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The career of causal analysis in American sociology†

ABSTRACT

The visibility of causal terminology in American sociology, from 1895 to the 1960s, has varied according to changing philosophies of science and patterns of scientific communication. Successive generations differed in their receptivity to the diffusion of causal concepts from traditions other than sociology. After briefly reviewing the status of causality in the pre-sociological period, causal terminology is observed across four successive cohorts of sociologists. The first generation, 1890 to 1910, spoke freely of 'causes' until the second generation, in command of method using English statistics until the 1930s, made talk of causes anathema. Following theorists called for a return to causal interpretations which commenced in the 1950s as new methods attuned to the prevailing philosophy of science were imported to sociology from other disciplines.

INTRODUCTION

Few terms have suffered either the infamy or the taken-for-grantedness of 'cause'. The word has often been deemed extreme and treated in kind. It has been overdefined, overly praised, and just as often misunderstood. Whatever the vagaries of terminology, however, causal inference is well known as the working tool of those seeking a 'because' to 'why' questions. Causality in sociology has proved to be a fragile core-concept. Its discussion has passed from uncritical adulation with young optimism to complete rejection and eventual pragmatic utilization.¹ The variations in this career of an idea cannot be explained by everyday commonsense applications of 'cause-talk' working surreptitiously into the jargon. Neither does internal rigour in logic or technology completely clarify the passage to acceptability. The innovators have always been on the periphery

or outside of the discipline; to the extent that they were successful impinging upon traditions they opened the audience to the possibility of causal analysis. For that reason this inquiry focuses on the agents and channels of diffusion as well as the receptivity of sociologists.²

CURRENT PRACTICE AND DAVID HUME

It would be misleading to suggest that the idea of causation has ceased to be controversial. That just the opposite is the case is demonstrated by many excellent discussions tackling a heavy burden of epistemological and ontological problems.³ What is the status of causality in modern day sociology? One answer is to consider it a part of everyone's 'background knowledge' that in science is pressed into service as provisional heuristic bridging theory and practice. Causal laws, following leading model theorists Herman Wold and Hubert M. Blalock, are working assumptions of the scientist involving hypothetical statements of the if-then variety.⁴ These laws can never be demonstrated empirically, with the result that causal inferences belong to a theoretical level of science, while actual research (both experimental and nonexperimental) can only establish covariations and temporal sequences. Causal assertions rely on the assumption that all the relevant variables have been controlled or can safely be ignored. For lack of unanimity in the scientific community the causal concept is best understood by the rules of use regulating it.

The rules of use — such as association, asymmetry, or lack of spuriousness — rest on a philosophical platform which variously postulates a hypothetical reality, thematizes an interpretive procedure by the investigator, or otherwise covers itself against attacks of determinism by some sort of *ceteris paribus* disclaimer. In other words, causal statements carry no connotations of lawlike necessity. They are always tentative and only the product of hesitant scientific acceptance which allows their use even though causes in the invariant sense can never be empirically confirmed or verified. The historical record shows sociologists most unresponsive compared to other disciplines where '...the philosophical problems have remained in the background, stimulating argument and a penetrating examination of the inferential process but not holding up development.'⁵ Sociological meekness follows from past misuse amid highly charged attacks on causality. As Bunge pointed out the concept has been declared a 'fetish' (Karl Pearson), the 'relic of a bygone age' (Bertrand Russell), a 'superstition' (Ludwig Wittgenstein) and a 'myth' (Stephen Toulmin).⁶

The most important contribution to the subject of causality in modern philosophy was made by David Hume who, in 1748, maintained that (1) any talk of efficacy or necessity said to exist in a

cause is meaningless, and (2) experience of regularities of succession allow us to discover causes and make inferences. Prior to Hume the notion of causality had passed from ancient animistic meanings, through Greek philosophy with Aristotle's four senses of cause, to the deterministic and theologically grounded first-cause ideas of Bacon, Descarte, Locke, Spinoza, and others. The advent of western science and a Galilean world-view gave rise to a search for 'laws of causation' which postulated the possibility of discovering and confirming invariable, unconditional, and necessary causal laws. That, said Hume, was a fruitless search since the belief in causation stemmed not from divine or natural reason accessible to logic but from 'habit' derived from repeated experience and 'felt expectation'. Hume, it should be remembered, did not discredit relations of cause and effect by his injunction against necessary causation; he only swept out of the picture the classical faith in man's ability to unequivocally and absolutely know them.

PRECURSORS AND FOREIGN DIFFUSION: MILL AND GERMAN EMPIRICISM

Talk of causes under the letterhead of 'sociology' harkens back to Auguste Comte and John Stuart Mill. Yet the tracks of terminology to the *American Journal of Sociology* (1896) appearing a half-century later were quite indirect. A number of intellectual traditions filtered through the American climate of thought to fit the idea of causation to the needs of a new discipline. Comte and Mill are important because they were recognized as spokesmen. Their standing was subsequently questioned by the English philosophers of science following Mill.⁷

Comte's positive philosophy, for all its concern with 'laws', quite accurately drew a line between the world of theory and empirical observation but declared the search for causes completely unfruitful. The positive approach, with its stress on 'reasoning and observation, duly combined', did not ask *why* but *how*?⁸ Whatever merits may be alleged for Comte's position the semantical sidestep with its rejection of causal terminology was not well-received. His best American audience was comprised of clergy ready to march religion to war against science.

The early appeal of John Stuart Mill is easy to understand. Mill attempted to circumvent the Humean problem through his philosophy of experience and association. In his hands this philosophy allowed that the uniformity presupposed in nature, and its corollary, the law of causation, could be confirmed. The causal laws of this deterministic world could, in principle, be known.⁹

Mill's contribution toward furthering that goal was an experimental

method reminiscent of Bacon that would both discover and demonstrate causal relationships. His five canons of inductive inference — agreement, difference, residues, agreement and difference, and concomitant variation — were even claimed by their author to have been responsible for the discovery of every causal law thus far known.¹⁰

With William Stanley Jevons as the leading exponent of the hypothetic-deductive method, attention was turned from Mill's justification of inductive inference to deductive confirmation of hypotheses. Much of his opposition to Mill (for whom he had an almost pathological animosity) centred on the idea of 'cause' which, once spoken, 'may thus act as a spell, and throw the clearest intellect into confusion'.¹¹

Even though Karl Pearson's *The Grammar of Science* (1892) was far less thorough-going than Jevons's major work it was more widely read because of its simplicity and because it was addressed to social scientists. Pearson maintained that it was pushing the concept of causation past acceptable limits to declare absolute causal relations. Instead one had to identify causes proportionate to the viewed statistical variation and therefore had to deal with 'The category of association, as replacing causation'.¹²

When we vary the cause, the phenomenon changes, but not always to the same extent; it changes, but has variation in its change. The less the variation in that change the more nearly the cause defines the phenomena, the more closely we assert the association or the correlation to be. It is this conception of correlation between two occurrences embracing all relationships from absolute independence to complete dependence, which is the wider category by which we have to replace the old idea of causation.¹³

He then proceeded to discuss various measures of variation and association, many of which he created or discovered (i.e. standard deviation, correlation-ratio, product moments). Pearson's authority was not impugned (he was in fact readily cited in American sociology), but the line of thought he represented did not immediately prevail against the methodological impress from Germany.

As many as seventy per cent of the leading members of first generation in American sociology studied in Germany for an average of one and a half years apiece.¹⁴ As might be expected the imprint from such a wholesale exposure was reflected in the endeavour of the first sociologists to differentiate their discipline and identify the subject and object of their inquiry. It showed in the concern with ethnological materials and *Völkerpsychologie*, the so-called 'ethical' approach, and not least, a particular kind of social statistics. The statistical orientation the American students picked up in Germany — with its attendant causal terminology — explains much of the first

generation's use of 'causes' as well as the laggard influence of the more sophisticated statistical technology offered by Pearson.

The American students had first-hand exposure to the *Methodenstreit* ('battle over methods') which resembled the debate taking place in England at the same time between proponents of Mill's inductive method and Jevons's deductive method.¹⁵ On one side was Gustav Schmoller, spokesman for the younger historical school, arguing for concrete-inductive-empirical study against Karl Menger (co-discoverer of marginal utility analysis with Jevons and Walras) who spoke for Austrian economists and abstract-deductive theory. By dominance in the German university system Schmoller would have been the winner had the debate not proven so wasteful with its familiar false antitheses. Most Americans either did not care or were not duped and assigned some legitimacy to both methods of logic for science.¹⁶ The end result, in any case, was a triumph for Schmoller for American sociologists.

They returned with admiration for his unbridled empiricism born of historical case study. In their minds and notebooks Schmoller's 'technique', heavy on data collection, light on inference, and infamous for advocacy, became synonymous with the proper 'positivity' befitting a social science.¹⁷ The actual determination of causes from mountains of data seemed more a problem of conceptual organization than methodological import.

The empirical bent that so fascinated American sociologists was learned in centres less innovative than the renowned experimental psychology laboratory of Wilhelm Wundt. Sociologists were attracted to the professors of the historical school taking up the *Soziale Frage* (the 'Social Question') rather than the exacting work of Lexis, von Kries, Ludwig, and Fechner, who were teachers of the first instructors of statistics in the USA (Franz Boas, Henry P. Bowditch, J. Cattell, and R. Falkner). Their style of statistical inquiry was slanted more toward historical statistics than the 'moral statistics' of Quetelet (promoted by von Mayr).¹⁸

SOCIAL DARWINISM AND THE FIRST GENERATION

The founders of American sociology began out of step with other disciplines. While kindred social sciences incorporated new statistical means to their empirical ends the sociologists were hobbled by the elusive technique they selected for a model. Richmond Mayo-Smith's 1902 textbook for sociologists was a classic in historical school tradition beginning Columbia's empirical reputation.¹⁹ He seemed convinced of the ability of researchers to establish statistical and sociological laws of cause and effect but he proposed little more than his 'reflective analysis' as a path to that goal.

An explanation of this phase of sociological method returns to Comte and Mill whose inductive legacy relied on historical evidence. Schmoller's data base took an encyclopedic plunge in the same direction. Even Franklin Giddings, at the peak of his leadership, upheld the position that the discernment of causes was to be accomplished within the temporal framework provided by history (wherein one performed unspecified inductive operations). Speaking of causes in the convention of historians was more than just a case of terminological residue from past practice.

Sociologists joined the 'institutional' analyses of the German historical school with the remnants of American Social Darwinism, in order to establish independent traditions and to define their own competence, method and purpose.²⁰ The milieu of those influenced by foreign traditions was one embroiled in the merits of Social Darwinism where, among other themes, the adequacy of causal analysis was in hot debate. Foremost among the opponents was William James who was as adamant as witty in his denunciation of the movement's unilinear, natural-law, determinism.²¹ The anti-Social Darwinist critique of the principle of causality, for all its statistical and logical merits, was nevertheless poorly received in sociological circles. Few noted the perseverance more succinctly than Thorstein Veblen. In his view, evolutionary science held to a 'spiritual point of view' that was 'unwilling to depart from the test of causal relaxation or quantitative sequence'. Sociologists tended to embrace this quantitative animism though they lacked the methods to actually 'test' causal relations.²²

The persistent attacks eventually exacted a toll in caution as sociologists found analogic retreats in physical metaphors such as 'forces', 'controls', and 'energies'. Indeed the 'social forces' formulation popularized by E. A. Westermarck, S. Patten, and L. Ward became synonymous with cause.²³

The intellectual sweep of Social Darwinism was over by the last decade of the nineteenth century when American sociology emerged as a struggling professional group. Lester Ward had administered telling blows against individualism while 'collectivist' arguments for social reform – another product of German training – spelled the end of *laissez-faire* politics. Not discarded was the belief in progress and human evolution. Sociologists continued to clothe their vocabulary in terms of biology and the social organism. When it became apparent that the explanatory value had been exhausted (as Stephen Toulmin puts it, Social Darwinism was 'generating a pack of pernicious over-simplifications'), abandonment was complete.²⁴ The associated causal terminology all but disappeared.

By 1901 what went by the name 'sociology' could boast of fifty full-time professors and course offerings in about 400 colleges and universities.²⁵ An early show of successful institutionalization was

not rivalled by anything approaching success when it came to defining the intellectual content or method of the new discipline. Albion Small and Franklin Giddings, leaders of the competing Chicago and Columbia style of sociology, were equally explicit in their talk of causal analysis in sociology.

It was Small's opinion that 'Sociology, in its largest scope, and on its methodological side, is merely a moral philosophy conscious of its task, and systematically pursuing knowledge of cause and effect within this process of moral evolution'. What Small wanted to see as the over-riding question for sociologists was: 'Just what kinds of nexus [sic] are there between antecedents and consequents in human experience?' It was, by fiat, causal. Like the historical school he emulated, causal inference for Small was a kind of historical backward glance once enormous amounts of data had been gathered together and classified into logical, conceptual pigeonholes.²⁶ Giddings's reputation as quantitative methodologist was built on practicing what Small had preached.

His *Inductive Sociology* (1909) was originally meant to be a textbook on socialization but he added a thirty page introduction to define what the scientific study of society was all about.²⁷ After claiming that the fundamental methods of sociology are historical and comparative, Giddings let it be known that scientific induction would 'lead to causation'. Mill's method of agreement and of difference allowed us 'to induce a cause' and to find out how, Giddings referred the reader to Comte, Durkheim, Marx, Spencer, and Tarde, without offering substantive help for a definition of causality.

A lack of definition was no obstacle to defence against perceived dissenters like Emile Durkheim. Though Durkheim was highly visible and recognized as foremost spokesman for French sociology he did not receive a warm welcome. His study of suicide and of *The Rules of Sociological Method* were attacked early by G. Tosti who subjected his 'social causation' to severe criticism.²⁸ One of Giddings's students, Charles Gehlke, wrote the first major analysis of Durkheim's contributions to sociological theory where a critical chapter was devoted to 'Social Facts and Social Causation'.²⁹ A final discrediting blow to Durkheim's fallacious reasoning came in 1917 with critiques by Alexander Goldenweiser.

Sociologists in England were accorded more respect. Rowntree, for example, discussed 'causes of poverty'; he was something of an exception for he differentiated these immediate determinates from 'ultimate causes' which, he said, 'would be to raise the whole social question'.³⁰ Social reform was a moving force behind the heralded work of Charles Booth, but one had to exercise care turning data to political ends. L. T. Hobhouse, when launching the *Sociological Review*, said 'relations of cause and effect dealt with facts and not

morality'.³¹ Still, sociology was in search of 'laws' according to Hobhouse as he followed the American example inflating the scientific status of sociology.

ENGLISH STATISTICS AND THE SECOND GENERATION

Confidence quickly gave way to statistical caution with the second generation, gatekeepers of method until the mid-thirties. The dearth of measurement techniques, the inability to control for outside factors or rarely even deal with more than two variables at a time made statistical generalization to causes painfully impossible. The first generation had used causal terminology in the spirit of Mill filtered through German teachers and a carapace of Social Darwinism; their students worked in the shadow of Mill's detractors, beginning with Jevons. Where the first generation blithely avoided an operationalized concept of causality the second shied away from interpreting and generalizing from their empirical results.

The greater the exposure to the theory or history of statistics the less the talk of causes. When F. H. Hankins discussed Quételet's types of causes – constant, variable, and accidental – he reminded his readers that Quételet's simple formulations on the study of causal relations had been taken over or 'advanced' to a method of 'quantitatively measuring the correlation of two variable elements throughout their distribution'.³² William F. Ogburn went further in his 1912 dissertation on child-labour legislation. His was *not*

a study to determine causes, but it is hoped that it may be used as a basis for a study of causes. It is a necessary study in description and measurements, preliminary to an investigation of the forces underlying child-labor legislation.³³

For two decades following it was a rare work that made it beyond the 'preliminaries'. In Ogburn's case they consisted of more than 90 tables left uninterpreted.

Hankins and Ogburn were representative of a new cadre of sociologists more enthused by English statistics than the task of maintaining disciplinary boundaries. Despite decreased chauvinism and greater receptivity to 'outsider' ideas the new sociologists were conservative. Most did not stay abreast of developments in statistical theory. Until Samuel Stouffer, at the end of the 1920s, there was no direct intermediary to England, the undisputed centre for statistical innovation. There the classical inductive model for demonstrating causes had been subordinate to methods of statistical inference pioneered by Francis Galton and Karl Pearson. Around these eminent geneticists and biometricians gathered a critical nucleus of statisticians who revolutionized the tools of their trade.

The fruits of their labours were diffused through the *Journal of the Royal Statistical Society* and their own *Biometrika*. They had, in addition, their own statistical laboratory in London, positions in the leading universities, and the renown of the world-wide 'eugenics' movement (initiated by Pearson). Their focus began with evolution.³⁴

Charles Darwin's *Origins of Species* (1859) won an immediate convert in his cousin Francis Galton who in 1869 published his first article on problems of heredity 'in a statistical manner'. Controversy over his book *Natural Inheritance* (1889) pitted Mendelian views championed by William Bateson against Darwinists who placed less faith in mutation. Karl Pearson, a Darwinist, had followed Galton's method and became the leader of the opposition with his student George Udny Yule. With other followers, they founded *Biometrika* in 1901 to air unfashionable views concerning the causes of evolution. Their seedbed of statistical method spread to the United States via James M. Cattell, A. L. Bowley, G. U. Yule, F. Y. Edgeworth, Charles Spearman, R. A. Fisher, Warren Persons, Henry L. Moore, and many others.³⁵

Sociologists had been alerted to the thematic when Pearson subsumed their field under the branch of biology. The accent on statistical method stemmed from teachers in other disciplines at Columbia and Chicago and textbooks by English authors. The first of these, by Arthur L. Bowley, appeared in 1901. Giddings and his students welcomed it and the immediate reprint. An elementary text for a wider audience followed (1910) and then, in 1915, came the much used *The Nature and Purpose of the Measurement of Social Phenomena*. Bowley's intent was to disseminate statistical method but 'causes' had their connection to the enterprise – as 'connection'.

The general problem of sociological statistics is to define or delimitate [sic] and enumerate classes, to specify attributes and describe their variation, and to discover relations and causal connections.³⁶

Columbia University served as portal for the English statistics. On the faculty there or at Teacher's College were L. Thorndike, F. Boas, H. L. Moore, and others who were familiar with and often visited the statistical laboratory in London. Franklin Giddings in the Sociology Department did not himself ever master the methodology of the new statistics but encouraged its pursuit. The Columbia graduates like Ogburn, Chapin, Chaddock, Rice, Gillin, and Gehlke introduced regression analysis, random sampling, correlation, confidence intervals, and other techniques. Their impact was formally recognized by the 1924 formation of a section and committee on social research chaired by Ogburn at the annual

association meeting. (In 1927 Ogburn was recruited by Chicago where his decisive influence was felt by Burgess, Stouffer, Hauser, and Duncan.) The competition at Chicago had also made its unique contribution to new styles of research. Best known were the ecological studies of the city promoted by R. Park and the case study- method which blossomed after W. I. Thomas, in collaboration with Florian Znaniecki, published *The Polish Peasant* (1918-19) just after his dismissal from Chicago. But before the arrival of Ogburn, James Field in the Economics Department taught the ideas of the English statisticians from the end of World War One.³⁷

At both Columbia and Chicago the social survey method, still an indulgence of fact-mongering meliorists (and considered by many to be a less scientific type of case study) was rapidly being developed into a major tool of sociologists. Most surveys were originally defined geographically, later in reference to special problems, and often failed to expend much effort on quantitative analysis.³⁸ Many of the first ones, such as W. E. B. Dubois's study of *The Philadelphia Negro* (1899) were directly inspired by Booth. Rowntree's work on poverty and Jane Addams's Chicago-based Hull House publications were also influential. The growth of the survey method is indicated by the volume (over 2,800) of such studies cited in the 1928 Russell Sage *Bibliography of Social Surveys*.³⁹

The decade of the 1920s could be characterized as the period ushering in a tardy equivalent to the German *Methodenstreit*. Renewed debate began on the subject matter of sociology. Explicit discussions of causality and its status in sociological inquiry were rare at this time. Stress was laid to interpreting statistical results with reserve.⁴⁰ Texts by Chapin (1920), Odum and Jocher (1929), and Gillin and Blackmar (1930) had few words for causality – usually a reiteration of Pearson's displacement of the terms with 'correlation'.⁴¹

Stuart A. Rice (1928) was another follower of Pearson's philosophy for furthering the 'statistical view of a perceptual world'. But he was well aware of its limitations.

Scientific method conceptualizes the perceptual data and treats them *as if* they were real and exact entities. This methodological process, like a great deal more of scientific method, is essentially fictional. Its justification is to be found in the results to which it leads.⁴²

The use of causal terminology was, however, incipiently distortive. Again, in *Methods in Social Science* (1931), which Rice edited, all discussions of causation were relegated to the section dealing with historical studies. The problem with causation for G. A. Lundberg (1929), as for Rice, was the connotation of law-like determinism. Causes (efficient or final) were 'a pretension which no scientist has

ever advanced, and which no science has ever accomplished'.⁴³ All relations were contingent.

The prevailing winds of the philosophy of the social sciences were brought to bear on sociology by Morris R. Cohen, who combined lessons from studies of logic, statistical method, the *Methodenstreit*, and the past naïveté of sociologists. Sociologists, he said, paraphrasing Poincaré, could not beg off defining their subject matter by engaging themselves entirely in their method. Such a tactic had led to the wasteful polemics about the differences between the social and natural sciences. At one end of the extremes were the behaviourists arguing that all social phenomena were physical events and the Darwinists and historicists overemphasizing biologic and genetic explanations; at the other extremity were teleologists with their final causal explanations who couldn't shake off the burden of distortive evaluations. If the social scientist was to speak of social causation he needed 'a knowledge of the quantitative adjustments of *all* the factors necessary to produce a desired effect'.⁴⁴ This being seldom attainable, and the variations often being large, one is left in the final analysis with interpretive 'guess work'. Cohen was not opposed to speaking in terms of 'social causation' so long as it did not imply 'proven causes' or was masked in one of the ubiquitous but misleading metaphors like 'social forces' or 'control'.

DISCONTINUITIES: DIFFUSION FROM THE 'TRICIANS'

The ebb of causal terminology reached its low mark, near elimination, in the early 1930s. The tide turned toward a re-issue when sociologists began to seriously concern themselves with the division in their ranks between theorists and empirical researchers. Gradually it was determined that the philosophy of science could allow a union between the two realms where causal inquiry was acceptable to both. There was a major prerequisite to such an understanding.

Sociologists had to believe that the alleged incommensurability between theory and research – formerly couched as deduction vs. induction – could be overcome. The first generation, for all their support of data-gathering, were theorists without method; the second generation had method but lacked theory. The next generation, while hearing calls to 'bring the men back in', were in search of theory more amenable to research and methods more attuned to the state of the art. For the latter one had to look outside the discipline where the new vehicles that would make causal inquiry viable were gaining acceptability.

With considerable time-lags sociologists imported path analysis from biometricians, factor analysis from psychometricians, and modelling theory from econometricians. In each case the path and

time-lag to sociology varied. A cursory review of their development independent of sociology shows that the diffusion of innovation is regulated by social factors – receptivity of the audience, agents, channels – beyond the merit of the technique.⁴⁵ It also shows that the tardiness of causality's re-entry to sociology represented marked discontinuities in communication over fences between disciplines.

Biometricians and Statisticians Americans had already profited from K. Pearson's views on the philosophy of science and through *Biometrika* were provided with the most advanced work yet to be done in the theory and method of statistics as he, Edgeworth, Galton, and Yule added their contributions to the literature. Few in the USA, besides statisticians like Franz Boas at Columbia were able to deal with the complicated mathematical equations. Yule was able to somewhat remedy the discomfort with a text on statistical theory appearing in 1911.

By 1918 the Darwinist/Mendelian conflict was well on the way to resolution by a synthesis of the arguments about natural selection vs. heredity through the mathematical and experimental work of R. A. Fisher, J. B. S. Haldane, and Sewall Wright.⁴⁶ Fisher established his fame, after correspondence with W. S. Gosset ('Student'), by solving the problem raised by Pearson of the reliability of a correlation coefficient derived from a sample of a much larger population. He later used the same data as Pearson to arrive at an opposite conclusion granting some legitimacy to the Mendelian position on inheritance.

About the same time Sewall Wright was becoming a recognized authority for articles treating 'deductions of properties of populations on the hypothesis of Mendelian heredity'. In 1917, writing 'On the Nature of Size Factors', he laid the foundation of path analysis.

Wright's method was designed to estimate the degree to which a given effect was determined by each of a number of causes. In this case he treated causes, namely, factors which affected general size and factors which affected parts, as independent.⁴⁷

From this work came the first formulation of path coefficients. In 1921, under the title 'Correlation and Causation', he elaborated the theory by treating correlated causes, nonadditive factors, and non-linear relations. Despite some major disputes the work of Fisher and Wright united in Haldane's *The Causes of Evolution* (1932) to dispel the opposition between Darwin and Mendel. The result was the study of population genetics.

Psychometricians and Education Statisticians At the turn of the century most statistics courses in American universities were taught

by psychologists who also worked in the field of education. The *American Journal of Psychology* founded in 1887 by G. Stanley Hall, declared there would be a statistical future for the discipline and numerous psychology laboratories lent corroboration to his prediction.

The American interest in individual differences (resembling but not a spin-off from Galton's studies) marked a major break from German psycho-physics and experimental psychology influencing Americans. James McKeen Cattell initiated the formalization in 1896 with his course on 'Mental Measurement' at Columbia University. There the influence on Cattell and Boas converged on E. L. Thorndike whose *Notes on Child Study* (1901) was followed by the sophisticated *Introduction to Mental and Social Measurements* (1904). Through this book and his students Thorndike assumed a central position in the diffusion of statistical methods (e.g. Strayer's introduction of the Pearson Coefficient of Correlation in his dissertation on City School Expenditures (1905)).

Charles Spearman, trained by Wilhelm Wundt and inspired by Francis Galton, contributed to the *American Journal of Psychology* 'General Intelligence Objectively Determined and Measured' (1904) wherein the idea of factor analysis was first clearly enunciated. An earlier article on the proof of measurement of association had already aroused the American audience. From his home base in London Spearman received the support of Yule and Pearson; in the USA he secured a prominent place in Thorndike's mental measurement text.

The growing debate in the 1920s on Spearman's single-factor method led to L. L. Thurstone's multiple-factor analysis which was the subject of his 1933 presidential address to the American Psychological Association. Thurstone was close to the founders of *Psychometrika* (1936) who came together to foster the quantitative development of psychology. The psychometricians, from Cattell on, paved a considerable portion of the road toward a resurgence of causal analysis by extending and defining the limits of statistical practice. R. A. Fisher's revolution in sampling and analysis of variance theories and his *Statistical Methods for Research Methods* (1925), found a receptive audience among them.

Econometricians Astronomers seeking to understand the displacement and movements of heavenly bodies were the first to carefully analyze time series for the discovery of causal relations. The study of data observed successively in time was early applied (Augustin Cournot's 1838 work on supply and demand curves is an example) to economics for forecasting and studying trends. Economic regularities in cycles over time had its counterpart in the study of crises. The correlation between sunspots and periodic business fluctuations

occupied Sir William Herschel in 1801, his son John (who anticipated much of the philosophic basis for modelling theory), and W. S. Jevons who bestowed a 'connection as cause and effect' upon the relation.⁴⁸

Jevons's work was followed by the British physicist J. H. Poynting in 1884 and by R. H. Hooker in 1901, each of whom contributed to the idea of serial correlation.⁴⁹ Here the work of Galton, Pearson, and Yule was freely employed as they too published on problems of serial relations. These Britons were closely studied in the USA after 1910 by economists like Warren M. Persons, H. L. Moore, E. F. Gay, and Alvin Johnson. Closer relations between the two countries during World War One and the 'institutional' school of Thorstein Veblen led by Wesley C. Mitchell kept the study of economic cycles vigorous in both theory and research.

As with psychology, sophistication in statistical methods supplemented forays into causal inference. H. L. Moore's *Economic Cycles: Their Law and Cause* (1914) was a classic work seeking to incorporate causal terminology into statistical analyses. The economists were well-equipped to deal with the problems that arose. Not only did some of the most eminent statisticians of the time become involved in their problem-solving (such as Yule on the issue of spurious or 'nonsense' correlations), but the economists were unrivalled contributors to statistical theory among the social sciences. Econometrics acquired its identity during the twenties primarily through the efforts of Ragnar Frisch and Irving Fisher. Its objective, according to Frisch, was

to promote studies that aim at a unification of the theoretical-quantitative and the empirical-quantitative approach to economic problems and that are penetrated by constructive and rigorous thinking similar to that which has come to dominate in the natural sciences.⁵⁰

Econometrica (1933) became the organ for articles by T. Haavelmos, Harold Hotelling, E. Slutsky, T. Koopmans, and others who were to carefully employ 'random causes' and 'chains of causality' in their inquiries.⁵¹ A significant step was taken by Herman Wold in 1938 when his *Study in the Analysis of Stationary Time Series* marked the beginning of a long list of pioneering contributions to modelling theory. Wold, a keen follower of literature in the philosophy of science, thematized the hypothetical character of the population from which causal inferences were drawn. He thereby avoided the inductive quandary plaguing naive statisticians hoping to infer causes from particular sample populations where high correlations existed. In *Demand Analysis, A Study in Econometrics* (1952) Wold supplied a pivotal position on causality at the critical time of a sociological turn to modelling theory.⁵²

THE RE-ISSUE OF CAUSAL TERMINOLOGY: 1930-1950

Causal analysis did not appear in sociology until the 1950s although considerable ground-breaking in the use of causal terminology for the two decades preceding helped clear the way. With the exception of work by Paul F. Lazarsfeld the innovators mediated or built upon the work of the 'tricians'. What Richard Von Mises said in 1934 held true: over the centuries causality has 'undergone transformations in accordance with the development of science. It is like a receptacle which is filled from time to time with a new content'.⁵³ The new content was made workable by transformed methods channelled into sociology at a pace set by ongoing negotiation between the poles of theory and empirical research. The pace was slow.

By the end of the twenties it was abundantly clear that the new professional ideology of sociologists was the one propounded by advocates of quantitative techniques. Presidential addresses by Gillin (1926) and Ogburn (1929) confidently foretold the future where 'every sociologist will be a statistician'. The expectation seemed assured.⁵⁴

An indication of increased statistical sophistication in sociology was apparent by the early thirties when C. C. Peters (1933) criticized Frank A. Ross's misconception of statistical significance. In the same volume of the *American Journal of Sociology* Samuel A. Stouffer offered the most advanced application of statistical method yet published in a sociology journal. Stouffer and Dorothy Swaine Thomas studied in England and were the earliest agents of diffusion for the work of econometricians, statisticians, and especially R. A. Fisher's positions on the null hypothesis, significance tests, and probability inference where uncontrollable elements might be randomized. With some hesitation (Stouffer was strong on replication) they alleviated the severe shortage of depth on the theoretical side of sociological statistics but this alone was insufficient to renew discussions of causality.⁵⁵

The reluctance of statisticians to directly confront the issue of 'social causation' was among the reasons R. M. MacIver's 1930 presidential address, 'Is Sociology a Natural Science?' was a reply to Ogburn and his followers. MacIver became the leader in the movement to reintegrate the terminology of causal analysis within the boundaries of social scientific method.

The service of statistical methods in the study of social causation is to prepare the way, to reveal more precisely the nature of the factors involved, to isolate quantitative indices of aspects of the situation and to show the degree of their coherence.⁵⁶

The study of the social does not preclude 'our study of "causation" where conclusions are readily given as "approximations", in the

discovery of negative correlations and of *meaningful* positive correlations'.⁵⁷

It perhaps seems ironic that a new advent for causality should come from the author of the 'dynamic assessment' and one with little methodological expertise. MacIver, however, was reactive; he was alarmed by dangers of statistical mystification. The same concern was apparent in *The Method of Sociology* (1934) by Florian Znaniecki, another continental scholar of like mind and measure in the 'humanistic coefficient'. More radical than MacIver, Znaniecki warned against the danger of withdrawing 'into the realm of pure mathematical concepts'.⁵⁸ The danger of stifling intellectual curiosity by overemphasizing technique had to be avoided. Yet instead of posing inquiry in terms of causal statements (but 'workable postulates' since Mill) he proposed his own method of 'analytic induction' for the 'discovery of exact laws'.⁵⁹ Both MacIver and Znaniecki lamented the deleterious consequences of speaking only of association in lieu of causes.

Similar reactions came from other quarters where, as at MacIver's Columbia, 'accident of tradition' had assigned a lower or minority status to theorists. From a marginal position at Harvard, Talcott Parsons was guided, in part, toward the final-causal elements embodied in his *The Structure of Social Action* (1937) by a belief in the maladaptability of physical efficient-causal explanations in popular use by behaviourists.⁶⁰ His keynote introduction addressing 'theory and empiricai fact' eventually led to Max Weber's method of causal imputation.⁶¹

A confusing array of causal vocabulary entered the jumble of debate where causality was but one of a host of issues tailing the call for relevance. Alexander Goldenweiser's article on 'The Concept of Causality in the Physical and Social Sciences' in the *American Sociological Review* (1938) did a good job of adding to the confusion while Hans Kelsen's excellent historical account of the idea of causation (1943) attracted little attention.⁶² Receptivity remained selective and largely confined to topical boundaries prescribed by sociological discussion.⁶³

Robert MacIver's *Social Causation* (1942) suffered from repeated strawman attacks on mathematics and, at times, fallacious reasoning, but his earlier points were delivered with more impact. The kinds of answers sociologists were producing – statistical ones – had a disjoint relation to the questions that prompted them. As sociologists sought 'because's' to various types of the question 'why' they often stopped at simple correlations or, following Mill, ended by demonstrating non-causality. While he didn't know how to go about it MacIver argued that for sociological insight one had to reinstate causal investigation. There was certainly no technique for verification in sociology but this did not deny 'causal attribution'.

What MacIver accomplished by publicizing causation could be construed as an unintended consequence of flailing at statistics. A victim of his own net, MacIver failed to fully deal with the 'interpretative principles of a non-empirical, metaphysical, more or less dynamic character'. One critic wrote:

The nearest we can come to a solution is through a kind of pragmatism. For 'practical' purposes. . . we must adopt the empirical, descriptive view; causality is a matter of discoverable and describable order in phenomena.⁶⁴

Causal analysis was possible not because the causal agency could be confirmed (which indeed it could not) but because the investigator 'ascribes', 'attributes', or 'imputes' the causal relation from his normatively established scientific platform which acknowledges that an interpretative act by the investigator takes place.

Paul F. Lazarsfeld was the first to supply an informed method to the imputation process. His 'elaboration' formula (first presented in Hans Zeisel's *Say it with Figures* (1947)) enabled sociologists to proceed into a kind of multivariate analysis previously taken little beyond two-variable relations. When Lazarsfeld spoke of a causal relation he always stressed that it involved interpretation. In his elaboration procedure a causal relation was imputed only after it had been demonstrated that it was not spurious or, in other words, not non-causal.

Of course, we can always think of additional factors to introduce, and empirically, we will never definitely establish a causal relation. It will only seem increasingly plausible as it survives one test after another. To put it another way: a bivariate relation is causal if it is non-spurious, a fact which can only be explored by an ever-longer series of elaborations.⁶⁵

It was up to the investigator to decide when the elaborations had been taken far enough to get around the bugbear of spurious correlations and justify the causal imputation.

There was no efficacy to the pronouncement of causality. The relevant variables were incorporated into an 'accounting scheme' which was admittedly arbitrary. After the methodological operations were complete there was a causal 'assessment' or 'discernment'. Thus Lazarsfeld and his students refused to ignore or jettison causal terminology simply because it conjured up deterministic arguments of the past or worried purists who stressed the inability of science to conform.

Why Lazarsfeld was the first in sociology to put causal terminology to work in analysis is an interesting question. Each innovator had a unique set of circumstances but those he shared with following pioneers of causal analysis illustrate some of the factors affecting

the career of the idea. First, he combined methodological proficiency with wide-ranging interests in history, the philosophy of science, and other disciplines (especially econometrics). This catholicity was further exhibited within his Columbia social circle where MacIver, Lynd, Lundberg, Stouffer, and his collaboration with R. K. Merton provided the opportunity to bring theory and empirical research together.⁶⁶ Second, he followed Pearson's dictum: 'After all, the higher statistics are only common sense reduced to numerical appreciation'. Lazarsfeld never lost touch with the basic questions, perhaps in his case the result of simple and direct studies commissioned by public concerns. Although he had written a statistics text in Vienna while working with the Buhlers he avoided a host of semantical problems by never making the claim that what he did was 'science'; the language of social research was fitted to the audience. Third, Lazarsfeld trained students. The strong institutional base afforded by Columbia with rich resource in students allowed navigation beyond established method to become a tradition – a school of thought – in its own right.

CAUSAL ANALYSIS

The precedent Lazarsfeld quietly set within sociology was soon matched by Herbert A. Simon, Herman Wold, Hubert M. Blalock, Jr. and later Otis Dudley Duncan, all of whom introduced additional approaches to causal analysis from outside the discipline. Through their work and that of those building on the platform they established, the career of causality in sociology entered its modern phase. As the lengthy lags in diffusion from the -tricians ended, novel applications and recruits to what Nicholas Mullins has called the 'new model army' took that kind of analysis from back eddy to mainstream.⁶⁷

Jan Tinbergen in the mid-1930s moved econometricians from unirelational to multirelational forecasting models by way of his causal chain systems; in 1943 Trygve Haavelmos introduced interdependent systems. The new domain known as 'simultaneous equations systems' was immediately embroiled in debate about the applicability and limits of new statistical methods for the analyses. Ragnar Frisch and Haavelmos, using the argument of symmetry between variables (or implicitly the notion of causal reversibility), rejected estimation procedures by ordinary regression and instead relied on 'maximum-likelihood' estimators. Herman Wold entered into the polemics early on demonstrating each procedure was consistent with the other when applied to causal chains and casting differences in terms of recursive (causal chains with directly observable variables) versus nonrecursive (interdependent with numerous

indirectly observable variables) systems. He championed recursive systems as more amenable to causal interpretation because of their clarity while Tjalling Koopmans supplied a rationale for the non-recursive side of the inquiry with his work on 'identification' that attracted numerous adherents. The question of causal ordering was addressed by Wold in the 1952 Wold-Jureen book on demand analysis. Here Wold proposed treating questions of asymmetry by a two-step definition of the causal relation: a stimulus-response model of experimental design served as framework for extending applications to nonexperimental situations.

Herbert A. Simon worked from the same general body of knowledge as his teachers Henry Schultz and T. Koopmans to his concern with the issue of identifiability and causal ordering. Ragnar Frisch's 'confluence analysis', J. Karschak on statistical inference in model building, and econometric distinctions between endogenous and exogenous variables stimulated the insights beginning to appear in 1952 and best known to sociology through his 1957 *Models of Man*. Simon was familiar with Sewall Wright's path analysis and impressed by Wold's use of the concept of causality. A greater influence stemmed from the Chicago classroom of Rudolf Carnap, where Simon was drawn deeply into the philosophy of science.

His chapter on 'Causal Ordering and Identifiability' appearing in *Studies on Econometric Method* (1953) carefully noted the 'unsavory epistemological status' of causality in the early 1950s. Humean and determinist problems were irrelevant, however, to discussion of models. A real problem for hypothetical systems of equations was the abandonment of causal terminology for synonyms despite the 'common sense' use of masked causal relations. If conditions of asymmetry occurred in the models then 'the question... of whether we wish to retain the word "cause" in the vocabulary of science' was affirmative. The term, 'carefully scrubbed free' of any undesirable philosophical adhesions, can perform a useful function and should be retained.⁶⁸ Simon did so by putting Lazarsfeld's elaboration formula in terms of a set of recursive linear equations, dealing with the definition of 'power' in political science and speaking to a diverse audience of statisticians and philosophers.⁶⁹

Meanwhile at Columbia University students of Lazarsfeld exhibited an increased willingness to come to grips with the discipline's inferential procedures. A case in point was the 'significance test controversy' springing from the mathematical statistics of Fisher. The increased accuracy that accompanied experimental control through randomization could lead to increased confidence in making the intellectual leap to causal interpretation. To the extent that such an intention was underlying a non-rigorous use of statistical tests of significance Hannan Selvin, in 1958, warned caution to those

making an interpretative trespass across the borders of the classical hypothesis-testing design.⁷⁰

Lazarsfeld had likewise generated considerable interest in latent structure theory with speculations on the fit of manifest data to latent structure models. Dimension scaling in factor analysis posed some conceptual difficulties. An attitude was not defined but inferred from the observed data. Inference concerning causation in explanatory surveys, with a detailed treatment of 'elaboration', appeared in H. H. Hyman's *Survey Design and Analysis* in 1955.

As sociologists in the 1950s began tooling their trade to the computer they confronted a formidable array of advances in statistical method and theory. Controversies previously remote — such as that between Fisher and Pearson-Neyman on estimation — assumed new importance to a fast-growing group of specialists. Among them was Hubert M. Blalock, Jr. whose major in applied statistics at North Carolina left him well-versed in multiple regression and the analysis of variance and covariance. With the 1964 publication of *Causal Inferences in Non-experimental Research* Blalock became the undisputed leading spokesman for causality in sociology.

Throughout his career Blalock repeatedly confronted the problem of theory's relation to empirical measurement and, with Simon, the operationalization of terminology that would allow the interpretation of data. Simon's 1954 paper, 'Spurious Correlations: a Causal Interpretation', provided the basis for a 'Simon-Blalock tradition' where causal inference from correlations used a model of recursive linear equations by connecting them with known correlations between manifest variables.⁷¹ By the early 1960s the causal modelling of Wold (in preference to nonrecursive models) was incorporated into Blalock's position as he relied more directly on the econometrics literature and began writing on causal models with measurement errors. The 'block recursive' work of Franklin M. Fisher beginning in 1966, was an additional resource for Blalock, who stood with Otis Dudley Duncan at the centre of an ever expanding network of sociological causal theorists by 1970.

A somewhat separate evolution of causal method culminated in O. Dudley Duncan's introduction of path analysis to sociology in 1966. What multiple regression methods could do in experimental situations, path-modelling with latent variables accomplished in nonexperimental analysis. Duncan turned sociologists to the path analysis of Sewall Wright and the path regression of John Tukey whose 'Causation, Regression and Path Analysis' article in 1954 had long been overlooked. Econometric path modelling had concentrated on manifest variables; by merging this method with the factor analysis and error estimation of psychometricians, Duncan cleared obstacles to the treatment of latent variables. Raymond Boudon, in France, was independently working in the same direction. Seminal

contributions followed from Robert M. Hauser, Arthur Goldberger, and again Herman Wold in 1972 with his NIPALS (Nonlinear Iterative Partial Least Squares) model.

The above is but a scant overview of the position of major theorists guiding the development of causal analysis to the mid-1960s. Growth since then has been so rapid and complex that it represents a whole new way of thinking. The innovators were motivated by the desire to integrate theoretical and empirical knowledge. Hypothetical models served the purposes of deductive theoretical strategy using inductive statistics. Methods that had formerly been reserved for the experimental test of hypotheses were introduced to modelling equations as computer technology opened the door to research of such scale or simulation that classical specifying assumptions were met. As never before respect was paid to inferential procedures and the philosophy of science.

CONCLUSION

Causal terminology and the kind of analysis it represents has had an adventurous career in sociology. As this survey of use indicates, the terminology has been subject to a number of factors producing its visibility.

Most important was the changing referent of the word. Causal notions in sociology first rose to acclaim on the strength of John Stuart Mill's eliminative method which was touted far in excess of its capabilities. A reactive wave of statistical method supplanted the old causal concept and at the same time philosophers of science seemed determined to once and for all rid themselves of the intractable Humean problem by declaring the terminology off-limits. The result was a sort of inferential limbo leading to synonyms. A counter-reaction to statistical caution redressed causality (minus Millsian baggage) in terms of interpretive need and the pragma of hypothetical constructs. A fundamentally altered idea was gradually enhanced by new methods and advances in statistical inference.

Causal terminology always ranged within the realm of statistical possibility. Millsian induction was replaced by the descriptive statistics and then by the theory and systematized use of correlation of the Pearson school. In the wake of R. A. Fisher's work, probability laws and methods of estimation broached anew a role for deduction in experimental research. The recurrent division between inductive and deductive reasoning — causality now on the deductive side — was first mitigated by econometricians. Charles Booth once admitted that 'As to deductive and inductive methods I seem to need both eternally and never could separate them in my mind nor decide which moved first'. Models supplied a working capital undermining

the question of which came first, the 'hen' of theorizing or the 'egg' of fact-finding.

Causal analysis became a special province of inquiry by virtue of receptivity to outside theory and methodology. Just as original ideas of causality were diffused into sociology, later generations drew from biometrics, psychometrics, and econometrics for innovation and legitimacy. Reconstituted causality was a synthesis convened as necessary to overcome the impasse of deduction vs. induction, theory vs. research.

The use of causal terminology in American sociology has to some extent reflected sociology's self-image. The first generation profited by associating their presumed method with that of the natural sciences. Theorists of the 1930s, a beleaguered minority, used the neglect of causality to amplify their voice. Although the causal notion weathered use and disuse to become a joint theoretic-empirical concept, it currently is controlled by specialists in methodology. Causal theorists, with their special vocabularies, esoteric knowledge, and highly influential network of experts,⁷² could conceivably develop too fast to be appreciated by other traditions in sociology.

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NOTES

*The author was a graduate student in sociology at the State University of New York, Stony Brook during the mid-1970s, and more recently, with his father, a tug-boat captain on the Columbia river and its tributaries in the north-western USA. Mr Bernert (age 30) died as a consequence of exposure when his tug capsized in a storm on 26 May 1982. This article has been revised for publication by Dr Martin Bulmer of the London School of Economics.

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1. Cf. chapter 9, 'New Causal Theory', in N. Mullins, *Theories and Theory Groups in Contemporary*

American Sociology, New York: Harper & Row, 1973.

2. Cf. J. J. Spengler, 'Notes on the International Transmission of Economic Ideas', *History of Political Economy*, vol. 2 (1970), pp. 133-50; H. C. Selvin, 'Durkheim, Booth, and Yule: The Non-Diffusion of an Intellectual Innovation', *European Journal of Sociology*, vol. 17, 1976, pp. 39-52.

3. M. Bunge, *Causality: The Place of the Causal Principle in Modern Science*, Cambridge, Mass., Harvard University Press, 1959; P. Suppes, *A Probabilistic Theory of Causality*, Amsterdam, North-Holland Publishing Co., 1970; G. H. Von Wright, *Explanation and Understanding*, Ithaca, Cornell University Press, 1971; H. Wold, 'Mergers of Economics and Philosophy of Science', *Synthese*, vol. 20, 1969, pp. 427-82. J. Weinburg,

'Causation', *Dictionary of the History of Ideas*, vol. 1, New York, Scribners, 1973, pp. 270-8; H. A. Simon, 'Causation', *International Encyclopedia of the Social Sciences*, vol. 2, New York, Macmillan and Free Press, pp. 35-6; H. Feigl and M. Brodbeck (eds), *Readings in the Philosophy of Science*, New York, Appleton-Century-Crofts, 1953.

4. H. M. Blalock Jr, *Causal Inferences in Nonexperimental Research*, Chapel Hill NC, University of North Carolina Press, 1964; D. Lerner (ed.), *Cause and Effect*, New York, Free Press, 1965; J. L. Simon, *Basic Research Methods in Social Science*, New York, Random House, 1969; G. Nettler, *Explanation*, New York, McGraw Hill, 1970.

5. M. G. Kendall, 'Statistics', *International Encyclopedia of the Social Sciences*, vol. 15, Macmillan and Free Press, 1968, p. 231. Cf. N. Mullins, 'Theory Construction from Available Materials', *American Journal of Sociology*, vol. 80, no. 1, 1975, p. 4 for a discussion of tactics dealing with causal terminology.

6. M. Bunge, *op. cit.*, p. 333.

→ R. N. Soffer, 'The Revolution in English Social Thought, 1880-1914', *American History Review*, 75, no. 7, December, 1970, pp. 1938-64.

8. A. Comte, *Auguste Comte and Positivism*, G. Lenzer (ed.), New York, Harper & Row, 1975, p. 72. L. Kolkowski, *The Alienation of Reason*, trans. N. Guterman, New York, Doubleday and Co., 1968, p. 54.

9. J. S. Mill, *John Stuart Mill's Philosophy of Scientific Method*, Ernest Nagel (ed.), New York, Hafner, 1950.

10. J. Losee, *A Historical Introduction to the Philosophy of Science*, London, Oxford University Press, 1972, p. 149.

11. W. S. Jevons, *The Principles of Science*, orig. 1874, New York, Dover, 1958, p. 221.

12. K. Pearson, *The Grammar of Science*, 3rd ed., orig. 1892, London, A. & C. Black, 1911, p. 156.

13. *Ibid.*, p. 157.

14. Mr Bernert's uncompleted Ph.D.

research dealt with 'Die Wanderjahre: the mass movement of American students to German universities between 1870 and 1910' (editor).

15. The German *Methodenstreit* was not re-enacted on American shores except for thematic episodes within departments (as with Ashley at Harvard) and debates about marginal utility or the 'Institutional school' of Veblen, Commons, and Mitchell. A. Oberschall, *Empirical Social Research in Germany*, New York, Basic, 1965, pp. 10-13 provides the best synopsis of background and issues.

→ J. Dorfman, 'The Role of the German Historical School in American Economic Thought', *American Economic Review*, vol. 45, no. 1, 1955, pp. 17-28.

17. C → A. Small, 'Technique as Approach to Science', *American Journal of Sociology*, vol. 27, 1922, p. 650.

→ W. F. Willcox, 'The Outlook for American Statistics', *American Journal of Sociology*, vol. 15, 1910, pp. 633-40.

19. R. Mayo-Smith, *Statistics and Sociology*, vol. 1, New York, Macmillan, 1902. See also A. Oberschall, 'The Institutionalization of American Sociology', in A. Oberschall (ed.), *The Establishment of Empirical Sociology*, New York, Harper, 1973, pp. 187-251.

20. J. Ben-David, *The Scientist's Role in Society*, Englewood Cliffs, Prentice-Hall, 1971. One of the best studies of 'rules of conduct' is by R. N. Soffer, *op. cit.*, p. 1956.

21. W. James, 'The Dilemma of Determinism', *The Will to Believe*, New York, Longmans Green, 1897 (orig. 1882), p. 147.

22. T. Veblen, 'Why economics is not a science', 1898.

23. Pearson, *op. cit.*, p. 32, was appalled by such use. See also: → S. Sherwood, 'The Philosophical Basis of Economics: A Word to the Sociologists', *Annals of American Academy of Political and Social Science*, vol. 10, January, 1897, pp. 58-92.

24. S. Toulmin, *Human Understanding*, vol. I, Princeton, Princeton University Press 1972, p. 321. For another perspective see H. Cravens, *The Triumph of Evolution*, University of Pennsylvania Press, 1978.

25. A. Oberschall, *op. cit.*, 1973, p. 213.

→ A. Small, 'Some Contributions', *American Journal of Sociology*, vol. 28, 1923, p. 715.

27. Giddings's 1895 *Statistics and Sociology* is justly uncelebrated.

28. G. Tosti, 'The Delusions of Durkheim's Sociological Objectivism', pp. 171-7, and 'Suicide', pp. 464-78, *American Journal of Sociology*, vol. 4, 1898.

29. C. E. Gehlke, *Emile Durkheim's Contribution to Sociological Theory*, 1915.

30. B. S. Rowntree, *Poverty: A Study of Town Life*, New York, Longmans Green, 1922 (orig. 1901).

31. 'Sociology, General, Special, and Scientific', 1906, cited in P. Abrams, *The Origins of British Sociology*, Chicago, University of Chicago Press, 1968, p. 252.

32. F. H. Hankins, *Adolphe Quételet as Statistician*, New York, Longmans, Green, 1908.

33. W. F. Ogburn, *Progress and Uniformity in Child-Labor Legislation: A Study in Statistical Measurement*, New York, AMS Press, 1968 (orig. 1912), p. 24.

34. H. M. Walker, *Studies in the History of Statistical Method*, Baltimore, Williams and Wilkins, 1929, pp. 102-6.

35. H. Walker, *op. cit.*, pp. 151-62.

36. A. L. Bowley, *The Nature and Purpose of the Measurement of Social Phenomena*, London, P. S. King, 1915, p. 7.

37. M. Bulmer, 'Quantification and Chicago Social Science in the 1920s: A Neglected Tradition', *Journal of the History of the Behavioural Sciences*, 17, 1981, pp. 312-31.

38. M. Jahoda, P. F. Lazarsfeld and H. Zeisel, *Marienthal*, Chicago, Aldine-Atherton, 1971 (orig. 1933), pp. 99-125.

39. 'Statistical Method', *Encyclopedia of the Social Sciences*, vol. 14, New York, Macmillan, 1934, pp. 366-71.

40. D. S. Thomas, while discussing the use and limitations of statistics in sociology in the *American Journal of Sociology*, 1929, p. 17, supplied six procedural prerequisites designed to counter the dangerous misuse of method.

41. F. S. Chapin, *Field Work and Social Research*, New York, Century, 1920; H. W. Odum and K. Jocher, *An Introduction to Social Research*, New York, Henry Holt, 1929; J. L. Gillin and F. W. Blackmar, *Outlines of Sociology*, 3rd ed., New York, Macmillan, 1930.

42. *Quantitative Methods in Politics*, New York, Knopf, 1928, p. 24.

43. *Social Research*, New York, Longmans Green, 1942 (orig. 1929), p. 13.

44. M. R. Cohen, 'The Social Sciences and The Natural Sciences', in W. F. Ogburn and A. Goldenweiser (eds), *The Social Sciences and their Interrelations*, Cambridge, Cambridge University Press, 1927, p. 464.

45. A good example, asking why the statistical ideas of Yule did not diffuse to Booth and Durkheim, is found in Selvin, *op. cit.*

46. W. Provine, *The Origins of Theoretical Population Genetics*, Chicago: University of Chicago Press, 1971, p. 157. See also S. Wright, 'Path Coefficients and Path Regressions', *Biometrics*, vol. 16, no. 2, June, 1960, p. 191.

47. H. Walker, *op. cit.*, 161ff.

48. Cited in W. H. Hamilton (ed.), *Current Economic Problems*, Chicago, University of Chicago Press, 1914, p. 256.

49. H. T. Davis, *The Analysis of Economic Time Series*, Bloomington, Indiana, Principia Press, 1941, p. 52.

50. R. Frisch, *Econometrica*, vol. 1, 1933.

51. H. T. Davis, *op. cit.*, p. 36.

52. See Lazarsfeld's reference to Wold in the Wold Festschrift, T.

Dalenius *et al.* (eds), *Scientists at Work*, Uppsala, 1970, p. 78.

53. R. Von Mises and H. Pollaczek-Geiringer, 'Probability', *Encyclopedia of the Social Sciences*, vol. 12, Macmillan, 1934, p. 43.

54. The first specifically 'sociological' statistics text did not arrive until 1941 with the publication of *Statistics for Sociologists* by M. J. Hagood and D. O. Price, New York, Holt, Rinehart & Winston. The authors noted sociology's lag behind other disciplines.

55. The proportion of space devoted to 'methods' in the *American Journal of Sociology* more than doubled in the 1930s compared to the 1920s. See E. Shanas, 'Through Fifty Years', *American Journal of Sociology*, vol. 50, 1946, table 2, p. 526.

56. R. M. MacIver, *Society: Its Structure and Changes*, New York, Long & Smith, 1931, p. 520.

57. *Ibid.*, p. 546.

58. F. Znaniecki, *The Method of Sociology*, New York, Farrar & Rinehart, 1934, p. 233.

59. R. E. L. Faris, 'The Development of Sociological Thought', in R. E. L. Faris (ed.), *Handbook of Modern Sociology*, Chicago, Rand McNally, 1964, p. 926.

60. See T. Parsons, 'On Building Social Systems Theory: A Personal History', *Daedalus*, vol. 99, Fall, 1970, 826-81.

61. T. Parsons, *The Structure of Social Action*, 2nd ed., Chicago, Free Press, 1949.

62. H. Kelsen, *Society and Nature*, Chicago, University of Chicago Press, 1943. Kelsen addressed the field of jurisprudence.

63. This discussion does not treat a number of disciplines whose influ-

ence upon the causal thinking of sociologists was haphazard at best. These include (at different times) physical anthropology, logic, Freudian psychology and modern physics.

64. F. H. Knight, 'Review of MacIver's Social Causation', *On the History and Method of Economics*, Chicago, University of Chicago Press, 1956 (orig. 1943), p. 137.

65. P. F. Lazarsfeld, A. K. Pasanella, and M. Rosenberg, *Continuities in the Language of Social Research*, New York, Free Press, 1972, p. 121.

66. H. C. Selvin, 'On Formalizing Theory', *The Idea of Social Structure*, Lewis A. Coser (ed.), New York, Harcourt Brace Jovanovich, 1975, pp. 339-54.

67. This section is based on personal communications solicited by Hannan Selvin and the author in the Autumn of 1975. Gracious replies by Herman Wold, Sewall Wright, Herbert A. Simon, Hubert M. Blalock, and O. Dudley Duncan were invaluable for reconstructing paths of influence and creativity. The transcript of Joan Gordon's (November 1961) interview of P. F. Lazarsfeld in the Oral History Collection, Columbia University, provided additional insights.

68. 'Causal Ordering and Identifiability', in Lerner, *op. cit.*, p. 159.

69. Simon's application of Lazarsfeld in 1954 is discussed by Lazarsfeld in 'A Memoir in Honor of Professor Wold', in *Scientists at Work*, *op. cit.*, pp. 87-9.

70. D. E. Morrison and R. E. Henkel (eds), *The Significance Test Controversy*, Chicago, Aldine.

71. Lazarsfeld, Pasanella, and Rosenberg, *op. cit.*, p. 53.

72. Cf. Mullins, *op. cit.*, ch. 9.