C. Wright Mills, the Bureau for Applied Social Research, and the Meaning of Critical Scholarship

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This article argues that C. Wright Mills's time at Columbia University's Bureau for Applied Social Research provided a crucial intellectual and institutional basis for his later work, and it uses Mills's career as an allegory for the history and self-understanding of modern-day, self-described "critical" scholars. Mills's own published writings, along with those of his biographers, encourage a view of his time at the bureau as an aberration. Yet, a careful examination of his letters and his work reveals that the critical position for which Mills is famous was actually nourished by his use of surveys, statistics, research teams, and other trappings of administrative research while at the bureau. The article explores the implications of this history for our own understandings of critical research today.

I was invited to New York City and began to direct a research staff [at the Bureau for Applied Social Research (BASR)]. This kind of adjustment is becoming a major academic pattern of success in the profession for which I was trained. The old-fashioned professor who quietly writes his books and teaches is passing from the academic hierarchy in social science as well as other fields, but that old role was one of the important roles I had in mind. In due course, when the opportunity came to move up in the manner of the new career, I promptly turned my back on the opportunity offered me to become an administrator and an entrepreneur of large-scale research; and that clinched the main line of my direction, the direction of independent craftsman.

—C. Wright Mills, 1957 autobiographical "Letter
to Tovarich" (2000, pp. 251-252)

For many scholars in the social sciences, the name C. Wright Mills conjures up notions of critical social research, academic radicalism, and the ideal of the public intellectual. Mills's *Sociological Imagination* (1959, pp. 24, 50-75, 100-

101, 124-125, 219) is famous for its attacks on "abstracted empiricism"—a slavish devotion to statistical methodology—and the "bureaucratic ethos," where social scientists led research teams that do work for hire for the government or private sector. In *The Sociological Imagination*, Mills cast himself as the defender of "intellectual craftsmanship"—the lone, critical intellectual who was devoted to sociological questions that really mattered. Yet, Mills the radical intellectual, the "Man Who Goes Into The Field, rather than sending four dozen researchers there" (Mills, 1954/1964a, p. 575), benefited immensely from work done by the very research teams that he would later disparage. In a passage that frames the epigraph to this essay, Mills shows a little circumspection by quoting Walt Whitman: "Do I contradict myself? Very well then I contradict myself, (I am large, I contain multitudes)" (Mills, 2000, p. 247). Indeed,

> Mills had lived through a sea-change in American sociology brought about by the confluence of the development of computers and the realization by a liberal government, and corporate community, of the practical uses to which social research could be put. (McQuarie, 1989, p. 292)

From the standpoint of intellectual history, the figure of C. Wright Mills does indeed contain multitudes. The career of C. Wright Mills the defender of intellectual craftsmanship and scholarly individualism was built atop the career of C. Wright Mills the administrative researcher.

This article offers an alternative interpretation of the meaning of Mills's career as an allegory for a forgotten dimension of the history of "critical" social research. I argue that Mills's connections with the BASR at Columbia University in the 1940s and 1950s made possible the "critical sociology" for which Mills is rightly famous. This is important because Mills's oeuvre is so often read against "administrative" sociology, whether the interpreters cast Mills as a dissenter or a failure. Mills has been alternately cast as an independent voice and an ignoble social climber (e.g., Oakes & Vidich, 1999; Press, 1978). This article juxtaposes what we know of the career of C. Wright Mills with a range of figures in his and others' writings. In so doing, I hope to show that beyond the administrative/critical divide, there lay a whole range of possible and actual intellectual and institutional connections among disparate methods and hermeneutics. Rather than being simply a critical or administrative researcher, Mills was very much both at once. As I will argue in the conclusion, this is an important allegorical lesson for us today: It helps deflate the myth of the individual scholar as academic superstar. It also challenges us to retain a healthy skepticism about the relationship between specific methods of research and specific philosophical position and political ideologies. In other words, much of the work done under the name of critical research is not necessarily politically progressive or, for that matter, philosophically robust. The converse argument can be made for more research that aspires to a social science model.

Mills's career can help us to imagine the possible relationships between critical and administrative research because he is such an iconic figure in American intellectual history. He is also a conflicted figure. For some, he stands as the paragon of a critical strain in American social thought; some think him a patron saint of the New Left. For instance, in a recent article in *The Nation*, Tom Hayden and Dick Flacks (2002) credit Mills as a major influence on the Port Huron Statement. According to them, Mills both inspired students to move beyond their political apathy and offered a model of humanist radicalism that was neither doctrinaire Marxism nor bland liberalism. For other writers, he was a daring figure who helped keep alive a critical and engaged sociological spirit in an age where the field was becoming more technocratic. Mills is cast as embodying the moral commitment to "reason and freedom" (Notestein, 1964, p. 49), as "'a noble gadfly' like Socrates" (Casanova, 1964, p. 66), as a "radical in the academy" (Press, 1978, preface), and as "the most influential American radical social theorist since Thorstein Veblen" (Tilman, 1984, preface).[1] Yet, even the otherwise laudatory Irving Horowitz (1983) concedes that of "the many people I met, talked with, and corresponded with, very few mustered positive sentiments toward Mills" as a person (p. 4). Whatever Mills's enduring significance, few of the people who knew him considered him to be a nice guy.

This personal side of Mills has led to a heterodox reading of his career. Guy Oakes and Arthur Vidich (1999, p. 176) view Mills as "an insensitive philistine but also brooding, self-absorbed, and driven by an egomaniacal ambition." Their C. Wright Mills is a conniving, opportunistic, manipulative, instrumental, and unscrupulous "big shot," seeking an ever-receding horizon of professional greatness and a failure at social theory and social research (p. 113). In an alternately endearing and harsh memoir, Harvey Swados (1963) (Mills had dedicated *The Sociological Imagination* to Harvey and Bette Swados) wrote that

> the unique thrust of his best work—I am thinking of the decade of the fifties, of *White Collar* and *The Power Elite*—derived directly from his egocentricity. These books would have been paltry if they had not been informed throughout with a sense of the magnetic self-assurance of their author. (p. 37)

For Oakes and Vidich, Mills's candidacy for standard-bearer of critical social thought is a sham: He was a careerist and a failure at serious, engaged social research. For Swados, Mills's value comes *in spite of* his inability to do serious research.

Presently, I will tell the story of Mills the administrative researcher by juxtaposing some of his writings alongside his letters, memos, and accounts by contemporaries. I want to be clear that I am *not* privileging Mills's letters as "what he really thought," the Columbia memos as "what was really going on," or the comments of his acquaintances as yielding insight into "Mills, the man." Even his own family acknowledges that he was often "fronting" others in his letters (Mills, 2000, pp. *xii-xiii*). Though this article will make some biographical

claims—they can hardly be avoided—my goal here is not to get to the "real" C. Wright Mills. I will merely treat the documents left behind by Mills and others as *documents*, all worthy of historical interpretation but none as deriving truth effects simply from their genre. This is not a story about C. Wright Mills the person so much as it is an attempt to rethink the significance of C. Wright Mills the historical figure, and through that, to rethink the meaning of critical research.

In 1941, when Paul Lazarsfeld coined the terms "administrative" and "critical" to describe two approaches to scholarship, he meant to designate them as ends on a continuum, two tendencies—not absolutes (Simonson, 2001). Lazarsfeld (1941) was mainly concerned with the *source* of funds: Administrative research was work for hire; critical research was not. Administrative research was "carried through in the service of some kind of administrative agency of public or private character," and critical research was "posed against the practice of administrative research, requiring that, prior and in addition to whatever special purpose is to be served, the general role of our media of communication in the present social system should be studied" (pp. 8-9).

Over the decades, these terms have taken on considerable baggage, and spread widely among scholars in social science fields. Administrative research conjures images of large institutes, research teams, statistical methods, and positivist philosophy. Critical research conjures images of lone professors, historical or deep ethnographic methods, and a commitment to social or cultural theory. Today, we have ossified Lazarsfeld's categories into an ontology of research methods and philosophical dispositions. As Docherty, Morrison, and Tracey (1993) point out, the administrative and critical models are somewhat incompatible, though it is certainly possible to do "benevolent" administrative research—this was Lazarsfeld's goal all along; it remains the goal of many administrative researchers today. But de facto, if not de jure, certain methods now "belong" to certain theoretical dispositions. It is almost impossible to talk about the 20th-century history of social research in the United States without referring to some version of this binary division. Like all binaries, it is easy to feel that we are left with two options—permanent difference or a false synthesis and supersession (see Lewis, 1997; O'Keefe, 1993). For the conveniences of argument and in accordance with scholarly custom (as opposed to Lazarsfeld's intention), this essay uses "critical" and "administrative" for their broader monetary *and* methodological connotations.[2] This is especially relevant to Mills's career: His work was administrative in both the monetary and the methodological senses.

If we take Mills's career seriously on its own terms, a more complex picture emerges than "Mills, hero of the critical tradition." As it emerges, so too does a different picture of the relationship between cultural-theoretical and empirical-quantitative styles of social research. Mills very much depended on his enduring relationship with administrative sociological research to produce his major critical works. To put it plainly, the administrative paradigm made Mills's later

critical work possible. "The Columbia period coincided with most of Mills' published output. Despite his misgivings about the Bureau, his own debt to it should be acknowledged" (Eldridge, 1983, p. 30). Mills depended on research teams, surveys, and statistics—all the things he would later lambaste in *The Sociological Imagination*—for his major works. This seems like a simple point, yet it goes against the grain of existing Mills scholarship: His hagiographers want to see Mills as a principled opponent of administrative research; his detractors want to read Mills as an opportunist, a sloppy researcher and writer, and a failure at the administrative paradigm (see, e.g., Shils, 1960).

The story I offer here rejects both positions. As I will show, Mills made use of his resources and position at the BASR to help build the classic studies for which he is now famous. Only later in his career did he begin to publicly oppose what we now call the administrative model. In fact, Mills almost achieved some academic notoriety as an administrative researcher. On his arrival at Columbia in 1945, Mills became an early collaborator in a study of public opinion formation in Decatur, Illinois. This study was eventually published as Katz and Lazarsfeld's (1955) *Personal Influence*, and it is a classic document of mass communication research from the postwar period. Had things gone differently, Mills might have found his name on the cover, and he knew that in losing his spot in the Decatur project, he would lose significant "professional acclaim" (Mills, 2000, p. 172). Yet, had his name appeared on that book, it would have been considerably more difficult to sustain the characterizations of Mills as the father of critical sociology or an uncompromising American radical that have such sway in the secondary literature.

Both Mills's proponents and critics view his work at the BASR during the 1940s and 1950s as something of an aberration. Some appraisals of Mills's work, such as Rick Tilman's (1984) 200-odd-page study of Mills as an American radical, fail to mention the bureau at all (see also Aptheker, 1960, and Press, 1978). Others, although acknowledging it, tend to diminish its significance. Hanno Hardt (1992) notes Mills's association with the bureau but largely reads him as a proponent of critical sociology and pragmatism, and an enemy of "abstracted empiricism." Howard Becker (1994) wrote that the bureau represented a "style of work [that] was not congenial to Mills" (p. 181). Mills's own published writings, along with those of his biographers, encourage a view of this period as an aberration. The wildly inaccurate self-assessment atop this essay and Mills's (1959) attacks on abstracted empiricism and research teams in *The Sociological Imagination* are examples of this tendency.

Mills's detractors, such as Guy Oakes and Arthur Vidich (1999), also cast his work at the bureau as "obligatory" (p. 65)—as getting in the way of the work he really cared about. When they do take it seriously, it is only to cast Mills as a hypocrite—someone who challenged the capitalist system of knowledge production in writings and partook of its benefits in his life (p. 112).[3] Harvey Swados's (1963) biographical account spends more time on Mills's literary aspirations than his debt to administrative research (pp. 39-40).

Yet, a careful examination of Mills's letters and work reveals that the critical position for which he is famous was actually *nourished* by the empirical research he did under the administrative rubric. This is not to say that administrative and critical research are the same thing, or that Mills should be considered an administrative researcher. Rather, he ought to be considered an interesting hybrid. His political sensibility, his essayistic style, and his sarcastic wit are central to his reception today, but behind this distinctive presentational style lies a career trajectory that is at least partly administrative. So Mills's biography suggests that critical and administrative modes of research have a more intertwined history than is usually acknowledged. But the administrative part is not actually concealed in Mills's prose; his relation with his subjects ultimately shapes the tone of his work. In his critique of Mills's *Sociological Imagination*, Norman Denzin (1990) notes that Mills speaks of, for, and through but not *with* "the people" of whom he writes: "nowhere in the pages of his work(s) do these little people and their personal trouble speak. Mills speaks for them, or he quotes others who have written about them" (p. 4). Mills's work is based on a massive wealth of survey research and interviews, but the surveys and interviews were done almost entirely by research assistants. Mills hardly ever spoke with the subjects of his texts, and this is one reason why his subjects rarely speak *in* his texts. The style of Mills's writing, as I will argue, reproduces the distance that he himself held from his objects of study. This distance was mediated, facilitated, and propped up by a largely forgotten fleet of researchers (most of whom were women) and an institutional position that was more characteristic of the administrative model of social research that he criticized than the critical model of social research he wanted to epitomize. As I will discuss below, Mills's exploitation of his research assistants was standard practice at the time, especially in team-based social science research; there are also famous cases in the sciences (e.g., Maddox, 2002; Sayre, 2000). Yet, Mills's unremarkable and entirely conventional relationship to research teams becomes worthy of remark when cast against the subsequent reception of his oeuvre as a central node in the critical tradition.

The rest of this article is divided into two parts. I begin with an alternative account of Mills's time at the BASR and its role in his career. I finish with an account of how Mills later reinterpreted his BASR and Columbia work to fit the administrative/critical model of social research.

Mills at the Bureau

In 1944, C. Wright Mills was a young professor at the University of Maryland who wanted to get to New York. He had had his share of difficulty with faculty colleagues, and although Maryland was ranked near the top of the country in terms of the quantity of graduate training possessed by its faculty, it lacked the prestige—especially among sociologists—possessed by schools like Columbia, Chicago, Harvard, and Stanford. Mills found this to be a cause for concern

(Horowitz, 1983, pp. 58-61). Mills had established a summer residence in Greenwich Village in 1943 with the hope that he could build connections in New York. When he accepted a temporary appointment at Columbia in the summer of 1945, he wrote to his collaborator Hans Gerth—in all capital letters, "I WANNA STAY IN NEW YORK AND GET OUT OF THIS HOLE" (Oakes & Vidich, 1999, pp. 38-39, 95-97, quote at 97). Mills was hired to teach two courses for $800, a modest sum for the time. By early 1945, with the help of Robert Merton, Daniel Bell, and Paul Lazarsfeld, Mills landed a full-time job at the BASR (Horowitz, 1983, pp. 77-78).[4] It should be noted that Mills was not very well qualified for overseeing statistical and survey research. In landing the job, Mills benefited from his own intellectual promise, the postwar shortage of qualified statistical researchers, and the New York connections that he had been cultivating for some time (see Horowitz, 1983, p. 210; Oakes & Vidich, 1999, pp. 99-111).

Mills was hired to supervise projects in mass communication and public opinion under the direction of Paul Lazarsfeld. Though the souring of their relationship is a well-known story, they got on well at first. During this early period, Mills was essentially an administrator, overseeing research teams, writing up results, and coordinating efforts (Mills, 2000, pp. 83-84, 171). Mills did relatively little firsthand research and relatively little statistical tabulation. Yet, this was a tremendously important period for his work; between 1943 and 1948 Mills would become known as a sociologist of stratification. The research done during those years would provide the foundation for Mills's classics *The New Men of Power* (1948), *White Collar* (1951), and *The Power Elite* (1956), along with studies less known today such as *The Puerto Rican Journey* (Mills, Senior, & Goldsen, 1950) and *Character and Social Structure* (Gerth & Mills, 1953).[5]

At the bureau, Mills's biggest initial responsibility was a study of opinion leaders in Decatur, Illinois. Mills appears to have been unable to direct the project to Lazarsfeld's satisfaction, but his failure helped shape the future direction of his research and his career. When the Decatur project eventually appeared as *Personal Influence* (Katz & Lazarsfeld, 1955), Mills was mentioned twice in the acknowledgements: once for "the whole organization of fieldwork for the study" and once for advice that was "extremely valuable, often opening up completely new perspectives on the data" (Katz & Lazarsfeld, 1955, p. *xiii*). These acknowledgements mark Mills's initial presence and later absence at the head of the study.

Mills and Lazarsfeld fought regularly over the interpretation of data. In part, this was a political matter. Lazarsfeld was primarily interested in a set of empirical questions: Who were opinion leaders in a community? How did they influence others' opinions? How did they relate to mass media content, and was their relation different from others'? Lazarsfeld hypothesized that opinion was shaped by "horizontal influence," that is, influence by peers. Mills, meanwhile, was firmly committed to notions like class and ideology, which suggested that

there might be "vertical" forms of influence as well. These conflicts emerged most stridently in a 1946 discussion draft of the Decatur project submitted by Mills to Lazarsfeld. Mills' several-hundred-page draft contained a wealth of data but also utilized notions such as "ideology" that Lazarsfeld found to be decidedly imprecise. Perhaps more important, the draft criticized some of the tenets of Lazarsfeld's empirical method, just as the lecture in Boston had suggested that important questions were not asked. But Mills was at a decided disadvantage in this exchange. Although Lazarsfeld was literate in the European social theory that had influenced Mills's thinking, Mills was essentially statistically illiterate. He was unable to argue with Lazarsfeld on Lazarsfeld's terms, though in his clearest moments Mills does point out some of the limitations of Lazarsfeld's method.

In an address presented to the American Association for the Advancement of Science, Mills (1946a) begins with the claims that the Decatur study was designed to "isolate and trace an actual flow of inter-personal influence in a community" and "isolate the types of people who would rightly be called opinion leaders." But to this he adds an important third project not realized in the final study:

> Since opinion leaders, detected by a simple criterion, had been found on all class levels, we wanted now to relate the influence flow which we were gong to trace (and the opinion leaders whom we were going to isolate) to the structure of social classes within a community: Does influence flow along the horizontal lines of each class or is it more likely to be a vertical flow down the class hierarchy? (pp. 2-3)

The address ends with a plea for help with interpretation, but the penultimate section deserves to be quoted in full because it is such a clear statement of Mills's struggles with his attempt to bring class as a factor into Lazarsfeld's research design:

> Our third general problem, that of stratification, remains: Does influence flow from the top of the class hierarchy downward, or does it flow more along each class level?
>
> As in the conception of the OL [opinion leader], we can infer from the specific flow or we can go directly to the images people have of the leaders, so here in this problem we have two ways of answering it and we encounter another tragedy insofar as our microscopic concern goes. For, first, we do not have enough cases of appropriately designated people actually linked together by specific opinion transactions for very reliable study; secondly, we do not have a long-enough stretch of flow, that is, we only have two flows, three sets of people involved, and thirdly, our leader chain is in politics and our "big numbers" on flow are in fashion—so we cannot readily compare the strata of the two kinds of people—the vessels of flow and the imagined leaders. Thus the problem of stratification and influence is very difficult to handle with these data, although we are still at work upon it. We are, however, in a better position when we try to answer this problem in terms of our *chain* of political leader, and here the answer is rather clear-cut:

> The chain of political leadership is definitely a vertical affair. If, for instance, we build a composite stratification scale out of occupation, socio-economic status and income, breaking the scale into the conventional four-fold break, we find that 68% of the leaders of follow-up group three are in the upper class, whereas only 6% of the members of the sample who do not know a leader are in the upper class; the intermediate links being appropriately graduated. (pp. 12-13)

In plain language, Mills attempted here to wedge a concept of stratification into Lazarsfeld's research design. Because Mills came late to the project—after the research program had essentially been fully designed by Lazarsfeld—read stratification into the data. The survey questions that generated the data did not really provide the information necessary to say much of substance about class as a factor in opinion formation. So in a weird way, Mills is actually correct, so long as we read this as a methodological critique of Lazarsfeld: They would have to ask different kinds of questions to get at the vertical influence Mills wanted to see.

Mills sent memos to Lazarsfeld on each of the 2 days following the presentation. In these two memos, Mills takes his position further—too far, really—in the hopes that stratification could be a major concept in the final report that would later become *Personal Influence*. In the first memo, dated December 30, 1946, Mills (1946b) attempts to sketch out a relation between macroscopic research, which would get at questions of social stratification, and microscopic research, of the kind in the Decatur study:

> We might begin with an example of a macroscopic statement such as "Bureaucracy centralizes the use of buildings" [in the text, "buildings" is scratched out and another, illegible word is scribbled over it in dull pencil—possibly "individuals" given the subsequent discussion]. Such a statement is ambiguous because one is not sure that it involves one variable or two variables. If it involves only one, then it simply spells out one of the implications of that variable. If on the other hand, it involves two variables, it may be a proposition, that is, a statement of a relation which may be true or false. Variable X is the organizational chart of a society; variable Y is the self-government of an individual. Thus we see the prepositional way to state this sentence is by making explicit that a bureaucracy (x) means the attribute of a certain kind of social organization which is related to (y) a psychological attribute of individuals.
>   It is clear that the bureaucratic and the non-bureaucratic society, as well as the self-governing and non-self-governing individual each form a scale which can, if one wishes, be turned into a dichotomy. One of the first things, then, that we have to watch for in macroscopic texts is whether or not a statement is a proposition or the unrolling of an implication. It is true that the propositional, as over against the conceptual implication, of many macroscopic statements is left ambiguous. (pp. 1-2)

In the rest of the memo, Mills attempts to explain to Lazarsfeld, in Lazarsfeld's own language, that it is not always possible to break down macroscopic concepts into microscopic concepts that can be tested on the level of an individual. In particular, Mills insists on the impossibility of reducing macroscopic con-

cepts to their microscopic components, because "as we go up the scale of complexity of indices and get into the bigger units, we must, in order to get comparative cases, either go to historical or cross-cultural analyses" (p. 4).

The second memo also attempts to convince Lazarsfeld, in clumsy mathematical language, that it is possible to diagram vertical influence in the Decatur results. Mills (1946c) begins with big, speculative assumptions

> the vertical influencer is more likely to be the same person than is the case with the horizontal influencer. The vertical flow is, therefore, one might say, more person-bound and the opinion leader who operates vertically may be a more 'generalized' kind of leader than the horizontal opinion leader who is more indiscriminate and more specific in leadership. (p. 2)

Or consider this one:

> One is influenced by many people, once or twice on one's own level, but one is influenced quite frequently by a few individuals who are above one. This might be diagrammed in the following way. Let N equal the number of people on each level who influences a respondent. Let S equal the average number of times the respondent is influenced by each individual on that level. Now, if S increases, the social distance between the influencer and the influencee increases. But if N decreases, the social distance between the influencer and influencee increases. Now flow can mean either S or N or S [times] N. (p. 2)

Here's the rub: Although his mathematical reasoning is fishy at best, he appears to be right on both counts: The data do not give him the materials he needs to make the claim about social stratification and vertical influence (as ideology or as interpersonal contact). But it is more than a little interesting that McFadden Publications, who commissioned the research (and published *True Story Magazine*), used the survey data in a vertical form—their presentation of it is a kind of textual proof of Mills's hypothesis. In other words, if vertical influence existed, it existed prior to or outside the domain of the study. So, if it is true that decisions in a community about which toothpaste to buy or which movie to see are generally made through horizontal influence—through talk among peers—it is also still true that the decisions about which movies or toothpastes arrive in the community are still a vertical matter—a choice made by elites outside the community. This point was not lost on McFadden, who put together a slide presentation that summarized the Decatur data for their own promotional purposes. Consider the excerpted slides (from McFadden Publications, 1946a) as shown in Figure 1.

Leaving aside the script's incendiary representations of advertising-as-propaganda (amusing as it may be), the clear aim of this pamphlet is to argue that magazine advertising facilitates exactly the kind of vertical influence Mills sought to identify. Here, the model is the same as with movie advertising. Ads in mass media publications function as top-down influences, but they are dis-

Visual	Voiceover
137. Shot of lighted match starting grass fire	Not as 50 years ago—where the limited few spread the fires of persuasion—slowly.
138. Shot of firebomb igniting section of city (stock)	But, as opinion leaders now exist in all strata of the population—this job of sparking mass advertising into action can take place on all levels—among all groups of people.
139. Direction arrow labeled "your campaign" pointed toward Opinion Leader Keystone from original 121 on which is "Reach, Tell, Sell"	In building today's and tomorrow's campaign—to best cash the advertising investments of both yesterday and today—build for the Opinion Leader—*reach* him—*tell* him—*sell* him—first!
140. Montage of production line—people buying	Today, mass production and mass consumption looms as the salvation of the American Way.
141. Shot of magazine articles on American themes	We must sell not only things—but also ideas—American ideas; and get those ideas spread faster and more effectively than ever before!
142. Spotlight of report (pretty)	If this report makes any contribution toward more effective selling, and idea spreading—however small, it will have fulfilled the aims of its sponsor . . .
143. Cover of *True Story*	*True Story* Magazine
144. "For 27 Year the Wage Earners' Favorite Magazine"	For 27 years the Wage Earners' favorite magazine.

Figure 1: Excerpt From McFadden Slide Show Based on Decatur Study

tributed through Lazarsfeld's two-step model. This is a stunningly banal point, but one that could not have been accounted for on Lazarsfeld's terms. The respondents in the Decatur study only knew of products through encountering them in stores, via opinion leaders, or through advertising. "Hand in hand with the growth of the new leader groups has been the growth in means of communication with them" exclaimed another McFadden (1946b, p. 43) publication for advertisers.

Similarly, a colorful booklet published by the Association of Screen Magazine Publishers (1946) used the Decatur study to argue that movie studios could advertise in their magazines and achieve exactly the kind of vertical influences Mills was talking about. Whereas it neatly outlined Lazarsfeld's two-step

model of influence and summarized some of the key Decatur study findings, the booklet's point to advertisers was clear: The Decatur data show that if they advertised in the magazines read by opinion leaders, they would influence more people to go to the movies, thanks to the word of mouth generated by those opinion leaders. Not surprisingly, the booklet also suggests that opinion leaders are less likely to read magazines such as *Life* and are more likely to read magazines published by members of their association, such as *Movie Life* and *Photoplay.* Advertising was exactly the kind of top-down influence that Mills was looking to identify (in the sense that it was generated by elites, not in that the content of advertising guaranteed a given result or "caused" audiences to do things), and here it was in the *use* of the study after the fact.[6]

Lazarsfeld's responses to Mills are not in the archives, but we know the final result. Incompatibilities with Lazarsfeld and other problems led to Mills's reassignment from the Decatur project to a study of Puerto Rican immigrants in New York City.[7] Along with Mills's Marxist notion of class, his hope for a discussion of vertical influence in published study lost out. But a demographic sense of class does indeed appear in the final version of *Personal Influence* as a conditionally significant factor in the guise of "social status," an agglomeration of education, rent, and employment (see Katz & Lazarsfeld, 1955, pp. 370-371). So Mills never received an authorship credit on *Personal Influence* because of intense personal and methodological disagreements. But he was at the very center of the study during its conduct, and as I will show below, his work on this study provides a rich empirical base for the later work for which he would become famous.

After their methodological rift became too great, Lazarsfeld reassigned Mills to the research team that would eventually publish *The Puerto Rican Journey* (1950). Treated by many Mills scholars as an aberration (e.g., Scimecca, 1977, pp. 68, 127), the *Journey* is officially a collaborative work, listing the names of Clarence Senior and Rose Kohn Goldsen on the title page alongside Mills's. Granting that the book does read differently than Mills's later works, we can still find distinctively "Millsean" passages in it (e.g., p. 156). More important, it was composed in much the same fashion as his better known later works.

The actual work of the project was heavily divided, with Mills ostensibly responsible for the design and execution of the study, but functioning in a supervisory role. Senior was the only member of the authorial team who had firsthand knowledge of the Puerto Ricans discussed in the project, probably because he knew Spanish. Goldsen did the coding and statistical analysis, and Ruth Harper reviewed the data and made "the final package work" (Horowitz, 1983, p. 81; Mills, Senior, & Goldsen, 1950, pp. *viii-xi*). In other words, Mills could not have done the study alone. In fact, because he did not know Spanish, he could not have done the study at all. The same is true for *The New Men of Power.* Mills oversaw the labor research division of the bureau, and this meant that he had a massive team at his disposal. One biographer argues that "one is entitled to wonder, given the extent of help acknowledged in the footnotes,

whether others also ought to have been listed on the title page" (Eldridge, 1983, p. 63). Helen Schneider, who is credited on the title page for her "assistance," prepared the "essential empirical memorandum and the analysis of poll materials." Hazel Gaudet also made significant contributions to the statistical work, Maud Zimmerman constructed, tested, and redesigned questionnaires, and Ruth Harper oversaw the final preparation of the manuscript as a whole (Horowitz, 1983, p. 213; Mills, 1948, pp. 295-296; Scimecca, 1977, p. 120). In the late 1940s, we can say without equivocation that Mills was at the head of research teams quite like the ones he roundly criticizes in *The Sociological Imagination* (see Mills, 1959, pp. 100-118).

There is also clearly a story to be told here about women in the bureau. The bureau employed many women, but as I mentioned above, the male-dominated field prevented their advancement or even recognition in the profession. Most significantly, Ruth Harper, who would become Mills's second wife, was essentially his coauthor in *The New Men of Power*, *White Collar*, and *The Power Elite* (Horowitz, 1983, p. 213; Mills, 2000, pp. 146-147). The bureau clearly provided for the employment of a large number of women (Horowitz, 1983, p. 213), but these women encountered a very low glass ceiling in the field— they had little chance for significant career advancement in a male-dominated field. Like his Columbia colleagues, Mills benefited enormously from this state of affairs. For instance, in a December 1946 letter to his parents, Mills mentions the *White Collar* project and lists seven women as research assistants and describes the work they are doing for him (Mills, 2000, pp. 101-102). He essentially published as an individual what had in fact been constructed by a team of women. His early career was built upon an unacknowledged foundation of women's work. It appears that the C. Wright Mills who occupied the title pages of several midcentury classics—that icon of midcentury intellectual machismo—was actually Charles Wright Mills, Ruth Harper, and a whole group of mostly female assistants.

It is worth emphasizing that this was a structural issue: Other sociologists of the time regularly used the work of research assistants—male or female—without crediting them. The young Mills was occasionally on the receiving end of this approach to research-team sociology, where his own work was published under someone else's name (see Eldridge, 1983, p. 63; Lazarsfeld, 1947). Still, the larger question of the gendered history of sociology and mass communication research requires a great deal of further study. Promising beginnings can be found in Susan Douglas's (1999, pp. 139-148) writings on Herda Herzog and the Office for Radio Research. Mills's career path suggests that lurking behind the founding fathers of modern social research and midcentury sociology is a whole group of founding mothers. This is a matter I will pursue at length in another essay.

I hope that you are already able to see some cracks in the edifice of C. Wright Mills, "the lone radical," or C. Wright Mills, "the public intellectual." His ultimate success and importance was a result of a whole collective effort among a

group of scholars. It was certainly a form of collaboration, though all collaborators were never on equal terms.

White Collar (Mills, 1951) is often cited as a turning point in Mills's career—where he essentially becomes the "intellectual craftsman," as he is later known (Gillam, 1981; Horowitz, 1983; Scimecca, 1977). It bears the unmistakable Millsean style and is now likely considered a more significant contribution to American social thought than it was at the time.[8] In part because of Mills's conscious pursuit of a literary style antithetical to much of the sociological writing of the time (see Gillam, 1981, pp. 7-9), it is easy to miss Mills's extensive use of research teams, quantitative analysis, surveys, and 128 intensive interviews in building the study. In fact, it is largely due to editing—rather than composition or method—that *White Collar* reads like a piece of critical research and not administrative research. On top of Mills's attention to literary form and style, Oxford University Press decided, at the last minute, to cut Mills's extensive scholarly notes because there were so many of them (Gillam, 1981, p. 9; see also Swados, 1963). In place of a more substantial scholarly apparatus, we find an eight-page essay that combines bibliography and acknowledgements (Mills, 1951, pp. 355-363). So although the final form of *White Collar* downplays Mills's association with the bureau, his work during this period could not have been done without it (Eldridge, 1983, p. 73). To be clear, it does not appear as if Mills actively *concealed* his debt to the bureau in *White Collar*—it is freely acknowledged at the end of *White Collar* and likely would have been spelled out in greater detail in the endnotes had Oxford University Press kept them. So, although we associate the kind of prose Mills wrote with research that was generated in the intellectual craftsman style, that is more a matter of misguided retrospection than accurate description. All this is to say that if we read *White Collar* as a turning point in Mills's career, we reduce the distinction between administrative and critical research to a *formal* distinction, a matter of writing style rather than object construction, funding sources, and modes of inquiry.

Mills's time at the bureau quickly diminished after his battles with Lazarsfeld in 1946, but he continued his association with it for another decade. That continued association brought him grant money and research support; he even wrote drafts of chapters for the Decatur study on into the 1950s (see Mills, 2000, pp. 141-142, 172). The bureau and Columbia proved invaluable to Mills and crucial to the construction of *White Collar*. His presence at the bureau helped him secure research grants and organize research teams for the 128 interviews that were an important empirical base for the book.[9] Most of those interviews were conducted by others, and according to one biographer, Mills interpreted them "with a fair share of imagination" (Gillam, 1981, p. 7). In the acknowledgments to the book, we can find references to a war plants grant to study six midwestern cities, the Decatur study ("a more intensive study of one middle-western city of 60,000 population, in connection with a research project undertaken for the Bureau of Applied Social Research"), and almost all

of his other research projects at the BASR (e.g., *The New Men of Power* and *The Puerto Rican Journey*)—"in all these jobs, I kept my eyes open for 'white-collar material'" (Mills, 1951, pp. 355-363, at 356). Mills also made use of the work of his students and ideas developed in collaboration with Hans Gerth (see Oakes & Vidich, 1999, pp. 106-111).[10] *White Collar* took a long time to complete, in part because Mills was so busy with other projects and teaching and in part because he needed the data from his other projects to complete his research for the book. As John Eldridge (1983) writes, Mills' data collection for *White Collar* was "in the best sense, opportunistic" (p. 73).

The sources of *The Power Elite* are less well documented in the secondary literature but appear to merely build upon Mills's research base from *White Collar*. Though Mills inhabits the title page all by himself, the notes at the end of the book clearly make Ruth Harper Mills—"my chief researcher and editorial advisor"—his coauthor (Mills, 1956, p. 364; see also Horowitz, 1983, p. 213; Mills, 2000, pp. 146-147). The acknowledgements for *The Power Elite* reveal that it depended on many of the same sources as *White Collar*: a fleet of research assistants and "research memoranda," a secretary, a grant from the Social Science Research Council of Columbia.[11] The notes confirm this hypothesis. We find extensive discussions of data collected and analyzed for the study—almost certainly the work of Harper and not Mills. The notes also contain references to the Decatur study, the six-city war plants grant study, the Puerto Rican study, and *White Collar* itself (Mills, 1956, pp. 365-412). Though Mills does not say it, one suspects that he could have added a note analogous to the one at the end of *White Collar*—"in all these jobs, I kept my eyes open for 'Power Elite' material."

In providing the institutional history of these publications, I have mostly set aside considerations of content and style. On even the most superficial reading, it is obvious that Mills's style differs markedly from the works of Lazarsfeld, or even Merton. But to cast these formal, stylistic differences in scholarly writing as a total renouncement of administrative styles of social science research is grossly inaccurate. Mills's substantive work was very much the result of administrative research techniques, even if it did not read like administrative research. So although we can acknowledge the ideological and stylistic differences between Mills and many of his contemporaries, it would be wrong to say that Mills totally broke with the administrative approach to social research. More accurately, he acquired data in the administrative fashion but then moved away from the standard positivist procedures for making use of them.[12]

If we have a different historical image of Mills, it is largely because he would later represent himself as having rejected administrative research more fully than he actually did. Mills's *self*-representation turns out to have a great deal of influence on how he is remembered today, so it is only natural that we must turn a critical eye toward the self-representations of "C. Wright Mills, the critical sociologist."

The Meanings of Mills

Though a few authors have acknowledged the importance of administrative-style research to Mills, he remains an icon of critical social theory, more famous for his attacks on research bureaus and statistical analysis in *The Sociological Imagination* than his appropriation of those techniques not 3 years prior. Guy Oakes and Arthur Vidich (1999) write—with some contempt—that Mills was portrayed as "a political idealist, an iconoclast, and a principled radical. Mills was the conscience of American social science. Unbending in his devotion to intellectual honesty, he exemplified a flinty integrity that ruled out any compromise with existing institutions." This view was perpetuated

> in the various "C. Wright Mills awards" that leading members of professional associations of social scientists confer on one another and is now enshrined as the received view of Mills, who has been canonized as a twentieth-century exemplar of the Enlightenment tradition of critical reason. (pp. 111-112)

Oakes and Vidich find the view of Mills to be more a product of his own imagination than the real deal: In their account, Mills relentlessly pursued success as "an intangible goal": "There was always another book to write, a bigger contract to close, a new audience to win over, and a more promising job on the horizon" (p. 113).

As I stated above, the purpose of this article is not to assess Mills the person. Even if we accept characterizations of Mills as a less-than-wholesome character, insinuations of clumsy interpersonal interactions and accusations of hypocrisy do not invalidate the content of his writings or their significance. Many of the intellectual heroes of the 20th century were not nice people, and I cannot help but think it is some kind of peculiar professional malady that leads us to think that they should have been nicer interpersonally (on the problem of charisma in assessing scholars' work, see Bennett, 1993).[13] So although Oakes and Vidich have a point about the Mills mythos and integrity, one can still find value in his intellectual work. This is the fundamental condition of textuality: Once published, the text is no longer simply an extension of the author's persona (Barthes, 1977; Chang, 1993; Derrida, 1976).

At the same time, there are real intellectual and political stakes in the Mills myth that Oakes and Vidich identify: Taken at face value, it assigns certain research methods to certain political views, a priori. I began this article by arguing that Mills's work at the BASR has been wrongly dismissed by both hagiographers and detractors. Now that I have shown the centrality of Mills's Columbia connections to his research, I turn to the question of how Mills's own writings gradually move toward a view of social research as divided into administrative and critical camps. Given that this was not a *necessary* interpretation of Mills's work, how did it emerge and become the dominant interpretation of Mills's career? A large part of the answer lies in Mills's own writings.

Mills was trained in the European critical and American pragmatist traditions of sociology. Though he was developing an interest in stratification issues, the young Mills was more or less a theorist and commentator. When he arrived at the bureau, Mills had no statistical training and no strong disposition toward empirical research. His new supervisor, Paul Lazarsfeld, meanwhile, was steeped in mathematics and committed to methodology as the path to sociological knowledge. In a mid-1945 letter to Hans Gerth, Mills wrote:

> Lazarsfeld I find a wonderful man to work with; he gives me ingenious technical advice when I ask for it. The fellow has got me to see the inside of statistical manipulation in such a way that I see the quantity-quality shuttle operation so as to work with it for the first time. 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, 2000, p. 171)

Mills's early BASR publications reflect an even more pragmatic attitude (in contrast to the received view): Use quantitative approaches where they work, go theoretical when it works. *The New Men of Power* opens with a simple statement on method:

> In our method we have combined the statistical and the qualitative. Marx once remarked that the calculation of averages is the most explicit form of contempt for the individual. And yet, without "averages" the modern understanding is greatly enfeebled. In this book we have tried to avoid both the arbitrary trivialization that results when understanding is on the level of the anecdotal biography and the sterile contempt for individual reality that results when understanding is reduced to the statistical summary. We have tried to proceed as systematically, accurately, and carefully as possible in an inchoate field of study. We hope this is not inconsistent with our further aim to be politically relevant. (Mills, 1948, p. 10)

Here there is no yawning split between administrative and critical research. Instead, there is the interplay of what Mills would later call the personal and the social— inner subjective experience and larger scale social activity. The introduction to *The Puerto Rican Journey* makes a similar point, that statistical research leaves out the larger and deeper texture of experience:

> Confronted with a subject as alive, deep, and varied as a people on the move, we have had to rely largely on the collective and somewhat distant experience which statistical research offers, catching it in bits and pieces which we then try to fit together into some sort of understandable pattern. (Mills et al., 1950, p. *vii*)

But Mills—the person—was definitely dealing with concrete methodological issues during this period. He and Lazarsfeld were constantly at odds over the Decatur study. Since Mills's first discussion draft of the Decatur material in 1946, Lazarsfeld had constantly criticized Mills and demanded revision upon revision. Mills wrote to Gerth in February of 1952,

> You remember that for six years now I've been writing and rewriting 'The Decatur' ms., with him advising and etc. Well he gave me the latest draft again last week and we met Wednesday morning, and he asked me to do a complete rewrite of about 130 pages of it. As nicely as I could I told him no. The time had come either to publish it or if he doesn't like it still, to rewrite it himself. I offered to sit with him for one full day a week and revise together but [said] that I felt utterly unable to listen to him for an hour and then spend six weeks trying to get into it what he had said. (Mills, 2000, p. 172)

Mills's status on the project had long since switched from oversight to a more contributory role. He may long have been revising the Decatur manuscript, but he had not overseen the project since 1946. Still, Mills characterized this fight as "a complete break":

> 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. . . . To hell with the professional acclaim I'll lose (p. 172).

Indeed, Mills was taken off the project and his name exists only in the acknowledgements. One could probably write an entire essay about Mills's involvement with the Decatur project and his battles with Lazarsfeld; the important point here is that Mills represented 1952 as a significant moment of disinvestment in Lazarsfeld's brand of empirical social science research. Still, we cannot make too much of it: As I have shown above, this personal break with Lazarsfeld was not the same thing as an institutional or intellectual break with research teams, statistics, and putting his name on collective efforts.

As Mills made a break with Lazarsfeld, he began to narrate his own work— and more generally the work of social research—in different terms. A shift in Mills's writing is apparent in two rarely cited essays that emerged from this period. The first, titled "Two Styles of Social Science Research," is more or less Mills's version of Lazarsfeld's administrative/critical dichotomy (cf. Lazarsfeld, 1941, and Merton, 1957, pp. 3-16). Mills cast macroscopic social research as growing out of the European critical tradition. Macroscopic social science dealt with

> total social structures in a comparative way; their scope is that of the world historian; they attempt to generalize types of historical phenomena, and in a systematic way, to connect the various institutional spheres of a society, and then relate them to prevailing types of men and women. (Mills, 1953/1964c, p. 554)

To this he contrasted molecular research, which deals with "small-scale problems and by generally statistical models of verification" (p. 554). Though his criticism of molecular research is measured, Mills is not kind to the field as a whole, denigrating it as coming out of marketing and mass communication research and portraying it as relatively easy work. Of *The People's Choice*, a groundbreaking study of decision making in a presidential election by

Lazarsfeld, Bernard Berelson, and Hazel Gaudet (1944), he wrote, "It is possible within a few years to train competent persons to repeat a Sandusky job; it is not so possible to train them to repeat a Middletown study" (Mills, 1953/ 1964c, p. 557). This is a rather unfair statement, given that Mills himself apparently could not be trained to do Lazarsfeld's style of social research. But immediately after this slam, Mills does acknowledge that the more methodologically standardized approach to social research not only allows for the possibility of checking results by reproducing them but for accumulation of knowledge, at least in some cases.

Mills's ultimate vision in this essay was not exactly a synthesis between macroscopic and molecular research, but rather more mutual attention—a kind of discipline-wide synthesis. "We must build up molecular terms; we must break down macroscopic conceptions" (p. 566). Mills realized that his goal could not be accomplished by a single individual, so he called for a "working agreement on a grand model" that would include both the macroscopic and molecular approaches. This working agreement would in turn require sociologists to develop a "clear consciousness" of the place of their various projects within the larger framework so that they could "aid another specialist in the architectonic endeavor" (pp. 566-567).

It is very tempting to read this passage biographically: as if Mills wanted some kind of reconciliation between his approach and Lazarsfeld's—that there was enough space on the Columbia faculty and in social science for both of them. As history would have it, there was indeed. But Mills was working through something more extensive. The published version of the piece was apparently toned down from an earlier version. "I had another 5 pages or so slanted against the Molecular stuff but left it out in reading because wanted a balance for once and besides no time to read more than this" (Mills, 2000, p. 145). Even here, we must be careful not to read the letter as representing Mills's true feelings on the matter—I can assure the reader that earlier drafts of the article in your hands do not necessarily represent my truer feelings on the subject of Mills's career. A more mundane reading is in order: At this point in his career, Mills took that safe and ultimately futile position of arguing for a "grand synthesis" of approaches in sociology. Grand synthesis arguments sound more pluralistic than they actually are: Although they attempt to suggest there is room for everybody in a field, they simultaneously install a hierarchy of position in the field. They are ultimately exclusive, not inclusive, gestures— and that is why they are ultimately futile arguments. A disciplinary synthesis of competing positions involves subjecting one to another—and if there is a true desire for pluralism in a field, this kind of thinking ultimately undermines it. Though Mills casts himself as at the center of professional sociology in a call for a synthesis, in his other writings he places himself at its margins.[14]

So Mills's "Two Styles" (1953/1964c) perhaps deserves its obscurity. But a second article from roughly the same period reads like the Mills more often remembered today. Titled "IBM Plus Reality Plus Humanism = Sociology" and

first published in the May 1, 1954 *Saturday Review of Literature*, this piece more or less accedes to the impossibility of "balance" in social research. It foreshadows many of the most well known claims in *The Sociological Imagination*: that too much attention to method (what he will later call "Abstracted Empiricism") trivializes sociology, that impenetrable prose and unending theoretical discussion renders sociology irrelevant, and that research teams turn sociology from an intellectual endeavor to a bureaucratic one. In this earlier essay, Mills trumpeted that American sociology was

> now divided into three main camps. . . . I hold that only one of these camps is worthy of the name sociology, and accordingly, I am not even going to mention the names of the leading members of the other two. Some of my best friends are in those camps, but they will have to blow their own horn. (Mills, 1954/1964a, p. 575)

Mills names the two "unworthy" camps the scientists and the grand theorists. The scientists have moved "from marketing research to the foundations, and so from toothpaste and soap to higher mathematics"; the grand theorists have moved "from textual interpretation of sociological classics to careful thinking about their own possible thought" (p. 575). Mills's scientists are statisticians who, "by the costly rigor of their methods, . . . succeed in trivializing men and society, and in the process, their own minds as well." The trivialization is important on two fronts: It renders the scientists incapable of describing or analyzing "the major problems for men of this historical epoch" and it renders them politically harmless and therefore "easier to 'administer.'" The grand theorists do not fare any better: "In turgid prose they set forth the disordered contents of their reading of eminent nineteenth-century sociologists, and in the process mistake their own beginning for a finished result" (pp. 569-570). Like the scientists, the theorists are straw figures—Mills's argument is nothing more or less than a deferral of theory, rather than an engagement with it (see West, 1989).

Mills's positive move is, of course, a version of critical sociology. He sides with sociologists who ask the following questions:

> (1) What is the meaning of this—whatever we are examining—for our society as a whole, and what is this social world like? (2) What is the meaning of this for the types of men and women that prevail in this society? And (3) how does this fit into the historical trend of our times, and in what direction does this main draft seem to be carrying us? (Mills, 1954/1964a, p. 572)

These are more or less the same arguments he will publish 5 years later in more developed form as *The Sociological Imagination*.

At this point, Mills appears to have driven the wedge between administrative and critical sociology. The flippant tone of the piece certainly downplays his own career debts. Although Mills's prose never achieved the sheer complex-

ity of a Talcott Parsons, he began his career as a sociologist of knowledge and commentator on Weber. Mills's debt to research teams for the vast majority of the empirical data in his major works disappears when he gets in a potshot at bureaus: In praising William Whyte, Mills portrays him as "the old-fashioned Man Who Goes Into The Field, rather than sending four dozen researchers there" (p. 574). Again, there is a temptation toward psychobiographical readings, where the Mills of the IBM essay and later *The Sociological Imagination* is not the Mills that was, but rather the Mills that Mills wanted to be. But the larger point is that in the mid-1950s, we see two distinct—yet nearly simultaneous—treatments of method and inquiry by Mills. "Two Styles" has a holistic view of sociology as a field where no one person can do everything and so everyone must work together. It seems that in his IBM piece, Mills moves toward a vision of the sociological man who can do it all on his own: William Whyte without the research team.

Reading this attitude against his career, it quickly becomes apparent that Mills the person clearly could not do it all. Had he been entirely responsible for his own research, Mills would have been considerably less prolific and would have written very different books. The irony is that without the administrative apparatus behind him and a fleet of women research assistants around him, Mills would likely have had less of an enduring significance as an intellectual craftsman or a critical sociologist.[15] His IBM piece would have been less likely to appear in the *Saturday Review of Literature* if he were still a professor at the University of Maryland. His own work would have had a significantly smaller empirical base. Most important, without his time at Columbia, his own ideas about critical sociology versus its foils would not have developed in the same fashion. In other words, without C. Wright Mills the administrative sociologist, it is quite likely that we would not have C. Wright Mills the critical sociologist.

Mills's most remembered book today, and certainly the one that has had the greatest impact upon this author, is one that Mills managed to write without the aid of a research team, and the one that is perhaps most empirically inaccurate. As with "Two Styles," Mills's final, published version of *The Sociological Imagination* was toned down from an earlier draft (Swados, 1963, p. 39). This is not to say that the final version was subdued. It extended his arguments in the IBM piece, this time naming names. Where he had merely suggested translating grand theory "into English" in the IBM piece, he actually translates long sections of Talcott Parsons's writing into a short paragraphs of Millsean prose (Mills, 1959, pp. 27-33). He repeatedly names Paul Lazarsfeld as an advocate—virtually an ideal type (or more accurately straw figure)—of abstract empiricism.[16]

It is easy to read this as a personal matter between Mills and Lazarsfeld. There is some evidence that the two men did just that (see Horowitz, 1983, pp.

95-97; Mills, 2000, pp. 230, 257). But a more interesting reading of *The Socio-logical Imagination* casts it as a product of an imagined administrative and criti-cal divide, and the force that imagined divide exerted in the field of social research itself. Juxtaposing *The Sociological Imagination* against Mills's earlier work, we find it a book that was enabled by the very types of research that it is often misread as dismissing. Mills's knowledge of abstracted empiricism and the bureaucratic ethos was firsthand knowledge.

More to the point, his appendix on intellectual craftsmanship *presupposes* the resources to do empirical work. Mills ultimately defers the question of method. Although this would be appropriate for the grand, field-surveying commentary that fills most of the book, it is most inappropriate for a supposedly methodological appendix. Whereas Mills is right to point out that questions of method are often much more mundane and study-specific than methodologists might claim, he ultimately obscures his own methods because he represents himself as a lone researcher. Mills's appendix to *The Sociological Imagination* must be read against the methodological commentary in *White Collar* and *The Power Elite*: He was wholly dependent upon the work of others to write the books he did. So although the methodological appendix to *the Sociological Imagination* reads persuasively, it is ultimately fantasy.[17] "*The Sociological Imagination* is a work of Mills' imagination. In it he thinks and writes vaingloriously. He constructs images and pictures of society, men (seldom women), and history real only within his 10 chapters" (Denzin, 1990, p. 2). To this list we might add method, because Mills the methodical individual is not the Mills who built three landmark social studies off the work of research teams and BASR-funded time off from teaching. Mills appears in *The Sociological Imagination* as an "objective observer" of American society and American sociology (Denzin, 1989, p. 279). Because of this authorial voice, *The Sociological Imagination*—a critique of the institution of sociology—ultimately obscures the position that made possible its writing.

So to treat *The Sociological Imagination* as an empirical description of the world of 1940s and 1950s sociology or Mills's career is to abstract the book from its historical and institutional context. In this sense, *The Sociological Imagination* embodies the contradictions of Mills's career quite neatly. Though it was based in reality—this time Mills's experience rather than others' research—Mills's work was once again "not merely a logical indictment which could be upheld or attacked, but a poetic vision" (Swados, 1963, p. 40). Whether considered a document of its time or a vital text of a living tradition, *The Sociological Imagination* works best as a rhetorical work (Denzin, 1989, 1990) and a manifesto. "The manifesto is the form that exposes the broken promises of modernity" (Lyon, 1999, p. 3), and indeed, there are many still-broken promises of social thought to expose; many of Mills's critiques of others are as applicable today as they ever were. Read with a little humor, parts of it retain their power: There are whole new generations of theorists waiting to be "translated into English" as Mills did with Parsons. Mills's claims for his era as

postmodern should tell us something about similar claims made for our own moment. Mills's basic precepts remain relevant today: Engaged academics should study the crucial problems of the day, avoid method for method's sake, and avoid theory for theory's sake. To do otherwise is to risk conflating the problems of disciplinarity and its limits with the problems of the social world outside the academy. But we can also turn the critical and imaginative sensibility so lauded in the pages of *The Sociological Imagination* against itself, and when we do, we see a very different picture of men and women in social science than the one painted in the pages of Mills's most famous text.

Conclusion

So it turns out that the career of one of the central critical social thinkers of the 20th century was more or less built atop the edifice of administrative research. The implications of this simple insight begin with the historiography of social research and reach out and forward into our own moment. Mills's dependence on the BASR, the department at Columbia, and a fleet of women research assistants should cause us to hesitate in accepting the baggage that comes with labels such as *administrative* and *critical* applied to midcentury sociology and mass communication research. Mills was not in any simple way a critical researcher as we might figure the term today. Words like *administrative* and *critical* serve very well in building infradisciplinary fiefdoms and in the parent-killing exercises inevitably undertaken by young and old scholars alike. But using these labels to describe midcentury research is destructive of both historical memory and future imagination.

More important, Mills was never a lone figure—C. Wright Mills the academic superstar is a myth, and it points to the larger mythology of academic superstardom. Sure, there are deserving people in every field whose work is rightly recognized as significant and central; there are deserving people who are asked to speak on radio, appear on television, and write columns for the papers. But the ideological individualism of these media, and indeed, the habits of conference invitations, university public relations departments, and professional associations encourages us to think of scholars as individuals. In reality, even the most critical researcher is connected to a broad network of other people, grounded in a particular institution, and enmeshed in a field of ideals and ideas. The myth of the academic superstar may be tempting because it is based on the assurance that we are, as individuals, ultimately in control of our destinies, able to do it all by ourselves. But the reality could not be further from this fiction: We are fully, totally, and wonderfully dependent on one another. Mills's legacy ought to be adjusted to acknowledge the contributions of the many women who actually did the research for his most famous books. And this is a line of writing I will pursue elsewhere. Oakes and Vidich (1999) have shown the degree to which Mills's reputation as a social theorist was bound up with the career of Hans Gerth, who fared less well in the collective memory of sociolo-

gists. So even as Mills wrote attacks on the social stratification and exploitation, he certainly benefited from the stratification built into sociology.

Behind every great scholar lies a whole network of people, institutions, ideas, and ideals. We ought to celebrate and nurture those networks rather than the individuals for whom they come to be named. It was true for Mills, and it is true for every other major critical thinker—whether we consider sociologists such as Giddens and Bourdieu, or even philosophers like Habermas, Derrida, and Deleuze. In a very real way, academics have yet to fully digest the critiques of authorship that they routinely cite. Perhaps the least remarkable point of this article is to show that like Barthes's (1977) dead author, we cannot get back to the true intentions of C. Wright Mills; and like Foucault's (1977) nonexistent author, the C. Wright Mills cited in so many books and essays more marks a position in a discourse than a biographical individual. But it is most remarkable how enthusiastically we cling to these fictions, often to the professional benefit of those atop the social hierarchy in any given field—and to the detriment of those at the lower rungs of the prestige ladder—especially those outside the tenure track. Mills's story challenges us to find new ethics for representing the creative and research processes behind all scholarship. It is time to outgrow our comfortable fictions.

The fact that important ideas *circulate* leads us to yet another important lesson from the Mills case. Rigid methodologism or theoretical loyalty, whether from the positivists or from the radicals, obscures the ways in which different paradigms can feed off one another in ways that are neither acceptable nor necessarily visible to partisans of any particular paradigm. The point is not that we need some kind of grand synthesis of theoretical and methodical, quantitative and qualitative, textual and ethnographic, cultural and political-economic, historical and contemporary—or any other of the binary sets of approaches in our field. Even Mills's vision in "Two Styles" of a field unified by purpose if not method is ultimately a damaging fantasy. There are—and should be—real conflicts among positions in our field that cannot simply be synthesized with one another. To seriously take in these points is precisely to put aside the administrative and critical divide as some kind of ontological division in research. Method and style cannot possibly guarantee politics. As both administrative and critical researchers have pointed out, there are many different possible roads for researchers to travel (Lazarsfeld & Rosenberg, 1955; Mills, 1959).

Creative work often cuts across academic binaries in ways that may not be acceptable to partisans of either side. This does not mean that we can guarantee or legislate the ways in which this cross-fertilization will occur. The best work is often the stuff that we do not see coming. Of course, we should be reading across fields, learning from one another, and looking for connections. There is no substitute for intellectual breadth and exploration. But we should not seek a field where we all write—or even care about—the same stuff. An orthodoxy of critical research is just as damaging and regressive as any other academic orthodoxy. In fact, one can find in the pages of journals and conference programs

endless examples of so-called critical research that has forgotten its own reason for being. The critical is no longer critical when it becomes an end in and of itself, merely an entrenched and clichéd position to take up in a conflicted academic field. This is one place where the *Sociological Imagination* still rings true as inspirational literature: The professional is not automatically the political. A Marxist or postcolonial analysis of a text no more makes its author a political radical than a quantitative analysis of a survey makes its author a conservative. Methods matter, to be sure, especially as we interact and represent the subjects of our research. But to borrow a classic line from Stuart Hall, we cannot guarantee in advance *how* or *under what conditions* they will matter.

Eric Rothenbuhler wrote in 1993 that he hoped the future would find the field in which I was educated, communication studies, "wildly pluralistic, broader and deeper, and more diverse than it is now" (p. 158). This would be a good future for any of the human sciences. One way to achieve that future is to look to our past for examples of pluralism, breadth, and diversity that we might have missed before. The case of C. Wright Mills, administrative researcher, offers us one such opportunity. Even as we deflate the Mills mythos to show the collectivity at its roots, even as we recover the history and memory of the women who made Mills's oeuvre possible, Mills's legacy shows us that the combinations of voices—some intelligible, some not—that hit our ears as cacophony today may turn out to be the building blocks of new harmonies we cannot yet imagine.

Notes

1. His association with Veblen in the secondary literature is no accident: Mills published a new introduction to Veblen's (1899/1953) classic *Theory of the Leisure Class* in 1953.

2. Feel free to add scarequotes around *administrative* and *critical* for the duration of this essay.

3. Without diminishing other parts of Oakes and Vidich's (1999) thorough critique of Mills, this is simply a silly charge. By this standard, any kind of principled opposition within a society would be impossible. More perniciously, this reasoning leads to a kind of conservatism where those in privileged positions are not responsible for criticizing and attacking social inequity, because they benefit from the system. It reduces oppression to a problem solely of the oppressed.

4. Merton was also instrumental in getting Mills his first job. After publishing an early essay on the sociology of knowledge (Mills, 1939/1964b), Mills sent Merton a copy and asked for comments. The two struck up a correspondence (Mills, 2000, p. 35). In 1942, Merton was approached by Carl S. Joslyn, chair of the sociology department at Maryland and a friend from graduate school. Joslyn was looking to hire a couple of young professors, and Merton immediately recommended Mills (Horowitz, 1983, pp. 58-61).

5. Strictly speaking, *Character and Social Structure* resulted from his ongoing collaboration with Hans Gerth, but through his collegial association with Merton, Mills persuaded him to write an introduction for the book (Eldridge, 1983, p. 54; Oakes & Vidich, 1999, pp. 57-90).

6. In a famous piece almost four decades later, W. Phillips Davison (1983) would show that people tend to assume others are more affected by media messages than themselves. This third-person effect is essential to the advertising messages in both the McFadden slide show and the movie magazine publishers' pamphlet.

7. Irving Horowitz (1983, p. 80) suggests that Mills's mismanagement of funds was the reason that he was taken off the Decatur study. It was probably a contributing factor, but given that Lazarsfeld also had a habit of going way over budget, it was most likely the conflict over approach that led to Mills's reassignment. Still, according to Horowitz (p. 80), by agreeing to do the Puerto Rico study essentially for free, Mills redeemed himself with many of his colleagues.

8. *White Collar* was quickly overshadowed by *The Lonely Crowd* (Riesman, 1950). Mills considered Riesman his rival as "maverick sociologist"; his greater success was apparently something of a blow to Mills (see Horowitz, 1983, p. 149; see also Kornhauser, 1967, for a comparison of Mills and Riesman).

9. Richard Gillam (1981, p. 4) rightly points out that the germinal idea for *White Collar* came out of a 1942 review of James Burnham's (1941) *The Managerial Revolution* coauthored by Mills and Gerth (1942/1963). But *White Collar* would have looked very different without a team of researchers to acquire data for Mills.

10. This is a particularly damning passage in their book in which Oakes and Vidich (1999) show Mills lifting significant passages from a student's paper and diminishing the role of Gerth in the conceptualization of the project. It should be noted, however, that some of the citation and attribution that Oakes and Vidich find lacking *may* (and I stress the uncertainty here) have been in the original apparatus that was cut by Oxford University Press.

11. Mills also credits a few anonymous informants.

12. If we take Mills's work seriously on its own terms, it would also be wrong to criticize his oeuvre—as some have—as an aberration or adulteration of administrative research. It was, in the end, an attempt to negotiate institutional forces, political or intellectual commitments, and stylistic aspirations.

13. In fact, Mills the person is very enigmatic. Capable of great interpersonal warmth and total self-absorption, marathon writing sessions and marathon sleeping sessions, tremendous gluttony and utter despondence, egotism and self-deprecation, Mills's biographies and letters read like a textbook case of manic depression (whether he actually was manic depressive is a question well beyond the purview of this article).

14. Mills had known for some time that he was on the margins of professional sociology in both style and disposition. In a despondent moment, he had written to Robert Merton that "I hadn't realized (in fact I had for some reason been refusing to examine the point) how very far I had wandered from really serious work in our discipline" (Horowitz, 1983, p. 182). Mills's own feelings and ambitions aside, his approach to social research as "interested" (rather than "disinterested"), his self-consciously literary

style, and his political and otherwise "big" content set him apart from many of his contemporaries. But he was not the only sociologist writing on stratification, the sociology of knowledge, or on the new middle class. Mills was in the good company of Merton, David Riesman, and many others who may well have outshined him in terms of professional stature among peers during the 1950s.

15. To be fair, one could say the same thing about Lazarsfeld as an administrative sociologist, though Lazarsfeld himself often made this point in his own writings.

16. It is clear that Mills's case against Lazarsfeld is overstated and unfair. Lazarsfeld certainly believed in the occupational ideology of science—and helped to forge one of its modern versions—but he was also more of a pluralist than Mills acknowledges. *The Sociological Imagination* is also marked by an absence of middle-range figures such as Robert K. Merton, who moved ably between social-scientific and humanistic models of scholarship (see Eldrige, 1983, pp. 105-106; Tilman, 1989, p. 286).

17. For the reader interested in a discussion of method attentive to some of the same issues as Mills but that also acknowledges the institutional context of social research, I recommend Bourdieu and Wacquant (1993, especially pp. 217-260; see also Bourdieu, Chamboredon, & Passeron, 1991).

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