair form the border areas of distribution overlap. It is this overlapping that makes it necessary to think of a race as a group set apart by a complex of traits inherited together within a limited range of variability. Since tall stature shades into short, long head into round, and dark complexion into light, it must be shown that with tall stature are found also a certain head form, eye color, shape of hair, etc. (1928: 96, emphasis in original)

This is the Boasian and Pearsonian doctrine of race, on which Giddings also relies. The underlying idea here is that a race is not definable by a single criterion, but by correlated statistical distributions of several properties. Analogously, as Giddings, speaking in Pearsonian mode, explained: “the fact that every manifestation of energy is associated with other manifestations, every condition with other conditions, every known mode of behavior with other modes” (1924: 196) justifies the practice of using a wide variety of measures, each of which can be plausibly used as an imperfect surrogate for the variable of interest. The imperfections were then treated as measurement error, that is, as a well-understood problem of traditional statistics.

Giddings did produce some interesting models of analysis, even at this primitive stage. The speculative concept of “consciousness of kind,” derived from Giddings’s rejection of other explanations of groups suggests the following deduction: “Concerted Volition,” including that which is expressed through forms of political association depends on sympathy or consciousness of kind. Hence, there should be a relationship (which he spells out at length, discursively, on the theoretical level) between the degree of social sympathy and what we might call political culture. He “verified” this by constructing an “index number” corresponding to a weighted formula based
on the addition of these fractions: proportion of the numbers of native born of native parents; of native born of native parents and native parents of foreign born to foreign born; and this proportion to the proportion of colored (1901: 287). The number was understood to correspond to the degree of homogeneity in the population. After calculating this number for each state of the union, he constructed three categories based on the scores. The relationship is verified by examining the lists of states falling in each category. In the highest index number category, he said “it will be observed that the states which are distinguished for a rather pronounced ‘Americanism’ in politics and legislation are chiefly found, as might be expected” (1901: 289). In those states where the population was “neither perfectly homogeneous nor excessively heterogeneous,” signifying the existence of more intellectualized ties as distinct from primordial group feelings, are to be found the highest degree of “progress and social leadership” (1901: 289). Had he measured “Americanism” and “progressiveness” in some fashion, the association could itself have been measured statistically. Here, simple inspection of the differences in index scores between categories sufficed.

This little statistical association between heterogeneity and progressiveness provides a kind of paradigmatic example of the kind of social research that became conventional in sociology by 1935, and omnipresent in the fifties and sixties. It was motivated by some “theoretical” ideas about the political effects of heterogeneity. Heterogeneity was an abstract variable measured indirectly. The association was imperfect, but it confirmed and metricized a theoretical idea. By the twenties, with the infusion of Rockefeller Foundation money in support of the transformation of the social sciences into statistical disciplines and in direct support of statistical research, a significant amount of research of this kind was taking place. In Giddings’s department the first dissertation using multiple regression was published in 1920, and a major effort using extensive partialling to determine causal order was published in 1924. The Rockefeller-funded Institute for Social and Religious Research supported a sufficient staff of statistical clerks to perform the tedious calculations required for this type of analysis, and produced a significant number of books using these methods.

4.0 The American Enemies of Quantitative Sociology

The critique of this particular form of the use of statistics and the identification of these usages with “science” and the “scientific method” developed parallel to its rise. The first synthetic thinker in this oppositional tradition was Charles Ellwood, one the earliest American Ph.D.s in Sociology and a student of Dewey and Mead. The book that he published in 1933, Methods In Sociology: A Critical Study, represented a synthesis of the critiques that had been developed by that time. The personal background to Ellwood’s rejection of narrow quantitative sociology is quite interesting, because it reaches back to his undergraduate teacher at Cornell, Walter Wilcox, who was important in many ways for Ellwood and who was the author of the very first empirical statistical social science dissertation on a sociological topic in the United States, The Divorce Problem published in 1891. Wilcox was a student of Richmond Mayo-Smith, who was the major conveyer of the European statistical tradition to American students during his career at Columbia, where he was one of the earliest members of the faculty of social and political sciences. Ellwood was also a student in Berlin of Simmel, and this encounter proved formative as well, also for negative reasons. Ellwood subsequently rejected apriorism of the kind represented by Simmel, whose philosophical practice and influential example will be discussed in the next section.
Ellwood first encountered these problems in the early 1890s with Willcox. Ellwood recalled much later his negative reaction to Willcox's own promotion of the special and unique validity of the statistical approach. But he also took from Willcox what he thought was a better idea, the offhand suggestion that perhaps the best way for the social sciences to advance was to pay more attention to the psychological. Those two elements, the critique of statistics and the importance of the psychological, in a way epitomized Ellwood's career, for he followed the hint of pursuing the psychological throughout his sociological work and also resisted not so much statistics (which he himself collected and employed) but what he considered to be a misapprehension about the importance of the statistical method in sociology.

To understand Ellwood's critique, it is necessary to start with some very broad contrasts. There is a general contrast between Ellwood (and his American peers and sources) and the European tradition in writing about these topics. Ellwood was not in any sense anti-science, nor did he reject the notion that sociology ought to have theoretical knowledge in the form of universal principles (1933: 103). Although he argued specifically that behaviorist methods were inadequate for the problem of culture, which he took to be central to the best available understanding of society, he did not, and indeed explicitly abjured, any explicit a priori account of culture or meaning, such as Hans Freyer's theory of objective mind, which would then serve as the special topic of a sociological science of culture. The nature of culture, for him, was a matter to be understood by reference to universal considerations about the process by which the contents of culture were transmitted, especially the consideration that culture was produced and transmitted in what he called an interlearning process between individuals—a claim that in his hands and the hands of his most famous student, the "symbolic interactionist" Herbert Blumer, precluded an autonomous (or ontologized) notion of culture (or for that matter of society).  

One can distinguish two basic ways of being scientific. One is to follow a method, the scientific method. The second is to follow no particular method, but to concern oneself with the substantive results of science and fit new extensions of science with established results. What Ellwood rejected was the idea of imitating natural science by extracting a "method" and applying it to social life. In his view, the social theorist was constrained by the biology and psychology of his subjects, and ought to be conversant with the science that was the source of the constraints, and indeed he himself was. For him, social theory is continuous with the substance of science, and is open to whatever "continuous" turns out to mean. This is what drove his account of culture. Learning and interlearning for Ellwood were the real processes in which culture was sustained, and in terms of which innovation and change needed to be understood. These processes were not inventions of sociologists; learning was taken from established psychology. Ellwood was well aware that society and culture were terms that had been given various definitions, notably, in the case of society, the organic analogy. His reasoning was that it could not be taken for granted that society understood as an organism corresponded to anything real or was necessary to the explanation of the things that we know pretheoretically about social life that the sociologist should be theorizing about.

Ellwood rejected the idea, made fashionable by Durkheim and later Parsons, that there was some sort of autonomous subject matter of sociology that was free of these constraints. The idea of sociology as an autonomous subject is an idea that depends on a methodological conception of science. In Durkheim it takes the form of the doctrine of treating social facts as things, and then subjecting these invented "things" to analysis using the methods of science. Ellwood understood the grounds for claims about the reality of collective representations, which
was that they were in some sense to be understood as objective facts. But he argued that there was an equivocation in the use of “objective” that was fatal for these arguments. Durkheim, Ellwood says, was “only half-hearted in his objectivism” (1933: 32). He sought to do away with psychic elements and the explanatory role of individual psychological phenomenon, but instead of going on to construct a sociology in terms of the behavior complexes of the aggregate [i.e. to be like a behaviorist about society], he accepted the hypothesis of “collective representations,” such as popular beliefs and social traditions. Thus, Durkheim’s objectivism was tainted with subjectivism of the worst sort [and the kind that behaviorists objected to in psychology], for his hypothesis of “collective representations” transcends his definition of fact.” (1933: 32)

This is to say that Durkheim pretended to be constrained by objectivist methodological considerations, but proposed a hypothesis that no such facts could establish, and which rested on such “subjective” facts as feelings of obligation, necessarily formulated in concepts that derived from ordinary language, which Durkheim inconsistently derided as inadequate for science. Ellwood’s point is familiar to anyone who has read the critiques of Durkheim found in writers like Jack Douglas (1967) and Peter Winch (1958). The only way into the identification of social facts is through “members” concepts, i.e., something “subjective” in Durkheim’s own sense, and Durkheim himself is unable to avoid appealing to them, for example, in his definition of suicide in terms of the subjective notion of intention. And this is a fundamental problem, because the claim to be made for Durkheim’s conception of social facts is that they are special facts of a new type, not accessible to other senses.

Beginning as he did, before the arguments of his opponents had fully taken shape, Ellwood was hard-pressed to define the issues, and characteristically the methodological problems arose first in fairly arcane forms and sources, or in sloganeering that was difficult to unpack and critique. Ellwood discusses, for example, “Karl Pearson’s aphorism that ‘science is measurement’” (1933: 100), and claims such as Ogburn’s and Goldenweiser’s claim that “the ‘scientific’ future of the social sciences depends on their amenability to statistical methods” (1933: 100), which Ogburn, in a notorious episode, had caused to be engraved on the facade of the Social Science Building at the University of Chicago in the form of a quotation loosely derived from Kelvin (Bulmer 1984). The fact that the basic model of methodology discussed in connection with Giddings was not the subject of a more fully developed methodological rationale or “philosophical” treatment forced Ellwood to find examples of thinkers who articulated some approximation of one. Weber, who faced the same problem, devoted his methodological writings to figures who had already become obscure or obsolete, such as the historical school economists Wilhelm Roscher and Karl Knies, the legal philosopher Rudolph Stammler, and the energeticist chemist Wilhelm Ostwald, who have been preserved for current thinking largely by virtue of appearing in these methodological writings. It was rarely the case that the proponents of the positions they were criticizing stated their views in the kind of overt manner that lent itself to analysis. Thus, in a strange way, the critique of positivism was compelled to invent its subject, which in turn allowed proponents to disavow the positions attributed to them and ignore the critics, a pattern which proved to be characteristic of later debates, notably the German Positivismusstreit of the sixties.
To sharpen the issue of objectivity in sociology, Ellwood turned to a student of Pavlov named Zeliony, who had attempted, as the behaviorists in psychology of the same era characteristically did not, to apply the methodological reasoning they employed in constructing psychology to the problem of the nature of a science of sociology, which he treats as a reductio ad absurdum of the implications of the conception of science professed by social science objectivists. Zeliony is a convenient mouthpiece, for he argues quite explicitly that, as Ellwood puts it, “the task of natural science is . . . simply the description of observable phenomena, the discovery of new phenomena, and finally the deduction of relations of law between phenomena” (1933: 33), which implies that appeals to concepts such as ideas, emotions, beliefs, desires and values, even in descriptive contexts, are illegitimate, and more generally that the mind of another cannot be considered as a phenomenon, nor as a fact. Consciousness must be ignored by the natural scientist, as it is not available for his observation, neither can it serve as a transcendental hypothesis (1933: 34).

This means that “the whole of modern sociology is full of . . . mistaken designations,” since such notions as crime and family “involve or build on the psychic side of individuals, and thus must . . . be barred from scientific sociology” (1933:34). This was of course Durkheim’s claim as well, but one which, as Ellwood said, he failed to carry through consistently.

The structure of Ellwood’s response to Zeliony’s argument, and his extension of it to his contemporaries, is this. He frames the problem in terms of “adequacy,” the question of whether this conception of science and method is adequate for the study of society. This is framed in part in terms of the question of what is the “essential” character or “nature” of social life (1933: 55), and Ellwood defends a kind of realism about the social processes of interlearning discussed above (cf. 1933: 70-71), which he takes to represent the essence of culture, including its psychic side and considerations of meaning. His characterization of “behaviorism” reflects its use as a term for the denial of any “reality” or “objectivity” to “subjective, non-material phenomena” or any “non-physical entities or processes” (unnamed source quoted in Ellwood 1933: 47), a rejection which was also ordinarily extended to the objects of theoretical concepts. Ellwood argues that this denial is simply a priori and dogmatic, and anti-scientific to the extent that it “does not preserve the experimental attitude in the matter of scientific methodology itself” (1933: 51), especially by denying all value to “sympathetic introspection” in connection with participant observation.

Supporting the claim that behaviorism is “adequate” requires “some metaphysical dogmatism” to the effect that the subject matter that behaviorism cannot accommodate is not real or that the only possible explanations are mechanical and deterministic (1933: 52). He is able to repeatedly quote Pearson endorsing the idea that the only facts are sense impressions, a claim that points to a crucial ambiguity in the position he is criticizing, which seems to imply not only that psychic phenomena are inadmissible to science because they are not observable, but more radically that no theoretical concepts of any sort are admissible to science. Ellwood is able to dismiss this as metaphysics.

This does not excuse him from defending his own claims about the essential character of social processes. But he does this by arguing that his account is “adequate” to the understanding of social life as revealed through the “historical” and “psychological” methods (1933: 77), results we have no a priori reason, outside of a metaphysical dogmatism, to dismiss, and then he turns to arguing for the inconsistency between the standard statistical approaches to social life and the
philosophy of science that is presumed to underlie it. The denial of introspection, for example, is central to “behaviorism,” but the statistical material which is studied by statistical sociologists includes such things as questionnaires (which, in effect, record the introspections of the respondent), personal interviews, and the study of historical records, which, as Ellwood put it, “imply something more than behaviorism, because none of these methods could be used in the scientific study of the behavior of animals below man” (1933: 59-60). The behaviorist rejection of “concepts” and imagination is also inconsistent, Ellwood noted, since they are themselves compelled to employ both. He concluded that a synthetic approach, which employs various methods in conjunction with one another, which would require the extensive use of “logic” or more generally theoretical reasoning, is the most desirable strategy for the field.

These arguments were to take a different form in other thinkers, notably in the continental tradition, and in the writings of later critics of what came to be known as “positivist” social science, so a few features of this particular argument bear notice. The first is that despite his appeals to “essential” features, Ellwood was not an apriorist who began with a fixed notion of the contents of sociology. He was a realist about social processes, but the claim to reality is grounded in a claim of explanatory utility and adequacy with respect to phenomena established through extant methods of inquiry whose rejection he argued is indefensible without reference to metaphysical dogmas. He embraced the idea of science, and although he spoke of the problem of meaning (1933: 56) and cited Werner Sombart on Verstehen (1933: 102) he avoided the claim that there is an autonomous realm of meanings, and similarly, though he rejected materialism as dogmatic metaphysics and appealed to a notion of “values” that he contrasted to the material, he did not think that this constitutes an ontological realm beyond the normal social processes of “interlearning.” Indeed, his conception of ethics is similar to L. T. Hobhouse’s notion of the rational good, in which the evolution of morals is accounted for as a product of learning (1921).

Ellwood had allies. He was engaged in a continuous three-way correspondence with Pitirim Sorokin and Robert MacIver, each of whom carried on the fight. Sorokin’s Fads and Foibles of Modern Sociology in 1944 went beyond reductio to ridicule of the scientific pretensions of the various “new Columbus’s” and quantophreniacs. It remained in print for decades. MacIver’s Social Causation (1942) also identified Karl Pearson’s philosophy of science as the source of the model of causal explanation that such American quantitative sociologists as Giddings and especially their students Ogburn and Chapin had been inspired by, and attacked it on standard philosophical grounds. This book was also in print for decades and continued to provide a justification for the rejection of the use of purely statistical grounds for claims about causality.

These texts reflected the bitterness of the division in American Sociology over quantification and its scientific status, a division which is crucial to understanding the later reception of Logical Positivism. The students of Ogburn and Chapin were prominent figures in American sociology during the period of the reception, and one of them, George Lundberg, author of the popular scientistic work Can Science Save Us? ([1947]1961), was a supporter of Carl Hempel’s entry into American philosophy and a participant in the politics of the journal Philosophy of Science. Yet there was an important difference between the older Pearsonian (and Machian) generation and the Logical Positivists. The older generation was hostile to “theory” and theoretical concepts, especially in sociology. The Logical Positivists enabled their successors, in the generation of Robert Merton and postwar social psychology, to accept theoretical concepts, and indeed to bend the restrictive notions of the role of theoretical concepts in Logical Positivism
in the direction of a justification of appeals to those unobservables that could be indirectly associated with measurement, such as the concept central to social psychology, attitude. Logical Positivism thus served as a source for claims to the status of “science,” and the Logical Positivist model of theory came to be invoked, or, in its own way, followed. This was a two-way relationship: for some years at Columbia in the fifties, Paul Lazarsfeld and Ernest Nagel co-taught a seminar (with frequent participation by Merton) on these issues, and Hempel wrote on functional explanation in a work on sociological theory (Gross 1959; Hempel 1965: 303-30.). The issue was shifted by this collaborative relationship to the question of what type of theory was legitimate in sociology, a question leading, especially in the sixties, to an extensive literature critical of “positivism” in sociology, some of which, ironically, relied on Logical Positivist writings as a resource.

The hostility to theory that marked the earlier quantification movement also persisted among quantitative sociologists, and took various forms. So did many of the other issues raised by Ellwood, including the challenge of “realism,” issues about the centrality of processes of social interaction to any realistic account of such abstractions as “society,” the validity of such “methods” as participant observation, and the pervasive problem of the tacit dependence of quantitative sociology on “data” which was the product of under-theorized processes of social interaction in interviews and questionnaires. These issues were an important impetus to ethnomethodology and symbolic interactionism, and eventually led to a large literature in defense of the qualitative methods, such as participant observation, that Ellwood had championed.

“Symbolic interactionism” as developed by Herbert Blumer, claimed to be based on the thought of George Herbert Mead, used many concepts from Mead, and kept Mead’s thought alive in sociology far more effectively than in philosophy itself. The relationship between this movement and Ellwood is spelled out in a letter from Blumer to Ellwood in 1944, discussing his argument that the role and character of culture precludes the use of “natural scientific methods” in sociology. Blumer rejects the idea that the “cultural approach” is adequate. To him it represents “a case of forcing on human behavior an abstraction that has been derived from an imperfect study of the so-called static patterns of simple folk people; and that it operates to obscure the fact that human beings are organisms that are active, seeking, avoiding, and imaginative.” He added that “what we need, I feel, is a new framework for analysis-- a framework that will do more justice to the character of human beings as we recognize them to be and act in our everyday experience” (1944: 2). He finds this, he says, in George Herbert Mead’s “recognition that the act is built up in the course of its execution, and that in being built up it may be anything but a mere release of habit or of fixed established pattern” (1944: 3).

The methodological critique developed by symbolic interactionism depended in part on this notion of the primacy of interaction, which is to say the interactive process by which the act is “built up.” The result is very similar, in terms of social ontology, to Ellwood’s notion of interlearning: “society” is no more than the fundamental, ongoing, process of interaction. The emphasis is, however, different: Ellwood was concerned with the persisting and changing results of interlearning, i.e., culture, its consequences for the agent, and its development. Blumer with the immediate and ephemeral character of the course of action itself. Ironically, the importance of “culture” eventually reasserted itself within symbolic interaction (cf. Becker and McCall 1990), and the emphasis on the creation of social reality in the immediate moment of interaction eventually diminished. But the idea of the primacy of “everyday experience” and its inevitable
conflict with “abstractions” forced on human behavior remained basic to the methodological critique by symbolic interactionism of standard sociology.

5. Weber: Sociology in the Language of Life

Although Ellwood was the most translated American sociologist of the pre-1945 period, neither his writings on methodology nor those of “mainstream” quantitative American sociology were widely discussed in Europe. One result of the emigration of scholars from central Europe was that after 1945 issues in the philosophy of social science tended to be discussed in terms of German sources, notably Weber. Weber came to be discussed as a sociologist in the United States only after 1930. His methodological writings were translated only in 1949. Yet Weber had the most coherent and elaborate account of social science methodology, and Weber was both a target and a resource for subsequent writers in the philosophy of social science, notably Alfred Schutz, Peter Winch, Karl Popper, and Alasdair MacIntyre. The reception of Weber’s thought produced confusion. The writer who most closely resembles Weber is the one who claimed to have the greatest disagreements with him, namely Popper, and the writer who claims to follow Weber, namely Schutz, has the least to do with Weber’s actual methodological writing. Moreover, for much of the 20th century, Weber was conventionally believed to be a kind of social scientific successor to Wilhelm Dilthey whose central methodological idea was Verstehen and who was properly understood as a proto-phenomenologist of everyday life. 21 It is perhaps useful to begin with the sources of this peculiar misapprehension.

Weber’s Economy and Society ([1968]1978) begins with a definition, which as Weber might have said was a matter of decision, of sociology as concerned with meaningful social (meaning relevant to others) action. In the passages that followed he argued that explanations must be both causally and interpretatively adequate. What was adequacy with respect to interpretation? Weber was trained as a lawyer, and as we will see the notion of adequate cause comes from a contemporary theory of legal causation which Weber elsewhere (1949: 167) characterizes as the appropriate model for historical causal explanation. In his discussions of interpretative adequacy, which are very brief, he uses a legal distinction as well. A direct observation or understanding is one in which the point or meaning of an act can be discerned by in effect the immediate application of typifications of the kind that are contained in ordinary daily language. The example he gives is of a man chopping wood. The interpretation with the most inherent plausibility, or as Weber puts it, Evidenz, is that the activities of the wood chopper are instrumental and have the purpose of chopping wood. Other actions must be interpreted in an indirect way which corresponds to a different category of legal evidence involving inferences. The wood chopper, for example, may have the further purpose of chopping wood to provide a wood supply for the fireplace over the winter for his family, or maybe chopping wood for the purpose of sale in a market. These purposes cannot be derived or justified by simple reference to the act of wood chopping accessible to direct observation, but must be made evident by connecting them to other observed facts. In these other cases, however, recourse to standard typifications is essential and the Evidenz, and therefore the adequacy of the interpretation, is the result of the way in which the connected facts, such as the related actions of the wood chopper, are clarified through the typification.

Weber argued that the greatest degree of evidentness attached to rational explanations or typifications, so that, for example, an account of the act of wood chopping that connected it to evidence of a rational strategy for the achievement of some goal and also connected a variety of
other aspects of the actions of the individual provided the strongest kind of understanding. But a similar degree of “evidentness” might well attach to an interpretation of the actions of an enraged husband shooting his wife’s lover. These interpretations are of course always, as in the law, corrigible, so that an even better interpretation that, for example, shows that the husband was only faking rage to avoid punishment by representing a killing for purposes like a crime of passion might prove to be the explanation that made the most sense out of the facts. Moreover, multiple, conflicting interpretations were always possible.

The problem of typification and type concepts was a conventional if not a terribly deep problem in phenomenology; and when this aspect of the argument was reconstructed and extended by Alfred Schutz in phenomenological terms. Schutz’s problem, unlike Weber’s, was the problem of other minds, that is, the problem of how an individual could understand another individual. So his question is what are the conditions for the possibilities of understanding, and the conditions of possibility are understood phenomenologically to be a question about the structures of consciousness that are the necessary preconditions for understanding another person as a person. But Schutz, as we will see, was adamant about rejecting the “Diltheyan” interpretation of Weber’s arguments about this topic. Perhaps the crucial document in the reception of Weber on this topic was a famous essay by Theodore Abel, entitled, “The Operation Called Verstehen” (1948), which, in the spirit of Bridgman’s operationalism, asked what sort of “operation” this was? In Weber, of course, there is no claim that Verstehen is a “method” or operation. It figures rather as criteria of explanatory adequacy relevant to the causal explanation of meaningful action, and as a means of distinguishing kinds of disciplines, those which necessarily use the “language of life” (Weber 1988:209) and those which do not.

The term “meaning” is used by Weber for a specific reason, namely as a substitute for purposive or intentional action in order to, as part of a larger concern, eliminate teleological descriptions from social science. This desire to avoid teleological explanations was provoked by, among other things, a desire for a purely causal social science which did not require for its completion a general account of human ends based on some sort of philosophical anthropology or historical teleology, both of which Weber rejected as intrinsically valuative rather than scientific. In practice, when Weber says “meaningful” he means “intentional,” except in such instances as the meaning of a mathematical proposition and the like. Weber’s account of understanding and interpretative action consequently is designed as a superficial one of matching conduct to typifications of intentional action that requires no significant exercise of phenomenological prowess or a “method” of Verstehen, and no special account of access to “meanings.”

Abel’s particular concern in this article involved the puzzle about what sort of basis one could have for the attribution of intention in the context of understanding a statistical correlation. The example Abel gives is the relation between marriages and crop prices, one of the oldest statistical issues (cf. Mary Morgan 1997). The problem of giving meaningful interpretations to statistical relations is in fact a topic which Weber discusses in passing in the “Introduction” to Economy and Society (1914) under the heading of meaningful and meaningless statistical relationships. Here it would have been resolved by identifying the correlation with “typical” and already understood courses of action, such as the peasant (or his intended bride) marrying after deciding that he had enough money to marry.

The ordinary form of the problem of relating causality to interpretive typifications, however, operated in the other direction, that is to say it began with the typification, not the statistical relationship. As Weber understood the problem of causal explanation in the social
sciences, it was to indicate the probability of some outcome given a particular typification, where the typifications were derived from or part of the language of life. If the question is one of motivation to commit a murder, for example, a description of the act which typified it as “the man went to dinner with his wife and disagreed with her about the choice of desserts and consequently went home and murdered her” would be perhaps meaningfully adequate in the sense that one might be able to conceive of a person so engaged by a dispute over dessert that he could actually commit murder as a result of it. Nevertheless, the sheer fact of the rarity of such murders suggests otherwise. A different typification, for example of “a couple with severe marital difficulties is bickering, and the man, who has a history of violence, becomes enraged over the woman’s selection of dessert and kills her,” would have a higher probability and thus be more credible as an explanation. In the case of the first typification, the standard of minimal probability of the outcome, even if it was set at a very low level, probably could not be met. The supposed cause was so trivial that it would not sufficiently increase the probability of the husband committing murder to be deemed a cause.

Although this perfectly legitimate account of causality strongly resembles the much later statistical relevance theory of Wesley Salmon (1971), it is a form of analysis of very little practical significance. So much depends on the correct assignment of a case to a reference class, which is to say so much depends on how the facts are described, that the added value of the probabilistic analysis of causes is negligible, and this means that in effect a plausible description will invariably, except in made-up cases, be causally adequate. In this sense the naive understanding of Weber as a Verstehen theorist is true, not because he believes in a method called Verstehen but because the description of an act in terms of an already understood ideal-type provides all or virtually all of the explanatory payoffs available to the social scientists with respect to meaningful action. This is an issue we will take up again shortly in relation to Alfred Schutz, who made this point forcefully.

Weber’s focus on meaningful action is associated with a series of other theses that have an important subsequent history in the philosophy of social sciences, notably what comes to be called by Karl Popper “methodological individualism.” Weber was a relentless opponent of holistic explanations and appeals to social holism and argued for a prudential, or as Popper would later put it, “methodological” individualism, on the grounds that the cognitive purposes of sociology, whose object is the “subjective meaning complex of action” (Weber 1978: 13), do not require them. Another of Popper’s key ideas, presented in The Poverty of Historicism ([1957]1961), that action explanations should be made in terms of what he called “the logic of the situation” such that considerations of rationality provide a model of how the decision to act would be made rationally and deviations from rationality are explained, which Popper calls the Zeroth method, is found in Weber in the form of a discussion of rational action as an ideal-type. The conflicts between their understandings of these models will be discussed in a later section in connection with Schutz and Ludwig von Mises. These conflicts rest on differences in their understanding of the problem of the a priori in social science, of the status of frameworks, and of the role of theory, which can only be understood against the background of the larger problem of the a priori in neo-Kantianism during the early 20th century.

The key to Weber’s understanding of the problem of the a priori involves the notion of values, which he formulated in a distinctive manner. Weber’s basic ethical position was that a very wide variety of value choices were possible and that rationality could do little to decide between them. Nevertheless, for Weber, rationality could perform an important unmasking
function by revealing the role of concealed and unconscious value choices and commitments as well as by showing that certain value choices could not be construed realistically as “this-worldly” value choices but could be adhered to consistently only as “other-worldly” value choices, that is to say, value choices based on the outcomes or considerations that arose from God or heaven. For Weber an Al Qaeda suicide bomber or, as he himself usually put it, an anarchist, could “rationally” or consistently sacrifice his life for the cause but only if he acknowledged the unrealizability in this world of the supposed goals of his movement. These same considerations applied to pacifism and, from Weber’s point of view, a great variety of the conventional moral doctrine of his time, including the kind of Christian social reformism that was prominent in Weber’s own background and in the thought of many of his contemporaries.

Weber of course did not invent the fact/value distinction, but he formulated it in relation to social science with great clarity in the course of a controversy within the Verein für Sozialpolitik (Social Policy Association), a group of largely a group of economic experts who generally favored bureaucratic solutions and state control as a response to the various issues raised by the so-called social questions of the 19th century, as well as, in his methodological writings, in a relentless critique of the implicit teleology, usually presented as part of the “scientific” work, of the historical school of economists from whom these “Socialists of the Chair” derived their authority. Weber’s strategy was to force them to acknowledge the valuative and thus arbitrary character of their commitment to the state governed economy and the contribution it would make to the ever-increasing bureaucratization of social and economic life. The term central to Weber’s formulation of the problem of values was “decision.” Selections between values which had been clarified by the process of identifying problems of consistency between various value choices, making explicit the value choices, and determining whether they were realizable in this world left the individual in a situation of decision for which no further rational guidance was possible. This doctrine had implications far beyond the idea of a positive policy science, however, it also implied that our judgments of such things as progress are essentially valuative, a point he made in relation to Simmel. Weber discusses the example of Simmel’s substantive “sociological” claim that progress consists in “differentiation” and argues the following:

. . . whether one designates progressive differentiation as “progress” is a matter of terminological convenience. But as to whether one should evaluate it as “progress” in the sense of an increase in “inner richness” cannot be decided by any empirical discipline. The empirical disciplines have nothing at all to say about whether the various possibilities in the sphere of feeling which have just emerged or which have recently been raised to the level of consciousness and the new “tensions” and “problems” which are often associated with them are to be evaluated in one way or another. (Weber 1949: 28)

The claim is that there is no fact of the matter about what progress is: it is a valuative rather than a factual question and could be turned into a factual question only on the basis of some sort of ungroundable claim such as having discovered in history the final human value. The claim would be ungroundable because such a discovery would necessarily be circular, for reasons that will become apparent shortly. 23

Weber, like the cultural relativists, tended to collapse cultural distinctions and distinctions between historical ethics into cases of value choice or nonrational value decisions, in the ordinary
language of a particular ethic as containing a concealed value commitment. The actual process of value change, however, was not understood as the conscious making of value decisions, though this is of course possible, but rather as the experience of a kind of disorientation where the diminished relevance of particular values at the close of some historical period is experienced as a kind of intellectual twilight. It is these passages that were appropriated particularly by Karl Jaspers. But some of the most important implications of Weber’s construction of these issues came from the claim that the language of life was intrinsically valuative.

Weber stressed, in a way that neither Durkheim (for the reasons we have seen) nor cultural relativism stressed, that the character of ordinary historical categories, necessarily expressed in the language of life, made them unfit for use in eternal or trans-historical “laws.” Weber’s argument here is strongly reminiscent of Donald Davidson’s notion of anomalous monism (1980b). For Weber, the social sciences, or, as he called them to emphasize the valuative character of the subject matter, the historical sciences, constituted their objects and their explanatory interests in a terminology that was already valuative. He argued, in a passage that appears to refer to Mill, that an astronomy of social science, even if it were possible, would fail to answer the questions that we posed in our own valuative constitutive terminology, but as we have seen, he argued that this did not preclude causal analysis (Weber 1949: 73). This Kantian picture points to a more fundamental problem, the problem of the a priori. Weber’s solution to this problem was to divide the a priori conditions for social scientific knowledge into the rational and the valuative. The valuative was necessary for constituting the subject matter: the language of life was itself valuative, and part of a valuative Weltanschauung. The determination of casual relations, consistency of means to ends, and the like was a matter of “logic,” which he understood narrowly. This approach left him open to the charge that his conception of the problem of knowledge was itself valuative or metaphysical.

But his formulation of the problem made a critical distinction: between the worldview of the subjects of research (which he took to be a construction for the purposes of the analyst and his audience) and the worldview of the analyst and the audience. Neither was “rational.” The historical scientist has no privileged starting point, and cannot escape the implications of the following considerations: the facts of “history” are themselves constituted for us by our own values and whatever we discover about the world constituted in this way can be meaningful only to others who constitute the objects of history in the same language, and they would do so only because they shared our values. But it is entirely contingent that they do so. Also, there is no privilege that arises from being at one or another place in the historical process of the development of values, because, as seen earlier, even the notion of progress is valuative. This meant not only that any valuative claims that could be thought to arise from history are relative to this starting point, but also that the questions themselves are intelligible only with reference to an essentially valuative and thus relative starting point. His solution to the problem of the a priori was thus to make the basic framework of the analyst relative to the analysts’ time and concerns, with the exception of those elements that were universal parts of thought, which he called to be “logic,” and narrowly construed to mean deductive reasoning and perhaps the kinds of calculation necessary to assess probabilities, and perhaps decision theory, but did not include the kinds of philosophical reasoning characteristic of Kantian philosophy as practiced at the time.

5. The Problems of the A Priori
5.1 The problem of the *a priori* I: Sociological system building

The problem of the *a priori* appears in a variety of guises in early 20th century philosophy of science but appears in many more hidden ways in connection with social science. The now standard historical story about the origins of Logical Positivism, as told by Michael Friedman, who sees the logical positivists as taking a step beyond neo-Kantianism, but also remaining largely within the framework of neo-Kantian problems. In the case of physics these were problems that arose from the fact, which Einstein’s theory of relativity had made evident, that physical truth was relative to the geometrical form in which it was expressed and there were no definitive reasons in particular cases to prefer one geometrical form over another. As Don Howard puts it,

In a series of essays and reviews in the early twenties, Einstein and Schlick agreed with the neo-Kantians that empirical evidence underdetermines theory choice, especially the choice of deep theoretical principles like the axioms of geometry; but whereas the neo-Kantians exploited the fact of underdetermination to insulate cherished principles from empirical refutation, and insisted that our choice between alternative theories equally compatible with experience is determined by *a priori* considerations, Einstein and Schlick argued that no principle is immune to rejection or revision in the light of experience, and insisted that the choice between alternative theories is a matter of convention, guided at most by considerations of simplicity. (1990: 369-70)

The neo-Kantians wanted to insist that there were coercive *a priori* grounds for preferring one formulation and therefore one statement of fact to others. Einstein, who was aligned to the Logical Positivists on this crucial point, rejected this and treated this particular kind of underdetermination as irreducible.

Apriorism in physics arose as a reflection on the conditions of the possibility of already established physical theory, and it was characteristic of the aprioristic projects of neo-Kantianism to begin with some already established domain, about which one could then reason transcendentally. In the case of sociology, the problem was different, and the order in which the problems arose was also different. Sociology was not an established discipline, but itself a possibility whose conditions needed to be established. The idea that it was possible to establish them in advance reflected a dominant German conception of the role of philosophy, which was understood as follows: philosophy was a discipline which inquired into, clarified, and established the rational credentials of the basic concepts in an area of inquiry or domain of validity, such as the law. This was a Kantian task in the sense that it sought the uniquely rational system of ideas or categories which disciplined thought was believed to require, and by establishing its unique validity this effort established the conditions for objective knowledge, which was knowledge arrived at in accordance with these concepts.

Central to this conception of the task of philosophy was a particular view of logic as a discipline. Logic could be construed narrowly or more broadly, but if construed narrowly (as for example Weber had construed it) it could not fulfill the role of establishing a scheme of concepts, and if construed more broadly it could. Discussions in this literature routinely denounce narrow conceptions of logic in the course of defending their own conception of their task. Consider a representative example, from Freyer:
By dissecting the structural elements of thought from an existing context of knowledge, the pure categories that make the experience of the human sciences possible in the first place will (that is the goal) be found. Especially if someone, through his own work, has achieved both the necessary reverence for a genuine particular science and the proper intuition of its activity, then this way to a local foundation of the human sciences will appear to him as the only appropriate and promising way. He will scrupulously avoid a philosophy of knowledge based only on a theory of logic. Must it not appear absurd to him to want to know the logical structure of the human sciences other than by raising the factual procedures of brilliant historians and philologists into conceptual consciousness, and by gathering the concealed presuppositions, effective basic concepts, and unconsciously practiced methods from their works of art into understanding? . . . Dilthey proceeded in this way when he studied the formation of the historical world in the human sciences (1998: 5-6).

Of course, the situation in sociology was different from the situation of the established disciplines of history and philology. In sociology the same task of producing a “logical structure” had to proceed by analogy to the established human and natural sciences, such as legal science and history.

The law was a favorite topic of neo-Kantian thought, and not surprisingly: the law is “generalizing” and “human” and shares the subject matter of the other social sciences, particularly sociology which took the law as part of its subject matter, and thus law provided one of the most consistent contrasts to sociology. The analogies can be seen clearly in a crucial text by Emil Lask, published in a Festschrift for Kuno Fischer, one of the founders of neo-Kantianism. At the time this was written Lask was a close friend and intellectual interlocutor of Max Weber, who was then publishing his key methodological essays (but not on “sociology,” a discipline then more closely identified with Simmel, who is discussed by Lask). Lask notes “the parallelism of methodological and pure value problems,” and says that “insight into this parallelism may again save us from confounding the empirical cultural concept [i.e. the concept of culture relevant to the cultural sciences] with that concept of culture which represents absolute value and world outlook” (1950: 24-5). The law is a paradigm case of the “dualism” that promotes confounding, as it is both “cultural meaning” and “cultural reality” (1950: 27). “The law may be either regarded as a real cultural factor, a vital social process, or examined as a complex of meanings, more exactly of normative meanings, with regard to its ‘dogmatical contents’” (1950: 27). Understood normatively, it makes sense to resolve it into a “teleological science,” perhaps to be understood as governed by the abstract notion of Recht, an abstraction which allows for new possibilities of conceptualization unanticipated in common sense or a naturalistic approach (1950: 31), possibilities that arise, for example, when novel legal fact situations have to be understood in relation to established but insufficient legal abstractions.

Each effort at intellectually organizing the material of the law “is a transformation– partly prescientific and partly scientific– of epistemological ‘reality’ into an abstract world related to particular cultural meanings” (1950: 27). The “sociological” approach is parallel, governed not by the concept of Recht, but by the concept of the social:

all cultural types may well involve the element of the social, which in its complete isolation and unadulterated purity could be grasped only by an ultimate, most abstract
analysis. That analysis would then be the “sociology” postulated by Simmel, which would then start from the final results of other disciplines and constitute their “general part.” (1950: 26)

In effect, then, sociology becomes an aprioristic inquiry into “the element of the social” which takes as its material the notions of the social found in the special cultural sciences.

In one sense this is a transcendental problem on the first order, which is to say about “the social” as an empirical phenomenon. And Simmel’s own “sociological” efforts focused on such questions as the conceptual properties of particular forms of social relation, such as the dyad, and proceeded by elaborating various implications about the nature of the social relations conducted under these forms, which he exemplified through historical and anecdotal examples. A simple example of this, which happens to compare directly to Weber, is Simmel’s conceptualization of authority. Like Weber, Simmel’s account begins by placing it under a more general heading, in Simmel’s case “interaction,” Wechselwirkung (more literally “reciprocal effect” (Wolff 1950: lxiv), and distinguishes three kinds: subordination under an individual, a group, or an objective force (social or ideal) (1950: 190). The categories are elaborated to cover many topics, such as the subordination or exclusion of minorities by groups through “out-voting” (Wolff 1950: 239), and the potential role of conscience (Wolff 1950: 254-256) and considerations of objectivity with respect to appeals to principles (Wolff 1950: 256-261) in the case of subordination to an objective ideal force, such as an ethical principle. The status of this classification is muddy, and it seems both abstract and ad hoc. Nevertheless, it was grounded, in the sense this neo-Kantian strategy grounded its constructions, in the extant conceptualizations of the historians and interpreters in the special sciences, such as law, placed on facts. So the results represented a kind of analysis of pre-conceptualized material rather than simply stipulative definition. Weber, unlike Simmel, denied that his more famous classification of types of legitimate authority into traditional, rational-legal, and charismatic, was derived in this manner, or had any claim other than utility, and was thus free to simply stipulate.

The contrast between Weber and Simmel is in their attitudes toward the problem of grounding systems of concepts. In the framework of the neo-Kantian methodology with which Simmel and the other thinkers in this tradition operated, there was an ongoing ascent, comparable to the semantic ascent of later analytic philosophy, to the transcendental epistemic level, particularly to questions of “methodology,” meaning questions about the conditions for the possibility of knowledge of “the social” or of this “element.” This ascent, into what Simmel called philosophical sociology, had a different significance than semantic ascent. As Simmel puts it, philosophical sociology is “the level on which factual details are investigated concerning their significance for the totality of life, mind, and being in general, and concerning their justification in terms of such a totality” (Wolff 1950: 23), which is to say in terms of a closed system of concepts:

evidently, this type of question cannot be answered by the ascertainment of facts. Rather, it must be answered by interpretations of ascertained facts and by efforts to bring the relative and problematical elements of social reality under an over-all view. Such a view does not compete with empirical claims because it serves needs which are quite different from those answered by empirical propositions. (Wolff 1950: 24)
Thus Simmel’s project reflected the characteristic neo-Kantian idea that objectivity comes from raising particular issues to a higher level of abstraction under the discipline of the construction of coherent systems of concepts, nevertheless, he also grasped that this strategy was only partly effective in sociology. Issues over important questions of philosophical sociology, such as “Do meaning and purpose inhere in social phenomenon at all, or exclusively in individuals?” arise from conflicting world views or party positions, which abstraction and systematization alone could not resolve (Wolff 1950: 25). This kind of origin of the pre-conceptualization of the material on which both sociological and at the next level philosophical analysis worked left open the possibility of the construction of a variety of different systematizations, taking different starting points and reflecting different worldviews and values. For Simmel himself, such considerations damped any drive to systematization, and commentators saw him as “unsystematic entirely by intention” (Bohner quoted in Steiner 1999: 215), indeed as arguing that every system falsifies our thinking, because it freezes our thought. One can hear in this language of the rigidity of concepts and the non-rigidity of life echoes of Lebensphilosophie, in which Simmel, by the time he wrote this in 1917, was already engaged, and prefigurations of Heidegger.

One might wonder why the strategy of system-building had so many adherents, given its unpromising character as a solution to its supposed aim: providing a univocal grounding and conceptual structure for the science of sociology. There were perhaps two reasons. One was that these inquiries, which in Simmel’s case, for example, covered such topics as fashion, often produced gems of insight which did represent in some sense the fulfillment of the promise of the strategy. This was particularly true for Simmel himself, whose essays “The Stranger” (Wolff 1950: 402-8) and “The Metropolis and Mental Life” (Wolff 1950: 409-23) and his long study of money ([1896]1990) became classics. Moreover, these now forgotten systems of categories did not on the surface appear superior to the stipulative system of categories provided by Weber himself in his Economy and Society ([1968]1978), which introduced such enduring notions as charismatic authority, and which remains the most significant achievement of sociology and perhaps social science of the 20th century. But, as good neo-Kantians, these thinkers, with the exception of Weber, who differentiated himself decisively from them, could not conceive of a science that was not constituted by a closed framework of categories. So for them, there was no alternative to the a priori project, regardless of its propensity to merely produce sociological systems on analogy to (and for the most part not very different from) the philosophical systems still being produced in the departments of philosophy from which many of these sociologists had themselves decamped, or from philosophical circles with which they were associated.

If one accepts the methodological notion that to be a science one needs a coherent set of concepts and that these should be produced by the kind of ascent I have described here, the problem of multiple possible schemes, the problem of underdetermination, becomes a problem that appears to be soluble and perhaps can only be solved by the method of ascent itself, that is to say by “higher” epistemological or metaphysical considerations that decisively establish one scheme as genuinely basic. In this respect, these thinkers were like Einstein, accepting of underdetermination on the level of substantive sociology. Differences in worldviews were a fact. They nevertheless sought a perspective on the methodological or epistemic level that accommodated this fact while acknowledging the conceptual dependence of sociology on the pre-conceptualizations contained in worldviews. Weber’s approach to this problem, which became the focus of a large amount of commentary and critical reflection, was to sharply distinguish that which could be said to be general, which for him consisted of logic in the narrow sense and the...
kind of calculation necessary for making probabilistic judgments, and that which was not general, as a result of historically specific conceptual pre-constitution of the subject matter. He stressed the error of treating ideal-types, which for Weber were value-related constructions rooted in worldviews or the language of life, as though they were uniquely valid descriptions of substantive reality. He also argued that the constructions which his predecessors in the German historical school (or for that matter Marxists) claimed to have “deduced from reality” would not be any more than value-related constructions rooted in worldviews or the language of life and subject to the consideration that worldviews (and ordinary language, which for him represented a worldview) were valuative and thus arbitrary in origin. It was precisely his narrow view of “logic” that entailed that the Simmelian project and its variants were misguided: for him there simply was no “logic” in the extended sense that such projects required. Thus in effect Weber, like Einstein, embraced underdetermination as the last word.

This was an unusual and controversial conclusion, for reasons that are important for understanding the subsequent literature not only of German sociology but of the philosophy of social science generally. As I have suggested, the more typical response was to seek a second or third order resolution to the problem of alternative conceptual systems. The examples are legion, but a few of the more consequential ones are these: phenomenology, which aspired to some sort of grounding of social knowledge in fundamental considerations, but which settled for grounding in a phenomenological account of the conditions for the possibility of Verstehen (exemplified by Alfred Schutz, to be discussed shortly); the idea of objective culture, grounded in the “fundamental” considerations about the conditions for the possibility of “meaning” (Freyer 1998; Cassirer [1923]1955); solutions to the problems of the historical relativity of values or worldviews based on higher level theories (Scheler 1963; Horkheimer 1972; Mannheim’s relationism (1936: 78-83); a priori theories of the ontological superior status of society over the individual understood through liberalism; a priori accounts of action which supported the claim that laws of economics understood as a branch of sociology could be derived from the consideration that human action was “rational” by definition (Mises [1949]1963); and accounts based on philosophical anthropologies (Gehlen 1940).

The rapid development of a debate over social science in the Wiemar era was a result of the confrontation, in the form of critique, of these different strategies with one another, which produced a vast literature. Weber was the focus of much of this critical discussion, and the literature on Weber during this period (and long after) was concerned with such issues as these: Weber’s implicit fundamental ontology (Löwith [1960]1982); his implicit philosophical anthropology and concept of freedom (Landschut 1929); his assumptions about rationality (Grab 1927), the class and historical Weltanschauliche character of his fundamental concepts, including his rejection of dialectical “logic” (Lukács [1962]1980; Horkheimer 1972; Neurath 1959); the character of his underlying theory of values and the fact value distinction; the sufficiency of his (or any) sociological approach to the concept of law and legal validity (Kelsen 1945); his assumptions about the nature of human action (Mises 1960); and his claims about the nature and limitations of Wissenschaft, which produced a large controversy (Curtius 1919; Lassman and Velody 1989; Salz 1921). Weber’s defenders, notably Karl Jaspers, replied in kind, by elucidating (or inventing) and endorsing the underlying ontology, value theory, and so on which were claimed to be implicit in Weber’s thought (Jaspers 1989; Henrich 1987).

Phenomenology was the vehicle for many of the attempts to secure some sort of third-order grounding for particular systems. Alfred Vierkandt, who attempted to ground his account of
society in a phenomenological account of instincts, is perhaps the paradigmatic example of this strategy. Mises’ brutal critique of his efforts is revealing with respect to the difficulties which the strategy faced. Vierkandt rejected the individualist theory of action, and was in this respect a typically German anti-liberal. As Mises comments, “he is unable to support his rejection of the latter [i.e., the individualist theory of action] except by repeatedly referring to the rationalist, individualist, and atomistic character of everything that does not meet with his approval” (1960: 56). Of course, from Vierkandt’s point of view, this derogation of assumptions he rejected was an exercise in showing liberalism to be ideological, and leveling the playing field. His own position was, as Mises characterizes it, that

human society is, so to speak, already foreshadowed in the relationship of the master to the dog he trains. The relationship of leader and led corresponds to the relationship of master and dog: it is healthy and normal, and it is conducive to the happiness of both, the master and the dog. (1960: 56)

Mises notes that “one cannot argue this point further with Vierkandt because, in his view, the ultimate source of cognition is

phenomenological insight, i.e. what we directly experience personally in ourselves and can convey to consciousness with apodictic [i.e. incorrigible] evidence. (Vierkandt quoted in Mises 1960: 56)

Thus did the project of second and third-order grounding reproduce the conflict between systems, and impel much of German sociological thinking into a project of the analysis of presuppositions.28

5.2 The problem of the a priori II: action and normativity

The most consequential phenomenological study from this era was Alfred Schutz’s The Phenomenology of the Social World ([1932]1967), a classic work in the philosophy of social science in its own right. Schutz was a member of the Mises circle and a childhood friend of Hans Kelsen, rather than a member of the Weber circle, and his approach reflects this. He tells us that he became convinced of the correctness of Weber’s approach ([1932]1967: xxxi) but believed that “his analyses did not go deeply enough to lay the foundations on which alone many of the important problems of the social sciences could be solved” and went on to add that only in Bergson and Husserl, “and especially in Husserl’s transcendental phenomenology, has a sufficiently deep foundation been laid on the basis of which one could aspire to solve the problem of meaning,” which he took to be the main unfinished task left by Weber. ([1932]1967: xxxi-xxxii). Yet even Husserl, he thought, had not yet solved the problem of the “Thou,” of genuine interactional knowledge of other minds, so he proposed to go forward, in the absence of secure foundations, by way of a critique of Weber ([1932]1967: 97-8). Weber’s account did not require a “Thou” understanding, but a “They” or third person application of categories of intentional action descriptions, in the form of ideal-types. Ideal-types are anonymized logical constructs ([1932]1967: 183, 189) that operate from the point of view of the interpreter and have nothing directly to do with the actual subjective states of the interpreted agent ([1932]1967: 188). They supply meaning rather than discover it ([1932]1967: 190). Schutz notes Weber’s preference for
rational action ideal-types, a preference which Weber bases on the consideration that interpretation seeks *Evidenz*, and that the ideal type of rational action is the type that is most clear and distinct. This, together with the “they” character of his interest in action, decisively separates him from Dilthey:

> We must never cease reiterating that the method of Weber’s sociology is a rational one and that the position of interpretive sociology should in no way be confused with that of Dilthey, who opposes to rational science another, so-called “interpretive” science based on metaphysical presuppositions and incorrigible “intuition.” (Schutz [1932]1967: 240)

The issue that concerns Schutz, however, is one that arises between Weber and other practitioners of rational science, particularly Mises, and through him, as we shall see, Popper and, the rational choice theorists, and ultimately Davidson.

Schutz revised Weber in various small ways, one of which is consequential for what follows. He objected to Weber’s way of formulating the distinction between adequacy at the level of meaning and adequacy at the level of cause. He notes the perplexing apparent pointlessness of the notion of causal adequacy: “If I start out from a real action as my datum, then every ideal-type construct that I base on it will already be in itself causally adequate” ([1932]1967: 232). Moreover, because having happened, it had to have happened causally. Rational action is a matter of choosing means that are appropriate to ends. To choose rationally would be to do so in accordance with past experience of the relevant causal relations. To do this requires the agent to conceptualize of past experience into “typically comprehended meaning-adequate relations” ([1932]1967: 233), thus past causal experience enters into the conceptualizations of these typifications. Thus causal adequacy, rather than being an independent criterion, is “a special case of meaning adequacy” ([1932]1967: 234). This argument removes an important obstacle to collapsing the whole problem of the explanation into considerations of interpretation.

A new issue then becomes apparent. What if there were, so to speak, a universal solvent to the problem of interpretation, an interpretive scheme which assured that actions could be understood at the highest degree of rationality? Weber assumed that there was and could be no such thing: for him, ideal-types of action were retail affairs, close to the language of life of the historically specific audience for the analyses, and useful for the limited purpose of making sense to this audience. They worked by abstracting from the details of individual action in a specific way: by selecting out and emphasizing certain features of the action shared with other actions, and often do not perfectly fit actual cases. Mises claimed to have something better with marginal utility theory, which he characterized as follows:

> For a long time men failed to realize that the transition from the classical theory of value to the subjective theory of value [i.e. the principle of marginal utility] was much more than the substitution of a more satisfactory theory for a less satisfactory one . . . It is much more than merely a theory of the “economic side” of human endeavors and man’s striving for commodities. It is the science of every kind of human action. Choosing determines all human decisions. In making his choice man chooses not only between various material things and services. All human values are offered for option. All ends and all means, the noble and ignoble, are ranged in a single row and subjected to a decision which picks out one thing and sets aside another. Nothing that men aim at or want to aim at remains
For Mises, the rationality of human action, understood as preference-fulfilling choice, is an *a priori* truth. Weber, in contrast, had treated marginal utility and self-conscious rational choice of means as ideal-types (even as distinct ideal-types) among an array of other ideal-typifications that allow us to understand action, meaning to describe it in terms of its subjective meaning in the language of life. One can ask whether, from Weber’s point of view, marginal utility necessarily played a secondary role because an account in terms of marginal utility, which relies on hypothetical estimates of subjective values and an assumed machinery of choice, is significantly less transparent and intelligible than an account in terms of the conscious rational selection of ends. Indeed, the marginal utility account gets its intelligibility by analogy to the case of conscious decision, not the other way around.

Weber makes an additional distinction between these cases of instrumental (or Zweckrational) rational action and other kinds of action, including a) traditional actions, which are on the borderline of meaningful action, shading off into pure habit; b) purely affectual behavior, also on the borderline, in this case to mindless reaction, and often a case of semi-rationalized sublimation; and c) actions guided by the "deliberate formulation of ultimate values" using a systematic philosophical technique, which is a specific type of “rationalization” ([1968]1978:30). His point about the boundaries between action and non-action is crucial: because intelligible action shades off into mindlessness, it may be intentional in a limited sense, and is therefore only “action” in a limited sense. Indeed, he gives a number of examples of action that is rooted in biology, in which the strength of the reaction can be accounted for by the cooperation of biological causes, such as a biologically rooted reaction to deviance (cf. Turner and Factor 1994: 88). Here the category of “action” shades off into the category of biological causation. From Weber’s point of view, then, rational action in the full sense of self-conscious articulate decision is rare, most “action” only approximates the ideal-types, and often, perhaps typically, actions are causally influenced by biological causes, habituation, and so forth, and thus can only approximate the interpretive ideal-types placed on them. The focus of his definition of action is thus subjective meaning, but meaning is not everything. Other causal elements play a role, and thus there is a difference of degree rather than kind between fully intentional and non-intentional. For Mises the focus is exclusively on the fact of decision.

Schutz deals with this wide-ranging conflict in terms of the conflict over the principle of marginal utility, which Weber treats as an ideal-type and Mises as an objective truth. The difference is significant, and Mises stresses a particular aspect of it. For Weber, all ideal-types are “historical” or at least potentially transitory in their utility— they are ideal-types “for us,” that is to say for our particular historical interpretive needs. “Rationality” is an interpretive framework that is especially important to us, as moderns, and making sense of the actions of past figures in these terms is useful and necessary for us, since this is our language of life. The idea that rationality is a potentially transitory framework is not odd for Weber, since the techniques of rationalization, such as the rationalization of accounting through double-entry bookkeeping or the rationalization of Continental law through techniques of law-finding which allow for the extension of legal principles consistently to new circumstances, are, quite clearly, historical products which may not have achieved their final form and in many cases vary from one place to another even in the present. The relevance of marginal utility to these cases is questionable at best. What Mises
objects to, however, is the implication that the laws of economics, which he takes to be a logical consequence of the principle of marginal utility, are not universal. The principles of rational action, Mises says, though “they are acquired by means of abstraction, which aims at selecting for conceptualization certain aspects of each of the individual phenomenon under consideration” (quoted in Schutz, [1932]1967: 243), result “not in a statement of what usually happens, but of what necessarily must happen” ([1932]1967: 245). For Mises, this was an implication of the principle of marginal utility itself, which Weber endorsed, but seemed not to consider employing except in specifically economic contexts. Thus, for Mises Weber is inconsistent or unaware of the real significance of the principle.

This is the conflict Schutz addresses. He accepts that, from an epistemic point of view, Weber is correct to see rationality as an ideal-type, in the sense that it plays a role in constituting its object of interpretation as an intelligible action. But he agrees with Mises that the principle of marginal utility is not the sort of thing that it makes sense to think of as non-universal or historically transitory, and thus nonobjective. He solves the conflict in a Viennese manner that the Vienna Circle itself might have suggested: by reinterpreting Mises as making the principle of marginal utility into a stipulative definition of economic action ([1932]1967: 245). Its objectivity is thus the a priori “objectivity” of the definitional relation between the principle and the term “economic action.”

Mises went even farther than “economic action,” however, noting that the principle had the effect of obliterating the distinction between economic and non-economic action because all choice involves considerations of the scarcity of means (1960: 61), thus for him all action is rational by definition.

The kinship between these claims and later discussions of reasons and causes and rationality are extensive, but I will limit the discussion to a brief overview. One can broadly distinguish between wholesale and retail versions of the problem of the explanation of action. Wholesale versions, like that of Mises, associate the whole of human action with the model of rationality, and replace the problem of explaining action with the problem of assimilating action to the model of human rationality. The justification for this is as follows: identifying a piece of behavior as action already amounts to identifying it as rational and teleological. The only appropriate next step in accounting for the act as an act is to subsume it more fully into the model of rational action itself, for example by more fully specifying the matrix of decision which makes the act “rational.” This “method” closely resembles Weber’s discussion of the problem of constructing and explaining the errors of generals in battle:

The more sharply and precisely the ideal type has been constructed, thus the more abstract and unrealistic in this sense it is, the better it is able to perform its functions in formulating terminology, classifications, and hypotheses. In working out a concrete causal explanation of individual events, the procedure of the historian is essentially the same. Thus in attempting to explain the campaign of 1866, it is indispensable both in the case of Moltke and of Benedek to attempt to construct imaginatively how each, given fully adequate knowledge both of his own situation and of that of his opponent, would have acted. Then it is possible to compare with this the actual course of action and to arrive at a causal explanation of the observed deviations, which will be attributed to such factors as misinformation, strategetical errors, logical fallacies, personal temperament, or considerations outside the realm of strategy. Here, too, an ideal-typical construction of
rational action is actually employed even though it is not made explicit. (Weber ([1968]1978): 21)

What distinguishes the two is this: Weber rejects any claim that the model of rational action is necessarily universally relevant. It is merely a contingent fact for him that rational explanations provide the greatest clarity of understanding and it may be that in many cases, for example of actions done out of emotion, they provide little understanding. Understanding is a retail affair: there is no universal device for understanding, but only typifications useful in particular situations and for particular audiences. Where does Popper’s account fit? Popper does nothing to ground the model of rationality that defines the logic of the situation in a more general *a priori* claim about human action, as Mises does. But at the same time he does not envisage an alternative to “logic of the situation” analyses of human action. Also implicit in Popper and Mises is the idea that an adequate rationalization excludes “causal” considerations of the usual sort, which is to say the kind that need to be established empirically. As Mises puts it, “The causal propositions of sociology are not expressions of what happens as a rule, but by no means must always happen. They express that which necessarily must always happen as far as the conditions they assume are given” (1960: 91). The “necessity” in question here is, presumably, logical, and the validity of the claim to necessity rests on “the cognition of what is essential and necessary in every instance of human action” (1960: 90-1). This same kind of reasoning, it may be observed, reappears in Davidson under different auspices, notably the indispensability of the axioms of decision theory for any description of action.

“Retail” versions of this idea that adequate rationalizations preclude causal explanation or render it gratuitous appear in Schutz himself. As noted, he argues that causal adequacy is a special form of meaning adequacy, rather than an additional consideration. Thus a fully adequate “meaning” characterization of an individual action would include and subsume all relevant causal considerations. Parallel “retail” versions of this account appear throughout the reasons and causes literature of the fifties and sixties. A description of an action in terms of its intentions, in this literature, cannot be causal because the relation between intentions or reasons and the action that is intended is an internal or logical “in order to” relation rather than a causal one. But at the same time an intentional action can be intended only if the intender meets the following test: the act must be intended under a description that comes from the stock of descriptions available to the agent. This stock of action descriptions is, so to speak, a collection of retail items. But once one has described the act intentionally, there is no place for causal explanation.

The reasoning in this literature is slightly different from that of Schutz and Mises, but the results are similar. Identifying an action as an action is already a matter of ascribing not only intentionality, but a specific intention, as there is no test of intentionality apart from the identification of a specific intention. A full description of an intentional act qua action thus contains or refers essentially to the outcome to be explained. The sentence a) “John drove to the store in order to get a bottle of milk” contains a description of the act to be explained. The sentence b) “John drove to the store and John got a bottle of milk,” in contrast, merely describes a pair of events, not an intentional act, and is not explanatory. But “explain” is a problematic and gratuitous notion here. Explanation requires the independence of the explainer and the outcome. Here there is no independence. A description in which the two events are independent, such as b), is not explanatory, since going to the store is not a cause of getting the bottle of milk.
The relationship between the intention and the outcome, however, is more than this. A sentence correctly ascribing an intention to ordinarily implies that the action will take place, or to put it differently, the evidence for the intention ordinarily, if the intention is correctly carried out, includes the thing to be “explained,” namely the act that is the outcome. The relation between intention and act is a relation we recognize as correct, as valid or intelligible, and is thus in present parlance “normative” rather than causal. When we recognize an act to be the fulfillment of a given intention we are, so to speak, recognizing the validity of the fulfillment. Ascribing intentions is a “retail” process, in this sense: attributing intentions is not backed by a more general account of rationality, but by the normative considerations particular to the application of specific intentional attributions, the considerations that make an outcome a correct fulfillment of an intention. As with Weber’s meaning-adequate ideal-types, this provides intelligibility to action by fitting facts to a typification drawn from a large tool kit of intelligible singular action descriptions: what came to be called the stock of descriptions available to an agent (cf. Anscombe 1958; Winch 1958; Turner 1980; Turner 2003b; MacIntyre 1962).

5.3 Hidden apriorism

The problem of the \textit{a priori} is the ur-problem of 20th century philosophy, yet it is also, in the context of the philosophy of social science, one of the most confusing. Winch was explicit in endorsing the thought that social science was an \textit{a priori} inquiry, or more precisely an inquiry of an \textit{a priori} kind, namely the elucidation of concepts, into an \textit{a priori} subject, namely the concepts of a particular group. But this seems odd in many ways. Is there no empirical casual knowledge in addition to knowledge about concepts? If not, then what is it that social researchers produce? The rationality of action is difficult to conceive in other than an \textit{a priori} way, so if rationality enters into explanation, it seems, the explanation is itself \textit{a priori}. Or should we say that none of this is, properly speaking, explanation at all, but only understanding, and that there is no explanation in social science? If the backing that we can supply to our singular explanations, such as our explanations of actions by reference to intentions consists of claims about rationality, it seems that we should say that there is no explanation in social science. Attempts to reconcile the two have not been satisfactory. Hempel, when he turned to the problem of rational action, converted rationality into a “broadly dispositional state,” that is to say into something causal, to avoid a conflict (1965: 472), a solution that persuaded almost no one (cf. Davidson 1980a: 272-75).

The idea that social science has its own special “presuppositions” in the neo-Kantian sense, that is to say as distinct from stipulations or definitions, is another source of puzzlement, as we have seen. Is there a correct set? And if so, how does one determine what it is? Phenomenology, as we have seen, leads to results that are both diverse and in some cases strange, as with Vierkandt. And the fact that these presuppositions may also be shared with ideological movements raises the question of whether acknowledging the role of such presuppositions in social science amounts to the admission that social science is fundamentally ideological and that all claims are true relative to ideological presuppositions.

Popper, in a section on Mannheim in \textit{The Open Society and its Enemies}, dismissed the search for presuppositions as irrationalist, and suggested that Einstein’s success had shown that the neo-Kantian problem of frameworks of thought was trivial, because frameworks were shed and replaced every time a theory was replaced by a better one (1962: 220). In the context of physics, this makes some sense, though it is not clear that the process of shedding the old framework is quite so trivial. But in the context of social science, shedding frameworks is not
even the same problem. As we have seen, Weber made the point that the conceptualizations we employ are dictated by our interests, meaning the interests of the audiences to whom we direct our explanations as well as the interests we define when we specify the cognitive purposes of a discipline, and he also made the point that we are faced with a generic problem of changing the subject that prevents us from creating an “astronomy,” a social science in a language other than the language of life, that would answer questions posed in the language of life. Changing this framework, in the context of historical questions, is changing the subject.

The problem of changing the subject is so deeply bound up with the project of sociology that it requires its own discussion. When Comte invented the term sociology, he formulated his main “sociological” law of three stages, in which thought in a given domain predictably passed through three stages, the theological, the metaphysical, and the positive, in which it ended. His thinking reflected the fact that the subject domain of sociology was so to speak, already occupied. There were what he called theological and metaphysical concepts that were already part of ordinary moral and political discourse characterizing the relations of dependence between individuals. To take this domain and subject it to scientific understanding required that the theological and metaphysical content of these concepts be drained from them, leaving classifications which enabled the making of nomic predictions. This idea was transformed by Pearson and Giddings into a model for sociology as a statistical discipline, something that Comte did not envision, and indeed was hostile to. Durkheim too was a careful reader of Comte and there is more than an echo of Comte in his idea that social facts in the real causal sense are concealed and obscured by our ideas taken from the marketplace, as he quotes Bacon, and his insistence that studying society using these ideas results not in an investigation of society but in an investigation of the implications of the ideas in an aprioristic project rather than a scientific one. Yet each of these thinkers was compelled to deal with the fact that their starting point was in some sense dictated by the language of life, as Weber put it.

Durkheim, despite his overt hostility to apriorism in sociology, took for granted that the primary thing which sociology was concerned to explain was the fact of obligation. Ellwood, as we have seen, recognized that this choice was itself a piece of apriorism. Yet from a Comtean point of view this is a legitimate project. If we understand obligation to be a pre-positive concept that will ultimately be replaced on a scientific one and are aware of the snares of thinking of obligation in an *a priori* way, as is done in Kantian ethics, for example, we can loosely say that sociology seeks to explain obligation, meaning instead that sociology seeks to replace our deluded, superstitious concept of obligation with a scientific one. But one may ask whether Durkheim’s acceptance of “obligation” as a topic amounted to acceptance of a problematic *a priori* starting point. Durkheim’s contemporary, the Swedish philosopher Axel Hägerström, who was himself emancipating his thought from neo-Kantianism, dealt with legal obligation as a fiction to be explained by other means, by identifying the magical sources of Roman legal thought and the magical notion of obligation on which it rested, criticized the philosophical reconstruction of these magical ideas in terms of the concept of the will of the sovereign or the will of the people as the basis of legal obligation, and replaced them with a notion of law as fact in which only the predictive aspect, the element of expectation, remains once the metaphysical elements had been drained away.

Durkheim did not go in this direction, or at least as far. And indeed there is a generic problem here which the contrast between the two points to. One response to Hägerström, which became standard in the philosophy of law, is that Hägerström had failed to explain law because he
failed to account for this essential feature of bindingness. Thus he had in effect explained nothing, but merely changed the subject. Hägerström’s point, of course, was that the binding element of the law is not constitutive, except in a revisable sense, but rather entirely mythical and therefore not something which need be “accounted for” as anything other than the error and illusion what it was. The fact that 20th century legal philosophy chose instead to stick with the notion of obligation without ever getting a particularly satisfactory account of it indicates how sturdy the metaphysical notion of obligation and the law has been. Durkheim was more chary of the problem of changing the subject. He was thus more respectful of obligation and took it as one of the givens to be explained. But here we see the delicacy of the problem of the a priori definition of subject matter. It is unclear what general grounds we might have for accepting a topic as in some fundamental sense genuine and part of the factual world to be accounted for and when we are entitled to ignore it or treat it as delusion and error. Slight differences matter. Spencer, for example, took “feelings” of obligation as the “data” of ethics, without taking obligations themselves as data. But it is questionable whether these substitutions work and some substitution seems unavoidable. Even Durkheim did not pretend to explain obligations in their own terms, but rather the hidden social fact of obligation which produced the feelings.

There was a body of criticism that directly addressed the problem of substituting “sociological” concepts for normative concepts. This line of argument appears in Schutz’s 1932 work, where he repeats the claims of his childhood friend Kelsen. Kelsen’s argument against Weber with respect to the law foreshadows the “normativity” issue of the present.31 Weber distinguished between “dogmatic” and “historical” questions about the kinds of events that figure in the history of law. The question of whether, say, the donation of Constantine, was a legal “fact” or “valid” was for Weber a dogmatic question appropriate to legal scholarship, but a quite different question when taken as an explanation of the actions of historical agents who believed, however wrongly, in its validity. But what about facticity of the law itself? The dilemma here is this: either it is a “legal fact” or not; if it is not, however, we are no longer talking about law, but about something redescribed in such a way that it is no longer law, or no longer identical with law. Kelsen put this directly, and in terms that might be taken from the writings of Joseph Rouse or John McDowell today: the sociological conception of the law depends on the normative conception. Thus there is not and cannot be a non-normative sociological study of the law.

Kelsen’s reasoning is this. Weber defines law in terms of certain beliefs in legality together with the probability that the law will be followed. This is to say that, in the end, there is nothing “sociological” to the law but effective acceptance, or legitimacy. For Weber additional claims about legality are “dogmatic,” that is to say part of the legal discipline of the law, but are gratuitous for the explanatory purposes of sociology. To say of a particular pattern of enforcement and command that is believed to be legitimate or legal and that is actually effective in the sense that it probabilistically predicts the behavior of the participants, that in addition that it is “really” law adds nothing to either prediction or to understanding. The subjective meaning of the acts is contained in the agents’ beliefs about the pattern; the causal part is established by the patterns that allow probabilistic prediction of their behavior. One of Kelsen’s replies to this is specious: he comments that this cannot be a definition of the law because the criminal does not need to have the law in mind for the criminal’s act to be a crime. Weber is not committed to this either, but is only committed to some of the agents holding these beliefs and some probability of the beliefs being acted on. Moreover, he is not concerned with whether the criminal’s act is a crime: this is self-evidently a “dogmatic” question. But whether it is a crime, and the difference between
“some” people and “everyone,” is important for theories which aspire, as Durkheim, Bouglé, and the later Sellars did, to derive the “fact” of obligation from “collective” commitment.

Kelsen makes another claim that is more interesting. He argues that to explain the law in terms of the beliefs of the subjects of the law is not to explain the law, because the question of what is law is not a matter of public opinion, but rather a legal question, which can only be settled by legal considerations. Indeed, beliefs about what is law can be false. So to explain the law, which is to say what is genuinely law as distinct from the various things that people believe, however erroneously, about the law, requires attention to what it is that makes the law genuine. The thing that makes the law genuine, the specific legal considerations mentioned above, are themselves statements of law determining legality. As Kelsen asks,

Is a constitution republican, for instance, merely because it announces itself as such? Is a state federal merely because its constitution calls it such? Since legal acts usually have a verbal form, they can say something about their own meaning. This fact alone betrays an important difference between the subject matter of jurisprudence, indeed of the social sciences as such, and the subject matter of the natural sciences. We need not fear, for instance, that a stone will ever announce itself as an animal. On the other hand, one cannot take the declared legal meaning of certain human acts at their face value; to do so is simply to beg the question of whether such declared meaning is really the objective legal meaning. For whether these acts are really legal acts at all, if they are, what their place is in the legal system, what significance they have for other legal acts– all these considerations will depend on the basic norm by means of which the scheme that interprets them is produced. (Kelsen quoted in Schutz [1932]1967: 246, emphasis added in Schutz)

This notion of the basic norm, in the modified form of the concept of rules of recognition that determine what is law, was taken over by H. L. A. Hart. This idea fits jurisprudence: there is indeed a judicial procedure of determining what is law and what is not.

One may question, as Weber would, whether introducing this concept changes the situation in the intended way. The Grundnorm, from the sociological point of view, consists of nothing but belief in the validity of the judicial procedure itself, and some probability that decisions, in this case by judges with respect to legality, are made in accordance with it. Hart recognized that the idea that legal norms could be created by utterance in this way was strange, and seized on J. L. Austin’s notion of performative utterance to replace it. But one may question whether the notion of performative utterance makes any sense without the presence of beliefs in the legitimate powers of the performers— in the case of the law, Kings or legislators— to make such commands. And one may then ask whether there is anything explanatory added by discussions of whether the powers are “genuine” as opposed to “believed in and thus effective.”

6. Functionalism and Parsons’ Synthesis

In a different form, the issue of normativity did, in the middle of the century, have very large, and as I shall suggest, continuing impact, through Parsonsianism. To understand this form of the problem of normativity it is necessary to begin with the very large body of thought that might be given the label “functionalist,” which culminates in the attempt by Talcott Parsons to organize the social sciences under the concept of “system.” Parsons’s attempt drew on and
incorporated both American “cultural anthropology” and British “social anthropology,” and
combined this with the “culture and personality” anthropology of the Freud-influenced middle
part of the 20th century. But it did much more, and its consequences for the history of not only
sociology, but anthropology and area studies, were substantial.

Anthropological research employed “function” as an organizing idea, in which such things
as rituals were interpreted as serving hidden or misrecognized purposes. Functional explanations
appeared to provide novel explanations of mysterious facts, namely the apparently pointless or
misguided rituals of primitive people. But much of this “explanation” was no more than the
invention of hypothetical teleologies leading to some supposed good, such as the Durkheimian
purpose of increasing social solidarity. There were differences in emphasis, between analyses that
emphasized functions for society as a whole and those that emphasized the collective meeting of
basic human needs. As “theory” these ideas, such as the idea of the functional requisites of
society, were truistic or definitional, and the late in life attempts by such figures as Bronislaw
Malinowski to formulate functionalism as a theory were unable to go beyond such results as these
“axioms”:

A. Culture is essentially an instrumental apparatus by which man is put into a position
where he is better able to cope with the concrete specific problems that face him in his
environment in the course of the satisfaction of his needs.

B. It is a system of objects, activities, and attitudes in which every part exists as a means
to an end. (1944:150).

Such claims were non-explanatory, and non-empirical: they were true, to the extent that they
could be said to be true, by definition, or were definitions is disguise. The definitions, however,
rested on a problematic intuition, the idea that institutions or customs that persisted over time
must serve purposes or they (or the societies of which they were a part) would fail to persist.32
This notion was an inheritance of the organic analogy, which contributed many other ideas as
well, such as the notion that societies were homeostatic, equilibrium seeking beings. But, like the
organic analogy itself, it was difficult to make it into more than an analogy. Societies did not
“die,” and the “ends” of “society,” such as solidarity, were hypothetical. The development of this
body of thought also presents difficult historiographic problems in relation to the philosophy of
science because there was no single strand of methodological reflection that paralleled it or
informed it. Indeed the philosophical affinities of organic and functional thinking changed
frequently, and the issues were often mixed up with considerations from other areas of
philosophy, notably political philosophy and ethics, as well as metaphysics. Alfred North
Whitehead’s account of the “organic” in his metaphysics, to take one especially outré example,
was one source cited by Parsons (1937: 32).

One may wonder why this mishmash of ideas became, as it did become, overwhelmingly
dominant in the middle part of the 20th century. Part of the answer has to do with the person and
position of Talcott Parsons, its main leader, his unique situation, and the role of his thought in the
definition of the disciplinary identity of sociology. Parsons was trained as an economist, but in the
German historical fashion, and was soon marginalized in the Harvard Department that originally
hired him. He found other academic protectors at Harvard, notably in connection with a group
known as the Pareto Circle, which was an interdisciplinary reading group with a membership
including various powerful Harvard grandees, such as L. J. Henderson and Walter Cannon.
Members of this group were central to the transformation of Harvard into a modern research university. Parsons was a remarkably adept political player. By the forties he had created his own department, called Social Relations, based on his model of social science theory, and including portions of psychology and anthropology, and had a significant role in the creation at Harvard of newly created fields of area studies, which also transmitted his ideas. He was generously funded in the late forties by the Carnegie Foundation. And the international impact of his thought was greatly increased by the new dominance that American social science had after World War II.

The intellectual basis of this enormous institutional success and intellectual influence was, remarkably (and for sociology unprecedentedly), a project in “theory.” How did Parsons construct such an influential set of ideas out of the unpromising and theoretically thin material of functionalism? The “philosophical” background provides some answers to this question. It would be a mistake to take the standard version of the story of the rise of functionalism and systems thinking provided by Parsons himself too seriously. But it is the inevitable beginning point. In the thirties, Parsons wrote an influential study, *The Structure of Social Action* (1961), which purported to be an empirical study of key thinkers in the social sciences, Marshall, Pareto, Durkheim and Weber, and claimed that between these thinkers there was massive historical “convergence” toward a model of social action that Parsons described in the book-- Parsons’s own model. But Parsons did not claim the model for itself: he claimed instead to be merely describing the “emergence of the theoretical system” (1937: 14). In many respects, however, this was a project of second order synthesis between approaches which closely resembled the *a priori* system-building of his German contemporaries, of which he was well aware. As a synthesis, it was a genuinely remarkable effort: the huge differences between Durkheim, Weber, “positivism,” and the model of rational action of modern economics have ordinarily been seen as examples of incommensurability and an argument for irreducible theoretical and methodological pluralism. Parsons took on the task of showing that they could be reconciled into a common “conceptual scheme.”

The salient feature of Parsons’s model of human action was normativity, and in particular his own attempt to resolve the conflict between what he called “idealism” and “positivism” (1937: 282). This attempt involved a rejection of positivism in the form of a reduction of values to something else, an argument with striking similarity to the normativist claims discussed in the last section. “The inner sense of freedom and moral choice,” he argued, “is just as ultimate a fact of human life as any other, and its consequent, moral responsibility. In fact, a psychological explanation of moral obligation really explains away the phenomenon itself” (1937: 290), and he specifically mentions the problem of the binding character of obligation which he takes to imply “metaphysical voluntarism” (1937: 289-90). The facts of morality thus implied that “the world of ‘empirical’ fact must only be a part, only one aspect, of the universe in so far as it is significant to man. . . . it is something transcending science” (1937: 290). Ends, in short, were real, essential to the proper understanding of action, and irreducible to the “scientific.” With this, he seems to have dismissed Mises’s notion that everything could be assimilated to the subjective theory of value, and indeed he makes comments to the effect that certain ends could not be understood as culminating in subjective states (1937: 288). The relation of this thesis to Weber was complex. Placing values on the side of “reality” distanced him from Weber, who assimilated them to choice. Yet Parsons was driven to his notion of the reality of “moral obligation” and values by the same means-end model that Weber employed in this account of *Zweckrationalität*: that ends are "precisely the element of rational action that falls outside the schema of positive science"
Where Weber had distinguished this-worldly and otherworldly ends, Parsons called the latter “transcendental ends” (1937: 219). And, crucially, he dissents from Weber’s view that the pursuit of ultimate ends leads not to a single good but to a situation analogous to polytheism, a kind of value pluralism (1937: 294).

Parsons claims to have “cogent reasons” (1937: 294) for rejecting value pluralism. But these turn out to be arguments relating to social order. In a situation of value pluralism, would be . . . a war of all against all—Hobbes’s state of nature. In so far, however, as individuals share a common system of ultimate ends, his system would, among other things, define what they all held their relations ought to be, would lay down norms determining these relations. . . . In so far, then as action is determined by ultimate ends, the existence of a system of such ends common to the members of the community seems to be the only alternative to a state of chaos— a necessary factor in social stability. (1937: 295)

Parsons goes on to claim that there is “much empirical evidence that such systems of ultimate ends exist and play a decisive role in social life” (1937: 295). He also identified a psychological basis for the link between action and “common ends,” “the fact of experience that men . . . in some sense try to conform their action to patterns which are, by the actor and other members of the same collectivity, deemed desirable” (1937: 76).

This “argument” is faulty on many levels. The “in so far” clauses are ambiguous between the analytic claim that each individual action with an end has an ultimate end, the claim that communities share systems of ends, and the hypothetical conditional applying to those communities, which may or may not exist, which have such a system of ends. Parsons can establish the first by definition, but the remaining claims do not follow and do not even seem to be plausible. As it stands, the reasoning is a variant on the erroneous leap, often attributed to Aristotelians, from the idea that all chains must end somewhere to the idea that there is a somewhere that all chains must end— in this case end in social values that are a guarantor of social stability. Weber, in contrast, thought that the simple fact of a common interest in a variety of intermediate ends, such as the authority of the state, provided a sufficient basis for social stability without any need to share ultimate ends, and also that “ideal” elements played a minor role in the cultivation of the stable patterns of action, and that habituation, convenience, self-interest, and rational responses to power were largely sufficient as explainers.

The conceptual analysis in *The Structure of Social Action* was basic to the first step in this argument— that all action was ultimately and essentially oriented to the valuative. As we have seen in connection with Mises, however, there is a problem with the logical status of such claims. Are they merely definitional and stipulative? Ideal-types? Parsons’s answer to this question was in terms of the concept of conceptual schemes. Normative considerations were part of a complete conceptualization of social action. And this produces its own puzzles, in the form of a question about what he meant by a “conceptual scheme” and what sort of necessities attached to one, and why (and how) completeness was a desideratum. Parsons did not attempt to provide any philosophical explication of this project, but rather took the view that his own activities as a “theorist” made sense to him, and that others could come up with a suitable philosophical rationale for the completed project. However, there was a basic motivating idea: providing a conceptual scheme was understood as part of the project of making sociology a science.
Parsons was acquainted with the neo-Kantian model of conceptual refinement, which he imbibed as a student in twenties Heidelberg. He was also well-aware of Whitehead’s Harvard writings of the twenties, which he cited repeatedly. But the term conceptual scheme has a specific Harvard history apart from these sources. It was a key term of his sponsor Henderson and part of a well-developed view of science presented in Henderson’s Harvard lectures and various other writings, also cited by Parsons (Parsons 1937; Henderson [1932]1970; cf. Henderson 1970). Henderson, like Parsons and Merton, quoted Whitehead with approval. Yet Henderson also quoted Percy Bridgman and Carnap with approval, especially in the 1932 philosophical article cited by Parsons. The thesis that claims about reality are meaningless is taken from Bridgman, on the grounds that “no operation can be agreed upon as a definition of the word reality” (Bridgman cited in Henderson 1970: 167), and the notion of meaningfulness is applied to a table of claims, divided into factual and meaningless, in which the notions of meaningfulness and having no corresponding operation are assimilated to Pareto’s notion of non-logical conduct (1970: 179-80). “Conceptual schemes” figure in this account as well: Poincaré, Einstein, and the error of “endowing the conceptual world with absolute qualities” (1970: 165). Henderson’s discussion of “fact” ([1932]1970), quoted at length by Parsons, defined it as “an empirically verifiable statement about phenomena in terms of a conceptual scheme” (quoted in Parsons 1937: 41). The concept was a precursor to Kuhn’s notion of paradigm, and shared the basic idea. Whitehead’s discussion of the desiderata for systems of speculative philosophy is a fair statement of the desiderata for conceptual schemes as well.

Speculative Philosophy is the endeavor to frame a coherent, logical, necessary system of general ideas in terms of which every element of our experience can be interpreted. By this notion of “interpretation” I mean that everything of which we are conscious, as enjoyed, perceived, willed, or thought, shall have the character of a particular instance of the general scheme. Thus the philosophical scheme should be coherent, logical, and, in respect to its interpretation, applicable and adequate. Here “applicable” means that some items of experience are thus interpretable, and “adequate” means that there are no items incapable of such interpretation. (Whitehead 1929: 5)

This formulation, with its special stress on “logic,” necessity, and completeness fits Parsons’s actual form of argumentation closely. Parsons did not package his thought as speculative philosophy, though in practice he worked, as speculative philosophers and neo-Kantians did, post hoc and on pre-existing conceptual material, and he wanted to provide the basis of a science, like the German system-builders. But the science to which he aspired was of a quite different kind. In The Structure of Social Action itself we are provided with pages of equations (1937: 78-82). And in “The Present Position and Prospects of Systematic Theory in Sociology (1945),” we are told that the millennium has arrived:

Sociology is just in the process of emerging into the status of a mature science. Heretofore it has not enjoyed the kind of integration and directed activity which only the availability and common acceptance and employment of a well-articulated generalized theoretical system can give to a science. The main framework of such a system is, however, now available, though this fact is not as yet very generally appreciated and much in the way of
development and refinement remains to be done on the purely theoretical level, as well as systematic use and revision in actual research. It may therefore be held that we stand on the threshold of a definitely new era in sociology and the neighboring social science fields. ([1949]1954: 212)

If this sounds like the proclamation of a Newtonian revolution, it is no accident. He explains that the model for such a theoretical system was classical mechanics, because of its “possession of a logically complete system of dynamic generalizations which can state all the variables of the system . . . All other sciences are limited to a more ‘primitive’ level of systematic theoretical analysis” ([1949]1954: 212). “Functional analysis” provided the surrogate for completeness. It “appears,” as Parsons says, “to be the only way in which dynamic analysis of variable factors in a system can be explicitly analyzed without the technical tools of mathematics and the operational and empirical prerequisites of their employment” ([1949]1954: 218). Here his scientific model shifts from mechanics to “structural functional analysis in physiology” as exemplified by Cannon’s The Wisdom of the Body (1932, cited in Parsons [1949]1954: 218). Cannon, a sometime member of the Pareto Circle, popularized the notion of homeostatic mechanisms. We will shortly see the philosophical reasons why mechanics and physiology seemed for Parsons to be similar.

The idea of a project of providing a conceptual scheme is odd, if one considers the Kuhnian notion of paradigm as the lineal descendant of the concept (as it was, by way of James Bryant Conant, Kuhn’s mentor). Yet in Henderson’s usage this made a certain amount of sense. Henderson thought of Gibbs’s physics as a model of scientific development, and understood Gibbs’s physical model of statical equilibrium as a paradigm case of the development of a conceptual scheme, and used it as an example both in his key 1932 philosophical article (1970: 163), and in a more elaborate way in his Pareto’s General Sociology: A Physiologist’s Interpretation ([1935]1967: 14). His comments match closely with those of Parsons:

Gibbs’s system is plainly a fiction, for no real system can be isolated. . . . So results are obtained and then extended even to systems that are far from isolated. Also, the enumeration of the factors, i.e. concentrations, temperature, and pressure, is incomplete . . . In other cases the consideration of other factors, like those involved in capillary and electrical phenomenon, cannot be avoided. Sometimes, however, such considerations can be introduced after the first analysis in the form of “corrections.” . . . such apparent defects are in truth consequences of very real advantages. They are but signs of the well chosen simplifications and abstractions that make possible a systematic treatment of complex phenomenon. ([1935]1967: 15)

The picture here is that the major task is to make a good approximation that captures the major variables, and fill in the details later. Henderson thought this was a good model for physiology, and this is what Henderson thought Pareto had successfully done for society. “Pareto’s social system contains individuals; they are roughly analogous to Gibbs’s components . . . As Gibbs considers temperature, pressure, concentrations, so Pareto considers sentiments, or, strictly speaking, the manifestations of sentiments in words and deeds, verbal elaborations, and the like” ([1935]1967: 16). Parsons’ model, which relied so heavily on aprioristic considerations, would not seem to lend itself to this interpretation, as we have seen, but Parsons himself understood matters differently. The theory was made scientific by the concept of equilibrium: social stability
was guaranteed by the equilibrating processes of society. Because Pareto was the point of comparison, these claims did not seem peculiar, nor did the fact that these speculations had little connection with empirical data. As Henderson says, “Pareto’s social system is an invaluable conceptual scheme, but . . . it is now, and will probably long remain, an implement of limited usefulness in the digging up of data” ([1935]1967: 95)

Philosophers who looked into the Parsons phenomena came away perplexed. The most elaborate study was made by Max Black, who concluded that, once translated from the jargon which Parsons invented, the theoretical claims that could be identified were truisms. This is a fair sample of Black’s translations: Parsons’s model of action becomes “Whenever you do anything—you’re trying to get something done”; his claim that normativity is essential becomes “Choosing means taking what seems best for you or what others say is the right thing”; the systems claim becomes “Families, business firms, and other groups of persons often behave surprisingly like persons” (Black 1975: 279). Black questions whether these really say anything, or, as he puts it, “whether it is plausible for fundamental social theory to be so close to common sense” (1975: 279). Parsons’s reply to Black retranslates these claims back into his dichotomous category scheme of pattern variables, and, unmoved by the question of whether they go beyond common sense, claims that “on the question of the pattern-variables, I think it can now be said that they are essential and that they are exhaustive” (Parsons 1975: 336; emphasis in the original).

By “essential,” he explains, he means that if the pattern-variables “were not used, essentially the same concepts under different names would have to be introduced” (1975: 336).

So what did Parsons provide other than a systematized jargon and truisms? The answer can be found in part in connection to the problem of disciplinary development and one non-commonsensical claim about central values. To put the point simply, the convergence that Parsons manufactured out of the writings of Weber and Durkheim had the effect of producing a distinctively “sociological” conception of values in which values were both essential to action explanation, thus providing an ineliminable and basic place for the science that studied them, and at the time made values essentially social, and, through the confused argument described earlier, in some sense necessarily univocal. This meant that values could no longer be understood as individual choices, as they were for Weber (and more generally for the economists), but had to be understood as something distinctively “sociological,” namely as the contents of a central value system which played the role in regulating action that the conscience collective had played for Durkheim.

The dogma, as one of Parsons’ students, Bernard Barber, puts it, is this:

The structure of values in a social system influences action at all levels, from interaction in small groups to that in the total society. This happens as very general values such as equality or rationality are made more specific in the form of norms for more specific interactive relationships. (1998: 39)

For Parsons the central value system was the key component of the process of equilibration that produced social stability (1975: 336). This language became intensely unfashionable in the sixties, when it was asserted to be an affirmation of an ideological representation of a consensus model of society that was at variance with the evident social and political conflicts of the period. Yet the model survived in other forms. It re-emerges, for example, in the writings of his student Clifford Geertz in the guise of interpretivism and in the form of the thesis that the mind is full of
assumptions, frameworks, and templates. The transition in Geertz’s own usage can be traced through his work. He continued, for a time, to think of society as having a strong center, which was “symbolic.”

At the political center of any complexly organized society . . . there is both a governing elite and a set of symbolic forms expressing the fact that it is in truth governing . . . It is these—crowns and coronations, limousines and conferences—that mark the center as center and give what goes on there its aura of being not merely important but in some odd fashion connected with the way the world is built. The gravity of high politics and the solemnity of high worship spring from like impulses than might first appear (1977: 152-3).

And Geertz provided a mechanism that was a surrogate for Parsons’ notion of a psychological basis of conformity, arguing that without the assistance of cultural patterns

a human being would be functionally incomplete . . . a kind of formless monster with neither sense of direction nor power of self-control, a chaos of spasmodic impulses and vague emotions (1973: 99)

But he also used the language of postmodernism, thus providing a bridge by which Parsonians, such as Ann Swidler and Jeffrey Alexander, could emerge as “cultural sociologists,” re-labeling the central value system as culture, which was conceded to be more “plural.”

As we have noted in relation to “mainstream sociology,” Parsonianism was not the only strategy for “scientizing” sociology during the mid-century period. But similar issues arise with the other major examples that attracted philosophical commentary. Alfred Louch examined the explicitly propositional behavioral theory of Parsons’ colleague and rival George Homans and concluded that the claims were true by virtue of the interdependence of the definitions of they key terms (1966). Symbolic interactionism, as theory, was, like Parsonianism, a set of conventions for the redescription of action in other terms. In addition to these “grand” theoretical approaches, there was a systematic attempt to reconstrue ordinary empirical social research in “theoretical terms,” motivated in part by the Logical Positivist idea that theory was essential to science. Paul Lazarsfeld and Robert Merton promoted the idea of what Merton called middle-range theory, and many books with the title “Theory Construction” and similar titles were published. But despite their collaboration and contact with Nagel in the classroom, and Lazarsfeld’s ongoing discussions with such philosophers as Patrick Suppes, nothing recognizable as “theory” in the classical Logical Positivist sense developed. This story, though it can be traced to debates in the late thirties, is almost entirely a post-1945 matter, and thus beyond the limits of this chapter.

7. Epilogue: After 1945

Apart from a few prospective remarks, I have closed this account at a time before the full impact of Logical Positivism and before Ordinary Language philosophy developed its distinctive critique of the idea of causal explanation of human action. The consequences of these two movements were profound for what was to become philosophy of social science, in the sense that they shaped the language and issues of philosophy of social science as it emerged as a subfield of philosophy. Their relations to the disciplines of sociology and anthropology are more ambiguous.
These fields were established as disciplines by 1945, and took on their current form in the years after the war. Philosophy increasingly faced those disciplines as established facts rather than as hypothetical possibilities. Many of the issues that arose in the course of defining the disciplinary boundaries and character of the disciplines, such as the problem of normativity and holism, persisted. But much changed.

In the form of active research disciplines, sociology and anthropology presented some challenges that earlier discussions had not grasped. The argument of Ordinary Language philosophy that “reasons” accounts precluded the possibility of causal explanation of action, applied to sociology by Peter Winch in his classic *The Idea of a Social Science and its Relation to Philosophy* (1958), was presented with the obdurate anthropological reality that the “reasons” that primitive peoples presented for their actions were often not unproblematic and *a priori* valid, but instead unintelligible as genuine “reasons.” His response to this problem in “Understanding a Primitive Society” ([1964]1970) produced a whole field in the philosophy of social science on rationality, and led to two influential volumes (Wilson *Rationality*, 1970; Hollis and Lukes *Rationality and Relativism*, 1982). These issues turned out to have significant implications for philosophy generally. Davidson linked this form of the problem of rationality with the problem of incommensurability in Kuhn, and salvaged the notion of rationality by defining it in terms that retained its *a priori* character at the cost of relativizing it to our purposes—a position reminiscent of Weber’s relativization of the explanatory concerns of social science to our interests—but noting that beliefs that we cannot understand as rational in something akin to our terms we cannot judge to be irrational.

Logical Positivism in the period of the unity of science movement had anticipated the transition from understanding social science as a hypothetical possibility to understanding it as a going concern and. The basic impulse of the unity of science movement was to show that all sciences and all scientific knowledge could be assimilated to a single, logically integrated structure. This went with an astonishing openness to the social sciences, even an eagerness, as Neurath said, “to abandon for good the traditional hierarchy: physical sciences, biological sciences, social sciences, and similar types of ‘scientific pyramidism.’” (1944: 8). The *International Encyclopedia of Unified Science* made an effort to include sociology and economics, and did so not in terms of hypothetical possibilities but in terms of actual fields of knowledge with a pre-existing logical structure open to analysis, and with its own methodological literature. Neurath’s volume *Foundations of the Social Sciences* (1944), is studded with references to George Lundberg’s methodology book *Social Research* (1942), and deals with such topics as the appropriate use of index figures, disapproving of the use of ordinal numbers as though they were cardinal numbers—and measurement (1944: 33-4), and recognizes the role of the ubiquitous correlations that social researchers actually deal with, rather than simply restating the model of scientific law. Indeed, the tolerance Neurath practiced here was extended to functional explanation in social science (1944: 22), and, as I have already noted, this was later echoed by philosophers such as Hempel and by Ernest Nagel (1961).

The emergence of Logical Positivism in the United States after 1945 as a kind of philosophical orthodoxy coincided with the rapid expansion of the “behavioral sciences” and the reformulation of their “scientific” ambitions, a reformulation in which the idea of “theory” had a large role. Indeed, the moment was defined by the rapprochement of theory with empirical sociology, under such slogans as “middle-range theory” and by a considerable optimism about the scientific prospects of these disciplines. The optimism contrasted with the empiricism of the older
Pearsonians, who believed that all that statistical sociology was going to yield was correlations. The Logical Positivists’ emphasis on theory, though it could only with difficulty be reconciled with the available “theories,” validated the idea that to be scientific a discipline needed theories, and this was consequential.

The two-way traffic between sociology (and to a more limited extent anthropology) and the philosophy of science shaped both sides of the exchange. Not only did philosophers take sociological and anthropological issues on board, sociologists and anthropologists took these philosophical issues about explanation and logical form seriously. Scientistic sociologists redefined science in terms invented by Logical Positivism. But the mutual attraction between the two was ill-fated. “Functionalism,” which the Logical Positivists attempted to accommodate, was, to paraphrase Sidney Morgenbesser’s remark on pragmatism, failed to meet the needs of these disciplines, and both it and the “positivist” program in sociology lost its credibility during the sixties and seventies. The “theoretical concepts” which Logical Positivism was used to legitimate, such as the social psychological concept of attitude, did not lead to “theories” of interest. Ironically, the philosophical critics of the scientistic pretensions of social science turned out to have a longer and more influential afterlife in sociology than the positivism they critiqued. The critique of sociology in Winch, for example, found a ready-made audience among the opponents of “positivism” and was absorbed into such movements as the Wittgensteinian version of ethnomethodology.

The discussion of the earlier 20th century methodological and philosophical literature in this chapter suggests that the issues contemporary philosophy of science raised were novel only in form. The problems of perspectives, pluralism, normativity and the a priori, as well as the problems of measurement, cause and correlation, and the nature of rationality and action, were there, and in a developed form, very early. In a sense these problems were obscured by the episode of mutuality mediated by Logical Positivism during the period of the new scientific ambitions of the “behavioral sciences” immediately after 1945, and have now returned to their original centrality.

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Notes

1 In this volume, especially the chapters by Risjord, Jarvie, and Wylie.

2 Much of the content of the famous journal *Archiv for Sozialwissenschaft und Sozialpolitik*, for example, consisted of such studies (cf. Factor 1988).

3 As Kincaid notes in his chapter on functional explanation in this volume.

4 The extensive muddles over teleology, necessity, and related concepts are discussed in Jones. Boutroux’s criticism of Durkheim’s use of “necessity” in *The Division of Labor* is particularly revealing as a motivation for Durkheim’s later care with respect to these usages (Jones 1999: 160).

5 This is the topic of Schmaus, this volume, and Schmaus (2004).

6 The difficulties of separating the notion of practice from a collective ontology are nevertheless considerable. Practice theory is discussed in this volume in the chapter by Rouse, but see also Turner (1994) and Schatzski (1996).

7 The development of these fields is discussed in the chapter in this volume by Zammito.

8 This paper is discussed from the perspective of the philosophy of social science in Turner (2003c).

9 For an attempt to restate Durkheim in terms of collective intentionality, see Gilbert (1994). For a more general philosophical introduction to Durkheim’s ethics, see Miller (1994 and 1996). For a discussion of collective intentionality and the philosophy of social science, see Zahle in this volume.

10 The methodological influence of psychology on sociology was substantial but complex, and largely unrelated to the correlational tradition discussed here, which was temporarily superceded at mid-century. Some of the historical aspects are discussed in Turner and Turner (1990). The psychology side of the issue has been discussed brilliantly in Danziger (1990), which is primarily focused on the shifting conceptual preconditions for psychological methods.

11 The chapter by Root in this volume deals with issues about racial concepts. The methodological lessons of these issues was crucial to later ideas about how to measure underlying traits with multiple possible indicators—a common situation in empirical sociology.

12 Many of Ellwood’s ideas, especially about culture, and his methodological critiques, were paralleled in Anthropology by Alexander Goldenweiser.

13 The relationship between Ellwood, Blumer, and symbolic interactionism will be discussed briefly below. It is discussed at length in a controversial work by Lewis and Smith (1980).

14 One of Ellwood’s later papers was an appreciation of James Mark Baldwin with whom he shared his evolutionary view of culture as interlearning (Ellwood 1936), discussed in this volume in the chapter by Kincaid.

15 The first textbook to define the basic methodological ideas and scientific aspirations of statistical sociology was Giddings’s of 1924, which was based on a series of articles published in *Social Forces*. 
To the extent that there was a “philosophy of science” literature on this topic in contemporary American philosophy, it was skeptical about the project of a physics like social science (cf. M. R. Cohen [1959]1978: 321.

Blumer was to attack this thesis in one of his own early methodological writings, “Science Without Concepts,” which Ellwood cites (Blumer 1931, Ellwood 1933: 22).

The chapter by Hage in this volume illuminates the idiosyncracies of this project of “theory construction,” as well as the different perspective which practicing sociologists had to the problem.

Discussed in Lynch’s chapter in this volume.

Blumer claimed to be inspired by Mead’s lectures, and Mead’s Carus lectures, one of which was entitled, “The Present as the Locus of Reality” contains the same idea (1932).

For a discussion of the reception of Weber’s methodological writings that deals with the issue of this misperception, see Eliaeson, 2002, esp. 41-46.

This is a point made by Schutz, who then proceeds to reinterpret Weber’s notion of causality as a subordinate element of the adequacy of interpretation, under the mistaken impression that Weber’s notion of causality can be subsumed under the notion of rational expectations for the causal outcomes of action (1967: 229-34). As shown below, Weber is also concerned with the fact that there may be other causes (e.g. of a biologically based kind) that co-operate in the production of interpretable phenomenon (such as, to choose one of his examples, succumbing to charismatic appeals). These added causes might affect probabilities but not be part of the agent’s expectations.

These arguments reappear with Popper, who assumes them. But the reconstruction of Popper’s sources is an exceptionally difficult task. His familiarity with Weber is nevertheless easy to establish. There are multiple references to Weber in The Open Society and Its Enemies (1945). The concept of decision used in essentially the same way, as a residual category of ethical choice, plays a central role in The Logic of Scientific Discovery (1959) in connection with Popper’s demarcation criteria, which is defended as a convention, grounded in nothing but decision, but which has the consequence of permitting and enabling scientific progress, which itself is a kind of contingent value choice.

Relativism and historicism, and especially cultural relativism, is discussed in the chapter by Jarvie in this volume. Many related issues appear in the chapter by Crasnow as well.

This topic is discussed at length in the writings of Michael Friedman (1999, 2000) where the technical details are given. His discussion of Carnap’s Aufbau and its critique of neo-Kantianism is particularly important. The direct relevance of this to the social sciences was limited, though indirectly there were strong connections, in that the episode stressed by Friedman in this book, and even more so in his discussion of Cassirer and Heidegger (2000), was part of a long-running crisis in the notion of localized or historicized synthetic a priori truth that was central to neo-Kantianism’s revision of Kant. As will become clear, however, the problems of the Geisteswissenschaften were significantly different from those of Naturwissenschaften, and this
was nowhere more evident that in the discipline of sociology, which worked with historical materials but purported to be a generalizing science.

26 Critical Theory is the concern of Bohman’s chapter and part of Outhwaite’s chapter in this volume.

27 This debate was recapitulated in only slightly changed terms after World War II by Henrich (1952), Strauss (1953), and Habermas (1971), and is dissected in a classic text by Bruun (1971).

28 This was also the strategy of Critical Theory, most fully developed in the thought of Habermas. Critical Theory is the concern of Bohman’s chapter and part of Outhwaite’s chapter in this volume.

29 Weber would have made a different point. Consider his remark that “many aspects of charisma . . . contain the seeds of . . . psychic contagion . . . These types of action are closely related to phenomena which are understandable either only on biological terms or can be interpreted in terms of subjective motives only in fragments” ([1968]1978: 17). This suggests that such “actions” are not fully explained by construing them in terms of subjective meanings, that to the extent they are the products of biological causes are only approximations to the ideal-type of rational action, and that action itself is an ideal-typical category which the actual course of events often only approximates. For more examples see Turner and Factor (1994).

30 “It may seem that I want to insist that decision theory, like the simple postulate that people tend to do what they believe will promote their ends, is necessarily true, or perhaps analytic, or that it states part of what we mean by saying someone prefers one alternative to another. But in fact I want to say none of these things, if only because I understand none of them. My point is sceptical, and relative. I am sceptical that we have a clear idea of what would, or should, show that decision theory is false; and I think that compared to attribution of desires, preferences, or beliefs, the axioms of decision theory lend little empirical force to explanations of action. In this respect, decision theory is like the theory of measurement for length or mass, or Tarski’s theory of truth. The theory in each case is so powerful and simple, and so constitutive of concepts assumed by further satisfactory theory (physical or linguistic) that we must strain to fit our findings, or our interpretations to preserve the theory. If length is not transitive, what does it mean to have a number to measure length at all? We could find or invent an answer, but unless or until we do we strive to interpret ‘longer than’ so that it comes out transitive. Similarly for ‘preferred to’.” (Davidson 1980a: 272-3)

31 Discussed, in conflicting ways, by Henderson and Rouse in their chapters in this volume.

32 Claims of this sort were an easy mark for Claude Levi-Strauss, who was later to generate many examples of rituals and ordering practices that had no apparent “function.” Nevertheless, the notion of function outlined by Malinowski is so broad that it is difficult to see how these examples, or any others, could conflict with them in a strict sense, since by definition they are part of a whole that does function in Malinowski’s minimal sense.

33 Even archeology was influenced by this novel alignment. The Kluckhohns, discussed in Wylie’s chapter, were part of the Parsons group, and the “normative” approach she discusses derives from Parsons.

34 This is an argument that is made explicitly in a text by Gustav Radbruch, Weber’s protégé in legal philosophy (1950).

35 As Bruce Wearne has shown in detail, Parsons actively avoided being pinned down on
philosophical issues about the nature of his theory (1989).

36 One might be misled, by Whitehead’s later reputation as a theologically inclined metaphysician, into thinking that this kind of argumentation stood at the opposite pole from the concerns of Logical Positivism. But this would be an anachronistic judgment. Whitehead was concerned with and wrote on the same issues of underdetermination that motivated the break between the Logical Positivists and the neoKantians, and Whitehead himself was still thought of as a logician and philosophical interpreter of science.

37 The idea that truisms might actually be the explainers that backed historical explanations was seriously explored in the philosophical literature (Scriven 1959).

38 This is the background to the story of the rise of cultural sociology told by Zammito in this volume.

39 Hage’s chapter deals with his own efforts in this direction, and there was a program of formal theory construction that attempted to provide such theories. Some of these books attempted to treat correlational analysis as “theory” (e.g. Stinchcombe, 1968, Blalock, 1969), while others, particularly a group of Stanford sociologists, promoted particular theoretical programs as exemplars of genuine theory (Berger and Zelditch 1993). These programs remained marginal, and did not concern themselves with topics that were conventionally regarded as important. Other works, by “theorists,” formulated propositions in the form “the greater the x, the greater the y,” following the influential book by Zetterberg, On Theory and Verification in Sociology ([1954]1963), under the impression that such statements could be treated as though they were confirmed by evidence of statistical correlations between x and y (cf. Turner 1974, 1987).

40 A title which itself reflected the view of social science as a hypothetical possibility rather than as existing intellectual enterprises.

41 The literature on this topic is discussed in the chapter by Lukes in this volume.

42 The history of this topic, which developed largely in psychology and in the areas of social psychology shared with psychology, is discussed in this volume in the chapter by Michell.