



# Paper Tools and the Sociological Imagination: How the $2 \times 2$ Table Shaped the Work of Mills, Lazarsfeld, and Parsons

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## Abstract

C. Wright Mills' *The Sociological Imagination* (1959) represents one of the most influential texts of postwar American sociology. The title has become a catchphrase that stands for a style of thought that transcends both theoretical dogma and the constraints of mere methodological rule following. This article sets out to show that Mills' vision of the sociological imagination had more in common with the then dominant lines of scholarship than his broadside against Parsonian grand theory and Lazarsfeldian abstracted empiricism in the main part of the book would suggest. Among the tools that Mills introduced as fostering the sociological imagination were  $2 \times 2$  tables. The article traces the use of such tables over time and across scholarly communities and shows that, contrary to Mills' own estimate, these tables describe a common nexus between his own work and that of Parsons and Lazarsfeld. All three scholars made ample use of this formal tool to construct sociological arguments at central places of their oeuvre. Given its shared use across otherwise divergent schools, the  $2 \times 2$  table is a prime example for what historians of science have called paper tools, i.e. statistical formulas, algorithms, tables, diagrams, graphs, etc., that structure scientific research across different schools of thought and theoretical approaches. Drawing on the notion of paper tools, the article advances a post-Kuhnian perspective on the history of sociology that shifts the research focus from substantive ideas to formal tools and demonstrates elements of commensurability among presumably incommensurable schools of thought.

**Keywords**  $2 \times 2$  tables · Paper tools · Sociological imagination · C. Wright Mills · Paul F. Lazarsfeld · Talcott Parsons

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## Introduction

C. Wright Mills' *The Sociological Imagination* (1959) represents one of the most influential texts of postwar American sociology. The book castigated what it described as the two dominant styles of scholarship in American sociology: grand theory, epitomized by the work of Talcott Parsons, and abstracted empiricism, as practiced by Paul Lazarsfeld. Mills described these two styles as twin evils, i.e. the fetishizing of the association and disassociation of theoretical concepts at the expense of empirical insight (in the case of Parsons' grand theory) and the inhibition to address real life problems by articulating only such research questions as can be answered within the methodological confines of quantitative survey research (in the case of Lazarsfeld's abstracted empiricism).

Both of these tendencies or schools exist and flourish within what ought to be pauses in the working process of social science. But in them what ought to be a little pause has become, if I may put it so, the entrance into fruitlessness. (...) As practices, they may be understood as insuring that we do not learn too much about man and society—the first by formal and cloudy obscurantism, the second by formal and empty ingenuity (Mills 2000 [1959]: 74–75).

In an appendix on intellectual craftsmanship, Mills chose the expression sociological imagination to describe an alternative style of research that he identified with his own work.

This article sets out to show that Mills' vision of the sociological imagination had more in common with the then dominant lines of scholarship than his broadside against Parsonian grand theory and Lazarsfeldian abstracted empiricism would suggest. Among the catalogue of tools that Mills introduces to foster the sociological imagination are  $2 \times 2$  tables, also known as fourfold tables, informing the reader that "I do not believe I have written more than a dozen pages first-draft without some little cross-classification" (Mills 2000 [1959]: 213). The article traces the use of the  $2 \times 2$  table in sociology over time and across scholarly communities.

The table initially entered the discipline as the most basic form of a contingency table, that is, as the cross-tabulation of two dichotomous variables that facilitates the calculation of correlation coefficients. How to construct and interpret such tables was widely discussed in the methodological literature in the late 1940s and early 1950s, as for example in Hans Zeisel's influential *Say It with Figs.* (1947). This statistical textbook was published by the Bureau of Applied Social Research at Columbia University and introduced by its director Paul F. Lazarsfeld. The use of  $2 \times 2$  tables in statistics was subsequently emulated by Talcott Parsons at the Harvard Department of Social Relations. Beginning with *Toward a General Theory of Action* (1951), he employed the table for the cross-tabulation of dichotomous pattern variables that formed a central point of reference in his emerging structural-functionalist theory. It later transformed into his by now infamous fourfold AGIL scheme. Like Mills, Parsons and Lazarsfeld used the  $2 \times 2$  table not merely as a neutral instrument for the display of data, but as a tool to stir their imagination in the process of formulating new research questions and theoretical arguments.

In recognizing that the use of the  $2 \times 2$  table has such innovative qualities, the article follows the material turn in the history of science that has shown how material objects

shape the process of scientific knowledge production (Latour 1987; Pickering 1992; Pickering 1993; Rheinberger 1997). In *Image and Logic: A Material Culture of Microphysics*, for instance, Galison (1997) has demonstrated that both the logical positivist (e.g. Carnap) and the anti-positivist (e.g. Kuhn) account of science are flawed. Both present a monolithic narrative line of how science develops, with the former attributing epistemic primacy to empirical observations and the latter to theory. Galison, in contrast, makes an argument for an intercalated model of scientific knowledge production that grants partial autonomy to at least three different strata – theoretical, experimental, and instrumental. This tripartite account assumes that science does not have a monolithic structure. Scientific change does accordingly not occur on all three levels at once, but each of the three quasi-autonomous strata carry their own periodization. In particular the partial autonomy of the instrumental dimension of science is something that both positivists and anti-positivists have ignored.

Extending this insight about technology, scholars have shown that things done with paper share many of the effects produced by objects that are more easily recognized as technology. Klein (2001, 2003) has coined the notions of paper tools (see also Cohen 2004; Kaiser 2005), Hess and Mendelson (2010; 2013) use the notion of paper technologies (see also te Heesen 2002; te Heesen 2005), and Krajewski (2010) has elaborated on paper machines to show how the characteristics attributed to large scale technological equipment used in the laboratory also apply to the seemingly more mundane paper objects used in the research process outside of the laboratory. Examples range from diagrams, graphs, periodic tables and chemical formulas, to the punch cards used to feed Hollerith tabulating machines and IBM counting sorters.

What makes the notion of paper tools, paper technologies, and paper machines meaningful is the insight that like the technologies described by Galison and others, paper is not a neutral instrument which is simply used to inscribe or illustrate already existing knowledge. Paper tools are an active force in the production of new knowledge. In the case at hand, it can be shown that the  $2 \times 2$  table as a paper tool was central to the creative process of knowledge production that Mills called the sociological imagination.

While research on material objects and instruments, paper tools included, plays a central role in the history of science, it has not yet been systematically taken up in work on the history of sociology (for a rare example see Guggenheim and Krause 2012 on model systems). Scholarship on the history of sociology focuses on a wide range of factors apart from theoretical allegiances and loyalties that shape the process of scientific knowledge production, including, for instance, the impact of personal networks, the organizational structure of scientific research institutions, or the larger funding environment of academic life. Paying attention to paper tools does not erase the importance attributed to any of these causal factors. The aim is instead to show that paper tools can crosscut these influences and provide an independent factor shaping both similarities and dissimilarities between different schools of thought. In the case at hand, it can be shown that the  $2 \times 2$  table as a paper tool was central to the creative process of knowledge production that Mills called the sociological imagination.

The fact that all three scholars, Lazarsfeld, Parsons, and Mills, used the  $2 \times 2$  table is not the result of a shared underlying theory of society. Drawing on the concept of paper tools, the article advances a post-Kuhnian perspective on the history of sociology that shifts the research focus from substantive ideas to formal tools and demonstrates

elements of commensurability among presumably incommensurable theories. It shows that, contrary to Mills' own estimate,  $2 \times 2$  tables describe a common nexus between his own sociological imagination and that of Parsons and Lazarsfeld. Far from being exclusively tied to the work of these individuals, this paper tool was moreover embedded in the research agendas of the leading sociological institutions of the late 1940s and early 1950s, the Bureau of Applied Social Research at Columbia and the Department of Social Relations at Harvard, and would shape the development of the discipline at large.

The following analysis employs archival documents from both institutions. It further draws on recent scholarship on Mills, most important among them a collection of his letters and autobiographical writings by Kathryn Mills and Pamela Mills (2000), a selection of his writings, some of them previously unpublished, by John H. Summers (2008), and a biography employing detailed archival sources by Daniel Geary (2009).

## The Bureau of Applied Social Research and Abstracted Empiricism

As Stigler (2002) has shown, the  $2 \times 2$  or fourfold table has a long and honorable ancestry in logic and in probability. It is based on a dichotomous classification scheme that goes back at least to Aristotle. Yet published tables of cross-classified counts were quite rare before the twentieth century, even though data that could have been cross-classified were not. The  $2 \times 2$  table only became popular at the late nineteenth and early twentieth century, when Udny Yule, Karl Pearson, and others introduced correlation coefficients (MacKenzie 1981). The table was thus first used as a tool for statistical analysis, that is, as the most basic case of a contingency table. Sociologists, like scholars in other fields, initially adopted the table in order to calculate correlation coefficients for statistical data.

One influential early publication in sociology that made use of the  $2 \times 2$  table was Samuel A. Stouffer and Paul F. Lazarsfeld's SSRC funded *Research Memorandum on the Family in the Depression* (1937). According to Lazarsfeld, Stouffer had introduced him to the table in 1937, drawing it on a tablecloth during a luncheon (Lazarsfeld 1968). In the memorandum, the two authors intended to calculate correlation coefficients for their survey data, yet had to admit that given the number of variables involved, the effort was almost futile. A survey with only eight questions with three possible answers each could produce as many 6561 logical combinations, which from a practical point of view was impossible to use as a basis for calculation.

In a methodological appendix to the volume, as well as in a review of the typological method published by Lazarsfeld (1937) in the same year, a distinction between three different ways to reduce the multi-dimensional attribute space of variable based survey research was introduced: functional reduction, based on a high correlation between variables; numerical reduction, using indexes; and pragmatic reduction, where different combinations of variables were either neglected or highlighted based on the research question under consideration.

In the memorandum, Stouffer and Lazarsfeld chose the latter strategy. The authors created a cross-tabulation of two dichotomous variables, which resulted in a reduction of the possible combinations to four. "We shall limit the discussion to the type of hypothesis which is reducible logically to the form of a fourfold table" (Stouffer and

Lazarsfeld 1937: 187). In a next step, they argued that the cells in the table stood for four different types of authority. Once converted into this format, Stouffer and Lazarsfeld were able to engage in an empirical test of their theory. Argument and data related perfectly to each other for the simple reason that both derived from the same  $2 \times 2$  table.

Lazarsfeld, together with Stouffer, thus used the  $2 \times 2$  table in two different albeit connected ways, that is, as a tool for concept formation and as a tool for statistical analysis. The *Research Memorandum on the Family in the Depression* is notable not only because it is an example of an early use of the  $2 \times 2$  in sociology, but also because it shows that it was the ability of the table to connect between the two, i.e. theoretical concepts and statistical analysis, that made for its appeal. Using the table in this way, Lazarsfeld can be described as both, a social theorist and an empirical researcher, although he himself tended not to use the term theory to describe his work on concept formation, but instead referred to the use of  $2 \times 2$  tables for the creation and reconstruction of conceptual typologies as qualitative analysis. Both ways to use the table would remain central elements of his work throughout the remainder of his career.

### Paul Lazarsfeld and the BASR

Lazarsfeld is today largely remembered for his contribution to a number of research techniques, including the panel survey, the sociometric survey, the contextual survey, and last but not least, latent structure analysis. At the center of all of these methods stood the  $2 \times 2$  table as a tool for multivariate causal analysis. While himself still a newcomer to the use of this table in the mid-1930s, Lazarsfeld would describe the table as a standard tool of sociological research during the annual meeting of the American Sociological Society (later renamed American Sociological Association) a decade later. “Whenever an investigator finds himself faced with the relationship between two variables he immediately starts to ‘cross-tabulate,’ i.e., to consider the role of further variables” (Lazarsfeld 1955 [1946]: 115).

At the time when Lazarsfeld gave this presentation, he was the founding director of the Bureau of Applied Social Research (BASR). The bureau was the preeminent survey research center in the United States and provided the role model for such centers at other universities. The BASR grew out of the Office of Radio Research (ORR) in 1944 and was officially affiliated with Columbia University (on the history of the BASR and its predecessor see Barton 1979; Converse 1987; Fleck 2011; Hyman 1991; Sills 1987). The connection to the university was already made in 1939, when Lazarsfeld was first appointed as a lecturer at Columbia. A year later, he became an associate professor in the university’s sociology department. The bureau together with the department were the training ground for some of the most influential scholars of postwar American sociology. No less than 75 MA theses and 122 PhD theses were produced at the BASR (and its predecessor organization) throughout the time of its existence from 1937 to 1977 (Barton 1979: 21) and not few of the directors of other research centers throughout the country, including Peter Rossi at the University of Chicago’s National Opinion Research Center and Charles Glock at Berkeley’s Survey Research Center were trained at the bureau (Converse 1987: 285).

The way statistical research was conducted at the BASR during the first years of its existence is exemplified by the methods textbook of Hans Zeisel (1947), Lazarsfeld’s

co-national and boyhood friend who immigrated to the United States in 1938. In the early 1930s, while still in Austria, Lazarsfeld and Zeisel had worked together with Marie Jahoda on the Marienthal study, now considered a classic of empirical social inquiry, or sociography, as the authors then called it (Jahoda, Lazarsfeld and Zeisel 2017 [1933]<sup>2017</sup>). Zeisel's *Say It with Figures* was no less influential. This pioneering statistics textbook for the social sciences went through as many as four editions by 1957. The  $2 \times 2$  table was introduced as the prime tool for analyzing statistical data and can be considered as the core of the book. The purpose of this methodological tool was the search for causal relations between variables. "The usefulness of careful cross-tabulation may be taken for granted. This is especially true when we are seeking explanations. For if we want to understand a particular result, refined cross-tabulation becomes indispensable" (Zeisel 1947: 203). The  $2 \times 2$  table was thus not introduced as a tool of social inquiry, but as *the* tool, as long as causal explanation was the goal.

Throughout the 1940s and 1950s, the cross-tabulation of dichotomous variables was the standard way to analyze quantitative data in the social sciences. Not only the question how to interpret such tables, but also how to construct them was taught in statistics textbooks (Platt 1996). Some data, such as for instance marital status (i.e. "married" vs. "unmarried"), is by itself a discrete dichotomous variable. In the case of continuous variables, such as for instance income, it was left to the researcher to construct the dichotomy and to distinguish between, say, "high" and "low" income.

The rationale for such a simplification of the data was spelled out in detail in another prominent publication on research methods by the BASR, Herbert Hyman's *Survey Design and Analysis* (1955). The volume grew out of a Planning Program in Advanced Training sponsored by the Ford and Rockefeller Foundations and would emerge as the most frequently used methods textbook in American colleges by the early 1960s (Sibley 1963: 120). Hyman was a faculty member of the Columbia sociology department, where he taught a required course in survey methods that either Lazarsfeld or Patricia Kendall (Lazarsfeld's third wife and research assistant at the BASR) had previously offered. He also worked on BASR projects since 1951 and would serve as an associate director of the bureau from 1957 to 1969.

The book came with practical instructions on how to punch cards, operate a counting sorter, and cross-tabulate data. According to Hyman, survey research had existed for a long time, but it had only now become possible to process large samples.

[A] revolution in the mode of treatment of such data, arising out of technological change, has more recently occurred. It is universal in current survey research that automatic or machine methods of processing data are used. It is therefore a further demand upon the survey analyst that he have considerable familiarity with machine methods (Hyman 1955: 19).

*Survey Design and Analysis* offered detailed instructions on how to operate an IBM Card Counting Sorter, Type 75, the machine then most common in social research. Operating such a machine was labor intense and thus time consuming, even for a trained operator. For every category of a variable that was to be cross-tabulated with another variable, an additional run was necessary. Hyman offered radio listening behavior and its relation to gender and education as an example: classifying education in seven categories, as actually reported in the survey, would require a total of



seventeen runs, while a re-classification into three categories would do with only nine runs (i.e. eight runs less in total: four runs less for the collapsed educational variable, times two for each gender). The conclusion was simple: the fewer categories a variable had, the lower the operating costs and the more feasible the research project.

Styles of engagement with this office equipment differed substantially between researchers. Lazarsfeld surrounded himself with research assistants who did much of the mechanical part of the analysis, while his former collaborator Stouffer was famous for running his own statistical tables on an IBM card counting sorter outside his office door in Washington, and later at Harvard (Sills 1987: 268–269). Neither of the two, however, could escape the mechanical constraints that the use of this equipment put on research in terms of the time, money, and organizational know-how.

According to calculations by Parton (1950), a card-operated sorting machine could process about 400 cards a minute, but it was such a delicate machine that it might give wrong results if cards were worn. Exchanging worn out cards was easier said than done. Punching cards was itself time consuming and involved one person to punch the cards, and another to verify the results. On average, puncher and verifier could produce only slightly more than 200 cards a day. It could thus take up to a week to punch the results of a survey with about 1000 respondents on cards. Researchers had accordingly a strong incentive not only to reduce the number of categories of their variables, but also to limit the number of variables in their models to make sure that the cards did not wear out.

Multiple regression analysis, now used if data for several continuous variables is available, only became a standard tool within sociology when more powerful computers (replacing mechanically operating counting sorters) and software packages such as SPSS (replacing punch cards) entered the market in the late 1960s and early 1970s. The method was known, yet as long as regression coefficients had to be calculated by hand, it was rarely practiced and for the very same reason also not discussed in statistics textbooks. Zeisel's *Say it with Figures*, for instance, discussed Spearman's rank correlation coefficient in three chapters, but did not even mention regression analysis once. Hyman's *Survey Design and Analysis* did not even discuss correlation coefficients, but instead focused on the more intuitive interpretation of  $2 \times 2$  tables. In the late 1940s and early 1950s,  $2 \times 2$  tables were the cutting edge methodological tool in social analysis and had the reputation to let the facts speak, even though the data, i.e. facts, used in this analysis had to be heavily selected and converted to make the tool applicable.

The observation also holds true for Lazarsfeld. He was among the first scholars to conduct a multivariate regression analysis in a sociological publication (Lazarsfeld and Franzen 1945), but this innovation was not taken up again in his subsequent work. His favorite methodological tool remained multivariate contingency tables based on attribute data. While Lazarsfeld created many indices at the ordinal level, which could have been adapted to research techniques for continuous data, he often re-simplified the indices into grosser categories and examined percentage differences rather than capitalizing on the ordinal property (Converse 1987: 296, see also Selvin 1957 on the absence of tests of statistical significance in the work of Lazarsfeld and his collaborators at the BASR).

According to Hanan Selvin, a former student of Lazarsfeld, limitations of time and money usually precluded multivariate regression analysis throughout the 1940s and

1950s, although data that could have been analyzed in this way were available. It was only with the introduction of the computer that statistical techniques capable of handling the task were placed into the reach of most investigators. This change was as momentous as the initial introduction of Hollerith machines and IBM card counting sorters.

The development of punchedcard machines in the first third of this century made modern survey research economically possible. (...) Now the large electronic computer has brought another new technology (...). The results of this change will be better, cheaper, faster, and more powerful analyses than can be done with tables, and some new kinds of analysis that cannot be done with tables at all (Hirschi and Selvin 1996 [1967]: 162-163).

Yet this was an assessment made after the fact, i.e. once an alternative tool for data analysis was readily available and its ability to produce novel results had been demonstrated. From the point of view of today's researchers who are well familiar with regression analysis and the use of multiple continuous variables, the translation of all data into dichotomous variables might look deeply limiting, if not reductionist.

It is important to note, however, that back in the 1940s and 1950s, the use of  $2 \times 2$  tables was perceived to be the exact opposite. Variables had to be dichotomized, yet the subsequent cross-tabulation of these variables opened up a new space of possibilities. In the past, Lazarsfeld argued, sociologists had been largely content with dualistic typologies, while the use of a multivariate attribute space, of which the  $2 \times 2$  table was the most basic example, enabled the construction of by far more encompassing typologies.

Already in his 1937 article on the typological procedure in social research, Lazarsfeld also described the reverse of the process of reduction. "This procedure of finding, for a given system of types, the attribute space in which it belongs and the reduction which has been implicitly used is of so much practical importance that it should have a special name; the term, substruction, is suggested" (Lazarsfeld 1937: 132). In the case of most typologies then in use in the social sciences, the attribute space from which it derived was merely implicit, in the sense that the originator did not articulate it and might not even have been aware of it. In such cases, the method of substruction could be used as a tool for discovery, that is, as tool to reconstruct the full attribute space of the employed variables. In this way, the  $2 \times 2$  table could be used not only for the analysis of empirical data, but also for the construction of new theoretical concepts. The argument was part of a larger picture of the development of the discipline painted by Lazarsfeld.

A hundred years ago the task seemed to be to make sweeping guesses as to the future development of society. Fifty years ago the interest focused on basic concepts which would properly classify the crucial social phenomena. Today the trend is toward singling out the basic variables from which all specific concepts and interrelationships can be derived (Lazarsfeld 1954: 3).

The special appeal of the  $2 \times 2$  table that lay in its ability to create a connection between theoretical concept formation and statistical empirical analysis that was already clearly



articulated in the *Research Memorandum on the Family in the Depression* reappears in this statement. By putting this connection into a temporal, i.e. historical, framework, Lazarsfeld was able to make an argument that the survey research carried out at the BASR did not oppose, but rather advanced the work that was part of the classical theory canon of the discipline.

The number of dichotomous typologies that had been produced in previous decades was in fact substantial. Howard P. Becker had compiled a list with more than a dozen conceptual dichotomies then in use in sociology, ranging from the works of Saint-Simon to contemporary American authors (Becker 1950: 258–261). With such dualistic typologies being omnipresent at the time, a call for their cross-tabulation was in fact a way to add complexity to the picture, rather than to reduce it.

The term abstracted empiricism that Mills would later use to describe Lazarsfeld's work was thus misplaced. The latter did not merely use statistical research methods in order to abstract or generalize empirical observations, but, to the contrary, had a very clear vision of the imaginary potential that  $2 \times 2$  tables afforded for concept formation. Mills did in fact learn about this when he began to work with Lazarsfeld at the BASR.

### C. Wright Mills and the BASR

C. Wright Mills joined the BASR as a research associate in 1945. In the following year he was appointed assistant professor at Columbia College, in the undergraduate wing of the sociology department as opposed to the department proper, located in the Graduate Faculty of Political Science (Geary 2009: 76). The connection to Columbia came about through Robert K. Merton, who had served as a mentor to Mills for several years. When Mills published his first journal article in 1939 he sent a copy to Merton, asking for comments and initiating a professional association that ultimately led to the invitation to join the BASR (Mills and Mills 2000: 35). Merton had a high opinion of Mills at the time. Writing to Albert Salomon at the New School, he described him “as the outstanding sociologist of his age in this country” (Merton Papers: Merton to Salomon, December 6, 1946), and announced that Columbia would respond to any attempt to hire him with a counter offer.

Mills was initially hired by the BASR to conduct a large-scale study of the effects of personal influence on individual decision making in Decatur, Illinois. The study that was to be carried out under the direction of Lazarsfeld appealed to Mills because it provided an opportunity to learn new research techniques. Mills considered statistical research techniques to be useful, as long as they were employed as one method among others, rather than as the sole method, as a letter written to his collaborator Hans Gerth shortly after his arrival indicates.

Lazarsfeld I find a wonderful man to work with; he gives me absolute freedom to do what the hell I want in all respects on the study and gives me ingenious technical advice when I ask for it. (...) There are all sorts of disadvantages also which I see now for first time. I wouldn't think of doing only this kind of research. In other words, it is a hell of a fine experience to do one big job statistically, but a guy ought not to go hog wild about it! (Mills to Gerth, June 28, 1945; in: Mills and Mills 2000: 171).

Merton, at that time an associate director at the BASR, also offered Mills advice on the Decatur study, suggesting that he get in touch with Ilse Zeisel, the sister of Hans Zeisel.

I think you will find it extremely valuable if the coding on Decatur material is done in consultation with Ilse Zeisel. She has an enormous fund of practical experience on coding operations, and is particularly sensitive to problems of card punching and tabulation (Merton Papers: Merton to Mills, June 11, 1945).

Mills was also in touch with Hans Zeisel himself, who thanked him in the acknowledgements of *Say It With Figures* for editing the manuscript (Zeisel 1947: xvii). The work of Zeisel was not Mills' only exposure to the systematic use of  $2 \times 2$  tables during his time at the BASR. Merton also made prominent use of the  $2 \times 2$  table in his work, beginning with an article on "Social Structure and Anomie" (Merton 1938). Mills would later reference one of Merton's applications of the table in a publication on *Discrimination and the American Creed* (Merton 1949: 103) as an inspiration for his own use of fourfold typologies (Gerth and Mills 1953: 267). The very same publication that inspired Mills was later referenced by Lazarsfeld and Barton as a positive example for the use of  $2 \times 2$  tables in the construction of typologies (Lazarsfeld and Barton 1982 [1955]: 255–256). Lazarsfeld had already recognized the similarity between his and Merton's views on the topic when the two were newly hired by the Columbia sociology department and began to exchange publications with each other.

I found your papers very interesting indeed. As a matter of fact, the one on Anomie and Social Structure I shall be using in my class in research as an example of a successful conceptualization of a complex subject matter. The scheme you use there is close to some ideas on typological classifications which I took up in the paper you were kind enough to ask for (Merton Papers: Lazarsfeld to Merton, February 20, 1941).

Mills was thus introduced to the  $2 \times 2$  table by the very people who made it prominent within sociology and was in total agreement with them about its relevance.

Mills was an integral member of the BASR during its early years. Of the first 11 books published by the bureau in the 1940s, nine were authored or edited by Lazarsfeld and Merton, and the remaining two by Mills and Zeisel (Hyman 1991: 207). Alongside Lazarsfeld and Merton, Mills was moreover the only other Columbia faculty member at the BASR when he was hired (Barton 1979).

Despite this promising start, Mills' work on the Decatur study ultimately led to a protracted dispute with Lazarsfeld and strained his relation to Merton. At the heart of the matter was his dissatisfaction of working as a mere employee of Lazarsfeld, rather than as an independent researcher, as he wrote in a subsequent letter to Gerth.

I've worked on that crap more than on any other book with which I have been associated and of course he will now take it away, but I do not care. (...) Nothing is worth the continual feeling that you're not your own man (Mills to Gerth, February 15, 1952; in: Mills and Mills 2000: 80).

This conflict with Lazarsfeld further fueled the long process by which Mills' path diverged from that of professional sociology. The book based on the Decatur study, *Personal Influence* (Katz and Lazarsfeld 1955), was a foundational text for postwar media studies, yet by the time it was published, Mills had long since been taken off the project (Geary 2009: 89).

As Sterne (2005) has shown, Mills' experience at the BASR nevertheless provided a crucial intellectual and institutional basis for his subsequent work. Before he joined the bureau, most of his sociological work was theoretical in nature. Upon arrival, he quickly caught up with the new methods of social research. His first book, *The New Men of Power: America's Labor Leaders* (1948), was based on research conducted at the BASR and made heavy use of statistical data compiled by the Bureau's staff. Mills proudly reported that Merton was highly pleased over this book, "especially its form (integration of tables and text, typologies etc.)" (Mills to Gerth, September 26, 1948; in: Mills and Mills 2000: 120).

*White Collar*, Mills' most influential book before the publication of *The Sociological Imagination*, was likewise based on material that he had collected during his time at the BASR, although it was published after the relation to Lazarsfeld had already turned sour. The book bemoaned the retreat of the ideal of craftsmanship and the disappearance of an independent middle class, which once had dominated American life. This old middle class was increasingly replaced by a new middle class of white collar workers, whose social position was as dependent and alienated as that of the traditional working class. While the industrial revolution had first given rise to the factory system, it was now beginning to take hold of the office.

The book presents the use of IBM machines as exemplifying a new division of labor that destroys the independence of the old middle class and its sense of craftsmanship. This sense of craftsmanship, which Mills saw on the wane in contemporary America, its universities included, was the very ideal that he would later try to rejuvenate in his appendix on intellectual craftsmanship. The first version of the text was in fact written only shortly after the publication of *White Collar*.

*White Collar* can also be considered as Mills' first publication to critique Lazarsfeld, albeit at this point still without explicitly addressing him by name. In a chapter on old professions and new skills, Mills produced a description of a new entrepreneurial type of professor at American universities that clearly fitted Lazarsfeld.

[The academic entrepreneur] is a consultant to large corporations, real-estate bodies, labor-management committees; he has built his own research shop, from which he sells research services and the prestige of his university's traditional impartiality. He becomes a man with a staff and with overhead. It is high overhead with a system of fees for given jobs that causes his business-like frenzy (Mills 1951: 134).

At the other end of the spectrum of this division of academic labor stood the college professor.

The type of man who is recruited for college teaching and shaped for this end by graduate school training is very likely to have a strong plebeian strain. His culture

is typically narrow, his imagination often limited. (...) Men of brilliance, energy, and imagination are not often attracted to college teaching (Mills 1951: 129–130).

After leaving the BASR as a result of the conflict with Lazarsfeld, Mills had effectively retreated into Columbia College. His low opinion of college professors provides more than a small hint that he was not entirely satisfied with the turn that his career had taken.

*The Sociological Imagination*, written during the academic year 1956–1957, expressed Mills' estrangement from professional sociology even more clearly. Sociologists remember the book for its broadsides against two of the most influential sociologists of the period, Lazarsfeld and Parsons. The major theme of the book was already on Mills' mind when the conflict at the BASR was still in full swing. As early as 1951, Mills advertised the planned book to Philip Vaudrin, an editor at the Alfred A. Knopf publishing house, and argued that the sociological discipline "is now split into statistical stuff and heavy duty theoretical bullshit" (Mills to Vaudrin, September 17, 1951; in: Mills and Mills 2000: 155).

In his correspondence, Mills made it clear that *The Sociological Imagination* was not so much an attempt to assess the state of the discipline at large, but first and foremost a way to defend his own way of doing research and to get back at people who had previously criticized him. "[N]ow I declare war," he announced while still writing on the manuscript (Mills to Birnbaum, November 22, 1957; in: Mills and Mills 2000: 257). Mills' desire to get back at people was unmistakable. He described the chapter on abstracted empiricism as "anti-Lazarsfeld" and the chapter on grand theory as "anti-Parsons" (Mills to Coser, April 4, 1957; in: Mills and Mills 2000: 233) and both of them combined as "a kind of 'anti-Duhring'" (Mills to Tovarich, summer 1960, in: Mills and Mills 2000: 296).

The appendix "On Intellectual Craftsmanship" was, like the book at large, intended as "a 'defense' (without appearing to be such) of the kind of stuff I've done" (Mills to Miller, March 14, 1957, in: Mills and Mills 2000: 230). It was originally written in 1952 and mimeographed in 1955 for circulation among students at Columbia College (Mills 2008 [1952]). The text was intended as a "Handbook for Students Beginning Independent Work," as the initial subtitle suggested, and described Mills' own method of work, which sought to demonstrate what the sociological imagination looked like in practice. Using his own research practice as a positive example, it is also clear that Mills considered his work to be free of the faults that he detected in the scholarship of Parsons and Lazarsfeld.

To answer the question how the sociological imagination is spurred, Mills outlined a few techniques, which in his experience increased the chance of coming up with innovative ideas. The initial memo outlined eleven such techniques, which were truncated into seven in the published version. The creation of typologies by means of cross-tabulating dichotomous variables was at the center of both versions.

The technique of cross-classifying is not of course limited to quantitative materials; as a matter of fact, it is the best way to imagine and to get hold of new types as well as to criticize and clarify old ones. Charts, tables, and diagrams of a qualitative sort are not only ways to display work already done; they are very often genuine tools of production. They clarify the 'dimensions' of the types, which they also help you to imagine and build (Mills 2000 [1959]: 213).

As already quoted in the introduction, Mills told the reader that none of his famous works on the different class segments of American society, i.e. organized labor, the new middle class, and the power elite, had been produced without some cross-classification taking place in the process of writing the first draft, although he did not usually display such diagrams in the published version. The appendix itself is an example for the application of a  $2 \times 2$  table that did not make it into the published text. The original mimeographed text written for circulation among his students at Columbia College used a  $2 \times 2$  table to illustrate how he made creative use of the work of Mosca to think about the status of elites in American society. Mills distinguished between minority and majority groups on the one hand, and organized and unorganized groups on the other. By way of cross-tabulating these two dichotomous variables he ended up with four different types of group status, even though Mosca himself had addressed only two of these, that is, the organized minority (i.e. the power elite) and the unorganized majority (i.e. mass society).

It was precisely this practice of filling out empty cells in a  $2 \times 2$  table that Mills provided as an example for the sociological imagination. “There are organized minorities and they run things and men. There are unorganized majorities and they are run. But: why not also consider the apparent opposite? In fact why not the full scale of possibilities?” (Mills 2008 [1952]: 50). The published version of the text in the appendix of *The Sociological Imagination* still elaborated on these four types, even though it did no longer provide a  $2 \times 2$  table as illustration.

Constructing such types is what Mills thought made his work creative. In *The Sociological Imagination*, he castigated Lazarsfeld’s research as abstracted empiricism that derived arguments in a mechanical and overly rule-bound way. This mechanical style of research practiced at the Bureau of Applied Social Research was set in contrast to Mills’ own presumably more creative approach, distinguishing Lazarsfeld as a mere data technician and academic entrepreneur from himself as a true craftsman and creative intellectual.

Mills failed to mention, however, that the  $2 \times 2$  table as a tool for creating typologies was used by Lazarsfeld in just the same way. As shown above, the latter did, like Mills, emphasize that he made creative use of the attribute space that derives from the cross-tabulation of variables. Lazarsfeld also highlighted this imaginative potential of  $2 \times 2$  tables in his presidential address to the American Sociological Association.

Many of you, I am sure, are acquainted with the notion of an attribute space. It starts with the observation that objects can be described along a number of dimensions. Think, for example, of an IBM card on which people are described by sex, race, education, etc. In such a space, regions can be combined to form typologies. (...) The relation between typologies and attribute spaces will be obvious to anyone who has converted people into questionnaires and finally into cross-tabulations (Lazarsfeld 1962: 759).

According to Lazarsfeld, the important point about this practice of cross-tabulating variables was that it aided the construction of typologies. Drawing on Weber’s model of a pure bureaucracy, he pointed out that the criteria that define this type can be cross-tabulated. For the purpose of the talk, Lazarsfeld focused on two of Weber’s ten criteria.

This gives a two dimensional space you can visualize easily as a traditional system of x-y coordinates drawn on a piece of paper. (...). And as a free gift we now know what an ideal type is: it is the region in the upper right corner (...). How about the diagonally opposite point, the one with the coordinates 0/0? No one, as far as I know, has worked out in detail what a non-bureaucracy looks like (Lazarsfeld 1962: 759).

According to Lazarsfeld, looking at social life through the lens of the  $2 \times 2$  table had the potential to foster the formulation of new research questions. As a tool for thought, the table stirred Lazarsfeld's imagination in just the same way that it stirred the imagination of Mills.

The example provided by Lazarsfeld in his presidential address was virtually identical with the example provided by Mills in his appendix on intellectual craftsmanship. Mills converted Mosca's argument on elites into a  $2 \times 2$  table and used his imagination to fill out the empty cells that Mosca had not addressed. Similarly, Lazarsfeld converted Weber's argument on bureaucracy into a  $2 \times 2$  table and used the same kind of imagination to address the resulting empty cell of a non-bureaucracy. Mills and Lazarsfeld both used the  $2 \times 2$  table as a tool for creative thought, rather than as a mere illustrative tool to aid the display of data.

The term abstracted empiricism that Mills used to describe Lazarsfeld's work was thus misleading. The latter did not merely use statistical research methods in order to abstract or generalize empirical observations, but, to the contrary, had a very clear sense of the imaginative potential that  $2 \times 2$  tables afforded. It was moreover Lazarsfeld who had formulated this insight as early as 1937 in his discussion of the process of substruction, a name that he himself had coined. Not only did the practice originate with Lazarsfeld, rather than Mills, but the former had previously also school-mastered his erstwhile BASR colleague about it at a point in time when their relationship had already been damaged beyond repair. An article on concept formation by Lazarsfeld and Barton (1982 [1955]) referenced the definition of craftsmanship that Mills had advanced in *White Collar* as an example for an unsystematic use of concepts that could benefit from the application of the procedure of substruction. Mills had defined craftsmanship based on six features, Lazarsfeld and Barton noted:

If all six of these attributes have the values indicated (...), we have the ideal-typical situation of 'craftsmanship.' (...) Actually the six attributes give sixty-four logically possible combinations of values; the intermediate, mixed combinations, however, do not enter into Mills' present discussion, which deals only with ideal-type cases and not with the whole attribute-space. (...) Formal devices such as attribute-space and relational matrices can often help to clarify concepts which are not systematically presented; sometimes they can even suggest significant possibilities not originally considered (Lazarsfeld and Barton 1982 [1955]: 260–261).

The fact that Lazarsfeld targeted the very notion of craftsmanship in Mills' work to make the point adds, by hindsight, more than just a small amount of irony to the attempt by the latter to claim both the ideal of craftsmanship and the discovery of the creative potential of the  $2 \times 2$  table for himself.



## The Department of Social Relations and Grand Theory

In the light of the affinities between the works of the two scholars, it is evident that Mills' attack on Lazarsfeld was rather misplaced. But what about his critique of Parsons? While Lazarsfeld served Mills as role model for abstracted empiricism, Parsons' work served as his example for grand theory. Compared to the confrontation with Lazarsfeld, Mills' antagonism toward Parsons was less severely colored by personal motives, yet a desire to get back at him was nevertheless present. Already in 1951, Mills had penned a review of Parsons' *The Social System* for *The New York Times Book Review*, although it ended up not getting published (Summers 2008: 282). While still in production, Mills had described the review as a kind of personal revenge. "I understand P has said to a small group up there [about] my stuff in *White Collar*, 'Well, the man can write some, but it is all impressionistic stuff.' Won't he be surprised!" (Mills to Miller, November 1951; in: Mills and Mills 2000: 158).

At the time, Parsons was the leading personality at the Harvard Department of Social Relations that he himself had helped to create in order to foster collaboration between the disciplines of sociology, anthropology, and psychology. The department was established in 1946, together with the Harvard Laboratory of Social Relations that was led by Stouffer. Beginning with fall quarter 1949, Parsons organized a seminar to produce a shared theoretical framework for the social sciences. The participants consisted of the faculty of the department, such as Stouffer, but also of guests whom Parsons invited, including the sociologist Edward A. Shils (Isaac 2010; Nichols 2019).

The seminar resulted in the joint publication of *Toward a General Theory of Action* (1951) which, in accordance with the purpose of the seminar series, was intended "to contribute to the establishment of a general theory in the social sciences" (Parsons and Shils 1951: 3). The most crucial contribution of the publication was a chapter authored by Parsons and Shils that introduced the concept of pattern variables. The pattern variables were designed as dichotomous pairings of value orientations that allowed actors to determine the meaning of a situation. Five such pairs of opposites were introduced: affectivity vs. affective neutrality, self-orientation vs. collectivity-orientation, particularism vs. universalism, ascription vs. achievement, and diffuseness vs. specificity. Parsons' considered this conceptual scheme to be his central contribution to the project, referring to them as "my pet, pattern variables" (Parsons Papers: Carnegie Project, Discussions Transcript, December 16, 1949).

Parsons was aware that binary oppositions or dichotomies as such were nothing new in sociology. He drew, among others, on Ferdinand Tönnies' distinction between community and society as a point of reference during the meetings. What was new were not these binary oppositions as such, but the use of the  $2 \times 2$  table as a framework to relate them to each other. The inspiration for this table were the contingency tables used by Stouffer in his statistics classes, as Shils recalled in his autobiography (Shils 1997: 85). Parsons did thus not merely use the same tool of thought as Lazarsfeld to formulate his theory, he was also introduced to it by the very same person.

Not all participants were enthusiastic about the concept of patterns variables and the use of  $2 \times 2$  tables. The most persistent countervailing voice was, ironically, that of Stouffer, who considered the scheme to be arbitrary and doubted that it could be translated into empirical research. Contrary to what is often asserted, the working alliance between Lazarsfeld as empirical researcher and Merton as social theorist at

Columbia did not find a match in the relationship between Parsons and Stouffer. Merton had actively tried to promote such a collaboration at Harvard, but to no avail (Fleck 2011: 217–218). The two scholars never engaged in any collaborative projects comparable to those carried out at the BASR by Lazarsfeld and Merton. Stouffer contributed a chapter to *Toward a General Theory of Action*, yet it remained largely unconnected to the argument that Parsons and Shils made in their contribution to the volume. During the seminar series, he took particular fault with Parsons' formalism as epitomized by the notion of pattern variables.

I don't have any love for symmetry especially and although it's nice if you can get it, I don't believe it's necessarily there in nature anywhere. (...) While there's a certain intellectual satisfaction (...) – the fact that when you get the kind of euphoric feeling about it (...) – it might actually be blinding a person to other ways of structuring the problem (Parsons Papers: Carnegie Project, Discussions Transcript, December 22, 1949).

Parsons and Shils objected to this criticism. Parsons insisted that the scheme was only a formal model that could not say anything wrong “unless there are logical errors and so on,” while Shils hastened to add that “you can make lots of errors in the concrete things which are put into the boxes” (Parsons Papers: Carnegie Project, Discussions Transcript, December 22, 1949).

Parsons explicitly stated that the pattern variables were intended as dichotomous variables and did not take account of differences of degree. “[I]n [I]n the pattern – in the pattern variable aspect (...) it's quite clear that we have not included degree as such. (...) [A pattern variable is a] scope variable; but that has to do with the mode of drawing the limits, not the degree” (Parsons Papers: Carnegie Project, Discussions Transcript, January 14, 1950). The procedure was a typological, not a statistical one, even though it was statistical thinking that provided the model.

Parsons did not consider this typology based on “scope variables” to be reductionist, because the cross-tabulation of the five dichotomous pattern variables allowed for as many as 32 possible combinations or types. These combinations were not merely logically exhaustive, as he pointed out in *The Social System*, published in the same year as *Toward a General Theory of Action*, but they actually introduced combinations that had not yet been observed in real life. When examining actual cases, Parsons stated, “we do not find that empirically observable structures cover anything like the whole range of theoretically possible variability; possible, that is, according to purely logical permutations and combinations of structural components” (Parsons 1951: 152). Parsons thus described the use of the attribute space, i.e. the cross-tabulation of dichotomous variables, as a tool that fostered conceptual innovation and went beyond the mere abstraction of empirical observations.

It was the same assessment of the  $2 \times 2$  table, i.e. in this case a  $2 \times 2 \times 2 \times 2 \times 2$  table, as a tool for creative thought as put forward by Lazarsfeld. The latter was accordingly appreciative of Parsons' notion of pattern variables and considered them to be a prime example for the logic of substruction at work and even went so far as to state that the “most elaborate use of systematic typologies is found in Talcott Parsons and Edward Shils' work” (Lazarsfeld and Barton 1982 [1955]: 256).

The formalism of Parsons' conceptual scheme was matched by Lazarsfeld's subsequent proposal for a typology of the disposition concept, which can be considered as a rival to the former's notion of pattern variables, although it never gained the same amount of influence (Lazarsfeld 1959: 8–10). By disposition, Lazarsfeld meant attitudes as defined by the social psychologist Gordon Allport or values in the sense used by the cultural anthropologist Clyde Kluckhohn, both of whom had been contributors to *Toward a General Theory of Action*. Lazarsfeld proposed a dichotomous scheme of dispositions along three dimensions – substantive scope, dynamics, and time range – that resulted in eight possible combinations in a three-dimensional attribute space, i.e. a  $2 \times 2 \times 2$  table. While Lazarsfeld was a psychologist by training, he failed to reference any empirical research that answered the question why these eight types should be exhaustive of human dispositions and why they should derive from a combination of pairs of opposites.

This omission of empirical evidence was not unique to Lazarsfeld. None of the three scholars discussed so far, Lazarsfeld, Mills, or Parsons, provided a substantive reason why continuous variables had to be dichotomized. Lazarsfeld, in his publication with Stouffer, provided practical reasons why a high volume of uncategorized data could not be analyzed; Mills argued that quantitative data on relative frequencies was not available for the kind of research questions he was interested in; while Parsons rested his rationale entirely on historical precedent, i.e. the authority of sociological classics such as Tönnies. Put differently, one scholar claimed that there was too much data to do things differently, the next one that there was too little data available to do so, and the last one did not even bother to address the topic because sociologists in the olden days had not done so either.

None of the three, it is important to notice, was actually arguing that the social world is structured in a dichotomous way all the way down. Marital status is, but not all social phenomena are like that. A theoretical argument for the practice of dichotomizing variables was lacking – the  $2 \times 2$  table itself was the argument. In the works of Lazarsfeld, Mills, and Parsons, the table had gained a life of its own: it had the reputation to be the most rigorous and innovative methodological tool in the social sciences and this reputation is what carried the theoretical arguments made by its users. As such, this paper tool had all the taken-for-granted qualities that Kuhn would later attribute to paradigms, with paradigms commonly understood as theoretical frames of reference.

The similarities between Parsons and Mills are as striking in this regard as those between Lazarsfeld and Mills. While Mills was highly critical of Parsons grand theorizing as epitomized by *Toward a General Theory of Action*, he used the table in just the same way. The dichotomous variables used to construct  $2 \times 2$  tables were according to Mills polar types that ignored relative frequencies and matters of degree, as he stated in the appendix on intellectual craftsmanship.

Often you get the best insights by considering extremes – by thinking of the opposite of that with which you are directly concerned. (...) This technique is also logically sound, for without a sample, you can only guess about statistical frequencies anyway: what you can do is to give the range and the major types of some phenomenon, and for that it is more economical to begin by constructing 'polar types,' opposites along various dimensions (Mills 2000 [1959]: 213–214)

In Parsons' work,  $2 \times 2$  tables, either in their initial version as cross-tabulated pattern variables or in their subsequent version as AGIL scheme (with fourfold subdivisions in each cell) did likewise ignore differences of degree and relative frequencies. Parsons instead devised cultural patterns and core functions of the social system as polar types, or scope variables, as he called them.

Neither Parsons nor Lazarsfeld made an effort to point out these similarities between their work and that of Mills. While they did not react to *The Sociological Imagination* in writing, they nevertheless tried to keep the influence of the book at bay (although Lazarsfeld would a decade later reprint Mills' chapter on abstracted empiricism in a response section of his collected essays on qualitative analysis, Lazarsfeld 1972). Based on the request of Lazarsfeld, Parsons convinced the chair of the program committee of the International Sociological Association (ISA) to withdraw an invitation to Mills as a keynote speaker for the association's 1961 World Congress of Sociology in Washington, D.C. (Summers 2006: 37–38). The personal conflict between the three scholars ended here. In the following year, at age forty-five, Mills died of a heart attack.

## Conclusion

Mills' critique of two leading styles of sociological research has turned out to be the most enduring aspect of his work. The very title of the book in which he put this critique forward has become a catchphrase and attained a life of its own. Based on an analysis of journal articles, textbooks, and autobiographical collections, Platt (2013) has shown that the term sociological imagination is frequently used without a reference to its originator and without being tied to a specific argument. It very often simply refers to whatever an author considers to be good sociological work. Yet arguably only few contemporary scholars who use the expression "sociological imagination" in an affirmative way would be prepared to also endorse the specific research techniques that Mills had in mind when he coined the expression (one of the exceptions is Swedberg 2014: 78–79, who recommends  $2 \times 2$  tables as a creative tool for social theorizing).

The label has aged well, the style of research to which it was initially attached did not.  $2 \times 2$  tables are no longer either the dominant or the most cutting edge methodological tool of the discipline. In his high appreciation of this methodological tool, Mills was not the outcast scholar who went against the grain, as he is frequently portrayed, but more than anything else a child of his time.

Equally important, *The Sociological Imagination* cannot be read as an accurate account of American sociology in the 1950s. It offered only an incomplete survey of dominant tendencies in the field. Though Mills criticized two acknowledged disciplinary leaders of the period, he neglected to mention a third one, Robert Merton. In one passage, Mills referred to a "statesman" who sought to reconcile grand theory with abstracted empiricism, denying the differences between the two. This statement has been interpreted as an allusion to Merton and his advocacy of "theories of the middle range," which sought to merge the best aspects of Parsonian-style theory and Lazarsfeldian survey research.

Mills had in fact not always distinguished between only two styles of sociological research. In an article published in 1954 that foreshadowed the argument of *The Sociological Imagination*, he added to the Higher Statisticians and the Grand Theorists,

as he than called them, a third camp of scholars who merged social theorizing with problem oriented empirical research, producing scholarship that was in his view the “only one (...) worthy of the name sociology” (Geary 2009: 172). In the subsequent book, however, Mills chose to present only his own work as an alternative to the styles of research that he critiqued, regardless of the long list of alternative voices that he had previously elaborated on. As an assessment of the discipline, the book was accordingly flawed even by his own account.

Whether acknowledged as part of a third camp or not, Merton rejected Mills’ juxtaposition of different styles of research in a paper presented at the International Sociological Association’s *Fourth World Congress of Sociology* in 1959.

[These] polemics have more to do with the allocation of intellectual resources among different kinds of sociological work than with a closely formulated opposition of sociological ideas. These controversies follow the classically identified course of social conflict. Attack is followed by counter-attack, with progressive alienation of each party to the conflict. (...) The consequent polarization leads each group of sociologists to respond largely to stereotyped versions of what is being done by the other. (...) Sociological orientations that are not substantially contradictory are regarded as if they were (Merton 1961: 29-30).

Merton was a student of Parsons, a collaborator of Lazarsfeld, and a mentor of Mills. In his view, the similarities between these scholars were more profound than the differences.

An analysis of the use of  $2 \times 2$  tables in the work of the three scholars provides evidence for the accuracy of Merton’s assessment and shows that Mills’ work did not escape his own critique. Through the use of  $2 \times 2$  tables, there are elements of the very grand theorizing and abstracted empiricism present in his work that he castigated in the writings of his colleagues. The analysis provides a telling example of the way in which methodological tools can cut across theoretical schools of thought and give rise to novel arguments.  $2 \times 2$  tables served as paper tools in mid twentieth century sociology and had an independent effect on how empirical data was analyzed and how theoretical concepts were designed. The partial autonomy of this methodological tool vis-à-vis data and theory is well captured by Galison’s intercalated model of science and the notion of paper tools that builds on it. Facts do not speak for themselves, as has long been recognized in the history of sociology, yet neither are theories all determinative of the way scientific knowledge is produced. The instrumental dimension of sciences is an independent factor that can cut across theoretical schools of thought. To think of the works of Lazarsfeld, Parsons, and Mills as exemplifying incommensurable schools of thought is to fall prey to the stereotyped accounts that scholars at times produce of each other.

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