

Producing Parsons’ Reputation: Early Critiques of Talcott Parsons’ Social Theory and the Making of a Caricature

ARTICLE ABSTRACT

This article examines the critical responses to Talcott Parsons’ first major work, *The Structure of Social Action* (1937) and his subsequent two books, *Toward a General Theory of Action* and *The Social System* (both 1951). Because Parsons’ work was the subject of such virulent debate, we cannot fully understand Parsons’ impact on the discipline of sociology without understanding the source and nature of those early criticisms. I trace the responses to Parsons, first through book reviews and private letters and then in the more substantial statements of C. Wright Mills, George Homans and Alvin Gouldner, from the largely positive but superficial reception of *Structure* to the polemics that followed Parsons’ 1951 works. In the late 1930s and 1940s, Parsons’ reputation grew steadily but there remained no careful reception of *Structure*, fostering resentment towards Parsons in some quarters while precluding a sophisticated understanding of his work. After 1951, a few critics capitalized on that tension, writing sweeping rejections of Parsons’ work that spoke to a much broader audience of sociologists. That dynamic, coupled with Parsons’ own indifference towards his harshest critics, produced a situation in which many sociologists simply chose not to read Parsons in the 1950s and 1960s, reinforcing a caricature and distorting perceptions of Parsons’ place in mid-twentieth century American sociology.

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Introduction

This article examines the critical responses to Talcott Parsons’ first major work, *The Structure of Social Action* (1937), and his two subsequent books, *Toward a General Theory of Action* (co-edited with Edward Shils) and *The Social System* (both 1951). Sociologists and historians sympathetic to Parsons have repeatedly noted that from the late 1950s onwards, the sociological community largely dismissed Parsons without a fair evaluation or any sophisticated response to the content of his theories, following the lead of authors like C. Wright Mills and George Homans. The unfairness of Parsons’ early critics is now such common knowledge that it is often referenced with a note of resignation by sociologists who perhaps wish it to be remembered as part of the historical record but are sick of contesting it. But despite the widespread recognition that Parsons’ initial rough treatment by his colleagues was not based on a fair appraisal of his work, there has yet to be a detailed historical narrative of how Parsons came to be so demonized in the first place. My aim in this paper is to shed light on why disagreement about the merit of Parsons’ work became so entrenched in the 1950s and 1960s by giving an historical account that takes seriously both Parsons and his most outspoken critics.

Many accounts of Parsons’ intellectual trajectory do exist, as well as efforts to contextualize his thought in terms of intellectual trends of the time, but Parsons’ meteoric rise to prominence and the subsequent backlash against him were such unusual phenomena that it is impossible to fully understand Parsons’ place in twentieth century intellectual history without taking into account the early critical responses that stuck so strongly to his image. Howard Brick has argued persuasively that Parsons’ work was much more socially aware than it first appeared to many: his seemingly “arcane theory”

was in fact an effort to envision a postcapitalist society in the United States, informed by and part of a much broader movement of academics and public intellectuals to imagine an alternative social order to postwar industrial capitalism (Brick, pp. 2, 121-151). While Brick is right to correct the stereotype of Parsons as a “pure theorist” completely disassociated from his social surroundings, I will argue here that we should be cautious about attributing to Parsons too deep an involvement in the movement Brick describes, a movement that was “handed down from one intellectual cohort to another, at each step replicated, deployed in new ways, or reshuffled, recast and supplemented by new additions” (p. 18). Not only did Parsons pay little attention to the antecedent traditions of philosophy and sociology in the United States, as has frequently been noted (see Camic, 1989, 1992; Joas and Knöbl, p. 25), he also often tried to keep as much distance from his critics as they did from him.

In a similar vein, we are at risk of overestimating Parsons’ influence on his intellectual milieu if we do not take proper account of his critics. Parsons was seen by many, supporters and detractors alike, to be at the apex of the sociological world in the 1950s and 1960s, but the breadth of harsh and often virulent criticism directed against him suggests that his imagined influence has often been overstated (on the myth of “mainstream” sociology, see Calhoun and VanAntwerpen). We find Gerhardt writing, for example, a detailed exegesis on the content of Parsons’ 1946 memo on behalf of the Social Science Research Council (SSRC), a document intended to convince Congress to provide funding for the social sciences through the newly established National Science Foundation (NSF). Gerhardt champions Parsons for his involvement in the war effort and as a spokesman for the social sciences, and she only briefly alludes to the fact that the

SSRC reviewers overwhelmingly found both the style and the emphasis of the final report disastrously unsuited to its intended audience and consequently declined to publish it (Gerhardt, 2002, pp. 153-160; for a fuller account of the content of and responses to Parsons memo see Solovey; Klausner). By focusing so little on the often-negative reception of Parsons’ work, Gerhardt’s version can only be a partial account of Parsons’ contribution to postwar debates over national funding for the social sciences. Charles Camic has paid particular attention to the strategic moves Parsons made in his career, including a detailed and extensive characterization of *The Structure of Social Action* as a “charter” for a radically reformulated discipline of sociology (Camic, 1989, 1992, 1996). However, Camic, too, has paid relatively little attention to the actual critical responses that Parsons’ work received and how Parsons in turn replied to his critics, a line of inquiry that will enrich our understanding of Parsons both as a highly ambitious theorist aiming to remake the discipline in his image and as a strategist operating in the sociological job market.

My account will center on three responses to Parsons in particular: Mills’ *The Sociological Imagination* (1959), Homans’ 1964 address to the American Sociological Association (ASA) and Alvin Gouldner’s *The Coming Crisis of Western Sociology* (1970). There were many responses to Parsons in the early 1960s, and the list of most influential could certainly be expanded. Gerhardt (1999), for example, mentions Wrong (1961) and Martindale (1960). However, Mills, Homans and Gouldner, in addition to their visibility and high standing in the field, best exemplify what I take to be the defining characteristics of the anti-Parsons sentiment of those years: a total critique of Parsons that insisted his work was not only wrong but an impediment to the sociological endeavor and

a sense that, as things stood, Parsonian sociology was a hegemonic force that dominated the entire discipline in the USA. Finally, in their critiques of Parsons, all three focused heavily on Parsons’ two major 1951 texts. That narrow focus was another recurring feature of anti-Parsons writings of the 1950s and 1960s.

My argument will proceed in four parts. I will begin by reviewing the (largely positive) critical responses to Parsons’ *Structure* in 1937. With the publication of that book Parsons won accolades from his colleagues and made a name for himself in sociology but did not have a major impact on the field at that time. I will then touch upon three key episodes in Parsons’ career during the period between 1937 and 1951, a time when Parsons’ own intellectual orientation changed substantially while his reputation remained relatively static. By discussing Parsons’ role in the creation of the Harvard Department of Social Relations (DSR), his role in the postwar NSF debate mentioned above and the reception of his collected *Essays in Sociological Theory* (1949), I aim to demonstrate the mounting tension between Parsons’ growing reputation and the lack of serious treatment of his ideas. This tension laid the groundwork for the explosive polemics that greeted Parsons’ 1951 texts, which I will address in part III. These polemics gained momentum but retained the same general tone for years to come, culminating in the major texts of Mills and Homans. The field of sociology in the 1960s was undergoing an immense transition, expanding its boundaries to include innovative methodological approaches and hitherto neglected areas of investigation. Craig Calhoun and Jonathan VanAntwerpen succinctly capture the diversity of sociologists eager to leave structural-functionalism behind in the 1960s when they characterize the lot as a “motley bunch” of “radical sociologists, Marxists, feminists, phenomenologists, symbolic

interactionists, and ethnomethodologists” (p. 376). I will briefly try to characterize that diversity and locate the positions of Mills and Homans within the discipline as a whole. In part IV, I will address Parsons’ reaction to the tide of resentment against his work, which played as crucial a role in producing his stereotyped reputation as did the critiques themselves, and the reception of the early work of some of Parsons’ most important students, namely Robert Merton, Bernard Barber and Robert Bellah. By addressing the general reception of the work of Merton, Barber and Bellah, I aim to complicate the apparently simple dichotomy of opinions for and against Parsons (although the rhetoric of many sociologists assumed and reinforced precisely that dichotomy). I do not attempt to address the ways in which Parsons’ ideas were taken up, reformulated and extended by the community of his “loyal” students and committed followers. Rather, I focus on explaining the production of Parsons’ reputation as it was understood by the much larger community of sociologists who collectively marginalized Parsons from the late 1950s onwards. In my conclusion I will reiterate the defining characteristics of the wave of anti-Parsons sentiment in the 1950s and 1960s and will argue once again for the relevance of this reception study to contemporary Parsons scholarship.

Finally, a note on methodology: I have relied heavily on published book reviews and unpublished personal correspondence to capture the spirit of Parsons’ colleagues regarding his work. While these sources give a necessarily incomplete picture and often leave the *content* of Parsons’ theory wholly unaddressed, they nevertheless have distinctive advantages as sources. Letters allow for the candid expression of sentiments that are often veiled or diluted in published works. I have given great weight to two sets of letters in particular: those addressed to C. Wright Mills upon the publication of his

1953 *Saturday Review* article and those addressed to Parsons upon the publication of Ellsworth Faris’ review of *The Social System*. In both cases, numerous of Mills’ and Parsons’ colleagues felt moved to write and express their reactions. The spontaneous expression of emotionally-charged responses from multiple sources at once, on both occasions, is a good indication of how polarizing an effect Parsons was having on the discipline. The letters also serve the purpose of validating the review articles as sources. Judging by the private responses, these review articles were not just shouts in the darkness. They were really being read, and, especially in a time when evidence suggests that relatively few were making the effort to puzzle through Parsons’ lengthy books, they were really shaping Parsons’ reputation.

This article should not be taken as an argument that we can fully understand Parsons’ reputation – or the full range of responses to his work – without first understanding Parsons’ works themselves. But precisely because the prevailing trend of critical response became so polemic and so detached from the detailed content of Parsons’ theory, it is possible to read them and understand how Parsons’ reputation was made in the 1950s and 1960s without delving deeply into the debate over the ultimate meaning or lasting value of Parsons’ work.

I

In 1937, some theoretically inclined reviewers greeted Parsons’ *The Structure of Social Action* (hereafter *Structure*) enthusiastically, seeing it as major step towards the creation of a systematic theoretical framework for further sociological research and theory building (Pinney; Gettys). The majority of early reviews, however, were ambivalent. Many critics were hesitant to endorse Parsons’ theoretical program,

wondering about the coherence or utility of Parsons’ theoretical constructs. Yet it was generally agreed that *Structure* was “almost unique in recent American sociology” (Gettys, p. 425) for its ambition to establish new terms of debate in the profession and its rigorous interpretative analysis of the writings of Alfred Marshall, Vilfredo Pareto, Emile Durkheim and Max Weber. Even reviewers who had serious misgivings about the obscurity and imprecision of Parsons’ language or his dubious inductive logic all conceded that the appearance of the book was a good thing for sociology in the US, given the fertile grounds it would provide for future debate.

In a review of *Structure* that appeared in *American Sociological Review*, Louis Wirth wrote a perceptive passage on Parsons’ language that expresses both the doubts many of Parsons’ colleagues had about the book and the potential they thought it had to constructively advance sociological debate. *Structure*, Wirth wrote,

does not, like some commentaries, confine itself to the sterile, exegetical exercise of starting with a text as if every term had a revealed and standardized meaning, but rather goes to some pains to translate the language of each of these writers [Marshall, Pareto, Durkheim and Weber] into that of others. While this occasionally involves drawing inferences from and imputing meanings to the texts which are difficult to sustain as intended by the writers, it does at least bring each of the authors within the orbit of a common universe of discourse. The resulting product, despite the involved and awkward language and unusually extensive volume, is highly interesting and helpful in the diagnosis of our present-day battle of theories – which, it may be added, is more than a battle of words (p. 400).

Wirth’s recognition that Parsons’ difficult style is employed in the service of bridging gaps between the theories of his several European writers, all of whom write in their own complex technical language, is important. Although he criticizes the book for its stylistic clumsiness, its debatable interpretative work and a number of other shortcomings, Wirth sees Parsons’ language as a major contribution to a serious sociological dialogue. By recognizing that Parsons’ conceptual apparatus, despite its idiosyncrasy and awkwardness, is deeply embedded in the writings of the European theorists, Wirth identifies myriad starting points from which the interpretative analysis and conclusions of *Structure* can be criticized, modified or extended. To anyone familiar with the work of Parsons’ European writers, *Structure*, difficult language notwithstanding, is a relevant contribution to the “present-day battle of theories,” not just a foray into a “battle of words.” The observation that *Structure* provides rich grounds on which to debate existing sociological ideas has its clearest formulation in Wirth’s review, but it is implied in others (see Kirkpatrick).

In fact, even the harshest reviews of *Structure* recognized it as a major statement on many pressing issues in US sociology. Robert Bierstedt, a former student of Parsons, wrote in the popular journal *The Saturday Review* that Parsons’ obscure language was unfortunate because it would deter many scholars and otherwise interested readers who found themselves lacking the “courage” to read it. Still, despite the “stylistic absurdity” and the fallacious logical reasoning that Bierstedt saw in *Structure*, he maintained that the book was “one of the most profound and learned contributions to social science in the last decade.” It was a shame, said Bierstedt, that Parsons’ linguistic style would deter readers, because the book offered an insightful interpretative analysis and a bold attempt at a

theoretical synthesis of some of the most important antecedent work in European sociology. Bierstedt’s ultimate judgment that the book was a major contribution to sociology, coming as it did in the midst of a generally condemnatory review, makes it clear that, like Wirth, he found the awkwardness and obscurity of Parsons’ language regrettable but certainly not grounds on which to judge the book’s utility or importance. Bierstedt called Parsons’ method of theorizing a “blind alley,” an articulation of “the direction sociology cannot go if it is to be an empirical science” while maintaining that *Structure* provides “an excellent map of the alley” of which professional sociologists ought to take notice. (1938b, pp. 18-19).

Indeed, Bierstedt himself embraced many of Parsons’ main concepts and terms. Just months after the *Saturday Review* article, Bierstedt wrote another for the *American Sociological Review* on “The Means-End Schema in Sociological Theory.” In the article Bierstedt wrote in general terms about an “intellectual climate” in which “many sociologists... have renounced the positivistic tradition,” turning instead to “voluntaristic theories of social action.” It is transparently clear here that Bierstedt is addressing *Structure*; he closely follows Parsons’ unorthodox definitions of the “voluntaristic” and “positivistic” traditions even while presenting them as components of a widespread theory in the existing “intellectual climate,” and relegating *Structure* to the status of “a recent systematic exposition of the voluntaristic theory” (1938d, pp. 665, 667). Bierstedt’s article presents a robust challenge to the voluntaristic theory, yet by presenting it as an acceptable and widely recognized position in the sociological profession, Bierstedt wholly embraced the new terms of sociological debate that Parsons hoped to create with *Structure*.

These responses came in the context of a discipline that was highly unsettled. In the late 1930s, sociology was still very much in a process of forming its own identity. Although several universities had already established themselves as preeminent centers of sociological research – the University of Chicago, the University of Wisconsin-Madison, Columbia University and the University of Minnesota foremost among them – US sociology in the years before *Structure* was a marginal field, lacking clear and universally accepted methodological and epistemological standards and without a clear mandate as an academic discipline. The period of greatest creativity of the first Chicago School was over by the beginning of the 1930s (Bulmer, pp. 208-224; Turner and Turner, pp. 49-50), leaving US sociology without any “school” of thought of comparable internal coherence and productivity. Another organizing principle highly influential in 1920s sociology was that of “scientism”: the aspiration to model sociology on the natural sciences by restricting its study to empirically verifiable observations and upholding the belief in value-free conclusions (Bannister; Ross, p. 390; Hinkle, 1994, p. 185). But by the mid-1930s, sociologists pushing in a number of new directions decisively overcame the weighty “moral authority” of scientism (Ross, p. 405). Some rejected it to turn their attention more directly to the social and political problems of the Depression, in an attempt to put sociologists on an equal footing with political scientists and economists, who had taken the lead among academics as policy advisors and public commentators on the national crisis. The American Sociological Society (later to become the American Sociological Association) contributed to the cause by establishing conferences and symposia to demonstrate the relevance of their discipline (Turner and Turner, p. 56; Camic, 2007, pp. 258-264). At Columbia, often considered a stronghold of quantitative

statistical analysis, Robert MacIver and Robert and Helen Lynd produced important ethnographic studies that stood as alternatives to the empirical fact-gathering and data analysis characteristic of scientism (Hałas, p. 31; Bannister, pp. 70-72). Pitirim Sorokin, the first Chair of Harvard University’s young Sociology Department, developed a fiercely anti-positivist sociological theory with a reliance on metaphysics that recalled the writings of an earlier generation of sociologists (Johnston, pp. 79-83). While these reactions against scientism allowed for more methodological creativity than had previously been the case, they also carried the risk – together with the decline in stature of the Chicago School – of disorienting the field and pushing sociology further from the public prestige and knowledge-producing efficiency that come from being a “high-consensus, rapid-discovery science” (for this language see Collins, 1994). Such was the state of sociology when Parsons’ *Structure* appeared.

Structure continued the trend of reacting against the values of scientism – Parsons’ principle targets were biological determinism and orthodox economic theory – but it also made a claim to establish an entirely new kind of sociology by declining to acknowledge or engage explicitly with any earlier work that had been done in US sociology, including the contributions of Robert Park, MacIver, the Lynds and Sorokin (see Camic, 1989). However, Parsons was remarkably successful in signposting *Structure* as an important book to the very colleagues whose work he ignored and presenting his arguments with a force that made the book recognizable to them as a paradigm-shifting work of sociological theory. He did so in part by making *Structure* highly deferential to another, far more expansively conceived intellectual tradition: the social-theoretical writings of European intellectuals from Hobbes to Durkheim and Weber.ⁱ The

congratulatory letters Parsons received upon publication of the book, celebrating the fact that he had “arrived” (Bierstedt, 1938a; Mannheim, 1938), provide one indication of the immediate visibility that *Structure* had to the sociological community, even before its arguments had been processed and discussed. The hope that Wirth, Kirkpatrick and Bierstedt all expressed in their reviews – that *Structure* would prove an weighty contribution to the theoretical debate in sociology, and indeed reorient that debate altogether – is another such indication.

But *Structure* did not immediately impact the discipline as profoundly as its first reviewers hoped it would. Bierstedt’s prediction may have been realized: that many lacked the “courage” to read it. While Parsons solidified his reputation, few American sociologists paid the book any close attention at all. It was reviewed in the two major sociology journals of the day (*American Sociological Review* and *American Journal of Sociology*ⁱⁱ) and a handful of others, but it sold few copiesⁱⁱⁱ and had little effect on the broader shape of the field (Joas, 1996, pp. 7-8; Wallerstein, p. 429). The major effect of the book, rather, was to establish Parsons as a major figure at Harvard. At that time, Sorokin was falling out of favor with the administration. By writing *Structure* Parsons established himself as a prime candidate to be a leading figure in sociology at Harvard in a post-Sorokin era. *Structure* thus laid the groundwork for the leading role Parsons took on in shaping Harvard’s new Department of Social Relations. Established in 1946, the DSR was a major innovation in social scientific research at Harvard, bringing together sociology, clinical psychology and social anthropology into a common site for research and teaching. Parsons became the first Chair and the department’s “guiding light” (Gilman, p. 73). The leading role that Parsons took on in advocating for and later running

the DSR demonstrated not only his administrative and organizational energy, but also his drive to institutionalize his vision of how social scientific research ought to be structured, clearly distinguishing “basic social science” (as Parsons termed the research questions and methodologies that were eventually brought together and institutionalized in the DSR) from the applied social scientific research that depended upon it (on the creation of the DSR see Gilman, pp. 72-112; Johnston, 150-158; Keller and Keller, 91-92; on Parsons’ conception of “basic social science” see Isaac, forthcoming; Allport, 1943; Buck, 1944; Parsons, 1944; Allport, Kluckhohn, Mowrer, Murray and Parsons, 1944a, 1944b, 1944c).

II

The fact that Parsons’ institutional standing grew faster than his theoretical influence turned out to be crucial. If the arguments of *Structure* had been more fully integrated into the main currents of sociological debate upon its publication, as Wirth and Bierstedt hoped, then the subsequent history of Parsons scholarship might look very different. As it happened, however, *Structure* did not receive that kind of attention early on. Parsons’ prominence continued to grow because of his position in the DSR at Harvard and his public visibility, both as spokesman for the SSRC during the postwar debate over NSF funding and as president of the American Sociological Association (ASA) in 1949. But before the publication of the 1951 works there was no widespread or sophisticated understanding of what *Structure* had to contribute to sociology. In what follows, I will address the usual disconnect between Parsons’ reputation in the late 1930s and 1940s and the amount of careful attention his work actually received.

The years after the publication of *Structure* were busy and transformational for Parsons himself. Howard Brick has charted Parsons' changing ideas, characterizing the general arch of his intellectual development as a “shift away from economics” (p. 135). Brick interprets Parsons in the light of Roosevelt’s New Deal and ensuing political and civil debates over the economic future of the United States, arguing that Parsons came to focus his work on the particular combination of sociology, cultural anthropology and social psychology known as “basic social science” or “social relations” at the same time as concluding that “the very notion of ‘capitalism,’ too wedded to economic reductionism, had lost salience in the analysis of modern society” (Brick, p. 123). As Brick rightly notes, Parsons was not only dedicated to making this transition in his own work but was also invested in implementing his vision of basic social science institutionally in the DSR (Brick, p.144; see also Isaac, forthcoming).

It would not be too strong a statement to say that, for Parsons, the two objectives were inseparable. Parsons had been involved with the creation of the DSR from its earliest conception, as a member of the “Allport Committee,” an interdisciplinary group of Harvard faculty who wrote to Paul Buck, then Dean of the Faculty of Arts and Sciences, urging a reorganization of the social sciences at Harvard (Allport, 1943). The Allport Committee’s proposed reorganization was the establishment of a new “department of basic social sciences” to absorb the existing Department of Sociology, the social and clinical branches of the Psychology Department and the social branch of the Anthropology Department. They understood the proposed department to be “basic” to political science, economics, experimental psychology and the other “applied” social sciences, “not with any implication of ‘more important’ but merely in [the term’s]

primary meaning of ‘underlying’” (Allport, Kluckhohn, Mowrer, Murray and Parsons, 1944a). So strongly did Parsons believe in this new organization of social scientific research that he wrote to Buck, “I will stake my whole professional reputation on the statement that [the emergence of ‘basic social science’] is one of the really great movements of modern scientific thought, comparable, for instance, to the development of Biology in the last third of the 19th Century” (Parsons, 1944). When the DSR was eventually established in 1946, it closely followed the model proposed in the original letters of the Allport Committee.

With the establishment of the DSR, Harvard put itself on par with the other leading sociology departments in the country. Because of its innovative organizational structure, the DSR attracted much attention, including that of the Carnegie Corporation, which generously funded the collaborative effort *Toward a General Theory of Action* (Gilman, pp. 84-92). As the Chair and the leading figure in the Department, much of that attention and prestige naturally accrued to Parsons, too. For his part, Parsons continued to work hard to align the character of the Department with his own vision of the social sciences (for Parsons’ own retrospective account, see Parsons, 1970), sometimes to the detriment of his relations with his own colleagues. This came out clearly several years later when, upon the publication of *Toward a General Theory* in 1951, Parsons presented that volume at a meeting of the faculty. In George Homans’ recollection, Parsons managed to “[imply], though without quite saying as much, that it ought to be adopted as the official doctrine of the department to guide future teaching and research.” Homans resisted and was supported by Samuel Stouffer (who had contributed a chapter to *Toward a General Theory*) and “the matter dropped” (Homans, quoted in Johnston, pp. 224-225;

see also Isaac, forthcoming). But the implication of that exchange is unmistakable: in Parsons' rise to fame and renown at the top of the profession he had fostered resentment, and resentment, furthermore, from colleagues who felt strongly that they ought not to be “put under any pressure to read” Parsons’ work, regardless of his status (Homans, quoted in Johnston, p. 224).

Another episode indicates that beyond the confines of the DSR, too, Parsons’ name was having more impact than his writings. When the debate over the creation of a National Science Foundation first came before Congress in 1945, the Social Science Research Council immediately recognized the importance of the situation for the social sciences. Although the initial proposal, authored by Vannevar Bush, the director of the Office of Scientific Research and Development, made no provision for funding social-scientific work, the SSRC weighed the costs and benefits of receiving federal funding but then acted quickly to advocate for inclusion in the NSF (Klausner, pp. 4-6; Solovey, pp. 397-399). After a first effort by Louis Wirth proved unsuccessful, the SSRC commissioned Parsons to write a policy memo on the situation and task of the social sciences to be sent to Congress.^{iv}

Because the stakes for the SSRC were so high in the debate, both in terms of potential financial gain and in terms of political stature and respect, Parsons’ manuscript, entitled “Social Science: A Basic National Resource,” was subjected to criticism and revision by over twenty reviewers after he completed a first draft. The “conflicting visions of social science” represented in the SSRC quickly emerged as a formidable obstacle. But so too did Parsons’ “notoriously rigid” writing style (Solovey, p. 414). Shepard Clough, a professor of economic history at Columbia, called Parsons’ writing

“somewhat turgid” and imprecise in its conclusions. He concluded, “the document is very ‘poor’ and will get little attention. It will not be convincing if read.” A review by J.S. Davis, while more positive about the content, was even more critical of Parsons’ writing style. Davis contended that the paper would be excessively difficult for “intelligent citizens” to follow, a sentiment echoed by William Olson, who concluded that the document “reflects the richness and complexity of the writer’s thinking but will tax the patience of all but the most devoted.” Numerous other reviewers came to the same conclusion (Klausner, pp. 23-27). Parsons returned to the document after receiving the reviews but only halfheartedly, focusing instead on his own academic work (Klausner, p. 27). After years of intermittent revisions, the memo still faced “paralyzing problems,” and Parsons’ work on it finally came to a complete halt in 1951 (Solovey, p. 415). The document was never sent to Congress.

Recent reassessments have tried to redeem Parsons’ ill-fated memo as a valuable and subtle piece of writing, expressing regret that complaints about style “served to repress the substantive issues” contained in an impressive social-theoretical statement (Klausner, p. 32; Gerhardt, 2002, pp. 153-167). That may have been, but it was hardly the most important standard against which the reviewers had to assess the document. The readers recruited by the SSRC were interested first and foremost in convincing Congress to fund the social sciences, and Parsons’ memo was not appropriate to that end, as Samuel Klauser and Victor Lidz have already noted (p. *xi*). The document is largely free of the technical terminology that Parsons would employ in his 1951 works, but it still suffers from Parsons’ highly theoretical orientation and an emphasis on “basic social science” at the expense of economics and political science, surely the two social sciences

that those on Capitol Hill would have the most familiarity with and sympathy for. Parsons' readers may not have given his work the careful reading it deserved, but his SSRC memo demonstrates that Parsons also had great difficulty making himself understood, either to a lay public or to his fellow sociologists. Both sides of that communicative impasse would turn out to be important in the reception of Parsons' 1951 works.

For all the waves that Parsons was making institutionally and politically, when the first edition of his *Essays in Sociological Theory: Pure and Applied* appeared in 1949 it generated relatively mundane and perfunctory reviews. While Arthur Davis (1949) gave Parsons his due as “a very eminent thinker,” and Lewis Coser in *The American Journal of Sociology* (1950) gestured at a way to relate Parsons’ structural-functionalism with Robert Merton’s work in *Social Theory and Social Structure*, every review to appear in a major peer-reviewed journal recognized that the emerging theory of action set out in the essay collection was incomplete (in addition to reviews by Davis and Coser, see MacRae; Nuesse; Francis, 1950; and Williams). Reviewers looked back to *Structure* for a more complete statement of Parsons’ theory or speculated about how his systematic theory might progress. The essays themselves were not taken to be especially significant.

The few episodes recounted here – Parsons’ role in the creation of the DSR and his subsequent conflict with Homans, his role in the NSF debate and the reception of his 1949 collection of *Essays* – are not intended to amount to a full assessment of Parsons’ development between 1937 and 1951. Brick, Gerhardt and others have already narrated those developments much more comprehensively. My objective here is merely to highlight the curious distance that emerged in the 1940s between Parsons’

steadily growing reputation and the thoroughness with which his work was received by his peers. It was widely recognized that Parsons was, in Davis’ words, “a very eminent thinker,” but his ideas did not receive the level of attention that we might expect for someone of that status. Part of the fault likely lay with Parsons himself. In writing the SSRC memo, Parsons was seemingly unable to adapt his language and his stylistic presentation to suit his audience. It would be wrong to attribute the relative lack of serious reception of Parsons solely to his own difficult writing style, however. His critics were also major contributors. Homans, as we have seen, thought Parsons’ idea of basic social science to be overbearing and rejected the idea that *Toward a General Theory* ought to be required reading for the entire Department. It was, paradoxically, precisely the extent of Parsons’ reputation and institutional power that prompted Homans to push back against a charitable reception of his ideas.

III

The tension between Parsons’ reputation and the sometimes negative, sometimes lukewarm reception of his work reached a breaking point in 1951. When *The Social System* and *Toward a General Theory* were published that year, they garnered immediate attention. The reviews, however, were overwhelmingly negative. Most were far more caustic than even the most ambivalent reviews of *Structure*. Bierstedt’s worry in 1938 that Parsons’ language would deter readers who might profit from engaging with *Structure* is modified and made more polemical in these later reviews. Readers, the reviewers suggest, are rightly intimidated because the conceptual and linguistic maze that Parsons erects in his 1951 works is neither necessary nor desirable. The difficulty of the language itself is reason enough to doubt the utility or empirical applicability of Parsons’

theoretical arguments. Ellsworth Faris’ review of *The Social System*, appearing in *American Sociological Review* in 1953, is an eloquent statement of some of the recurring criticisms in negative reviews of the two 1951 works. Questioning Parsons’ failure to explicitly refer to any antecedent theorists in US sociology or the theoretical concepts that they employed, Faris asks, “why does Parsons do it?”

Two possible answers suggest themselves. The first is lack of familiarity with our literature and tradition...It is not impossible that our author has not had the time to familiarize himself with the large output of our literature before he came into the field [from economics] and that his seeming indifference to our conceptual tools results from mere unfamiliarity.

The alternative explanation would be that his purpose is not only to construct a conceptual scheme but to originate the concepts instead of availing himself of the body of terms that have been accumulated through the years.

The right to do this is unchallenged; the wisdom of doing so is questionable...To be the Linnaeus of sociology is a high ambition and the risk of failure is great (p. 104).

Faris argues that Parsons, in his ambition to be “the Linnaeus of sociology,” has deliberately isolated himself from his sociological colleagues. He sees Parsons as proposing a conceptual framework and body of terms that one must use (and thus implicitly accept) if one is to engage at all with Parsons’ work. Declining the invitation to engage with Parsons’ work in Parsons’ terminology, Faris instead focuses on critiquing the terminology itself, arguing that Parsons’ conceptual terms are so many “slip-shod creations” that betray “unclear thinking” (p. 105).

Parsons’ *Essays in Sociological Theory*, which had generated such little attention upon their original publication in 1949, occasioned much more robust commentary when a revised edition was released in 1954, further exemplifying and solidifying the polarizing effect of Parsons’ work. A review in *American Anthropologist* by Lloyd Fallers observes the basic emerging dynamic in critical response to Parsons:

Unfortunately, there appear to be only two views concerning the work of Parsons. There are the disciples, for whom all problems are exegetical or, at most, involve slight elaboration of “The Frame of Reference”; and there are the scoffers, for whom Parsons is merely a latter-day scholastic with an unintelligible writing style (pp. 870-872).

Fallers argues that the new edition of the *Essays* provided a useful bridge between *Structure* and the later works, which became “progressively more unintelligible,” but he was not optimistic that the new volume would do anything to lessen the gulf between Parsons’ “disciples” and “those few who remain uncommitted.”

Robert Bierstedt offered a vigorous attack on the revised *Essays* in the *American Sociological Review*, clearly more disenchanted with his former teacher than he had been in 1938. The flattering and aggrandizing treatment he had given Parsons’ theory in the article, “The Means-End Schema” in 1938 is turned on its head, and Bierstedt attacks Parsons for treating his own work in essentially the same fashion as Bierstedt had a few years before: Parsons is mocked for his “tendency to usurp the function of the critic and, in passages unmarked by even a modicum of modesty, to trace the phases of his own intellectual development, and to praise his own theoretical achievement” (1955, pp. 124-125). Bierstedt dedicates most of the short review to discussing Parsons’ language (which

“results simply in a failure to communicate,” he writes, adding that, “for this the author cannot shift the responsibility to his reader”) and excessive sense of self-importance.

Towards the end of the review Bierstedt writes, “our remarks so far, suffering perhaps from the Parsonian influence, have also concerned method rather than substance.” There follows a very short discussion of the “substance” of Parsons’ essays, but it is superficial and clearly not Bierstedt’s priority in the review.

Bierstedt’s comment about prioritizing method over substance, intended humorously and made without any apparent caution for the implications of treating Parsons’ work thus, epitomizes the way Parsons’ work was imagined and addressed by many after 1951. With the benefit of hindsight it is hard to miss the weight of that comment. Because Bierstedt refuses to employ Parsons’ conceptual terminology on principle, he finds himself pushed into talking about method rather than substance, unable to enter into a precise discussion of Parsons’ arguments. As Fallers’ and Bierstedt’s reviews confirm, many felt themselves unable to connect Parsons’ work in fruitful or interesting ways to other contemporary contributions to sociology. The general tone of critical response remained essentially the same for years. The rich theoretical and empirical promise of *Structure* that had impressed Louis Wirth remained unaddressed, and Parsons’ work more and more acquired an aura of impenetrability and inflexibility. The dilemma facing Parsons’ colleagues was that, as Matteo Bortolini has written, “Parsons’ wager was extreme: one had to buy either ‘the whole package’ or something different” (Bortolini, forthcoming).

Those reviews of the 1951 works and the *Essays*, as well as the more elaborate polemics against Parsons that were shortly to follow, need to be understood in the context

of a discipline that had changed considerably since the late 1930s and the emergence of *Structure*. The extraordinary intellectual ferment of the 1960s has been widely commented upon, and the confluence of events leading to the explosion of creativity in sociology in the 1960s – much of it directed in opposition to a constructed “mainstream” of which Parsons, Robert Merton and Paul Lazarsfeld were the leading figures (see Calhoun and VanAntwerpen; Bourdieu, 1991) – defies any simple causal explanation. However, there are some key features of the discipline of sociology that provide a helpful context for understanding the potency of the collective reaction against Parsons in those years. By the 1950s, generous funding from philanthropic organizations had become widespread, resulting in milestone achievements like the Carnegie Corporation-funded *Toward a General Theory*, Gunnar Myrdal’s *An American Dilemma* (Carnegie, 1962), David Riesman’s *The Lonely Crowd* (Carnegie, 1950) and *The Authoritarian Personality*, by Theodor Adorno and others (American Jewish Committee, 1950) (see Turner and Turner, pp. 92-101; Gilman). Many universities had established new sociology departments, and Chicago and Columbia, two departments with long and storied histories, had reinvented themselves to remain at the forefront of the discipline. The Columbia Bureau of Applied Social Research (BASR), established in 1943 under the direction of Lazarsfeld, was at the leading edge of statistically oriented sociological research, operating with a high level of prestige and financial independence (McCaughey, pp. 372-375; Hałas, pp. 38-39). A “second Chicago School” emerged at the University of Chicago, where Herbert Blumer and a younger generation of students, including Erving Goffman and Howard Becker, rejuvenated the first Chicago School’s creative interest in

Mead and Dewey with the elaboration of symbolic interactionism, a term first coined by Blumer in 1938 (Colomby and Brown; Gross, pp. 197-201; Joas and Knöbl, pp. 130-131).

As well as the benefits of better funding and firmer institutional standing, sociology of the 1950s and 1960s had clearer, although certainly not more homogenized, theoretical bearings than it did in the late 1930s. Along with Parsonianism, BASR-organized survey projects and symbolic interactionism, already mentioned above, several other well-defined theoretical frameworks had established themselves in the field. George Homans’ perspective was that of neo-utilitarianism, an interdisciplinary approach that took its bearings from the psychology of B.F. Skinner and classical utilitarianism, updated to respond to criticisms from the institutionalist economists and Parsons’ *Structure*.

Ethnomethodologists – behind the leading figure of Harold Garfinkel, a former student of Parsons – adopted an approach similar to the symbolic interactionists, likewise eschewing Parsons’ systems theory for a more micro-level analysis of individual behavior and meaning-making (on the development of all these theoretical paradigms, see Joas and Knöbl, pp. 94-173; Alexander, 1987, pp. 156-280). Perhaps because of the more secure institutional and financial standing enjoyed by the discipline as a whole, along with myriad promising theoretical and methodological avenues to explore, sociologists in the 1960s found it easier to vocally and categorically reject Parsons’ enormously ambitious effort at theory-building than they had in the years immediately following *Structure*.

Nowhere was the rejection of Parsons’ effort more forcefully expressed than in C. Wright Mills’ *The Sociological Imagination* (1959). In that text Mills parodies *The Social System* but makes a serious point in doing so: that Parsons’ high level of abstraction and insistence on an exhaustive conceptual scheme logically preclude his work from being

empirically applicable or useful as a basis for substantive new insights in sociology.

Mills’ criticism of Parsons helped to make it a standard of interpretation to treat Parsons’ language as an “[impediment] to meaning” rather than just an annoyance (p. 27). Mills, like Bierstedt, had not always adopted such an uncompromisingly negative stance towards Parsons. In an earlier, unpublished draft review of *The Social System* that formed the basis of Mills’ critique in *The Sociological Imagination*, Mills prefaced his comments by acknowledging that, “in his The Structure of Social Action, Professor Parsons revealed himself to be an excellent although difficult textual scholar” (Mills, 1951). But in the final published critique, Mills dropped this opening line. Although in *The Sociological Imagination* Mills explicitly states that he is “not...trying to judge the value of Parsons’ work as a whole” in his attack on *The Social System* (p. 27), he makes no suggestion of how any of Parsons’ other works could be considered more valuable than *The Social System*. Indeed, his critique of Parsons stands in for an indictment of what Mills calls “Grand Theory” generally, and the breadth of his claims make it hard to interpret his attacks on language in *The Social System* in particular as anything less than attacks on Parsons’ writing style and theoretical framework in the most general terms possible. The entire stylistic approach to sociology, Mills writes,

is drunk on syntax, blind to semantics. Its practitioners do not truly understand that when we define a word we are merely inviting others to use it as we would like it to be used; that the purpose of definition is to focus argument upon fact, and that the proper result of good definition is to transform argument over terms into disagreements about fact, and thus open arguments to further inquiry (p. 34).

With this blanket charge of abuse of syntax and neglect of semantics that Mills levels at all of Grand Theory and its practitioners, Mills directly and powerfully denies what Wirth, in reviewing *Structure*, took to be the redeeming attribute of Parsons’ awkward and involved language: that it stands the chance of being “helpful in the diagnosis of our present-day battle of theories – which...is more than a battle of words.”

When he wrote *The Sociological Imagination*, Mills was appealing to a by then broad base of existing frustration with Parsons’ work. Another prototype of Mills’ attack on Parsons was laid out in a 1954 *Saturday Review* article accusing the “Grand Theorists” of the sociological profession of creating “an elaborate method of ensuring that no one learns too much about man and society, the first by formal but empty ingenuity; the second, by formal and cloudy obscurantism” (p. 22). The private response to Mills’ article was overwhelmingly enthusiastic. He received eight letters of unqualified congratulation from colleagues, including letters from Herbert Blumer and Robert Bierstedt, who found the article “brilliant” (Bierstedt, 1954; Blumer; see also letters from Davis, L.; Duncan; Eliot; Hacker; Hunter; and Owen). However, the most conspicuous feature of those letters is not their enthusiasm for Mills’ humanistic approach to sociology, but rather their willingness to accept Mills’ assertion that “the Grand Theorists” (along with “the Scientists”) dominated professional sociology.^v One correspondent wrote that the dominance of “the Scientists and the Theorists...has seemed a long tide, which I hope is now turning” (Eliot). Blumer also adopted an embattled tone, writing to Mills that he “read the article with much satisfaction and, of course, obviously enjoyed the provocative form of [Mills’] treatment.” Blumer had decamped Chicago for the University of California-Berkeley in 1952, where he was Chair of a prestigious, West

Coast sociology department worlds away from the DSR, the epicenter of Parsons’ influence. The fact that even he took such delight in provocations to the Grand Theorists is evidence of how long a shadow Parsons cast over the entire discipline in 1954.

It is difficult to fit Mills neatly into one of the newly emergent theoretical frameworks mentioned above, in part because he consciously cultivated an image of himself as a maverick outsider throughout his career (Geary, pp. 1-13). He worked at Columbia for much of his career, although he was antagonistic to both Merton and Lazarsfeld and uncooperative in his first project within the BASR (Geary, pp. 89-96). Mills’ early interest in pragmatism and the sociology of knowledge establishes a significant parallel between him and the symbolic interactionists (Mills, 1939, 1940a, 1940b; Gross, 2007, pp. 204-206), but later in his career he was engrossed with macrosociological problems of power, politics and the “sociological imagination,” topics that demonstrated a broad explanatory ambition and did not rely explicitly on the methodological tools of the symbolic interactionists. Mills’ audience enthusiastically went along with his self-characterization as a “maverick on a motorcycle,” which inflated his fame but inhibited serious critical reception on his work (Geary, p. 2). With regard to his critique of Parsons, however, it seems to have served exactly the purpose Mills wanted it to serve. When in the process of writing *The Sociological Imagination*, Mills wrote to a colleague that he hoped to appear to the rest of the discipline as “a prophet who comes in from a desert” (Mills and Mills, eds., pp. 229-230), and to an impressive degree, he succeeded. Because of his compelling personal image and the urgency with which he expressed his hopes for a public and political sociology, Mills appealed to many

from disparate methodological and theoretical orientations, both consolidating and propelling forward a high level of anti-Parsonian sentiment.

In 1964, George Homans continued the assault on Parsons from a place of similarly high visibility: the annual presidential address to the ASA. The potential impact of the speech was not lost on him. The conflict between structural-functionalism and its opponents was “still not a settled one,” Homans asserted, and it was important enough that “if I have only one chance to speak *ex cathedra*, I cannot afford to say something innocuous. On the contrary, now if ever is the time to be nocuous” (Homans, 1964, p. 809). Homans’ speech is, like Mills’ discussion of Grand Theory, a polemic against structural-functional sociology in general, but the prominence of Parsons as the principal target is clear. Furthermore, although attacks on Parsons and structural-functionalism were commonplace by 1964, Homans ascribed the same power to a hegemonic structural-functional orthodoxy as had Mills’ correspondents after his 1954 *Saturday Review* article.

An important exception to the narrative presented so far is Lewis Coser, whose book *The Functions of Social Conflict* (1956) was the first major statement of conflict theory in the United States (Joas and Knöbl, pp. 174-198). He was neither within the fold of Parsons’ students and supporters nor among the many sociologists who, like Mills and Homans, categorically rejected Parsons’ relevance to sociology. Coser managed to situate himself in a constructive relationship to Parsons, clearly departing from Parsons’ scheme but nevertheless engaging with it seriously and showing deference to structural-functionalism as an important theoretical movement. Furthermore, Coser’s early reviewers recognized *The Functions of Social Conflict* as a significant theoretical statement and appreciated the constructive departure that Coser made from Parsons’

theory. In his reaction to Parsons, Coser was commended for making “the fighting...clean and the blows well aimed” (Freedman; see also Rose; Ross, M.; and Jameson).^{vi}

Coser’s work notwithstanding, to see just how long the wave of anti-Parsonian feeling and rhetoric lasted in the US lasted, and how much momentum it gathered, we should turn our attention to Alvin Gouldner’s *Coming Crisis*. Writing in 1970, in many respects Gouldner gave a more subtle analysis of Parsons’ work than did Mills, but with regard to Parsons’ relevance to future sociological research, *Coming Crisis* only reinforces the damning indictment put forward in *The Sociological Imagination*.

Although Gouldner historicizes Parsons’ work to a certain degree – clearly distinguishing between “The Early Parsons” (which he associates with *Structure*) and “Parsons as a Systems Analyst” (which he associates with the 1951 works) – Gouldner gave no serious attention given to Parsons’ work after 1951 as substantially different from the theory presented in *The Social System* and *Toward a General Theory*. He is as firm as Mills in his contention that Parsons’ later systems theory had no applicability to empirical issues in sociological research. While Gouldner gives Parsons’ students credit for their scholarly achievements in making Parsons’ theory accessible and relevant to a broader audience than Parsons would otherwise have enjoyed, at the same time he emphasizes the elitism and detachment of Harvard as a major source of the “unearned prestige” that had accrued to Parsons’ work (p. 201).

Finally, Gouldner declares his intention to move beyond the constant “snickering about Mr. Parsons’ tortured style” to begin “serious thought about what it might mean” (p. 200), but he ultimately only encourages the lack of communication between Parsons

and his critics. This comes out in one of the most striking claims of the book: “Parsons is above all a metaphysician” (p. 218). Gouldner provides further explanation of what he means when he attacks Parsons’ notion of a system as a picture of social reality derived by “entirely literary means.” That is to say, systems analysis has never been shown to explain any empirical problem more effectively than another mode of explanation. Parsons’ professional success and renown as a theorist can only be attributed to his persuasive force as a writer. This Gouldner takes to be the enduring paradox “for those who merely complain about Parsons’ literary style,” and it is the aspect of Parsons’ work that Gouldner most wants to reveal to that skeptical audience (p. 212). For, if it were seen that Parsons’ literary rhetoric and not the explanatory power of systems theory is his greatest achievement, then there would remain little or no reason to try to become conversant with his theory. For what could a metaphysician – an antiquated philosopher – have to say of interest to a twentieth-century sociologist?

The easy answer to that question given by many of Mills, Homans and Gouldner’s peers was, “nothing at all.” Those three writers, together with some of the early reviews of *The Social System* and *Toward a General Theory*, made powerful statements that sociology would be better off without structural-functionalism. However, the reductive treatment of Parsons’ work was due as much to the effect that those major statements had on the rest of the field as to the statements themselves. In *The Sociological Imagination*, Mills had enumerated four attitudes it was possible to adopt towards Grand Theory:

To at least some who claim to understand it, and who like it, it is one of the greatest advances in the entire history of social science.

To many of those who claim to understand it, but who do not like it, it is a clumsy piece of irrelevant ponderosity. (These are rare, if only because dislike and impatience prevent many from trying to puzzle it out.)

To those who do not claim to understand it, but who like it very much – and there are many of these – it is a wonderous maze, fascinating precisely because of its splendid lack of intelligibility.

To those who do not claim to understand it and who do not like it – if they retain the courage of their convictions – will feel that indeed the emperor has no clothes.

(p. 26).

While Mills styled himself as a member of the minority that understood Grand Theory and had the courage to expose its irrelevant ponderosity, he became a champion of those who wished to declare that the emperor had no clothes without investigating the matter very carefully themselves, and he contributed enormously to making that stance intellectually respectable in the field of sociology.

IV

In the face of such a backlash, Parsons did not respond to his critics in the 1950s and 1960s as effectively as he might have. After Faris' review appeared in *American Sociological Review*, Parsons received several letters from friends and colleagues assuring him that he need not take Faris seriously because “the review is not even addressed to the content of the Social System” (Warner; see also Merton, 1953; Spencer; and Spiegel). Parsons did not reply to the review and assured his supporters that he was “not seriously worried about the effect of the review,” given the letters of support he had received (Parsons, 1953b). Thus, Parsons and Faris each completely failed to reach the

other as a receptive audience in much the same way that Homans and Parsons faced each other at the DSR, each with the conviction that the single department was not big enough to house both of their sociological projects. In general, Parsons was not easily swayed into reformulating his ideas in response to critics. In response to comments from the many readers of his SSRC memo, Parsons abandoned the project rather than overhaul the document. He had been similarly unreceptive to criticism in his earlier career, but with a crucial difference: in response to the early reviews of *Structure*, Parsons engaged with his critics, sending lengthy letters to Louis Wirth, Robert Bierstedt and Alfred Schütz, an Austrian immigrant and social theorist who had privately sent Parsons an enthusiastic 74-page critique of *Structure* (Parsons, 1938a, 1939, 1941a, 1941b, 1941c; Schütz, 1941a). Parsons was not willing to cede any ground to any of them (including Wirth and Schütz, who had written laudatory reviews in the first place),^{vii} but at least he had engaged. In the aftermath of the reviews of his 1951 works, Parsons determined that those who rejected the very basic methodological and conceptual assumptions of his work simply did not merit responses. Regarding the Faris review, Parsons wrote to Merton that he thought it best “to let this particular sleeping dog lie” (Parsons, 1953a). Unfortunately for Parsons, it soon became clear that Faris’ polemic was anything but a passing phenomenon that he could afford to ignore.

Merton, in fact, did much more to rescue Parsons’ reputation in those years than Parsons himself. Merton’s “theories of the middle range” can be read as an appeal to the sociological community to integrate Parsons’ work more meaningfully with other advances being made in sociology. Much of Merton’s work can be seen as an attempt to reconcile the disparate stands of sociological work in the 1940s and 1950s that so

frequently failed to speak to each other constructively. A magician in his youth, Merton became a consummate strategist and bridge-builder as a professional sociologist. As a PhD student at Harvard, he studied with both Parsons and Sorokin. He maintained good working relationships with both throughout his career, although he later wrote to Sorokin that “such twin loyalties” occasionally put him “under severe strain” (Merton, 1952). Likewise, Merton worked closely with both Mills and the quantitatively oriented Paul Lazarsfeld at Columbia. He was instrumental in convincing Lazarsfeld to bring Mills to Columbia to work in the Bureau of Applied Social Research under Lazarsfeld’s direction (Geary, pp. 75-76), and in 1947 he excitedly wrote Parsons that one of his classes at Columbia was interested in and receptive to his intention “to build a bridge between [Parsons] and [Lazarsfeld]” (Merton, 1947).

By 1948, Merton had situated himself as a sympathetic and constructive critic of Parsons, in a reply following Parsons’ “The Position of Sociological Theory” in *American Sociological Review* (Parsons, 1948, pp. 164-168). By making both analogies to and careful distinctions from the natural sciences, Merton argued, “complete sociological systems, as in their day, complete systems of medical theory or of chemical theory, must give way to less imposing but more adequate sets of limited theories.” While he denigrated any ambitious conceptual schemes that would “[run] so far ahead of confirmed special theories as to remain a program rather than a *consolidation* of these theories,” Merton also took care to emphasize the extent of his agreement with Parsons. In particular, he expressed optimism that the model of basic social science embodied in the DSR would yield “a new era of large theoretic gains” (pp. 165-166).

Although none of Parsons’ students reached Merton’s level of recognition in the 1950s and 60s, Robert Bellah deserves mention as another who contributed to a diversification of the way Parsons’ concepts were understood. In 1957 Bellah published *Tokugawa Religion: The Values of Pre-Industrial Japan*. In that book, and indeed throughout Bellah’s career, he exhibited a clear debt to Parsons’ theory. Reviewers of *Tokugawa Religion* consistently recognized Bellah’s debt to Parsons as well as to the older and more definitively established “classic” theorists, Durkheim and Weber (see Yinger; Cole; and Liu). A review in *Social Forces* listed Weber and Parsons as the primary sources of Bellah’s conceptual frameworks, suggesting that the study “might perhaps be called, ‘The Samurai Ethic and the Spirit of Political Capitalism (Yinger, pp. 49-50).’” Bellah’s work thus offered a promising example of how Parsonian sociology might be creatively utilized and integrated with other ambitious theoretical projects (such as Weber’s), even by sociologists who did not wish to commit wholly to Parsons entire theoretical framework. Bernard Barber likewise earned attention as an important sociologist in his own right. His first publication, *Science and the Social Order* (1952) was a study in the sociology of science. Although that topic was inspired by Merton’s antecedent work, Barber himself was adamant about the importance of Parsons as a source of inspiration in *Science and the Social Order* (Barber, 1986). To Barber’s readers, however, it was the debt to Merton that was more immediately obvious. Four reviews of Barber’s *Science and the Social Order* (1952) made reference to the “long, thoughtful, and altogether excellent Foreword” by Merton and commented on what it added to Barber’s own project (Bierstedt, 1953; Zirkle; Lessa; and Crane). By contrast, only one review in a major journal mentioned Barber’s debt to Parsons (Fleming).

Merton, Bellah and Barber established themselves in constructive relation to Parsons' work and, at least in part because of that association, were able to establish successful careers of their own that melded Parsons' theory with a more flexible attitude towards his opponents' work. However, Parsons' more committed critics did not take the opportunity to modify their approach to Parsons' works themselves. Reviews of Merton's and Bellah's books duly applauded the usefulness of their theories and noted their indebtedness to Parsons, but those positive responses to applied structural-functionalism did not translate into more positive responses to Parsons' work. Not a single review of Parsons' books that lamented his style and his high level of abstraction mentioned that some of his ideas were to be found – in a more applied and accessible form – in the work of Merton and Bellah. If Parsons' students had any effect on the interpretation of Parsons amongst his committed detractors, it was by placing a “lower bound” on the kind of interpretation that most sociologists gave to Parsons' work. As the critical reaction to Parsons grew stronger and more widespread, Parsons' work began to be understood as simply more abstract and incomprehensible than Merton's, without any qualities to recommend it beyond in addition to Merton's work. Gouldner, for example, (formerly Merton's student himself) gives a reading of Parsons and Merton along these lines (1959, pp. 242-251). The more traction such readings of Merton gained, the less reason there was to try to engage seriously with Parsons. Despite the constructive uses made of his writings by students like Merton and Bellah, for many sociologists Parsons' reputation continued to be formed primarily by a combination of the virulent polemics hurled by critics and Parsons' own conspicuous silence.

Conclusion

Gouldner’s *Coming Crisis* was overly dramatic in its depiction of the Parsonian hegemony. By the 1970s, the theoretical alternatives to Parsonianism of symbolic interactionism, ethnomethodology, neo-utilitarianism and others were already well established, and it became less fashionable to depict Parsonianism as a giant looming over the entire discipline. Indeed, by the 1980s it became possible to speak of a Parsons “revival.” The angry backlash and quick dismissal of Parsons were frankly acknowledged, and Parsons was celebrated by many as the first to attempt the grandly ambitious synthesis of the older, established classics of sociology (see especially Alexander, 1983), and his work became a cornerstone in more recent attempts at synthesis in sociological theory from the likes of Jürgen Habermas, Anthony Giddens and Niklas Luhmann.

However, in the course of that Parsons revival, little attention has been given to the role that Faris, Mills and Homans played in shaping his early reputation. I have argued here that those most prominent attacks are inextricably linked to the production of Parsons’ reputation and that they shed an important light on his intellectual legacy understood in the most general terms. When we examine those early critiques, clear patterns emerge. Although it did not go uncontested, there was a *single* caricature of Parsons that emerged, collectively produced. It treated Parsons reductively, focusing inordinately on his language and his mode of presentation. What references it made to the content of Parsons’ theory treated his 1951 works as definitive, although a strong consensus has since emerged that Parsons’ later contributions – especially the *Working Papers in the Theory of Action* (with Edward Shils and Robert Bales, 1953) and *Economy and Society* (with Neil Smelser, 1956) significantly advanced his theory, providing a

more sophisticated notion of a system than that of *The Social System* (Bortolini, forthcoming; Alexander, 1983, pp. 73-118; Habermas, 1987, pp. 225-226; Joas and Knöbl, p. 75). In the face of Parsons' own theoretical innovations, however, the caricature of his 1951 works was perpetuated for years to come. For Parsons' critics in the 1950s and later, his corpus was not simply the "blind alley" that Robert Bierstedt saw in *Structure* and thought well worth exploring, it was a roadblock to the discipline, something to be evaded at all costs. On the strength of those claims, Mills and Homans invited the generation of sociologists coming of age in the 1960s to simply not read Parsons. The contributions of Parsons' students, although they helped to keep some of his ideas in circulation, did little to rescue Parsons' personal reputation or direct a wider readership back to his writings. Caricatures though they were, the polemics of Mills, Homans and others moved a remarkable number of sociologists, and they ought to be remembered whenever we try to characterize Parsons' impact on the field of sociology and the legacy he has left behind.

ⁱ *Structure* is often credited with introducing US audiences to the work of Marshall, Pareto, Durkheim and Weber, the four European writers at the heart of Parsons' synthetic project. But that assessment ignores a sizable body of work that had been produced in the US before *Structure* and thus obscures the significant resonance that Parsons' European writers had with his US colleagues. Marshall's work, needing no translation, was familiar to US economists and the broader social science community (Yonay, pp. 30-36; Ross, p. 174). Pareto's work, too, was well known to a number of influential sociologists and

other social scientists before the publication of *Structure*. Parsons’ own interest in Pareto began during his involvement in a 1932-34 seminar on Pareto at Harvard organized by Lawrence J. Henderson. Members of the prestigious seminar group included sociologists George Homans and Elton Mayo, historian Crane Brinton and economist Joseph Schumpeter (Heyl). Beyond the Pareto Circle at Harvard, several scholars who were to engage with Parsons in the coming years had written on Pareto before 1937 (see Faris, 1936; House, 1936; Sorokin, 1928; and Homans and Curtis, 1934). Weber’s work was gradually being introduced to US readers, in part through Parsons’ own 1930 translation of *The Protestant Ethic*. As for Durkheim, by 1937 US scholars had been familiarizing themselves for years with his work and where it stood in relation to more familiar American social psychologists and philosophers (see especially Faris, 1934; Karpf, 1932, pp. 108-122; Sorokin, 1928, pp. 463-480; and Merton, 1934). It would be more correct to say that Parsons played the key role in *establishing* these figures as classics among US sociologists, but even that fails to explain why only Durkheim and Weber attained that status, while Pareto’s star diminished considerably in the US after 1937.

ⁱⁱ The review in *American Journal of Sociology*, by Floyd House, was cursory and did not contain substantial editorial opinions on the book.

ⁱⁱⁱ As Parsons noted in the 1968 reprint of *Structure*, its initial printing only sold out after approximately 10 years in print. (Parsons, 1968, v).

^{iv} Solovey argues that Parsons was chosen as spokesman for the SSRC on the grounds that his vision of social science’s standing relative to the natural sciences matched up well with the official position the SSRC wished to promote (p. 413), but Parsons’ name recognition and the prestige of his institutional standing were also certainly factors in the

decision. Klausner writes that, given his “breadth of understanding” and “academic seniority,” Parsons was “all but destined to be invited to prepare the paper” (p. 17).

^v Although I have focused here on Mills’ challenge to Grand Theory, his argument in *The Sociological Imagination* and in the 1954 *Saturday Review* article is two-pronged. Mills is equally dismissive of Parsons’ work and what he calls “Abstracted Empiricism” (identified with “the Scientists” in the 1954 article). By “Abstracted Empiricism,” Mills meant the quantitative, statistics-laden sociology that was largely identified with the Columbia BASR and especially the work of Paul Lazarsfeld (Mills, 1959, pp. 50-75).

^{vi} Coser was not the only conflict theorist to critique Parsons constructively through a deep engagement with his work, but the other significant writers to do so came from outside the United States, leaving them somewhat distant from the center of debate that concerns us here. David Lockwood and Ralf Dahrendorf, from Great Britain and Germany, respectively, both produced major statements on Parsons in the mid 1950s (see Lockwood, 1956; Joas and Knöbl, pp. 183-184). Although they departed from Parsons strongly, both “still spoke of the desirable extension of Parsonian theory rather than its refutation” (Joas and Knöbl, p. 184). Readers of Lockwood’s and Dahrendorf’s articles would find no encouragement to simply not read Parsons, as the audiences of Faris, Mills and Homans did. Parsons thus enjoyed a more or less continuous status as an important thinker in Europe, and especially in Germany, from the early 1950s to the more recent projects of Habermas, Luhmann and Anthony Giddens. On Parsons’ more recent reception in Germany, see Alexander, 1984.

^{vii} Parsons sent three letters in reply to Schütz, a cumulative 20 pages long. The tone of all three letters is well summed up by this passage towards the end of the third:

As I have said before at a number of points, I feel able to assent on general grounds to your non-critical formulations because they seem in accord with my own experience. I do not, however, see their relevance to what is my field of interest, the development of a systematic theoretical scheme for empirical use in the social field. Practically every statement of yours, so far as I have understood it, which might, if its implications were followed up, be relevant to that seems to me to rest upon inadequate understanding of my own argument (Parsons, 1941c).

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