

182-1  
24

TESTING GOULDNER'S COMING CRISIS THESIS:  
ON THE STATE OF CONTEMPORARY SOCIOLOGICAL THEORY

by

James John Chriss

Thesis submitted to the Faculty of the  
Virginia Polytechnic Institute and State University  
in partial fulfillment of the requirements for the degree of

MASTER OF SCIENCE

in

Sociology

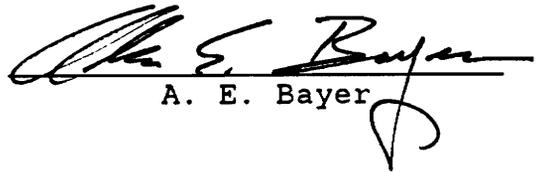
APPROVED:



E. R. Fuhrman, Chairman



G. A. Hillery, Jr.



A. E. Bayer

December, 1990

Blacksburg, Va.

C.2

LD  
5655  
V855  
1990  
C469  
C.2

TESTING GOULDNER'S COMING CRISIS THESIS:  
ON THE STATE OF CONTEMPORARY SOCIOLOGICAL THEORY

by

James John Chriss

Committee Chairman: Ellsworth R. Fuhrman  
Sociology

(ABSTRACT)

Alvin Gouldner, in his 1970 work The Coming Crisis of Western Sociology, put forth the proposition that the discipline of sociology would soon witness the decline and eventual entropy of the then dominant functionalist theoretical perspective. This thesis is tested vis-a-vis the measurement of influence of the leading figures of four competing theory groups, namely Talcott Parsons (functionalism), George Homans (exchange theory), Erving Goffman (the dramaturgical wing of symbolic interactionism), and Harold Garfinkel (ethnomethodology). The findings provide illuminations of both the past and current state of sociological theory.

## ACKNOWLEDGEMENTS

There are few things in this oftentimes confused and bewildering life that one can point with any degree of certainty toward which epitomize and provide balance, foundation, and constancy. My personal Rock of Gibraltar, those individuals that have always believed, that have gone beyond all expected or even knowable boundaries of compassion, loyalty, understanding, and love, are of course my parents, John and Angie Chriss. Their constant and unequivocal support has provided me with a mountain of opportunities and rewards, the accumulation of which I know I will never be able to fully repay. To them I dedicate this work.

Heartfelt gratitude and love is also extended to my brother Bill, who always stood by me and believed in me even during those times when I never really gave him good reason to do so. This, I suppose, is truly what it means to be a brother.

Finally, thanks go out to the faculty, staff, and fellow students of the Virginia Tech department of sociology for providing innumerable perspectives of the social world from which to pick, choose, (and even ultimately reject). A

special round of thanks go out, of course, to the members of my committee--Ellsworth Fuhrman, Alan Bayer, and George Hillery, Jr.--all of whom gave of their time and intellectual resources in providing advice and guidance throughout the writing of this thesis.

## TABLE OF CONTENTS

LIST OF TABLES AND FIGURES.....	vii
<u>Chapter I</u> INTRODUCTION.....	1
1.1. Preliminary Remarks.....	1
1.2. The Problem.....	6
1.3. Confirmation and the Philosophy of Science..	13
<u>Chapter II</u> GOULDNER'S CRISIS.....	24
2.1. A Review of the Reviews.....	24
2.2. The Importance of Gouldner.....	30
2.3. Gouldner's Views.....	38
2.3.1 On Goffman.....	40
2.3.2 On Homans.....	43
2.3.3 On Garfinkel.....	47
2.3.4 On Parsons.....	53
<u>Chapter III</u> METHODOLOGY.....	58
3.1. What is Influence?.....	58
3.2. Measuring Influence.....	65
3.3. Citation Analysis.....	72
3.4. Cumulative Advantage.....	79
<u>Chapter IV</u> THE CHOSEN WORKS.....	86
4.1. Harold Garfinkel.....	90
4.2. Erving Goffman.....	105
4.3. George Homans.....	119
4.4. Talcott Parsons.....	131
<u>Chapter V</u> INTERPRETATION OF FINDINGS.....	154
5.1. Overview.....	156
5.2. <u>American Sociological Review</u> .....	173
5.3. <u>American Journal of Sociology</u> .....	183
5.4. <u>Social Psychology Quarterly</u> .....	187

Chapter VI CONCLUSIONS.....194

6.1. Critique of the Study.....194

6.2. Strengths of the Study.....202

6.3. Coming Crisis in Retrospect.....207

6.4. James Coleman and Rational Choice Theory....213

BIBLIOGRAHY.....223

VITA.....242

## LIST OF TABLES AND FIGURES

	<u>PAGE</u>
Table 4.1. Number of Citations in <u>SSCI</u> 5-Year Cumulative Indexes to Authors' Selected Works	89
Table 5.1. Total Cites to Five Key Works of Each Author in Three Selected Journals, by 5-Year Intervals	161
Figure 5.1. Total Citations in Three Selected Journals to Each Author's Chosen Works, 1960-89	164
Figure 5.2. Authors' Single Most Cited Work in Three Selected Journals, by Years Since Release	168
Table 5.2. Total Citations to 5 Key Works of Each Author by Selected Citing Journals, 1960-89	175

"It is not likely that the devotees of the Psychedelic Culture will find Parsonsianism congenial; indeed, the mind boggles at the thought of a Parsonsian hippie."

- Alvin Gouldner (1970a:160)

"Many practitioners of cognitive enterprise tend to allege that our knowledge of the external world is directly 'given' by the 'nature' of that world and that humanly formulated 'theory' unless inferred by the strictest rules of induction alone is distortion. However, there is an equally, indeed recently even more prominent, tendency to maintain that humans have directly authentic experience of organically grounded motivation and that to 'free' such authentic motivation from alleged distortions it is necessary merely to brush away the symbolic structuring of human action systems. In this sense, if Hume be the great apostle of cognitive empiricism, then Rousseau must be identified as the apostle of direct motivational 'experience'; Schutz stands [as well] in this prophetic line. Both extremes are equally unacceptable to an unreconstructed Kantian like myself."

- Talcott Parsons (1978:413)

"The weakness of sociology has continued to lie in theory. Not only have most sociologists failed to accept an adequate conception of what a theory is, but they have allowed themselves to fragment into a number of allegedly incompatible theoretical schools. These could be united by an individualistic behavioral psychology."

- George Homans (1986:xxix)

"For ethnomethodology the objective reality of social facts, in that and just how it is every society's locally, endogenously produced, naturally organized, reflexively accountable, ongoing, practical achievement, being everywhere, always, only, exactly and entirely, members' work, with no time out, and with no possibility of evasion, hiding out, passing, postponement, or buy-outs, is thereby sociology's fundamental phenomenon."

- Harold Garfinkel (1988:103)

"Indeed I've heard it said that we should be glad to trade what we've so far produced for a few really good conceptual distinctions and a cold beer. But there's nothing in the world we should trade for what we do have: the bent to sustain in regard to all elements of social life a spirit of unfettered, unsponsored inquiry, and the wisdom not to look elsewhere but ourselves and our discipline for this mandate. That is our inheritance and that so far is what we have to bequeath."

- Erving Goffman (1983:17)

## I. INTRODUCTION

### 1.1 Preliminary Remarks

Alvin Gouldner's The Coming Crisis of Western Sociology, published in 1970 on the heels of the tumultuous string of revolts, uprisings and protests which had wracked the American university system during the 1960s, proffered a challenge to establishment sociology, asking us all to once again undertake a reassessment of the situation, to not be satisfied with merely resting on our laurels. Through this reevaluation of sociology by way of an inward examination of ourselves and the tenets which we hold dear (i.e., positivism), Gouldner hoped that this new spirit of heightened self-awareness would act as a contagion which would ultimately serve to instill within each of us the conviction that, in order for sociology to be transformed into a more liberative, humane science, society itself would require transformation.

The Coming Crisis is thus an enormous, ambitious undertaking, one which offers a novel approach--that being reflexive sociology--for the transformation of sociology in particular and of society in general. Gouldner chose, for

very good reason, to begin his book with a quote from Nietzsche: "Here are the priests; and although they are my enemies...my blood is related to theirs" (Gouldner 1970a:vi). Gouldner thus sees himself in the romantic tradition as the martyr giving of himself for the betterment of mankind. He is literally the messenger who, being the bearer of bad news, is ordered beheaded by the king. And the sociological establishment did indeed, through book reviews, symposia and the like, come to a consensus concerning Gouldner's sentencing. The verdict: "Off with his head!"

I have already stated that I believe Coming Crisis is indeed a monumental intellectual achievement, but taking note of an accomplishment and agreeing wholeheartedly with its tenets are two different things altogether. I will have much more to say concerning my own views of the merits of Gouldner's project as it relates to its substantive impact upon the discipline later. But before proceeding with this investigation any further I wish to emphasize a point which I feel has been lost, or at least overlooked, in much of the sociological literature of late, particularly when it comes to questions concerning ontological and/or epistemological differences between theorists and representatives of competing perspectives. What I would like to suggest--and this really should come as no

surprise--is that sociology is certainly not open heart surgery, nor should the nature of its discourse be confused or identified with what goes on in the political arena. That is to say, matters of life and death do not hinge upon whether Parsonian functionalism is conservative, whether there is actually a convergence taking place between functionalism and Marxism, or whether Alvin Gouldner's prediction of a coming crisis in sociology has come true or not.<sup>1</sup>

I say this because during the course of this investigation I have had the enlightening (though at times troubling) task of gathering the plethora of book reviews, critiques, letters-to-the-editor, and symposia which, having evolved into a minor industry of its own, Gouldner's The Coming Crisis of Western Sociology has generated. To

---

<sup>1</sup> Granted, these are interesting and provocative intellectual questions and ones which will be addressed over the course of this investigation. However, as Mannheim (1936:38) has so eloquently argued, scientific or intellectual discourse is (or at least should be) autonomous in pursuing its own ultimate ends, those being the generation and advancement of knowledge. This of course requires from us the acknowledgment of the simple fact that multiple intellectual universes do indeed exist (and coexist), each one disseminating from its own unique perspective particular knowledge claims. On the other hand, political discourse is seen "...as conflict and tends to be a life-and-death struggle," with each side's ultimate goal being the obliteration of the other. For more elaboration on the types of ultimate values indigenous to sociology, see Parsons (1935b, 1978:352-433).

say that this book struck a raw nerve within the discipline would be the understatement of the last two decades. If Gouldner's singular goal in writing this book had merely been to shake up the sociological establishment, then Crisis fulfilled its mission admirably. Persons will say and do strange things in response to a perceived career threat, and Gouldner's sustained polemic against academic sociology was surely taken in that spirit.

The controversy surrounding the interpretation of Crisis and the overwhelmingly negative initial response which emerged (the discussions of which will be presented more fully in section 2.1) both serve to emphasize a fundamental point undergirding this entire investigation. This point is simply that Gouldner's book, by drawing a line in the sand and calling to the battlefield those sociologists who, like him, sensed an ill wind blowing through the discipline, attempted to shatter once and for all the long held notion that value neutrality is attainable and merely an innate occupational verity of science. By arguing for the linking of internal sentiment structures with the eventual products of scientific thought (i.e., theory), Gouldner thus brought to the fore the role of ideology in the dissemination and production of sociological thought and knowledge. But Gouldner's line in the sand also served to foist upon the discipline, via the tyranny of dichotomization, another

false barrier to informed and reasoned intellectual discourse. That is, the trajectory of the discussion--especially the conservative/liberal component--tends toward the perception that what we have here is an irreconcilable dichotomy, i.e., an "us versus them" mentality. Thus forced into a corner, and for ontological peace of mind, one must, while demonstratively displaying an appropriate sense of urgency, make a stand one way or the other regarding the merits (or lack thereof) of Gouldner's project. That is to say, a study of this sort forces one into a violent confrontation with one's own domain assumptions (or in Jeffrey Alexander's [1983] terminology, one's ideological presuppositions).<sup>2</sup>

On a personal level, one of the primary tasks of this investigation will be an attempt to sort out the duality of my own epistemology, because I have an affinity for aspects of both Parsons' and Gouldner's theoretical programs. That is, there are components within each theorist's work which resonate well with my own theoretical predilections. For

---

<sup>2</sup> However, Alexander (1983) does not agree with Gouldner that a theorist's sentiment structure (or domain assumptions) necessarily impinges on or informs the form of the theory. Alexander thus argues for the "multidimensional" nature of theory construction, suggesting that a theorist's ideological assumptions cannot properly be said to be causally traceable to the choice of methodological strategies, analytical techniques, or the final form which the theory takes.

example, I am inclined to accept both Parsons' functionalism, for methodological purposes, and Gouldner's reflexive sociology. In fact, through this investigation I hope to come to a more complete understanding of the limitations and points of demarcation regarding each perspective, and to ultimately look into the possibility of arriving at, say, a reflexive functionalism. This particular program of personal inquiry and discovery will be incidental to and a residual of the larger task of testing Gouldner's crisis thesis (as I explain more fully below), but one which will nevertheless find explicit articulation at various points in the analysis.

## 1.2 The Problem

This work proposes to assess the accuracy of a major prediction put forth by Alvin Gouldner in his 1970 work, The Coming Crisis of Western Sociology. That prediction-- that the discipline of sociology would soon witness the decline and eventual entropy of Parsonian functionalism-- was based primarily upon two claims or observations: 1) the emphasis on the development of middle range theories directed toward social problems research within the matrix of the rapid post-war expansion of the American Welfare State would be shown to be incongruent with functionalism's implicit social utilitarianism and its view of society as a

self-regulating system; 2) the emergence of a new, inchoate sentiment structure within the New Left would cause the then current wave of young student sociologists-to-be to find the tenets of functionalism distasteful and/or irrelevant, which in turn would lead to a wholesale rejection of functionalism as a viable working paradigm within sociology (Gouldner 1970a:161-62).

Central to Gouldner's claim is the notion that the theories of Talcott Parsons dominated the field of sociology from WWII through the sixties, and that any critique of contemporary academic sociology must necessarily be directed against Parsons as its leading figure.<sup>3</sup> There exists little if any evidence to doubt this claim; both opponents and proponents of Parsons alike seem unified in their acknowledgement of the primacy and extent of Parsons' impact upon the discipline.<sup>4</sup> Guy Rocher

---

<sup>3</sup> Gouldner (1970a:167) describes contemporary Western sociology as being comprised of two major, competing divisions, namely, Marxism and Academic sociology, with Parsons being the leading representative of the latter category.

<sup>4</sup> There are, of course, a few observers who do not agree with the claim of Parsons' intellectual/theoretical domination of the field. Besides those critics of Gouldner which I discuss below, Alan Sica (1983, 1989) best reflects the contemporary sentiments of this small but vocal opposition. His opposition to Gouldner's position comes by way of his questioning the routine invoking of the Parsonian domination theme which the great majority of sociologists engage in. For example, Sica (1989:504) claims

(1975:153), for example, has pointed out that no contemporary theorist so dominated and influenced sociology during this period as did Parsons, and that the scope of his influence extended into such diverse fields as "...the sociology of knowledge and science, the sociology of religion, economic sociology, political sociology, the sociology of education, the sociology of social change,"<sup>5</sup>

---

that there never was an "orthodox consensus" within the discipline, and that theorists such as Smelser (1988b, 1988c) and Alexander (1988) are simply wrong in claiming that Parsons ruled the "theoretical roost" in the 50s and early sixties. Sica goes on to claim that there exists firm evidence which supports his position, suggesting that an "...examination of the journals between 1945 and 1965, or the remarks of other notables of the era, does not support their belief." In any event, this investigation will be attempting to shed some light on this area of controversy, and for purposes of this analysis I will agree with Gouldner's contention of the domination of Parsons until such time that the data suggest otherwise. Finally, my investigation will hopefully isolate the peak of Parsons' influence and illuminate the period of transition from "orthodox consensus" to "theoretical polycentrism."

<sup>5</sup> Some might disagree with Rocher that Parsons contributed substantially to the study of social change, especially those who contend that Parsons' theories contain an inherent static bias. Gouldner himself claims that Parsons' theories progressed through two distinct stages, and that this second stage--wherein Parsons became concerned with such things as evolutionary universals in society and the investigation of the processes of structural transformation within certain aspects of social systems--came about as a response to earlier criticisms which contended that his theories could not adequately account for change and conflict in society (Dahrendorf 1958, 1959; Mills 1959). Thus, Gouldner (1970a:368-370) claims that during this second phase Parsons' theories began to converge to some degree with Marxism in that there was a tendency to place greater and greater emphasis upon

and the history of social thought."

Richard Munch (1987) is absolutely correct in stating that Parsons' greatest and most enduring contribution to sociology is his elaboration of a highly technical scheme for the general analysis of human social action. As Munch (1987:1) explains,

Sociologists of the most different persuasions are at least unanimously agreed on the importance and the extent of Parsons's contribution to the theory of action. Equally, all are agreed on the immense scope of his work and hence its significance for all sciences of human action, well beyond the confines of sociology as such.

I stand with Munch's assertion that action theory represents the core concern of the science of human action, and that Parsons' unwavering, single-mindedness of purpose in elaborating such an action scheme was truly a remarkable accomplishment, especially in the face of consistent and repeated assaults from dissenting factions within the discipline who for one reason or another found themselves

---

the structural exigencies of social change. Gouldner sees indications of this general convergence trend in the works of such functionalists as Merton, Smelser and Etzioni. For example, Gouldner claims that "Merton used Marx to pry open Durkheim," the quote referring of course to Merton's typology of adaptation and his formulation of anomie which, while overtly following Durkheim and tacitly following Marx, indicates the cultural contradictions of capitalism by showing the various manifestations of the disjunction between means and ends, i.e. between effort and attainment.

at odds with Parsons' approach.<sup>6</sup>

Why did this backlash against Parsonian functionalism occur within sociology, and what are the more general implications of the Parsonian deconstruction when viewed from a sociology of knowledge perspective? What good reasons, if any, are there to believe the claims of Gouldner that a sociology of sociology, and that particularly his notion of a "reflexive sociology," is what is required in order that sociology be lead out of the morass into which functionalism unwittingly dragged the discipline? Finally, given the vociferousness and intensity of the critique against Parsons, did this sustained polemic lead in fact to any appreciable decline in Parsons'

---

<sup>6</sup> Robert Friedrichs (1970:26-29) has traced the historical response to functionalism in general and Parsons in particular, suggesting that Dahrendorf's "Out of Utopia" (1958) was the catalyst for orienting sociologists toward a repudiation (or at least a more thoroughgoing questioning) of system theory and functionalism's presumption of stability and equilibrium. Dahrendorf continued his assault on Parsons by carrying the discussion into his 1959 work, Class and Class Conflict in Industrial Society. Other prominent critics of Parsons from the same period were Barrington Moore (1958), C. Wright Mills (1959), Dennis Wrong (1959, 1961), William Kolb (1961, 1962), and Max Black (1961). A strong second wave of critiques were to follow in the late sixties/early seventies, led by Gouldner (1970a, 1970b, 1971, 1973a), Reynolds and Reynolds (1970), and, of course, Friedrichs's own work. It should also be noted that both Friedrichs (1970) and Mullins (1973) rely heavily on Kuhn's (1962) conception of paradigm and paradigm shift in the sciences. This of course contrasts against Gouldner's (1970a) view insofar as he overtly chose not to embrace Kuhn's formulation.

influence; that is, was there truly an entropy of Parsonian functionalism?

The first several questions will be dealt with during a brief analysis (carrying on from the introductory discussion above) of the two major components of Gouldner's coming crisis thesis. I will present Gouldner's own reasoning and justifications for the assault on Parsons, and I will further briefly look at some voices sympathetic to Gouldner. Finally, I will analyze various defenses of Parsons in the face of the Gouldner assault.

With regard to the accuracy of Gouldner's prognostication of the decline of Parsons' intellectual domination of sociology, one of the major tasks of this investigation will be the elaboration of an acceptable methodological approach for dealing with this concept of scientific or intellectual "influence." As I stated earlier, there appears little doubt that Parsons was, during the period in question, indeed as influential and dominating as Gouldner claims. The question that remains to be addressed, then, is how do we measure "influence"? As I explain in the methodology section below, it will be argued, and justification will be offered, that a citation analysis of Parsons' work will be adequate for the purposes of measuring the general levels of influence his works have generated within sociology over time.

However, in order to assess more thoroughly Gouldner's prediction of a coming crisis in Parsonianism, we must also consider the other competing paradigms and perspectives which Gouldner sees as potentially gaining ascension within sociology relative to the decline of functionalism. According to Gouldner, these competing perspectives are 1) Erving Goffman's dramaturgy (1970a:378-390); 2) Harold Garfinkel's ethnomethodology (1970a:390-395); and 3) social exchange theory as represented primarily by George Homans and Peter Blau (1970a:395-396). Because Gouldner concentrates primarily on Homans in his discussion of exchange theory, no formal analysis of the theories of Peter Blau will be offered.

Gouldner's chapter 10, "The Entropy of Functionalism and the Rise of New Theories" (1970a:373-411), is therefore an important component of our analysis because it is here that he provides an alternative explanation for the direction and future possibilities for sociology. Crisis, therefore, goes beyond being merely a blind railing against and denunciation of academic sociology; instead, Gouldner suggests some specific intellectual projects which, according to his analysis, seem primed to fill the void created by the decline of the Parsons project. Gouldner's thesis thus informs and directs this investigation into yet another, interconnected set of considerations. We are not

here concerned merely with measuring, vis-a-vis a citation analysis, Parsons' relative levels of influence; we must also consider the works of Homans, Garfinkel, and Goffman.

In order to test Gouldner's thesis, then, it would seem appropriate to include citation analyses for these three theorists as well. Therefore, citation analyses will be conducted for each theorist covering the citing period 1960-1989; citation rates for these theorists will then be compared over appropriate intervals in an attempt to establish any discernable patterns of movement among the theorists. 1960 has been chosen as the start date for this investigation because it coincides well with the first wave of responses to the Parsons project beginning in the late fifties. Also, with Gouldner's Crisis having been written in 1970, the decade of the 60s should allow us some baseline measure of each theorists' citation rates before the second wave of anti-Parsonian critiques began.

### 1.3 Confirmation and the Philosophy of Science

There is a philosophy of science question which must be addressed pertaining to what exactly counts for purposes of this study as confirming or disconfirming evidence in the test of Gouldner's crisis thesis. For this purpose Hempel's (1965) discussion of the logic of confirmation is well worth reviewing.

First, we must realize that the school of thought known as logical positivism emerged ostensibly as a reaction against various metaphysical formulations (e.g., Kant and Hegel) which, as claimed by the empiricists, were devoid of either (or both) logical and empirical meaning. Ian Hacking (1983) has suggested that in their attempt to establish a formal, rigid set of criteria of cognitive significance certain philosophers like Quine opened up a whole new can of worms by arguing that we should not (as far as our theories go) talk about things, but rather we should talk about the way we talk about things. Observation is thus reduced to observation statements, that is, the words used to report the observation. It is easy to see, I believe, why the logical positivists and the analytical philosophers went this route: by reducing analysis to semantics it was hoped that the essence of theory talk could be captured within a finite, highly regulated vocabulary, the rules of the analysis being guided by the laws of predicate logic.

Thus the logical positivist program was born. In his discussion of the logic of confirmation Hempel (1965:3-51) reviews the logical positivist and later empiricist positions, especially in terms of how a sentence counts as

being a cognitively significant assertion.<sup>7</sup>

The two ways that an assertion can fulfill cognitive significance are (1) the sentence itself is either analytic or contradictory, i.e. the classic deductive nomological syntax of logic, or (2) the assertion corresponds to some experiential evidence in the world, i.e. rather than purely logical significance, the sentence has empirical significance.

This is, as Hempel suggests, the testability criterion of the meaningfulness of sentences. The logical positivists thus set before themselves an ambitious but somewhat flawed goal: the linking of logical utterances (or hypotheses) about a given phenomenon with the empirical statements (or observation sentences), this linking in turn serving to either confirm or disconfirm the theory.

In an attempt to verify the criteria of significance, that is, when the question was asked what counts as a significant versus a nonsignificant sentence, the verifiability requirements of the logical positivists were found lacking in that the conditions of logical consistency which obtain from such an attempt to codify the conditions

---

<sup>7</sup> "Cognitive significance" as understood here means merely that the sentence is constructed such that the statement can be directly shown, by the rules of sentential logic, to be either true or false.

of verifiability tended to produce overly restrictive and, ultimately, unworkable schemes for the task at hand (Proust 1989).

In an attempt to improve upon the weaknesses of the verifiability requirement, later philosophers introduced the requirement of definability, which asserts simply that within a cognitively significant sentence any empirically significant term must be definable by means of observation terms. But even definability was shown to be too restrictive because of the problematic nature of such disposition terms as, for example, "soluble," "malleable," or even "fragile" (Hempel 1965). That is to say, disposition terms throw a monkey wrench into the analytical program because the terms as such are not easily translated into the language of predicate logic, thus the attempt to attach cognitive significance to them is seriously impeded.

Carnap improved the situation somewhat by introducing reduction sentences, that is, sentences which do not attempt a sweeping, universalistic explication of definitional terms. Rather, the definition of the term in question is conditional or only partial. Thus, as the program of explicating and clarifying cognitive significance and the verification of their tenets has progressed over the years (paralleling the transition from earlier logical positivism to logical empiricism), we

witness the continual weakening of the rules and laws which, as proffered in each succeeding incarnation, have attempted to establish in exhaustive and inclusive fashion the foundations upon which an empirical conception of significance could be built.

Thus, the logical positivist program of confirmability, which attempted to state how sentences are meaningful either by deduction from a more general set of propositions or as confirmed by present evidence, gave way to logical empiricism and its weakened verifiability which admitted that not all evidence could ever be gathered at hand. The empiricists thus gave up the ideal of absolute verification in favor of "high certainty." We see in Hempel's case, for example, a philosopher who began his career solidly in the positivist camp but later softened his own position, moving toward perhaps what we might describe as a Peircian pragmatism.

Hempel's (1965) retrospective of the failings of the logical positivist program has been elaborated here because it helps to clarify how I am attempting in this study to talk about confirmation. In the following discussion I will attempt to link Hempel's observations with what I see as a paralleled set of observations in the thought of Talcott Parsons. What will be emphasized is that intellectual lineage beginning with Kant which led through L. J.

Henderson and finally into the heart of Parsons' theoretical program, all of whom detailed the importance and the role of the conceptual scheme in any project of theory construction.

Hempel (1965) ended up suggesting that in light of the failings to produce a consensually rigorous theory of cognitive significance, in the final analysis the twin projects of theory formation and concept formation go hand in hand. That is to say, when one goes about the task of isolating the requirements of cognitively significant sentences apart from any connection to its theoretical objective--that is, treating this process as an end in itself rather than as a means toward testing the validity of one particular statement within the framework of one particular theoretical system--then one is, obviously, engaging only one component of the process. One must recognize that other types of sentences, such as, for example, isolated sentences, play a symbiotic part in making the theoretical system a totality. The program of excluding certain so-called nonsignificant (and thus undesirable) sentences for the purpose of establishing the means of attaining a truly and wholly cognitively significant scientific theory may indeed not be feasible. The best that we can do, argues Hempel, is talk about degrees of cognitive significance of entire systems rather

than attempting to isolate only a few select sentences within these theoretical systems. Again, it is stressed that extralogical and logical components together are required for a scientific theory, and that by comparing, say, the relative number of extralogical to logical features in a system we may, so says Hempel, come upon further guidelines for identifying the degree of cognitive significance of competing scientific theories.

Hempel's notion of the symbiotic relationship between logical and extralogical components of a theoretical system brings us to a similar line in Talcott Parsons' (1937) thinking. In a somewhat modified argument Parsons views the empirical as the "logical" and the philosophical as the "extralogical" symbiotic components of scientific theory. That is, Parsons (1937:21) suggests that philosophy "...is the attempt to achieve a rational cognitive understanding of human experience by methods other than those of empirical science." Philosophy thus exists as a residual category; however, since according to Parsons all theoretical systems tend to move toward theoretical closure (because it is simply in the nature of reason to strive for a rationally consistent explanation of all experiences which come within its range), and further, since these propositions are brought to the attention of the same mind, then it follows that there is a tendency to bring these

seemingly disparate methodologies (i.e., scientific and philosophical) into relations of logical consistency with one another (Parsons 1937:21). Parsons goes on to suggest that there are no logically "watertight compartments" in human experience, thus rational knowledge is a single, organic whole. But since at any given time we only have a fragmented view of the world or any particular aspect of it, not all explanatory variables contained within a theory will be explicitly defined. That is to say, given that all theories tend to move toward logical closure, and given that not all postulates or variables can coincide with empirical definition (because otherwise there would be logical closure), this means that there are always implicit assumptions contained within each scientific theory. The residual category of philosophy, then, helps to check the plausibility of certain of these implicit assumptions of empirical fact which have yet to be sufficiently tested or experienced. Philosophy as a nonempirical system of thought, then, plays a vital, symbiotic role in any empirical, scientific theory. Thus the connection back to Hempel's conception of logical and extralogical components of theory is complete.

This notion of the fragmentary nature of the world as available to human perception and as constitutive of knowledge is central to my use and understanding of

confirmation. We have seen in the example of the progression of thought from the logical positivists to the logical empiricists and beyond the continual weakening of the ideal of verification. That is, with each step of the way we have gone from absolute verification to high probability to, as van Fraassen (1989)--who happens to be a contemporary pragmatic antirealist--has suggested, empirical adequacy. That is, theories can be said to be competing if their models share the same empirical substructure. Nevertheless, theories represent (through the models we build) how we choose for our own purposes to slice up the world. The particular problem which we have chosen to pursue and which we define as problematic requires that a theory vis-a-vis a conceptual scheme is formulated so that we may say something about the phenomenon of interest.

Our phenomenon of interest here is Gouldner's theory of the coming crisis of Western sociology. There are obviously many ways of approaching the theory and many ways of making sense of its multiple components. For example, as Hacking (1983:94-95) (in agreement with Putnam) has said, "We cut up the world into objects when we introduce one or another scheme of description." We see as central the notion of intellectual influence, and as a result have chosen to define influence in a particular way such that it will be

measurable via citation analysis. The data which will be collected from the empirical substructure (i.e., selected sociology journals covering a specified period of time) will therefore count as an empirically adequate representation of the key measurable variable, namely influence. Just so long as the definitions which we choose to guide this study are adhered to, that is, as long as consistency is maintained in the attempt to link the empirical observations to the theory, then we can safely speak of the evidence as either confirming or disconfirming the theory. But keeping in mind the above discussion pertaining to the fragmentary nature of human perception, it should be stressed that our findings will count only insofar as we have chosen to "slice up the world." Thus, Gouldner's thesis is being tested within a clearly demarcated range of parameters (all of which will be more fully developed as the work progresses). In setting up the problem in this way we hope to say something novel and important concerning this particular aspect of Gouldner's thesis.

The next chapter offers an in-depth analysis of The Coming Crisis of Western Sociology. In addition to reviewing the many published responses to and commentaries on Gouldner's book, I will provide a summary and examination (in section 2.3) of Gouldner's reading of

Parsons, Goffman, Homans and Garfinkel in order to ascertain exactly what he believes each theorist has to offer and how each is judged as contributing to sociology's coming crisis.

## II. GOULDNER'S CRISIS

### 2.1. A Review of the Reviews

Although one can find interspersed throughout the literature occasional nuggets of wisdom and thoughtfully articulated arguments--both for and against Gouldner--which contribute constructively to an ongoing reevaluation and interpretation of Crisis, an alarmingly large number of these responses are, sadly, obnoxious, puerile, mean-spirited, and even downright scurrilous. Clearly the most bathetic examples--those critiques and missives which represent the apotheosis of this type of enraged, perverted discourse--are those of Jackson Toby (1972), Lipset and Ladd (1973), and Gouldner's (1973b) own rejoinder.

Believing that Gouldner's reading of Parsons was distorted and misleading (possibly true) and that he is a poor source to turn for an explanation of functionalism (probably not true), Toby (1972:109) suggests that Gouldner wrote the book for purely economic reasons, that he made a

"shrewd assessment of what would 'resonate' in the marketplace for books." Toby should be ashamed.<sup>1</sup>

Lipset and Ladd's (1973) article--an exercise in trivial, wretched excess if ever there was one--was a response to Gouldner's response to their original (1972) critique. The authors spend almost the entire article attempting to pin down Gouldner on fifteen allegedly sympathetic references to Parsons which appeared in his (1963) textbook co-authored with Helen Gouldner.

But even Gouldner was unable to escape from this madness, from this orgy of injudicious tit-for-tat, because in his (1973b) rejoinder to Lipset and Ladd he claims they exhibit an "unthinking male chauvinism" for automatically assuming that the alleged sympathetic references to Parsons are attributable to Gouldner alone, without stopping to consider coauthor Helen Gouldner's possibly having something to say in the matter. This is an unconscionable accusation, and Gouldner surely knew it. He apparently felt, however, that it was the only way to "get back" at Lipset and Ladd for their own foolishness. Exchanges such

---

<sup>1</sup> Toby is intimating, of course, that Gouldner turned on Parsons only recently because he sensed a rising tide of anti-Parsonian sentiment within the discipline. But this simply is not true; one need only check Gouldner's 1955 paper, "Metaphysical Pathos and the Theory of Bureaucracy," to see that Gouldner had been critical of Parsons long before the release of Crisis.

as these leave one scratching one's head in puzzlement to ask, is this the sort of "rational discourse" Gouldner envisioned for his fledgling reflexive sociology?

Although I am not about to defend the tactics employed by the majority of Gouldner's critics, I believe that Gouldner, whether knowingly or unknowingly, brought this wrath upon himself because of the literary style he chose. One may note, for example, the chapter headings and subheadings he used in discussing the career of Talcott Parsons: in reference to Parsons' social system concept the title was "The Conceptual System as Icon" (205); an entire chapter was titled "The Moralistics of Talcott Parsons" (246); another subheading read "The Piety of Functionalism" (262); for his discussion of Parsons on power and wealth he chose "Making American Society Whole: The Importance of Being Rich" (297). Aside from Parsons, Gouldner had some choice words for the anthropological functionalists Bronislaw Malinowski and A. Radcliffe-Brown whom, he intimated, were conservative apologists for European colonialism. Evidence for this, according to Gouldner, is that they rarely objected to European domination and, while "some Functional Anthropologists conceived it as their societal task to educate colonial administrators, none

thought it their duty to tutor native revolutionaries" (Gouldner 1970a:132).<sup>2</sup>

Gouldner's consistent use of headings and arguments such as these, wherein he denigrates Parsonian functionalism for its piousness, its conservatism, and so on, together with a somewhat moralistic discussion of his own reflexive sociology near book's end led a great many observers to suggest that Gouldner had impaled himself upon his own rapier. This is nowhere more clearly articulated than in Leo Kaplan's (1971:66) excoriation of Gouldner:

His book is saturated with innuendos about the venality of others (indeed, of the entire sociological profession), their stock manipulations, sexual lives, mistresses, political connections, inner-departmental dogfighting and power seeking and so on--in short, the wholesale selling of the sociological soul to mammon. Meanwhile, the saintly Gouldner, alone in his pristine purity, feels disgust, and, fighting off the seven deadly sins that surround him on all sides, pursues relentlessly, dialectically, reflexively, critically, intellectually, and self-consciously the holy grail: the grand synthesis of Marxism with academic sociology.

---

<sup>2</sup> See Murray Wax's (1970:217) defense of Malinowski and Radcliffe-Brown. Here he claims that Gouldner's history is simply wrong, reporting that "What I read, I found appalling, for instead of careful analysis I found innuendo and smear. Gouldner's discussions [of Malinowski and Radcliffe-Brown] are so inadequate that it is hard to take them seriously." In a parting shot Wax suggests that "His cry for what he calls a 'reflexive sociology' can scarcely be achieved if we are to begin that enterprise by a series of distortions of the work of those who helped to found the discipline."

As this strong renunciation by Kaplan indicates, an exploration of the reviews of Gouldner's Coming Crisis reveals that a good number of them were indeed negative.<sup>3</sup> Among the various other criticisms of Gouldner's analysis, several reviewers have suggested that he overestimated the centrality of Parsons' place within Western sociology. As Dahlstrom (1972:374) states, "Gouldner's identification of Academic Sociology with Parsonianism is questionable." Others voicing similar sentiments include Lipset and Ladd (1972), Urry (1972), Zeitlin (1972), Peterson (1971), and Roach (1971). What these reviewers are suggesting, of course, is that the Gouldner/Sprehe (1965) article which Gouldner cites as being evidence of Parsons' hegemonic domination within the discipline is simply not sufficient for the task at hand.

Another line of criticism against Gouldner suggests that his analysis of the historical development of social theory

---

<sup>3</sup> There were, of course, a few partially or wholly positive reviews, such as Young (1971), Hinkle (1971), Heritage (1971), Berger (1970), Friedrichs (1971, 1972), Flacks (1972), Deutsch (1971), and Peterson (1971). Berger (1970:280) best articulates the sentiment of most of these reviewers: focusing in on only the various shortcomings of Gouldner's analysis is like "...accusing an eagle of walking awkwardly instead of marveling at the majesty of its flight. Gouldner's book walks awkwardly now and then, but it is a soaring achievement, the most learned and eloquent book on the sociology of sociological ideas I know."

is flawed (e.g. Rhoads 1972, 1973; Urry 1972; Wax 1970; Lipset and Ladd 1973). Others suggest that his reading of Marx and Marxism is ill-informed and/or superficial (e.g., Touraine 1972; Alt 1981; Shaw 1971; Flacks 1972; O'Neill 1972).<sup>4</sup> Still others believe that Gouldner's analysis is overly-ideological (Ritzer 1970; Simpson 1971; Zeitlin 1972; Lipset and Ladd 1973), or that the heuristic benefits which accrue from the methodological approach of a sociology of knowledge are either not apparent or at best dubious (Hirst 1972; Roach 1971). Finally, many critics felt Gouldner's reflexive sociology--a deus ex machina offered up near book's end as a corrective to the ills of contemporary sociology (and, for that matter, Western society)--represents nothing more than the useless and

---

<sup>4</sup> For a more recent critique of Gouldner's reading of Marx, see Giddens (1987). Additionally, O'Neill (1972:171) also makes the valid point that Gouldner's treatment of the emergence of new theoretical perspectives virtually ignores entirely the sociology of C. Wright Mills and adherents to his work. A plausible explanation for this oversight comes to light when one considers, as Alt (1981) has reported, that during the 1960s (while faculty members at Washington University) Gouldner and Irving Horowitz, a sociologist who spent a good deal of time documenting Mills's intellectual and biographical history, were engaged in intense political conflict and departmental infighting. Also, a rift between Mills and Gouldner can be detected in light of the confrontational academic milieu of the 1960s. For example, during that time radical student movements were embracing the (perceived) anarchy of Marcuse and Mills and rejecting, because it was perceived as unacceptably "establishment," the kind of intellectual and theory-building programs being espoused by Gouldner.

unworkable utopian social schemes (cf. Marxism) which have been propounded now and again throughout the ages (e.g. Swanson 1971; Lindstrom 1972; O'Neill 1972; Bernstein 1972; Kaplan 1971).<sup>5</sup>

## 2.2 The Importance of Gouldner

For many of the reasons that critics found Gouldner's analysis unacceptable, others found in these very same points of contention reason to applaud and praise Coming Crisis. As I touched on briefly above, the major point of contention which served to polarize opinion on the validity and/or worth of the book was surely Gouldner's attempt to

---

<sup>5</sup> Echoing Kaplan's (1971) sentiment, Swanson (1971:321) suggests that Gouldner's call for a reflexive sociology comes off as incredibly sanctimonious, especially in light of Gouldner's (1970a:510) suggestion that the ultimate task of a reflexive sociology is the integration of human existence which "...was once the task of religion." Thus, seen as a new secular religion and with Gouldner as head priest, this reflexive sociology calls, according to Swanson, for the perpetual renunciation of the moral and social order (read "status quo") until such time that humans are finally free from and unencumbered by false consciousness. But the problem with this vision of man, as Swanson continues, is that "It strips sustaining value from everything about persons that is particular to their time and place and bodies and it leaves, as a characterization of men and women, only a few biological imperatives around which to build the fundamentals of their existence: sex, hunger, motility, and death." Only then, reduced to nothing more than primitive species beings, will men and women realize this loftier morality.

link Parsons' social circumstances with the purported conservative nature of his theories.

Simpson (1971) best articulates the parameters of the ideological debate which Gouldner's book unleashed.<sup>6</sup> Following closely points made by Bendix (1970), Simpson suggests that since the book was ostensibly a product of the turbulent sixties, Gouldner's vision is skewed in that his words reflect what was sensed in the intellectual air of the time, that being, of course, the nebulous but ominous sense of impending doom. That is, Parsons' overly technical and detached social theories were seen as a pedantic celebration of theory building and the empty pursuit of knowledge simply for the sake of knowledge. What was needed, so the sixties sentiment went, was not so much pedantics but social activism, i.e., the linking of theory with praxis.

This rebellion against reason or, using Simpson's (1971:661) phrase, this "smashing of scientific icons," is surely not new or novel in the history of human thought. Marx, for example, elevated to a new level of respectability the art of attacking academic scholarship. What was being attacked, of course, was the scientific

---

<sup>6</sup> Almost the exact line of thought can be found in Lipset and Ladd (1972, 1973), and Rhoads (1972, 1973).

credo of value neutrality and indifference; anyone in such a position would undoubtedly be siding--if not overtly then surely tacitly--with the status quo.

Simpson thus sees Gouldner's book (as well as Friedrichs's A Sociology of Sociology) in this light, that is, as a predictable assault on mainstream, establishment sociological theory. As such, the analysis by Gouldner is instantly labeled as being ideological and thus antiscientific, and, further, his relying so heavily on biography as a causal factor in the shaping of intellectual thought (as well as the final form which the theory itself takes)<sup>7</sup> smacks of the most vulgar form of radical subjectivism.

Simpson (1971:664) summarizes these points by explaining why Gouldner's choice of analytic strategy is, ultimately, so distasteful:

...[W]hen we pin an ideological tag on a theory by calling it repressive, prophetic, or whatnot, we say something about the validity of the theory. This notion is alarming, for it would turn sociology into substandard moral philosophy with the resonating of sentiments replacing reason and observation as the basis for constructing and judging theories.

---

<sup>7</sup> This is, of course, nothing more than the classical sociology of knowledge perspective, its signal hypothesis being that there is, to some as yet unspecified degree, an existential relationship between knowledge and its social bases (Fuhrman 1984).

Since Parsons represented at the time the most prominent and distinguished sociological theorist in the field, Gouldner's attack against functionalist theory thus amounts, contends Simpson, to an attack on the "rational underpinnings of the entire discipline." Thus exposed as the loud, whining, and extended polemic against an entire discipline which, Simpson insists, it most assuredly is, Gouldner's analysis can hardly be taken seriously as an intellectual pursuit. And although Simpson (1971:664) admits that a few young sociologists would undoubtedly be attracted to the work and discover that Gouldner's attack resonates well with their own personal proclivities, most sociologists, seeing the book for what it is, would nevertheless reject Gouldner's analysis and "...continue to do what they and other scientists have always done, using reason to construct theories and evidence to evaluate them."

This strategy of labeling Gouldner's analysis as ideological (and thus antiscientific)--that is, equating ideology with "antiscience"--is itself dangerously ideological, and, drawn into a shouting match which they could not hope to win, Gouldner's critics were themselves thus opened up to the same charges, e.g. conservatism, being entrenched in the academic establishment, and so

forth, which were originally leveled against Parsons.<sup>8</sup> That is, the ideological critique merely served to play further into the hands of Gouldner and proponents of his work.

Thus, those agreeing with or sympathetic to Gouldner's analysis tended to appropriate the charge of conservatism against Parsons and academic sociology, using this sentiment in turn as a battering ram or a validating force by which emphasis was placed overwhelmingly on this one component of the analysis, in the process largely ignoring such things as Gouldner's historical analysis of the development of sociological thought, or even his call for a reflexive sociology. With the overall importance of Coming Crisis thus illuminated, Young (1971:280), for example, could claim in 1971 that the greatest accomplishment and the most enduring legacy of the book will be that "Gouldner has given us the means to liberate our institutions from the custodians of conservative theory."

In light of the mood of the late sixties/early seventies, it is easy to see why most contemporary supporters of the book chose to latch overwhelmingly onto

---

<sup>8</sup> Since they couldn't win, Gouldner's critics, whom together collectively constituted the bulk of academic sociology, simply walked away from further consideration of Gouldner's analysis after the last round of rejoinders and responses subsided, thereby taking comfort and consolation in the delusion of strength in numbers.

the ideological component of Gouldner's analysis. After all, the charge of academic sociology's conservatism was easily the most irksome point for the majority of Gouldner's critics. But for a more tempered, less urgently felt analysis of the merits and importance of Coming Crisis, it might be useful and instructive to consider how the book has stood the test of time, that is, how it is perceived today, some twenty years after its release.

Several observers (Fuhrman 1984; Miller 1981; Flacks 1990) have suggested that Gouldner contributed substantially to an advancement in the sociology of knowledge perspective which went well beyond Mannheim's (1936) incipient formulation. Especially salient here is Gouldner's critique of Parsons' analytical realism. Remember, Parsons' (1937) conceptual scheme suggested that by admitting into evidence the convergence of thought among four European theorists toward a voluntaristic or normatively-oriented view of human action, this in turn served ipso facto to undermine the validity of the previously dominant utilitarian position. Parsons' strategy, then, was to replace what he saw as an unacceptably naive theory--utilitarianism--with his own new and improved voluntaristic theory of action. However, Gouldner argued that efforts such as Parsons' are unacceptable in their own right, that is, that they are

concerned only with correcting "...a naive, inductivist epistemology and replac[ing] it with an equally naive view of theory which suggests that there is no 'tacit dimension or infrastructure to theory'" (Fuhrman 1984:289).<sup>9</sup> This was, according to Fuhrman (1984) Gouldner's first of three significant problem shifts (i.e., his rejection of transcendence) within the sociology of knowledge.

Although many observers (e.g. Wilner 1985; Jacoby 1987; O'Neill 1989; Buxton 1985; Savage 1981; Alexander 1983) feel that Gouldner's reflexive sociology (or reflexivity in general as an epistemological position) has yet to manifest itself to any degree within sociology's working environs (particularly with regard to theory building), others suggest that for various reasons reflexivity as articulated by Gouldner has been a success.<sup>10</sup> And although some of

---

<sup>9</sup> Although the seeds of a more sophisticated sociology of knowledge analysis were planted in Coming Crisis, Fuhrman (1984) suggests that it was not until his Dialectic of Ideology and Technology that Gouldner's attempt to analyze the social bases of social theory and ideology fully blossomed.

<sup>10</sup> For example, Savage (1981:6-10) suggests that Gouldner's sociology of knowledge (and for that matter, sociology of sociology) critique is flawed in that it conflates two distinct spheres, these being: one, the instruments with which the sociologist operates in going about his or her daily work, i.e. the system of concepts and analytical schemes; and two, the human subjects who operate these tools (concepts) of their trade. Gouldner's mistake, according to Savage, is that he reduces sociology to sociologists. Discourse is thus reduced to a set of

these observers sympathetic to Gouldner's position are still unsure about the extent to which Gouldner's formulation is relevant to contemporary sociology's theoretical and metatheoretical concerns (see especially Flacks 1990 and Hagan and Vaughan 1990), others (e.g. Fuhrman and Snizek 1990; Fuhrman 1990; Johnson 1990; Colvard 1990; Levesque-Lopman 1990) are, for various reasons, quite convinced of Gouldner's contemporary relevance.

This is no more apparent than in the sentiments articulated by a handful of feminist writers. This group (see especially Levesque-Lopman 1990 and Johnson 1990) has suggested (among other things) that contemporary feminist epistemology is compatible with Gouldner's formulation of a reflexive sociology. That is to say, since as Gouldner (1970a:46-9) suggests each of us bring certain background assumptions into our theories, all theories are imbedded with an ideological infrastructure which functions (oftentimes implicitly) as the ontological engine by which

---

social relations, that is, it is little more than a reflex of the social (which, of course, serves to bring in the notion of "reflexivity"). This type of flawed analysis is for Savage (1981:7) analogous to, for example, the attempt to analyze "a lump of coal or a pick-axe in terms of the social and personal experiences of the coal miner." In this light, Alexander (1983) and Buxton (1985) offer similar lines of critique.

all facets of the theoretical enterprise are sustained. Since, in Gouldner's estimation, functionalists have been overly preoccupied with the normative order of society and those (presumably) institutionally legitimated role relationships which contribute to the maintenance of that order, feminists such as Levesque-Lopman (1990) can thereby appropriate Gouldner's general critique of functionalism and read his reflexive sociology as a direct call to reexamine and reevaluate the normative boundaries of gender relationships in particular.<sup>11</sup>

### 2.3 Gouldner's Views

As discussed in section 1.2, this section briefly examines Gouldner's views of the theorists whom he envisions as playing a central role in the coming crisis of sociology. To set the stage for this discussion, it might be instructive to review in slightly greater detail his

---

<sup>11</sup> Of course, Levesque-Lopman admits that nowhere did Gouldner make explicit the role of women and the extent to which they would play a part in the development of such a reflexive sociology. Nonetheless, it is argued that Gouldner paved the way, perhaps even unwittingly, for the emergence of a feminist epistemology, i.e. by Gouldner's own example of critically questioning the ideological assumptions of the male-dominated, conservative academic sociology of his time; this in turn, then, is seen by feminist theorists as compatible with the values inherent in their own project of challenging the contemporary establishment hierarchy.

(1970a:410) own point by point summary. The coming crisis of Western Sociology (and of Academic Sociology in particular) shall, according to Gouldner, manifest itself:

1) by the drift of Parsonian functionalism toward the previously hostile and antithetical Marxist theoretical position;

2) by the emergence of a new wave of sociologists that will find the tenets of Parsonian theory in particular and functionalism in general incompatible with their own newly formed sentiment structure;

3) by the tendency for these newly alienated forces to express collective opposition and rejection of the dominant functionalist paradigm;

4) by the continuing and growing technical criticism of functionalism, both from within and without;

5) by the transition from these negative and critical observations to the active development of positive alternative theories embodying importantly different ideological infrastructures, the foremost of these being the theories of Homans, Goffman, and Garfinkel;

6) by the continuing emphasis on middle-range research which is oriented ostensibly toward the values of "freedom" and "equality," as opposed to functionalism's major preoccupation with "order."

From these observations Gouldner encapsulates the three primary driving forces of the coming crisis, these being:

1) the appearance of new infrastructures directly opposed to that of functionalism, particularly with respect to those closest to the university milieu (i.e., students) within which theory is formulated and transmitted;

2) the fragmenting of functionalism from within, that is, the loss of Parsons' hegemonic domination, due in large part to the growing variability and individuation of the large numbers of students which Parsons himself produced;<sup>12</sup>

3) the development of the welfare state; its new spirit of interventionism and regulation is seen to be at odds with the values of Parsonian "voluntarism" and laissez faire, value-oriented action.

With Gouldner's views of the coming crisis freshly in mind we may now review his discussions of Goffman, Homans, Garfinkel and Parsons so that we may understand more fully

---

<sup>12</sup> Of course, Harold Garfinkel, a former student of Parsons', is a perfect example of this. Merton and Barber's (1976) notion of "sociological ambivalence" comes into play here as well. Although the first generation of students remained fairly loyal to Parsons, with each succeeding generation the ties back to the intellectual mentor become successively weakened, a perfectly appropriate and natural progression by which a unique identity is forged by former students attempting to escape from beneath the shadow of the great teacher.

the ways in which Gouldner sees each theorist contributing, whether positively or negatively, to the coming crisis.

### 2.3.1 On Goffman

Any discussion of Gouldner's alternative thesis must necessarily include an analysis of Goffman's career for the simple reason that Gouldner (1970a:378-390) devotes the bulk of his attention to him. Gouldner explains the importance of dramaturgy, and the reasons for its consonance with the sensibilities of the newly emergent sentiment structure, as resulting from its overt rejection of conventionalized notions of hierarchy (1970a:379).<sup>13</sup> That is to say, Gouldner rightly acknowledges functionalism's--and therefore Parsons' own--convergence with Platonism (1970a:422-423) in that both perspectives stress as their foremost intellectual concerns moral value and social order. As Gouldner states, "Functionalism is the descriptive counterpart of Platonic moralism." Social order comes about, and the necessary hierarchical structuring of

---

<sup>13</sup> Gouldner is suggesting that since Goffman is underemphasizing hierarchy in his sociology this leads him into an inappropriate view of social life as episodic to the extent that actors are disengaged from social structure and thus cut off from sociohistorical understanding of their place in society. This charge of Goffman's ahistoricism is flatly rejected by Rogers (1980), who argues instead that Goffman's theories contain important insights into hierarchy, power, stratification, and status.

society exists, because of the complex patterns of socialization which feed appropriate cultural value-orientations into social actors. Values thus are transmitted in hierarchical fashion from outside the individual; in Plato's time the explanation for such a process was seen as cosmical, for Durkheim it was the collective conscience, and for Parsons, values are transmitted culturally (1970a:423).

Other than this refutation of hierarchy, what does Goffman's vision of society have to offer us that could be perceived as an improvement over Parsons' world view? According to Gouldner, Goffman's epistemological grounding is not in social structure per se, but in the very fundamental process of human interaction. It is thus seen as an "episodic," more fluid analysis of social life, one that is not constrained by highly technical and abstract notions of well-bounded social structures. Goffman's notion attempts to give back to the actor the fundamental power of human agency, disengaged as he is from social structures as such. This opens the door for Goffman's dramaturgical foundation, a social theory anchored within the concept of human actors purposively taking on and being able to choose whatever role is deemed appropriate for achieving the desired end. As Gouldner (1970a:379) suggests, in Goffman's world human actors "...are seen less as products of the

system than as individuals 'working the system' for the enhancement of self."<sup>14</sup>

### 2.3.2 On Homans

Gouldner (1970a:139) suggests that exchange theory as espoused by Homans had its roots in postwar America; specifically, it was an extension of the revisionist social utilitarianism implicit in the works of Robert K. Merton's version of functionalism. In essence, Merton "treated the

---

<sup>14</sup> Although Gouldner spends a good deal of time on Goffman and gives the general impression that his theoretical program is vibrant and one to be reckoned with, i.e. that Goffman is highly regarded within sociology, writers such as Posner (1978) believe just the opposite, i.e. that Goffman is rarely discussed, and that when he is writers provide him only lip service. In responding to Posner's contention Oromaner (1980:287), citing such evidence as book reviews, citation counts, journal articles, and even Gouldner's own treatment of Goffman as discussed here, states in contradistinction that Goffman is in fact widely discussed and highly acclaimed: "I doubt that the work of any other contemporary sociologist, with the exception of Talcott Parsons, has received this much attention in such a variety of academic and academically oriented publications." In her rejoinder, however, Posner (1980:293-4) does not accept the evidence which Oromaner offers, especially with regard to his citing of Gouldner: "...it is clear to anyone that has read Gouldner that he has little understanding of the work of Goffman and many of the so-called new theorists who he attempts to summarize in The Coming Crisis." However, the negative assessments by Posner here and Rogers (1980) (noted earlier) of Gouldner's analysis of Goffman appear to constitute a minority position (which of course does not mean to imply that these positions are wrong).

subjective orientations of persons (the voluntaristic component) in a completely 'secularized' manner."

Homans roots in functionalism, however, are seen to evolve beyond Parsons' concern for the moral implications of the motivational exigencies of social action, as Homans suggests a more deeply rooted substructure upon which to base this morality. Homans was not satisfied with the Parsonian notions of culturally structured social roles and the concomitant types of social institutions which serve to legitimate and sustain them. He believed instead that there exists deeper structures still, structures which do not rest largely or solely upon morality for their continued legitimation (Gouldner 1970a:395). Homans thus shifts the emphasis away from established institutions and its parallel role structures and toward the notion of the marginal utility of gratification derived from voluntaristic, market-playing actors engaged in social exchange. Again, just as with Goffman, we see an emphasis on the disengagement of the individual from perceived overly restrictive social roles and institutions; persons are no longer simply members of a particular society but see themselves as members of a much broader "species."

By discarding Parsons' emphasis on the structural solidarity which roles provide to the social system Homans is confronted with some theoretical tradeoffs, most

important of which is the realization that with this new freedom to peddle one's own affectual wares on the open market comes the awareness of the precariousness of the world and the fragility of the bonds which allow for social interaction.<sup>15</sup> This precariousness, again, derives from Homans' negation or rejection of the very fundamental role which, in Parsons' analytic scheme, institutions play in legitimizing social norms. Homans attempts to describe normative legitimation as deriving from and being interwoven within the intimate, face-to-face interactions

---

<sup>15</sup> Along these same lines, John Rex (1981:43-44) has suggested that traditional functionalism (specifically Parsons' version) has viewed social exchange unproblematically in that it merely represents an aspect of the stable integrated structure of social relations. However, in agreement with Gouldner's analysis, Rex points out that although exchange has its stabilizing mechanisms vis-a-vis the pursuit of self interests within the broader normative, institutionalized framework, Parsons' failure to articulate more explicitly the precariousness of exchange can be understood upon noting that "...exchange is also an unstable, disturbing and dangerous element in the structural-functionalist Utopia [and, consequently] the theorisation of this aspect [of exchange theory's precariousness] is an essential complement to structural-functionalist analysis." Rex believes that Homans has provided an improvement over Parsons' reading of exchange by suggesting that the very process of pursuing social rewards within a matrix of mutual exchange actually conditions actors to pursue acceptable or desirable forms of conduct (rather than relying upon guidelines imposed by some a priori normative structure). However, Rex feels that both Homans' psychologistic and Blau's economic models of exchange still do not go far enough in explaining social conflict because both versions were born within the traditional functionalist perspective whose prime directive has been the attempt to explain social order.

that take place between actors, characterized by the ongoing push-and-pull, bartering, and exchange for social gratifications.

"Bringing Men Back In," then, is Homans' attempt to liberate persons from socially conscripted roles which, in Parsons' formulation, confine actors to a narrow set of institutionally prescribed actions, with the range of these actions being defined by Parsons' pattern variable schema.<sup>16</sup> Homans, like Goffman, is attempting to give back to social actors some fundamental measure of human agency; in Homans' scenario persons are now seen as bartering over the rules of the game rather than playing by an already cast-in-stone set of action guidelines. Again, like

---

<sup>16</sup> Friedrichs (1970:27) traces the beginnings of Homans' "Bringing Men Back In" theme to his 1964 presidential address before the American Sociological Association wherein he repudiated the functionalist position which had dominated the discipline and which he himself had espoused. For Homans, the reaction against functionalism came in the form of Skinnerian behaviorism; the animus of this repudiation was Parsons' insistence that deductive logic is sufficient for the purposes of showing interrelationships between human action and their supporting institutional legitimation. Homans, in contradistinction, felt that descriptions of these relations derived deductively were simply insufficient as causal explanation. In order to explain why social relations are what they are (and not merely what they are) we must return, according to Homans (1964a:225; 1964b), back to the individual as expressed in behavioral psychology, and, in addition, the final crucial variables needed to derive a sufficient explanation of human action based on the hedonics of exchange would likely be found in biology (Friedrichs 1970:41).

Goffman, we see Homans' actor as being not merely a receiver and transmitter of culturally defined roles but as a builder and user of social structures (Gouldner 1970a:396). Homans, although stressing the importance of operant conditioning in the structuring of human action systems, nevertheless attempted to go beyond the behavioristic psychology of his antecedent, B.F. Skinner, as well. Homans' answer to Parsons' conception of morality is, in effect, that actors must get beyond the illusions of morality, that they must get down to the "no nonsense, tough-mindedness" reality of social life wherein persons are in a perpetual give-and-take and actively involved in shaping their world via the exchanging of social gratifications.<sup>17</sup>

### 2.3.3 On Garfinkel

Gouldner suggests that by the 1960s Western sociology had become increasingly polarized due to the polycentrism

---

<sup>17</sup> As is the case with much of Crisis, Gouldner's analysis of Homans seems to agree with both Homans himself and other scholars of Homans' work. Turk and Simpson (1971b:3), for example, concur with Gouldner's observations as expressed here, and they summarize the fundamental differences between Homans (1971), who emphasized that the problem was to understand how institutions arise through interaction; and Parsons (1971), who by contrast suggests that sociology's most enduring problem is to understand how and the extent to which institutions shape interaction.

occurring within both Marxism and academic sociology. This polycentrism became manifest, especially in the case of American sociology, vis-a-vis the emergence of new theoretical projects which challenged those traditional centers of intellectual domination, namely Chicago and Harvard Universities (Gouldner 1970a:159). Amidst this flurry of activity within the American academic milieu rose even more specific challenges to the Harvard wing and to Parsonian functionalism in particular. These challenges were articulated most successfully, as I have already pointed out, by George Homans and Erving Goffman, and, as I elaborate more fully here, by Harold Garfinkel.

Like Parsons, one of Garfinkel's primary concerns is the explication of how social order is possible. Unlike Parsons, however, Garfinkel attempts to avoid the notion that the social world is held together by actors consciously working to make their actions conform under the auspices of a morality tinged with the sacred, as if there is some a priori notion of duty or moral constraint by which action is guided. Garfinkel chose instead to concentrate on those tacit assumptions which people live by in their everyday life, those "...ordinarily unutterable...rules and knowledge that make stable social interaction possible"(Gouldner 1970a:390).

This project, like Homans' notion of "bringing men back in," attempts to emphasize the importance of the average person as he goes about the daily routine of his life. The understanding of social life is to be found in the mundane, in the everyday commonality of living, rather than in "critical events or dramatic public incidents." By getting back to a fundamental understanding of the routine nature of life (as opposed to working from some overblown, highly technical theoretical vantage point wherein sociologists lose touch with their own humanity, with their own routineness), sociologists can return to the task of formulating common sense perceptions of the world around them. This then is a return to viewing social life from the "inside," the project of communicating the true, more mundane, routine sense of things from a very fundamentally human vantage point. Following in Nietzsche's footsteps, Garfinkel is displaying an hostility for those obtuse, theoretically abstract projects (such as Parsons') which have, for all their technical sophistication and serpentine language, seemingly allowed the objects of their study--people--to quietly slip out of the picture (Gouldner 1970a:391).

This return to a common sense conception of the world is seen by Garfinkel as attainable via the ethnography of a culture's folkways and the ways in which shared meanings

find expression in the face of societal dissolution. Like Goffman, for Garfinkel the social bonds which hold people together in a societal community are rather fragile; the foundation upon which these bonds rest is the "taken-for-grantedness" of a culture's folkways. In effect, the system of tacit rules operating within any culture provides the foundation for social order. Ethnomethodology is the process or technique by which these tacit rules may be unearthed or discovered. The system of tacit rules, however, does not itself constitute a "structure" in the Parsonian sense; rather, there is a sense that each culture survives on rules and procedures which can be better described as being commonsensical and even arbitrary (Gouldner 1970a:392).

But unlike Goffman, the important aspects of Garfinkel's world are not of appearance and the senses of perception but of the mundane, the taken-for-granted, the "practically invisible." Garfinkel's task, then, is to strip the culture of its taken-for-grantedness in an effort to expose the truly fundamental components of human action and social solidarity. But Garfinkel's excavation of the social world is not confined to some presupposed notion of reality as espoused in any particular extant theoretical tradition, nor is it intended to isolate only certain segments of the cultural tradition in an effort to display to its denizens

the paucity, and therefore the untenability of, say, morality. Garfinkel's theoretical brush attempts to make a much broader sweep of the social landscape; his attack, in fact, is upon the common sense of reality (Gouldner 1970a:393). The world thus becomes its own laboratory, and trained ethnographers are sent out into the field in an effort to confront, in their own setting, common folks' notions of the world around them, to play the foil in an attempt to shake up the unquestioned perceptions of reality which each of us hold.

Through these demonstrations of challenges to the "ordinary world" Garfinkel can claim some startling results. For example, a typical strategy for uncovering the tacit conventions of everyday interaction could entail a researcher answering the routine, innocuous question "How's it going?" or "What's up?" with "What do you mean, 'How's it going?'" This challenge to a very fundamental, generally taken-for-granted component of interaction appears at first glance to be nothing more than a juvenile prank, described as merely "harmless fun." But, as Garfinkel (1967:43-48) suggests, after awhile this "harmless fun" begins wearing thin, and the "victims" of these demonstrations become "nervous and jittery...[and] irritation and exasperated anger [generally ensue]."

The exasperation and pained responses of normal persons to challenges of their conception of social reality and the violation of a sacred trust imposed during the ethnomethodological demonstrations confirms, and is evidence enough for Garfinkel of, the existence of a set of tacit rules which govern social relations (Gouldner 1970a:393). In the final analysis Garfinkel's strategy is to bring into question the unchallenged notion of reality which most of us share. It is a poorly disguised effort to challenge the dominant ideology and values perpetuated by the status quo. For Gouldner, Garfinkel's ethnomethodology represents the type of challenge to the legitimacy of the status quo that would be welcomed in the new era of sociological skepticism and ambivalence. The sentiment structure of the ethnomethodological project (or rather, the domain assumptions which representatives of this perspective hold), then, is seen as resonating well with the new, up and coming wave of sociological practitioners.<sup>18</sup>

---

<sup>18</sup> Although most observers find few faults in Gouldner's treatment of Homans and Goffman, this is not the case with Garfinkel. For example, particularly galling to O'Neill (1972:171-172) is Gouldner's whimsical view of the methodological techniques employed by ethnomethodologists in eliciting those tacit, hidden underpinnings of social action. O'Neill suggests that Gouldner's comprehension of Garfinkel falls short because of his lack of reflection upon a key element in Garfinkel's thought, namely Shutzian phenomenology. This, then, leads to Gouldner's naive

#### 2.3.4 On Parsons

Because Talcott Parsons plays such a central role in Gouldner's crisis thesis, I shall be able to outline here only the major points of his analysis which are pertinent to this investigation.

---

reading of Garfinkel, "...whom he imagines to be something of a street artist like Jerry Rubin." On the other hand, Berger (1970:277) defends Gouldner's less than enthusiastic reading of Garfinkel by pointing out that Garfinkel's ethnomethodology never seriously addresses the question of power in social relationships or how, for instance, any one definition of social reality becomes accepted over another. It should also be noted that, although Gouldner looks to these three (Homans, Goffman and Garfinkel) theorists as providing challenges to functionalism, they each share (with functionalism) one major fault: their theories are, in the final analysis, ahistorical (Gouldner 1970a:385, 391, 395). This is unacceptable to Gouldner because, as Swanson (1971:328) has pointed out, history (for Gouldner) is the source of moral values. Although Gouldner did not make explicit in *Crisis* the importance of history in the methodology and implementation of his reflexive sociology, he certainly elaborates on its role in several follow-up articles. See especially Gouldner's (1972:15) discussion of hermeneutics, a form of investigation which offers commentary on the meanings of human events and arrangements; this approach stresses interpretation over the formulation of scientific laws. Consequently, "from this standpoint the theorist is regarded as being more like an art or architectural critic than a physicist." Liberated from the chains of an ersatz methodology, the sociologist, through a heightened sense of self-awareness, is able to carry out such a reflexive sociology. Hermeneutics, then, is the tool for discovering the kind of moral code which could provide an appropriate infrastructure for sociology (and for society in general), one which would ultimately bring men and women into a rational theoretical discourse free from false consciousness (Gouldner 1972:13).

In order for Gouldner to proceed with his analysis he must first document the ways in which one's social milieu intimately affects and shapes one's social theories. Invoking images from C. Wright Mills, Gouldner (1970a:147) states that, aside from simply examining a theorist's theories,

...we must also examine the concrete manner in which history and culture intersect with individual biography. In short, we must come closer to the individuated way in which culture becomes embedded in personal reality and influences theory.

Thus Gouldner notes that because Parsons was born in 1902 and came of age professionally during the Great Depression, his earliest writings--from 1928 through the mid-30s--reflected the twin concerns of 1) formulating an American response to Marxism, especially in light of the communist Bolshevik revolution of 1918, World War I and so forth, and 2) shoring up capitalism and the American system in general, especially for those who would perceive the depression as a sign of system weakness or failure.

With the release in 1937 of The Structure of Social Action Parsons had formulated a "voluntaristic theory of action" which stressed that individuals, through hard work and an appropriately internalized Protestant ethic, could succeed through sheer tenacity even in the face of a temporarily distressed economic system. Parsons accomplished this voluntaristic thesis by carefully

choosing four European thinkers whom he claimed had converged, independently of one another, upon a normative, nonrational (as opposed to utilitarian) action schema. From Max Weber Parsons borrowed German idealism, especially with regard to notions of inner-directed, value-oriented action; from Emile Durkheim he borrowed the French functionalist tradition which stressed the importance of shared norms and values in maintaining social order; Parsons also found the work of Alfred Marshall, formerly a classical economist working within the utilitarian doctrine, important because he had broken away from this tradition to emphasize instead the importance of "wants" in economic (thus human) activity. That is, the utilitarian tradition operates from a perspective which views ends of action as random and which sees individuals seeking out on an individualistic basis those gratifications which in sum produce for society "the greatest good for the greatest number of people." But ends of action are not random, stresses Parsons; individuals internalize conceptions of appropriate means for achieving specific, culturally defined societal goals. Rather than a strictly utilitarian, rationally-oriented action scheme, individuals orient their action along normative, value-laden vectors; for Marshall the breakthrough which brought him to this realization (that is, from the economics end) was the formulation of marginal

utility which solved the value problem inherent in utilitarian demand side theory (Parsons 1937:130)<sup>19</sup>; and from Vilfredo Pareto Parsons borrowed a well-developed social system scheme wherein logical (or economic) elements of action were distinguished from nonlogical (value) elements; this value element coincided with Marshall's conception of "values" in economic activity (1937:300). Finally, Pareto had noted the instability of the utilitarian scheme as well, suggesting that it tended to break down toward a "radical positivistic" direction (1937:299).

Gouldner goes on to suggest that the individualism inherent in Parsons' earlier voluntaristic scheme served him well for the period in which he was writing, but that eventually Parsons would have to adjust his scheme to keep pace with the shift in American governmental policies, specifically with regard to the move from laissez-faire individualism toward the more collectively-oriented

---

<sup>19</sup> It is interesting to note that before the publication of The Structure of Social Action Parsons was prepared (during the 1934-35 academic year) to substitute Hobhouse for Marshall in the early formulation of his "Marshall-Pareto-Weber" convergence thesis (Parsons 1970). However, as history has shown, Parsons eventually relented, recasting Marshall back into the convergence thesis, adding Durkheim, while eliminating Hobhouse along with several other prominent theorists under consideration, most notably Simmel and Znaniecki (Camic 1989:59).

policies of the New Deal and its concomitant welfare legislation (Heritage 1971:455). The demands for applied, policy-oriented social science research, the need to understand more fully such things as the types of dysfunctional tendencies produced by social systems (in order to deal with the indigent, the mentally-ill, and the criminally-prone, for example); all of these factors sent Parsons scurrying toward an embrace of Freud in order to unlock the mechanisms by which socialization produces motivational capacities in social actors ("ego" and "alter") and to help specify those dysfunctional elements which occasionally creep into the socialization process (and beyond) which produce (or contribute to) various forms of deviance within social systems (Parsons 1951; Parsons and Shils 1951). Above all, Parsons was challenged with specifying the boundaries of and the interpenetrations between the various subsystems (cultural, personality, and social) in order to lay out an appropriate grid or mapping of the social environment. This entailed additionally the elaboration of an entirely new technical language (which was the animus of much of Mills's [1959] critique), for Parsons felt that there was no extant general theory sufficient to the task of explaining voluntaristic social action of individuals within the constraining institutional matrix of the social system.

### III. METHODOLOGY

#### 3.1 What Is Influence?

Insofar as this investigation is concerned and as it pertains to scientific activity, "influence" is the ability of a scientist or a group of scientists (headed ostensibly by one or several central figures) working in a particular subfield to extend their own methodological, theoretical, or empirical programs into positions of prominence, either through information dissemination or transfer (largely via academic journals), or as represented by students trained by the scientist for purposes of carrying on the research agenda, within the discipline at large. For example, as Gouldner (1970a) has discussed, Talcott Parsons, the leading figure of the structural-functional school within the discipline of sociology, was able to sustain a lengthy and dominant theoretical program which impacted not only those researchers within his own immediate academic milieu, but also extended into the various schools of sociology in general. He did this through his many journal articles and monographs--made possible in great part thanks to the cooperation of a major publishing house, the Free Press, of

which Parsons had nearly free and routine access to (Parsons 1970)--through his institutional affiliation at Harvard which allowed him the luxury of attracting the best and brightest students in the field. Another advantage of his Harvard affiliation was that through the connections which such an elite institution provides Parsons was in constant contact with other prominent researchers outside of his immediate research group; this then allowed for collaborations which otherwise might not have been possible.<sup>1</sup> Gouldner (1970a:168) summarizes these observations by suggesting that

...[I]t is Parsons who, more than any other contemporary social theorist, has influenced and captured the attention of academic sociologists, and not only in the United States but throughout the world. It is Parsons who has provided the focus of theoretical discussion for three decades now, for those opposing him no less than for his adherents.

Consistent with several other dimensions of influence

---

<sup>1</sup> As will be discussed later, after years of frustration at Harvard Parsons finally succeeded his nemesis, Pitirim Sorokin, and by 1945 his dream of a multi-disciplinary department, centered of course on Parsons' lifelong intellectual pursuit of a general theory of action, came to pass. With the Parsonian system still a very much open and unfinished project, the promise of contributing to the completion of such an all-encompassing theory attracted a great deal of attention, both in terms of garnering the interests and inputs of various colleagues as well as a full-blown migration of several generation of students (Gouldner 1970a). On a personal level, Parsons was receptive and attentive toward the enlisting of new followers, and as a result a clearly defined intellectual tradition rapidly emerged (Johnston 1986).

mentioned above, Gouldner (Ibid.) goes on to suggest how Parsons' influence has manifested itself beyond the realm of his written work:

Parsons' influence has exerted itself not only through his own prolific writings but also through his students, particularly Robert Merton, Kingsley Davis, Robin Williams, Wilbert Moore, as well as more recent students. They have been important because of their intellectual work, as well as their dominant roles as officers of the American Sociological Association and as editors of its journals....More than any other modern academic sociologist of any nationality, Parsons is a world figure.

As Merton's "Matthew Effect" suggests, influence is a phenomena isolated largely within a relatively small group of scientific elites, that is, that group of individuals which for whatever reasons has been able to sustain long and successful theoretical programs which retain the unique flavor and bear the stamp of the author. Although this conception of influence might lead some persons to suggest that the formulation provided here reflects a definitive elitist bias, it should be noted that the four theorists that Gouldner discusses, namely Parsons, Goffman, Homans and Garfinkel, are themselves examples of that rarified pinnacle of attainment which only a select few scientific elites are able to achieve. In this sense, they follow similar patterns of scientific elitism which Zuckerman (1977) reported on in her study of Nobel laureates (this latter class of individuals being, of course, the ultra-

elite of science). This stratification in science--wherein only a very few rule the scientific roost while the remainder go virtually unnoticed--has been reported on and confirmed by a number of writers (e.g. Zuckerman 1977; Merton 1968, 1988; Cole and Cole 1972, 1973, 1987; Crane 1972; Hagstrom 1965).

The similar pattern of elite attainment which I alluded to above can be seen when one considers, for example, that Parsons, Homans, Garfinkel and Goffman were all connected with elite institutions during the crucial early periods of their academic training; these institutional connections and the concomitant opportunities entailed therein contributed in turn to the highly successful theoretical and research programs which each was able to forge.<sup>2</sup> Regardless of the validity (or lack thereof) of any possible charge of elitism, this study's purpose is first and foremost to stay true to Gouldner's own discussion; my conception of influence, therefore, is merely a reflection

---

<sup>2</sup> Specifically, Goffman, trained at Chicago, went on to teach at Berkeley and Penn; Garfinkel, trained at Harvard, went on to teach at UCLA; Homans was trained and remained for his professional career at Harvard; and Parsons, trained at Amherst, the London School of Economics, and Heidelberg, eventually spent the entirety of his academic career at Harvard University (Mullins 1973).

of that offered up by Gouldner.<sup>3</sup>

For purposes of this study, in light of the multidimensional nature of influence, we could theoretically have at our disposal a variety of alternatives for speaking of or measuring influence. For example, we might decide to look at how many students of each theorist went on to take prominent positions within the discipline in their own rights; connected with this, we might investigate which representatives of which theoretical traditions went on to take important official positions within the discipline (such as, for example, ASA president or editor of a prominent sociological journal). However, it seems that the simplest, most straightforward approach would be a citation analysis of the major academic journals within sociology; these totals should give a good indication of how each theorist's work has impacted on the discipline at the crucial area of the research front.<sup>4</sup>

---

<sup>3</sup> It should also be kept in mind that Gouldner himself was contained within this small group of sociological elites, having studied under Merton at Columbia during his master's and Ph.D. studies, then going on to spend the bulk of his career at Washington University in St. Louis. See Merton (1982, 1987) for more on the circumstances which brought Gouldner to Columbia and the factors which shaped his own research interests.

<sup>4</sup> What this means is that, as Line (1979) has pointed out, journals appear to be cited nearer the frontiers of knowledge than any other type of publication (e.g. monographs, magazines, etc.). As Line (1979:273) states,

Now that we have discussed what, for purposes of this study, influence is (that being scientific productivity as measured by citation analysis), we must briefly state what it is not. For this we can turn to Wiley's (1979) discussion of the rise and fall of dominating theories in American sociology and the multidimensionality of intellectual influence as briefly alluded to above. Wiley (1979:48) suggests that for any theory to become dominant or influential within a discipline it must fulfill three conditions: (1) the ideas themselves must be clearly elucidated and represented by a central charismatic figure who has the ability to push forward the concept and make it appear viable and meritorious amongst a group of peers; (2) flowing from condition (1), the ideas must be organized around a theory group and in addition must be perceived as desirable by the next generation of students who ideally serve to effectively carry on the original work while providing important modifications and extensions to the theory as current needs and trends dictate; (3) for the idea to grow and be disseminated among the widest possible

---

"Most journal articles deal with one topic, often a narrow one, and often of current interest; they will therefore tend to refer to recent material." Additionally, as King et al. (1981:200) report, scientific journals are used "...as a current awareness mechanism and as a permanent record of scientific accomplishment."

audience the theory group must have access to or control of the means of intellectual production (e.g., professional jobs, graduate students, access to journal publication or publishing houses, and money for research).

Thus, our measure of influence, which will be discussed more fully below, will concentrate on the ideas themselves, that is, Wiley's condition (1), as represented by scientific productivity. Continuing on, our notion of influence will not greatly entail condition (2) because we are not here concerned with the manifestations of entire theory groups, rather, we are skimming from the surface the most elite spokespersons from four theory groups, namely functionalism, dramaturgy, exchange theory, and ethnomethodology. Finally, from condition (3) we are interested in certain means of production, i.e. certain high impact sociological journals, but we will not be directly concerned with e.g. monographs, the extent of access which the various theory groups have to publishing houses, professional jobs or appointments (e.g. ASA president or journal editor<sup>5</sup>), and the number of students

---

<sup>5</sup> This component of influence as seen in the number of editorial positions members of a particular school have attained is an interesting one, especially when one considers the findings of Sievert and Haughwout (1989). In short, the authors have found that since editors and editorial boards of journals serve as gatekeepers in their respective discipline they can directly influence the types of citation patterns exhibited by the journal. However,

each perspective has produced. Although some of these factors will be dealt with indirectly, they will not be treated in the sense proposed by Wiley.<sup>6</sup>

### 3.2 Measuring Influence

The discussion to this point has been an attempt to lay a theoretical foundation for the justification of the methodological approach, namely, a citation analysis of Talcott Parsons in order to describe his relative levels of influence within the discipline of sociology. By extending such a citation analysis to include the works of Homans, Goffman and Garfinkel as well we will be in a position to test empirically Gouldner's assertions that a) Parsons' theoretical program played a central, dominant role within the discipline of sociology from WWII through the early

---

because of the limitations of this study, i.e. lack of space, no systematic treatment of possible editor effects on citation patterns can be presented here, (although this area of concern will be addressed in peripheral fashion as the investigation progresses).

<sup>6</sup> For example, although we aren't interested per se in the number of students each perspective can claim to have produced, obviously since our measure of influence depends on citation analysis this gives at least an indirect indication of the strength of student ties back to the teacher. That is, cocitation analysis (e.g. Bayer, Smart and McLaughlin 1990; Merton 1988; Lawani and Bayer 1983; Aversa 1985; Oromaner 1977, 1981) shows strong correlations between a citing author and the theory group affiliation of the cited author.

seventies<sup>7</sup>; b) by the late sixties/early seventies there began a noticeable decline in the relative levels of Parsons' influence, and, finally, c) concomitant to the decline of the Parsons project there would arise within the discipline a greater theoretical "polycentrism," and that among the various intellectual projects competing for prominence those authored by Homans, Garfinkel and Goffman appeared primed, for reasons mentioned above, to emerge as leaders.<sup>8</sup>

My working hypothesis is that Gouldner's coming crisis thesis, as reflected in the above three propositions, is accurate. That is, I do expect to find a) that the rate at

<sup>7</sup> Even though Gouldner suggests that Parsons' influence could be felt beginning in and extending as far back as World War II, this investigation's measurement of influence begins only in 1960. However, we may corroborate Gouldner's contention by looking to Mullins (1973) and especially to Oromaner's (1979) work in which he traces Parsons' influence vis-a-vis citation analysis covering the period 1936-50. Oromaner does indeed find evidence to support the widely-held contention of Parsons' lofty position within sociology.

<sup>8</sup> Gouldner (1970a:410) is nowhere more explicit about this than in his discussion of the six components of sociology's coming crisis. Although I have already touched on most of these major points, the important thing to note here is Gouldner's statement that relatively sight unseen and bubbling just under the surface during the period of growing technical criticism of Parsons' functional theory there arose the formulation and elaboration of certain, more substantial alternative theoretical projects "...embodying importantly different sentiments and assumptions, such as Goffman's, Garfinkel's, and Homans'."

which Parsons' works were cited over the period in question will coincide with relatively high levels of influence; b) that a noticeable drop in Parsons' influence will become apparent at some point (presumably between 1960-67); and c) that relative to the decline in Parsons' influence there will be a concomitant rise in the influence levels of Homans, Goffman and Garfinkel.<sup>9</sup>

As I have already briefly touched upon in an earlier section, it has been decided that the most appropriate procedure for measuring intellectual "influence" within the scientific community is to look at the ways in which authors pay their respects to and acknowledge the works of others in their own writings. The simplest, most effective way of garnering these type of data is by examining the

---

<sup>9</sup> Of course, it would surely be wrong to hold Gouldner to the rigid expectation that all three theorists would or should exhibit dramatic improvement in their levels of influence. More than anything, this component of the test will hopefully ascertain exactly which projects lived up to Gouldner's prognostications and which ones achieved only modest gains. For example, it is my suspicion that, of the three theorists, George Homans will probably enjoy the least substantial gain over time in our measure of influence. This is because of the selection bias imposed in the early stages of this investigation when the decision was made to not include Peter Blau's version of exchange theory. However, we must also keep in mind that at the time Gouldner was writing Homans was much more representative of the exchange perspective and had published a far greater number of works than had Blau. In this sense, then, and for purposes of staying true to Gouldner's thesis, the selection of Homans over Blau appears vindicated.

material efforts of scientists, these being the articles and books which are produced in each respective field. The citations to other scientists' work contained within these documents represent the most objective, reliable public record of this admittedly subjective, somewhat nebulous notion of "influence" (Cronin 1981:17). In other words, our operationalization of influence is simply the count of the actual number of citations that a particular author's corpus of work receives over a specified period of time. As Gilbert (1977:114) has stated, "...the number of citations obtained by an author has been used to measure the impact of his or her work on the scientific community."

Much evidence can be marshalled as to the validity of using citation analysis for measuring "influence." As Ziman (1968:25) states, "Being a successful scientist is not just winning prizes; it is having others cite your work." Similar sentiments can be found in Bayer and Folger (1966), Lawani and Bayer (1983), Bain (1962), Stigler (1987), Porter (1977), Gilbert (1977), Smith (1981), and Cole and Cole (1968, 1972). Citation analysis as a valid measure of influence is therefore judged as appropriate here even in light of some of the criticisms of the technique raised by, among others, MacRoberts and MacRoberts (1986, 1987). They raise many issues concerning the potential drawbacks of citation analysis as a measure of influence, such as e.g.

self-citing and citing review papers. Some of their suggestions will be taken to heart; for example, neither self-citations nor citations taken from regular book reviews will be included in my measure of influence.<sup>10</sup>

Another problem to consider when attempting to measure influence by way of citation analysis is that the process

---

<sup>10</sup> Linda Smith (1981) has provided an excellent summary of the limitations and possible misuses of citation analysis. These are, briefly, 1) although citation of a document implies use of that document by the citing author this is not always the case. For example, content analysis of citations uncovers the fact that many times the cited document is not thoroughly read or simply misapplied by the citing author; this then can lead to various "sins of omission and commission"(1981:87); 2) the assumption that the citation of a document reflects the quality or significance of that document can be erroneous in that documents can be cited for reasons irrelevant to their perceived quality or merit (although it should be noted, contrary to this assertion, that Bayer and Folger [1966:381] report that measures of influence derived from citation counts do indeed have high face validity); 3) it is not always the case that citations are made to the best available works; and 4) all citations are not equal (which has led to the formulation of weightings and citation typologies by various writers [e.g., Cole and Cole 1973; Moravcsik and Murugesan 1975; Bain 1962; Swatos 1980; Chubin and Moitra 1975] in an effort to refine the analysis). But even with the documentation of these limitations, however, Cole and Cole have offered a robust reply, one which resonates well with this author and which also provides further justification for the methodological approach employed here: "This argument [that citations are not a valid or true measure of scientific influence] has been made ad infinitum and although it is true that citations remains far from an ideal indicator of quality of work or influence, the measure has proven to be of great use to sociologists of science and bibliometricians. No better indicator has been discovered"(Cole and Cole 1987:345, my emphasis).

by which authors decide to cite a work is ultimately a private one. That is, the subjective nature of the act of citing means that attempts to impute some concrete meaning onto these cites are largely conjecture. As we shall see, various methods and typologies have been devised in an attempt to work toward a more generalized "theory of citing" (Cronin 1981).

As I have already summarized above, we will be considering not only Parsons but Homans, Garfinkel and Goffman as well. Each theorist's rate of citation, covering the period 1960-89, will be recorded from three major core sociological journals (which will be explained more fully below). Although an on-line citation analysis was available through the Social Science Citation Index (SSCI), it was felt that a manual analysis would better serve the objectives of this investigation. Many writers have suggested that it is not enough simply to make note of the raw, absolute number of citations which each theorist receives (e.g., Moravcsik 1973; Moravcsik and Murugesan 1975); rather, it is suggested, we must conduct a content analysis of these cites as well, noting such things as the centrality (versus the peripherality) of each cite, the reason behind the utilization of the cite, and the tone of

the cite (whether positive, negative or neutral).<sup>11</sup>

Chubin and Moitra (1975) have reviewed the literature concerning the content analysis of references and have constructed a preliminary citation typology (which should ideally facilitate statistical analysis of such data). However, I feel that at this point none of the typologies presented are sufficient as a basis for quantitative analysis.<sup>12</sup> Other authors have suggested that an

<sup>11</sup> These categories correspond somewhat to Moravcsik and Murugesan's (1975:88) reference typology. The four dimensions of the typology--1) conceptual or operational, 2) organic or perfunctory, 3) evolutionary or juxtapositional, and 4) confirmative or negational (with each dimension having a "neither" component as well)--represent both the connectedness of the literature to the cite and also how the cite was used by the author (Chubin and Moitra 1975:424).

<sup>12</sup> The typologies provided by Chubin and Moitra (1975) and Moravcsik and Murugesan (1975) are steps in the right direction, but as Cronin (1981:19) has suggested, "...the experiences of both these teams seem to suggest that a single reference typology may be unrealistic." Attempts by others (such as Swatos 1980) to provide weights (for example, by assigning a negative cite a score of "0", a neutral cite "1", and a positive cite "2", then running statistics on these weighted totals) is also deemed inadequate. Therefore, in order to stay within the scope of this investigation, the extent of my qualitative analysis will involve only the documenting of the names of each citing author and the peripherality versus centrality of the cite in relation to the totality of the citing author's work. Obviously, because of the vast numbers of names of citing authors which will surely be produced over the course of the thirty years which the investigation entails, no formal presentation of this data (for example, as would be represented in tables or graphs) will be presented. Rather, I will mention, where appropriate, certain citing author's names and perhaps their theory group affiliation

appropriate statistical analysis<sup>13</sup> could be conducted on the raw citation counts for each theorist, thereby providing a further content analysis of the citations. However, as explained in the above note, since no formal content analysis will be provided in this investigation I will be able to offer only selected qualitative analyses of the citations by connecting certain citing authors and their paradigm affiliation to the works they are citing.

### 3.3 Citation Analysis

In reviewing the literature concerning the utilization of citation analysis within the field of sociology one finds that introductory sociology textbooks have been a particularly popular source for measuring intellectual influence or impact (Bain 1962; Swatos, Jr. 1980; Oromaner 1969). Swatos's findings--that as of 1974 Marx made a

---

during the discussions of the interpretation of the data in chapters V and the conclusions in chapter VI.

<sup>13</sup> I realize that "appropriate statistical analysis" as used here is very much a loaded phrase. In fact, no formal statistical analysis will be presented. Rather, it is felt that a presentation of a graph or series of graphs comparing each theorist's total citations (the Y-axis) over time from 1960-89 (the X-axis), will be sufficiently compelling and intuitive in and of itself to illuminate a visual representation of the test of Gouldner's thesis, thus eliminating the need for further statistical testing or verification.

surprisingly strong showing (having never appeared before that time) while Parsons "disappeared" from the list of top ten most cited authors--would seem to give credence to Gouldner's thesis that there indeed has been a major diminution of Parsons' influence over the period in question. However, we must approach these findings cautiously; as Oromaner (1969:334) has pointed out, sociologists write for different academic audiences: undergraduate students, graduate students, and colleagues, and that "...the functions of the literature produced for these audiences are likely to be different." Thus we suggest here that for our purposes "influence" means the types of cognitive activity/productivity directed towards primarily graduate students and colleagues. It may very well be the case that Parsons, because of the difficult nature of his theoretical program, is not suitable for elaboration or even representative presentation in introductory sociology textbooks.

The best sources, then, for measuring influence across the discipline are sociological journals (Brown and Gilmartin 1969). I take as my major theoretical model the work of Oromaner (1979). Here Oromaner conducted a citation analysis of Parsons' work from 1936-1950 for the purpose of judging his relative influence on sociology over this period. His data include citations of Parsons' work from

articles published in four major journals, American Journal of Sociology (AJS), the American Sociological Review (ASR), Social Forces (SF), and Sociology and Social Research.<sup>14</sup>

These journals were chosen because they are general in nature and not representative of any one particular sociological subspecialty. They are also regarded as being highly influential and widely-read journals: "...the AJS and the ASR are and have been the most prominent and influential journals in American sociology" (Oromaner 1979:77); and "The ASR and AJS are the two most prominent core American sociological journals" (Oromaner 1985:4).<sup>15</sup>

We choose for our investigation only AJS, ASR and Social Psychology Quarterly,<sup>16</sup> which we feel are sufficiently

<sup>14</sup> Oromaner included the journal Sociology and Social Research in his study because it is representative of the Western region (University of Southern California), which he felt was crucial to include in light of the fact the other journals together represented, at least in his estimation, an eastern and southern bias.

<sup>15</sup> Oromaner's justification for including Social Forces is simply that it is representative of the southern region and would thereby serve to balance the "Eastern establishment" bias of the two big macro-oriented journals, AJS and ASR. My own choice for attempting to reconcile the AJS/ASR macro-bias is by including the micro-oriented, ASA publication Social Psychology Quarterly.

<sup>16</sup> The Journal Social Psychology Quarterly was originally titled Sociometry. In 1978, Sociometry changed its name to Social Psychology. Finally, Social Psychology changed its name once again in 1979 to the current Social Psychology Quarterly.

representative of core sociological journals.<sup>17</sup> It seems that not much would be served in extending the scope of the analysis beyond these three journals; as Price (1986:257) has pointed out, in terms of the Science Citation Index (SCI), approximately 75 percent of all cited literature within a discipline can be derived from only 1/15 of that discipline's most influential source papers.

By focusing on only these three most influential and widely-read sociological journals we are then afforded the opportunity of gauging with a good degree of certainty the

---

<sup>17</sup> Eugene Garfield, the originating force behind (and currently editor-in-chief of) the Social Science Citation Index, has come up with a measure of how relatively frequently articles from particular journals are being cited in the journal universe at large. As the definition suggests, the journal "impact factor" is a "...measure of the frequency with which the 'average article' in a journal has been cited in a particular year" (Garfield 1989). When we look at SSCI Journal Citation Reports (which were made available beginning in 1981) we see that among sociology journals both AJS and ASR finished among the top three ranked by impact factor every year since 1981. In terms of social psychology journals, Social Psychology Quarterly finished in no lower than sixth place in impact factor rank, finishing as high as third in 1986 and 1988. One must keep in mind, however, that of the few journals finishing ahead of SPQ, all can be considered to be psychological journals (or are actually published by the American Psychological Association). Thus, among social psychology journals, SPQ is consistently the highest ranked journal published by the American Sociological Association. Besides their high impact factors, these particular journals have been chosen because they cover the gamut of research and theoretical interests within the discipline; that is, AJS and ASR are macro-oriented, and SPQ is micro-oriented.

extent of and change in Parsons' (as well as Garfinkel, Homans and Goffman's) influence. Absolute citation totals from year to year (possibly five year intervals would be more ideal) will then be compared to test whether there has been any significant change or visible trends from one period to the next. According to Gouldner's hypothesis, we would expect to see a decline in Parsons' influence beginning at some point in the sixties.<sup>18</sup>

Another component of the alleged decline of Parsons' influence is the sentiment that indeed there had been a decline in his influence, but that beginning in the early eighties (which coincides with his death in 1979) there was a revival of interest in Parsonian theory (Alexander 1983; Sciulli and Gerstein 1985; Jacoby 1987; Rocher 1975; Bryant 1983; Munch 1987; Luhmann 1982; Bourricaud 1981). Thus, it will be a task of this investigation to pay particular

---

<sup>18</sup> Huaco (1986) suggests that Parsons' decline began in 1971, paralleling the loss of America's world-wide ideological hegemony which was due in large part to the economic decline which began in the 1960s. Huaco, then, sees the question of Parsons' relative influence on the discipline as largely ideological in nature, rather than as a result of the actual substantive content of Parsons' theories. For more elaboration on the declining fortunes of the West during this period and the ways in which these larger sociohistorical events could ramify on intellectual pursuits (and theoretical agendas) see Dahrendorf's (1988:118-140) superb chapter six, "Crisis in the 1970s."

attention to this period of time as well.<sup>19</sup>

Finally, the operationalization of "influence" is central to and ties back into the larger theoretical discussion because Gouldner banks heavily on Parsons' domination of sociology from WWII through the seventies and how certain sociohistorical trends and events conspired to bring this about. Ultimately, the struggle to refine this concept of influence ramifies into important sociology of knowledge questions and contemplations, particularly those that offer models which explain how various schools of thought within intellectual disciplines find sustenance and continue to flourish (and also, at least as importantly, the types of conditions which bring about their decline or downfall). In the final analysis, then, this investigation

---

<sup>19</sup> Hans Adriaansens (1989:613), a Dutch sociologist who has spent a good deal of his professional life studying the works and career of Talcott Parsons, states that "During the 1970s and the greater part of the 1980s not many sociologists, certainly not in Holland, were willing to reopen the already closed book of their Parsons interpretations." This seems to be further testimonial to the accuracy of Gouldner's prediction of the coming entropy of and loss of interest in Parsonian theory. But, as Adriaansens continues, around the time of Parsons' death it appeared that a major reinterpretive effort was underway, led by the works of such theorists as Habermas, Luhmann, Alexander, and Munch. Robertson and Turner (1989:554) report that, especially in the eyes of many European theorists (and specifically Richard Munch), the singular event which may have actually sparked the revival of interest in Parsons' work was the 1976 release of the Loubser et al. two volume Festschrift to Parsons, Explorations in General Theory in Social Science.

takes on the task of analyzing the plausibility of the explanatory model which Gouldner has offered. The most astonishing aspect of this study is how, upon reading this book, one becomes witness to antithetical interpretations of seemingly benign, innocuous intellectual endeavors. An example which vividly illustrates this is Parsons' notion of "analytical realism," that is, that the observation of a convergence of thought between four distinct European thinkers was evidence enough (for Parsons) of a prima facie truth about the fundamental nature of human social action (Parsons 1937). One might easily accept this formulation as being a legitimate intellectual enterprise in and of itself, with little or no thought given to its ideological implications.

However, Gouldner's view of "analytical realism" is much more problematical, because he sees Parsons' formulation as an overt, continuing attempt to professionalize the discipline of sociology by playing to certain sensibilities and characteristics which were deemed by Parsons (at the time) to be essential to the legitimation (and, in Gouldner's eyes, the wholesale selling) of sociology in the eyes of the academic establishment. These sensibilities are "continuity, codification, convergence, and cumulation" (Gouldner 1970a:17), an ideology which embodies, for Gouldner, an extension of nineteenth-century sociological

positivism (which, of course, felt the need to stress societal continuity in the face of the negative criticisms arising from the French Revolution and the philosophes). As Gouldner (1970a:18) continues, Parsons' attempt to solidify and unify sociology's methodological position by way of this "continuity-convergence" (and thus positivistic) ideology, while serving "to strengthen the mutual solidarity of professionals," nevertheless at the same helped to create a "mood of consensus that smothers intellectual criticism and innovation." For Gouldner, then, methodology equals ideology, and Parsons' attempt to impose a rigid positivism upon the discipline by invoking the names of some great minds and their alleged convergence of thought is the unmatched exemplar of this.

### 3.4 Cumulative Advantage

Now that we have discussed the method by which influence will be measured, we must concern ourselves with the universe from which to draw our data. We have already discussed that three prominent sociological journals have been chosen which will provide the database from which to derive cite counts from each author's (Garfinkel, Goffman, Homans, and Parsons) corpus of published work. As indicated briefly above, because the available literature from which to draw citations is growing at a rate of about 7 percent

per annum (Price 1976),<sup>20</sup> we must necessarily limit, or control for, the number of journals allowed in our study. As previously indicated, this decision resulted in choosing three journals from which to count cites to the authors in question.

Besides implementing controls at the level of the literature (i.e., in order to control for its growth from year to year), we must also control for how much each author has published. That is to say, when we get down to the task of deciding which and how many works will count for purposes of measuring influence, we are left with either of two choices pertaining to the treatment of this data source. These choices, then, are: 1) counting everything, that is, the entire corpus of work which each theorist has amassed over the years, or 2) limiting the database by selecting only a restricted number of representative works from each theorist.

---

<sup>20</sup> As Price (1976:303-4) has discussed, the population of journal literature is definitely growing, and he calculates this growth at between seven and eight percent per year. As a review of the literature on literature growth indicates, however, these estimates vary slightly from author to author: e.g. Line and Sandison (1974) estimates the growth rate at approximately 5 percent; Aversa (1984) suggests it is around 4 percent; and Garfield (1980a) gives estimates as low as 3 percent. In any event, the fact that the literature is growing suggests that in order to disentangle obsolescence effects from the effects of the growth of the field itself, we must control the number of journals admitted to our study.

There has already been great precedence established for opting for the latter choice, that is, the restricting of the number of works allowed into the study. The reason we must opt for a limited universe is that scientific productivity as discussed here is an activity which is circumscribed by a stochastic phenomena known as "cumulative advantage."

What is cumulative advantage? In brief, it is simply a social phenomena in which, as Price (1976) has suggested, success seems to breed success. Merton (1968,1988), who has coined his own term--"The Matthew Effect"--for speaking of this phenomena, suggests that Lotka (1926) was the first person to attempt a quantification or articulation of a law of cumulative advantage. That is, Lotka's famous "inverse square law" of scientific productivity suggests that "the number of scientists with  $n$  publications is inversely proportional to  $n^2$ " (Merton 1988:611). What this relationship implies is that for a variety of scientific disciplines there is a tremendously skewed distribution in the sheer number of scholarly works produced. For example, if we apply Lotka's asserted relationship to the typical scientific discipline we find that a very small percentage (5 to 6 percent) of highly productive scientists who publish at all end up producing about half of all papers in

their discipline (Price 1986).<sup>21</sup>

Price's (1976:303-4) discussion of cumulative advantage is well worth noting. The process begins, obviously, when a scientist gets an article or book published. Immediately upon publication the paper is judged by peers working within the particular research environment; this first burst of citation activity connected with the new paper is extremely important, and as Price (1976:303) notes, this initial rate of appropriation of the article "in most cases probably determines all future citation history."

What this implies, then, is that if an article does not attract attention early on it is very likely that the article will receive few if any cites. In essence, the pulse size--that is, the initial response to the paper or book--determines the effectiveness of the paper at the research front. Conversely, it is very rare for books and articles which do not attract much cite attention during

---

<sup>21</sup> This seemed like a fairly plausible approximation of the situation when Lotka first introduced the inverse square concept in 1926. However, this formulation has recently been shown to (slightly) overestimate the domination of the few highly productive scientists. For various approximations of cumulative advantage, see (Price 1976), Griffith et al. (1979), Line and Sandison (1974), Cole and Cole (1972, 1973), and Oromaner (1985, 1987). Actually, Price's (1976) final approximation of the cumulative factor is fairly close to Lotka's, as he suggests that the relation, rather than being a perfect inverse square, is closer to  $1/n^{1.5}$ .

this crucial period to be "discovered," so to speak, later after laying dormant for any appreciable period of time.

This initial reaction to the publication--be it positive or negative--has a feedback effect which has implications for the researcher and his or her productivity (which then of course leads into the phenomena of cumulative advantage). As Price (1976:303) continues, "We must suppose that the size of the initial pulses modulates the author's behavior and causes either a continuation of publication or cessation." That is, for those authors fortunate enough to have their works appropriated (i.e., cited) early, the author's self-estimation is bolstered, and those in positions to judge the author's worth (especially in terms of promotions and tenure) will accommodate that particular author just that much more, enabling the author to make another attempt at successful publication. As this process continues after a certain number of iterations a threshold is reached wherein past successes, piled one upon the other, lead to larger and larger cumulative advantages which in turn lead to a built in mechanism for the successful continuation of publication activity.

Although this is a somewhat idealized account of the process of cumulative advantage, it nevertheless serves to illustrate the general process by which an author becomes highly visible, and how this visibility in turn sustains

the author at specific lofty heights while simultaneously denying space for lesser known authors who are publishing and vying for recognition in shared research environments. For our purposes of citation analysis, however, we should keep in mind, as Price (1976:304) has noted, that the correlation between citations and productivity is very high; citations are thus a good measure of influence, which we are here attempting to measure.

Thus, for purposes of this study we must, in lieu of the phenomena of cumulative advantage, restrict the number of works allowed into consideration. This is necessitated because of the differential amount of published works which have been produced by Parsons, Goffman, Homans, and Garfinkel. As will be discussed below (in chapter IV), relative to the other three authors Talcott Parsons has a very large body of work; if we were to count influence as the measure of total cites to all authors' works, Parsons would, merely because of the sheer volume of his publications, be shown to have the greatest influence on the discipline. Therefore, we must control for this inequality in scientific productivity by considering a select, equal number of works from each theorist. As such, this study will isolate five published works from each theorist which are deemed as representative of their total corpus. In order to ensure that these choices do not appear

capricious, spurious, or arbitrary, the criteria by which these works have been chosen (and justifications for each choice) will be explicated below.

#### IV. THE CHOSEN WORKS

The following section will delineate the various reasons and justifications for choosing the five works from each theorist; along the way, in order to help substantiate specific historical events which contributed either directly or indirectly to the trajectory of each theorist's intellectual development, pertinent biographical notes will be included as well.

The primary criterion for inclusion in this study was based on a search of the Social Science Citation Index (SSCI), the purpose of which to provide a preliminary profile of the general citation patterns for each work under consideration.

Without regard to the citing journal, I went into the pages of the SSCI and noted the column space dedicated to each author's published works. From this cursory check into the citation patterns I isolated a group of publications which, because they were obviously more heavily cited (i.e., took up more column space) than other competing publications, were therefore easy to point to as being

potential candidates for inclusion in the study.<sup>1</sup> For example, when one checks any five year cumulative index from 1966 on one finds that, with regard to Harold Garfinkel, his Studies in Ethnomethodology dominates the column space regarding cites to his published works. Situations such as these--wherein a pattern of clear dominance by a particular published work is discernable--made the task at hand of choosing five works from each author largely unambiguous.<sup>2</sup>

---

<sup>1</sup> That is to say, it is very much the case that when one references the SSCI 5-year cumulative indexes one notices instantly certain works which are much more highly cited than others. Because these particular heavily cited works provide clear visual justification for inclusion in this study, there was no need to include exact enumerations when the total to any single publication exceeded 300 cites. Table 4.1 (page 89) indicates the comparative citation totals for each work under consideration. This table serves only to provide justification for the five works which were ultimately chosen for inclusion in this study. These five works from each theorist and the amount of citations each garnered count for purposes of this study as representative of our measure of intellectual influence. But since the SSCI provides citation data from a wide range of social science journals, the data in table 4.1 should not be construed as impinging directly on this study since we are concerned here primarily with Gouldner's reading of the state of sociology, not the social sciences, per se. Nevertheless, I will at points throughout the data analysis note interesting contrasts between our own data culled from the three selected sociology journals and the general SSCI data in Table 4.1. (Note that the 5-year indexes span the periods 1966-70, 1971-75, 1976-80, and 1981-85; earlier periods are not yet indexed.)

<sup>2</sup> Of course, the only possible drawback to this method is that it necessarily entails combining citations to monographs as well as journal articles. That is to say,

From the group of candidate publications listed in Table 4.1 I conducted a hand count of each in order to determine the five most heavily cited works for each theorist. The counts which I have provided are accurate insofar as the limitations of the SSCI indexing presentation allow. That is, as Braxton and Bayer (1986:35-36) have noted, such things as name homographs, misspellings, incorrect reporting of the work's year of publication, and the like produce a certain amount of variability whereby one is apt to miss a few citations to an author's work. For example, when one sits down to conduct a search of the cite counts to the works of Talcott Parsons one finds listings to his work under, for example, the names "T. Parson" and

---

when we are analyzing journal citations to the various article and book publications of the pertinent authors we must realize (as Line 1979; Line and Roberts 1976; and Motylev 1989 have indicated) that the rate of obsolescence of the various types of literature varies. As Motylev (1989:98) states: "The intensity of the decline of information usefulness ('aging') is about 10% a day for newspapers, 10% a month for magazines and journals, and 10% a year for books." This might be seen as particularly problematic for Garfinkel because among his five most cited works four are journal articles. Since citations to articles "age" much faster than to monographs, our measure of Garfinkel's influence is slightly different than those of the other authors considered here simply because they have many more books in publication. However, I believe that the presentation of the data (in chapter V) will vindicate the method chosen because it will be shown, as it turns out, that the mixing of journal articles and monographs has little adverse effect on the interpretation pertaining to Garfinkel's unique situation.

Table 4.1. Number of Citations in SSCI 5-Year Cumulative Indexes to Authors' Selected Works

Author	Work (Year of Release)	1966-70	1971-75	1976-80	1981-85
Parsons	Struc Social Action (37)	147	214	300+	300+
	The Social System (51)	300+	300+	300+	300+
	Tow General Theory (51)	194	153	153	126
	Working Papers (53)	70	47	53	39
	Family, Soc, and Int (55)	209	209	300+	276
	Economy and Society (56)	60	81	74	42
	Structure and Process (60)	94	83	127	116
	Social Structure & Pers (64)	55	42	74	54
	Societies: Comparative (66)	66	109	130	80
American University (73)	-	41	78	60	
Homans	The Human Group (50)	227	211	231	188
	Marriage, Authority (55)	26	16	12	8
	SB as Exchange (58)	29	70	59	54
	Soc Behav: Elem Forms (61)	300+	300+	300+	300+
	Sentiments & Activities (62)	15	10	21	12
	Bringing Men Back In (64)	36	40	25	27
	Nature of Social Science (67)	15	41	38	29
Goffman	Presentation of Self (59)	230	300+	300+	300+
	Asylums (61)	300+	300+	300+	300+
	Encounters (61)	80	121	149	97
	Stigma (63)	152	268	300+	300+
	Behavior Public Places (63)	70	108	158	84
	Interaction Ritual (67)	26	122	186	231
	Strategic Interaction (69)	4	33	67	55
	Relations in Public (71)	-	101	144	180
Frame Analysis (74)	-	11	177	240	
Garfinkel	Inter-Racial Homicides (49)	2	8	18	23
	Degradation Ceremonies (56)	26	54	76	46
	Experiments with Trust (63)	6	19	29	18
	Routine Grounds (64)	26	37	27	16
	Studies in Ethmeth (67)	49	219	300+	300+
	Formal Structures (70)	-	24	65	43
	Discovering Science (81)	-	-	-	23

"Parsons" with either no initial or an incorrect one. And when one locates the correct "T. Parsons" file one must be wary of such discrepancies as publication dates being misreported, the many ways titles of works can and are abbreviated, and the various listings of publication year if the work happened to have several editions. All of this and more contributes to the very real possibility that the totals represented in Table 4.1 might very well be slightly underreported. Nevertheless, for purposes of choosing the five most heavily cited works from Parsons, Homans, Goffman and Garfinkel, the procedure described here suffices.

Again, as mentioned above, Table 4.1 indicates how the works under consideration compared to each other as a result of the SSCI search. For the most part, these numbers alone provided the decision rules for choosing which works eventually were chosen for inclusion in the study. For various reasons, however, there were several deviations from the decision rule; these will be elaborated, along with the more general discussion of the works which were eventually chosen, below.

#### 4.1 Harold Garfinkel

As Mullins (1973) has noted, Garfinkel received his Ph.D. in 1952 from Harvard under the supervision of his teacher and mentor, Talcott Parsons. Parsons' influence on

Garfinkel (at least at this early stage of his career) is quite evident, especially when one notes the title of Garfinkel's dissertation, "The Perception of the Other: A Study in Social Order." The concern with "social order" is, of course, reflective of Parsons' lifelong fascination with the subject, an area of study which Parsons elaborated on in great detail following his initial foray into the Hobbesian problem of social order which was treated at length in his first book, The Structure of Social Action.

Of course, as events unfolded and as chronicled in his numerous published works, Garfinkel drifted quite rapidly away from Parsons' emphasis on the structural determinants of social order toward Alfred Shutz's phenomenological approach. Indeed, the first part of the title of his dissertation, "The Perception of the Other," can be seen to be appropriated directly from Schutz, that is, from his treatment of how actors make sense of a situation (the famous "definition of the situation").<sup>3</sup> Rather than the

---

<sup>3</sup> As Richard Grathoff (1978:141) has noted, since he was both a student of Parsons' and also thoroughly acquainted with the work of Schutz, Garfinkel is somewhat unique in that he represents an intermediate position between the two theoretical extremes of the "Schutz-Parsons divide." Grathoff highlighted these theoretical differences by making available the series of private correspondences in which Parsons and Schutz engaged in the early forties; however, at least one important commonality comes to light when one realizes, as Garfinkel did, that both Parsons and Schutz agree that the study of social action must necessarily begin with the individual actor. As Grathoff

structure or the situation per se defining the acceptable parameters of behavior Garfinkel, following Schutz, sees the rules of interaction as more or less an emergent phenomena based on tacit codes of conduct which are worked and reworked by actors in a situation (but, it must be warned, definitely not in a Goffmanian sense).

This briefly sets the stage for the discussion concerning which of Garfinkel's works were chosen for inclusion in this study and why. These works are as follows:

1) "Conditions of Successful Degradation Ceremonies": This short article, published in AJS in 1956, still stands as one of Garfinkel's finest applications of ethnomethodological thought. This was an important and influential work; in fact, Mullins (1973:183) takes special notice of this article as representing a "program statement," that is, a work which stands as an exemplar of the types of investigations or positions which a particular theory group or paradigm is attempting to establish within a particular discipline. For ethnomethodology, Garfinkel's

---

(1978:129) notes, "While Parsons takes account of the individual actor in terms of situational references within action systems, Schutz insists that a grasp of the concrete actor and his performances, of his interests and motives should become accessible through the study of the structure of relevances in everyday life."

"Conditions of Successful Degradation Ceremonies" stood as a program statement because it took the bold position that all societies have mechanisms for dealing with individuals who violate group norms or broader societal sensibilities. These mechanisms are known as "degradation ceremonies," formal, ritualized events which work to transform the transgressing individual's status into an identity which is lower than the group's or lower than that which the individual previously held (Garfinkel 1956:420). Garfinkel then elaborated the sociology of the process, introducing such concepts as the "redefined person" (the denial of the previously held view of the self in favor of the now degraded, "true" self); the various degradation tactics which are employed in achieving these ends; the types of violations which prompt degradation ceremonies; the effectiveness of various degradation ceremonies, "timing rules," the role of witnesses and the concomitant status claims of the participants, as well as the use of power and prestige in influencing any particular outcome (Garfinkel 1956:424).

2) "A Conception of, and Experiments with, 'Trust' as a Condition of Stable Concerted Actions": A 1963 article which appeared originally in the volume Motivation and Social Interaction. Garfinkel begins with Parsons' notion of what constitutes stable interaction situations: that is,

ego and alter's ability to make sense of any particular social situation (and the concomitant interpretive consequences which follow) flows out of the actors' successful incorporation or internalization of the society's common cultural norms and values into the superego (Garfinkel 1963:187). But Garfinkel introduces a new twist here, suggesting that the overweening focus on social order leads sociologists into becoming complacent concerning the tacit, everyday, commonsense, nearly invisible rules which lay at the foundation and at the very heart of human social interaction. It is not sufficient, therefore, to study merely the perceived normal environments of interaction (or what Garfinkel refers to as "game events"); researchers must also unearth the hidden or substructural components of action. The way to expose these tacit conventions of everyday life is by violating the normative or taken-for-granted commonsense environment of events (Garfinkel 1963:215). Garfinkel thus employs Shutzian phenomenology (by way of Husserl) vis-a-vis the violation of the trust inherent in (for experimental purposes here) the basic rules of a game. For example, in an innocuous game of tic-tac-toe an experimenter is ordered to respond to an opening "X" move by placing his "O," ambiguously, on a border rather than in an appropriate square. This breach of trust by way of violation of the

rules of the game creates confusion similar in ways, Garfinkel insists, to Durkheim's notion of "anomie," the sociological consequences of which a) allows us to recognize what will produce confusion in which particular social setting or "game"; b) provides a more rigorous understanding of "community" by allowing the identification of those individuals or groups of persons who are reciprocally bound by the same constitutive order of actions, or, as Garfinkel (1963:197) describes, by those who are "playing the same game." In the final analysis, the real point which Garfinkel is trying to make of all of this is important but somewhat difficult to fathom. If one were to attempt to summarize this paper, then, one should point out the following: Garfinkel is concerned with the ways in which attitudes toward daily life are experienced merely as perceptions by which the common cultural legacy, internalized through socialization and other appropriate mechanisms, is seen as the unquestioned environment of social reality. Sociologists, being merely people as well, unacceptably "gloss over" the environment of everyday life with the term "common culture" (Garfinkel 1963:235). The attitudes of daily life, then, generally unquestioned or unexamined as they understandably tend to be, constitute or are reflective of, as Garfinkel explains, "the institutionalized common understandings of the practical

everyday organization and workings of the society as seen 'from within' (Ibid.). This tendency to accept the picture of society which reveals itself through the codes furnished within and by the society's socialization process provides its inhabitants with what for them is a "real" social environment, one which defines reality in a commonsense, taken-for-granted fashion. However, it can be shown to be, according to Garfinkel, a somewhat simple matter of altering persons' perceptions of what constitutes "normal" social relations and interaction by producing modifications which serve to transform socially defined environments of real objects "into another environment of real objects" (Ibid.). These modifications can be shown to be producible experimentally by violating basic rules of game playing or, say, the tacit rules which govern conversational interaction, thereby exposing the function of "trust" as being merely one of the many normative mechanisms by which society maintains an ordered, manageable environment. However, the point is made that this environment is not the only environment, and that it is not the only, unalterable dimension of reality knowable or comprehensible to its inhabitants.

3) "Studies of the Routine Grounds of Everyday Activities": A 1964 article originally published in the journal Social Problems, and which was later republished,

in somewhat modified form, in Studies in Ethnomethodology.

"Routine Grounds," along with the previous two articles just discussed, garnered approximately the same amount of cites in the SSCI, and were therefore chosen for inclusion in this study.<sup>4</sup> This paper continues the theme of the previous article, that being the unearthing of those tacit, oftentimes overlooked or glossed over components of human interaction which hold the key to a deeper, more fundamental understanding of social action. Garfinkel juxtaposes Kant's concern for the moral order "within" with

---

<sup>4</sup> As Table 4.1 indicates, there were three other articles under consideration for inclusion in this study, only one of which would eventually be chosen. The three articles closely contending for this final spot were as follows: First, Garfinkel's 1949 Social Forces article, "Research Note on Inter- and Intra-Racial Homicides," a work which attempted to shed some light on differential sentencing patterns relative to race of plaintiff versus defendant, i.e., white-white, white-negro, negro-negro, and negro-white. Although a fine article, it was felt that for purposes of this investigation a more general theoretical article would be more appropriate, and therefore was not included. Second, Garfinkel's 1981 co-authored article appearing in Philosophy of the Social Sciences entitled "The Work of a Discovering Science Construed with Materials from the Optically Discovered Pulsar," a superb ethno-historical microreconstruction of the events leading up to and including the 1969 optical discovery of a pulsar. However, because this article was of interest primarily only to philosophers and sociologists of science, it was not included here. Finally, the article which was ultimately selected for inclusion (because, as shown in Table 4.1, it received more overall cites than the above mentioned competing articles), the Garfinkel/Sacks co-authored article, "On Formal Structures of Practical Actions," the discussion of which follows in part five below.

recent sociologists' concern with the moral order "without," that is, in the Parsonian sense, the concern with the structural concomitants and exigencies of social order as understood in a nexus of social actors receiving cues from one another as to what constitutes acceptable behavior (that is, ego's choice of actions is to a large extent contingent upon and informed by alter's expectations, the actions themselves guided of course by an appropriately internalized set of cultural norms and values)(Garfinkel 1964:225). Again, as Garfinkel articulated in the previous article, this contemporary concern of sociologists reflects more or less the notion that the moral order consists essentially "of the rule governed activities of everyday life" (Ibid.). But in their haste to investigate or theorize about social action the bulk of sociologists have accepted this very essence of everyday life as being merely a "natural fact of life" without stopping to fully consider or comprehend the deeper implications, that is, that the world of tacit rules and governing laws represent a legitimate area of study in its own right, worthy of the attention of sociologists and social psychologists alike. Here Garfinkel gets into a more full-blown elaboration of conversational analysis and the types of modifications (brought about, again, by violations of the conventions of everyday conversation or talk) which

transforms one perceived set of real objects into another environment of real objects (Garfinkel 1964:249).

4) Studies in Ethnomethodology: As previously mentioned, this book is by far and away Garfinkel's most heavily cited work, a volume which pulled together all the major strands of his thought from the mid-fifties up to and including 1967, the year the book was published.

Garfinkel makes a point early on of informing his readers as to exactly what the point of this volume is, with the first chapter being titled, appropriately enough, "What is Ethnomethodology?" Again, as Garfinkel has been so determined to point out, ethnomethodology is concerned with the study of the routine, everyday activities of life, stressing that the commonplace--as opposed to spectacular events or extraordinary phenomena which usually grab the attention of researchers--is deserving of systematic study in its own right. Garfinkel (1967:1) further suggests that the accounting practices that we each employ as social actors--wherein we produce and reproduce "the managed settings of everyday life"--ordain within us as ordinary citizens the powers of observation and reportability which are usually accorded only to professional researchers. Garfinkel is making the case, therefore, that we are all to a large extent "lay" sociologists, and that professional sociologists do not have any greater claims concerning the

awareness, understanding or explication of social life and the moral order.

The book's most intriguing discussion occurs in chapter five, "Passing and the managed achievement of sex status in an 'intersexed' person." Garfinkel, following the same type of formulation employed in his paper "Successful Degradation Ceremonies," suggests that all societies have rigid, formal controls over the transference of status which individuals occasionally experience. Beginning with Parsons' notion of ascription as a "relation concept" (Garfinkel 1967:133), Garfinkel then describes how an ethnomethodological study of societal ascription of sex roles works in the case of a male transsexual attempting to deal with the conversion--or "passing"--to the female sex status. The observational techniques which ethnomethodology employs are put to the test and, as Garfinkel suggests, offer brilliant illuminations of certain processes and mechanisms of everyday life, such as in the case of the types of management devices utilized by "Agnes" in his/her attempt to pass successfully from the male to the female sex role. In fact, according to Garfinkel, ethnomethodological analysis of the impression management of self serves to undermine Goffman's own formulation of impression management which he elaborated in The Presentation of Self in Everyday Life. Garfinkel (1967:165)

suggests that although Goffman's formulation of self management was correct on a superficial level--especially relating to how "Agnes" achieved for herself the natural female role by successfully managing situations of risk and uncertainty, such as playing the role of female in the presence of anonymous others while realizing all along that this concealed knowledge of herself was "regarded as potentially degrading and damaging" if disclosed--the Goffmanian approach nevertheless falls short because of its methodological implications of strategy or game analysis, or what we might call the conscious role playing aspect of dramaturgy. That is, strategic analysis works only so long as events of impression management remain episodic in their formal structure. However, as Garfinkel continues, Goffman's analysis cannot account for the effect of time and how episodes of impression management occurring over time loop back upon and inform the next relevant course of action. Management devices must be enumerated over time, with each successive episode forming a structure or texture of relevances which direct his/her action toward the mastery of his/her practical circumstances (Garfinkel 1967:166). Ultimately, then, Goffman's analysis fails to take into account three important ethnomethodological

concerns, namely, trust, routine, and rationality.<sup>5</sup> As Garfinkel (1967:173-174) explains:

Agnes' passing occasions and her management devices throw into relief the troubled relationship in her case between routine, trust, and rationality. By considering these passing occasions and management devices with respect to this troubled relationship we may be able to break free of the "diagnosis" or Goffman's episodic emphasis.

5) "On Formal Structures of Practical Actions": As briefly mentioned above, a 1970 article co-authored by Garfinkel and Harvey Sacks which appeared originally in the McKinney/Tiryakian edited volume, Theoretical Sociology. This is an extremely difficult article in that the authors assume that the reader is perfectly familiar with the terminology routinely employed by ethnomethodologists and sociolinguists in discussing the nuances of conversational analysis.<sup>6</sup> Terms such as "indexical," substitutability,"

---

<sup>5</sup> Regarding rationality, Garfinkel's stress on the "rational properties" of everyday activities becomes abundantly clear in chapter eight, wherein he presents, following Schutz, a laborious taxonomy of the fourteen ways in which social actors can be understood as displaying rational behavior (Garfinkel 1967:263-68).

<sup>6</sup> This charge of incomprehensibility against Garfinkel is certainly not new or novel. A sampling of comments from reviews of Studies in Ethnomethodology will suffice to make the point: "...the reader is left to do an inordinate amount of work...the reader who is short of patience in grasping the sense of the strange, and often anguished, writing will be justifiably tempted to set the book aside prematurely" (Wilkins 1968:643); "Redundancy is a general problem in the style, and it violates ordinary syntactical expectations to such a degree that reading becomes difficult. In some passages, normal English grammar is

"prosody," "glossing practices," "definiteness," "token reflexives," and so forth are bandied about as the authors attempt to drive home their point. And just what is their point exactly?

Garfinkel and Sacks are arguing, as far as I can tell, that the great majority of contemporary social scientists, in their fervor to explain or to propagate a particular theory of human social behavior, inadvertently utilize or manufacture, in an attempt to stay true to the objective nature of scientific reasoning, objective expressions about the phenomena under investigation rather than using the readily available "indexical phenomena" of natural language (Garfinkel and Sacks 1970:339). The authors are not content, therefore, to remain at this second level of abstraction, that is, at the level of conceptualizations, formulations, models or definitions of social life.

---

almost completely abandoned" (Wallace 1968:125); "...Garfinkel elaborates very greatly points which are so commonplace that they would appear banal if stated in straightforward English. As it is, there is an extraordinarily high ratio of reading time to information transfer, so that the banality is not directly apparent upon a casual reading" (Coleman 1968:130). Even a generally positive review such as Guy Swanson's (1968:122) alludes to the incomprehensibility factor: "I may not understand this book as Garfinkel hoped his readers would, but I was mightily helped by coming to it when I did." One gets the impression at times from reading Garfinkel that some of the more egregious communicative disorders of his teacher, Talcott Parsons--notorious for his own tortured literary style--surely must have rubbed off on him.

Following Husserl's early phenomenological inquiries, the authors suggest that the only way of truly understanding human society is by redirecting this study back toward the fundamental structures of social action as disclosed in natural language, that is, back to the study of the phenomena of consciousness itself.<sup>7</sup>

Finally, however, the authors make an important programmatic statement concerning the peculiar niche ethnomethodology is attempting to carve for itself within the social sciences. Garfinkel and Sacks begin by reaffirming ethnomethodology's primary interest or focus, that being the formal structures of everyday activities. These formal structures are discoverable through societal members' accounts of these structures as observed and realized in natural language. However, unlike other professional sociologists, Garfinkel and Sacks (1970:345) argue that ethnomethodologists seek to "describe members' accounts of formal structures wherever and by whomever they are done, while abstaining from all judgments of their adequacy, value, importance, necessity, practicality, success, or consequentiality. We refer to this procedural policy as 'ethnomethodological indifference.'"

---

<sup>7</sup> It should be noted that much of the interpretation offered here is indebted to the introduction provided by McKinney and Tiryakian, especially pages 17 and 18.

This "indifference" is simply an indifference to the typical methodologies which sociology holds dear, such as positivism, statistical analysis, survey research, or even general theorizing. Garfinkel and Sacks see no gain in (or rather, are indifferent toward) what we might call the project of "sociological reasoning"; the aim, then, of ethnomethodology is simply the mastery of natural language. As the authors (Garfinkel and Sacks 1970:346) go on to explain:

Professional sociological reasoning is in no way singled out as a phenomena for our research attention. Persons doing ethnomethodological studies can "care" no more or no less about professional sociological reasoning than they "care" about the practices of legal reasoning, conversational reasoning, divinational reasoning, psychiatric reasoning, and the rest.

#### 4.2 Erving Goffman

Erving Goffman received his Ph.D. in 1953 from Chicago under the general supervision of Herbert Blumer, one of the leading figures in the establishment of American symbolic interactionism (Mullins 1973:92).<sup>8</sup> Unlike Garfinkel, who published only one major book (Studies in

---

<sup>8</sup> The title of Goffman's dissertation was "Communication Conduct in an Island Community," a work which evolved into his first book, The Presentation of Self in Everyday Life. His master's thesis, completed in 1949 and titled "Some Characteristics of Response to Depicted Experience," was also written at Chicago (Smith and Waksler 1989:173).

Ethnomethodology), Goffman enjoyed a prolific book publishing career, producing eleven book-length volumes between the years 1959 and 1981 (Smith and Waksler 1989).

The same decision rules discussed earlier were applied to the selection of the five works pertaining to Goffman. That is to say, the works included here were selected on the basis of the amount of citations each volume garnered as recorded in the Social Science Citation Index, with the top five most heavily cited works ultimately being chosen for inclusion. This selection strategy was deemed desirable because it provides a fairly objective criterion for choosing the five works from each theorist. Further, relying on the SSCI in this manner provides an internal consistency which cuts across all choices and all theorists involved, thereby eliminating some of the more subjective impingements which any project of choosing is subject to.

However, with the advantage of hindsight several trends became noticeable regarding the potential inadequacy of this strategy in relation to the works of Goffman. Recall I mentioned that the visits to the SSCI entailed merely a cursory examination of the crude amount of citations to each work without regard to citing journals. In the case of Goffman this criterion clearly indicated three volumes which stood head and heels above the rest of the group, namely, The Presentation of Self in Everyday Life, Asylums,

and Stigma. The final two choices came down to deciding between four other books which were nearly equally cited (but, still, slightly less so than the three previously mentioned), these being Behavior in Public Places, Interaction Ritual, Relations in Public, and Frame Analysis (enumerations for all of which provided in Table 4.1).<sup>9</sup>

However, during the data collection phase of this investigation (which entailed rummaging through the pages of AJS, ASR, and Sociometry in search of cites to each selected work) it became apparent that, because we are dealing here with sociological journals per se, a good deal of research interest seemed focused on two books not included in the study, namely, Behavior in Public Places and Relations in Public.

So here is the rub: Although heavily cited, Asylums and Stigma are books which have a much broader base of appeal beyond the confines of sociology. That is to say, beyond being pertinent to the interests of sociologists these books also were brought to the attention of, for example, psychologists, the health care community, and other institutional caregiving concerns, governmental or

---

<sup>9</sup> As discussed below, Frame Analysis and Interaction Ritual were eventually chosen from among these four. It should also be noted that Encounters and Strategic Interaction were heavily cited as well, but, nevertheless, fell just short of the four volumes being discussed above.

otherwise. Thus, the cites to these two books were "inflated" due to the nature of their subject matter and the ramifications into other disciplines.

The implications of all of this is simply that Relations in Public and Behavior in Public Places--two volumes which made a consistent showing in the sociological journals included in this study--were bypassed in favor of several other works of Goffman's which happened to garner more gross cites as per the SSCI. However, because the decision rule clearly called for the tallying of citation totals without regard to citing journal, Asylums and Stigma were, for reasons already explicated, chosen.

On the other hand, even with the acknowledgment of this potential oversight, it is hard to argue against including Asylums and Stigma in favor of perhaps several of Goffman's other works. Suffice it to say that this minor headache is merely symptomatic of the grand task at hand: that of choosing only five works from a theorist who has produced an extraordinary amount of important, highly influential (and thus highly cited) works.

The following, then, briefly discusses the representative works of Erving Goffman as selected for purposes of this study.

- 1) The Presentation of Self in Everyday Life: The 1959

publication which launched Goffman's career.<sup>10</sup> This book, the most heavily cited of Goffman's published works according to the SSCI, served to solidify the paradigmatic status of dramaturgy within sociology. The essentials of a dramaturgical perspective, according to Goffman (1959), describe the social actor in a situation according to how these actors present themselves in the course of carrying out their daily routines of living; the ways in which social actors attempt to manage the impressions of their daily performances; and a description of the types of normative rules and constraints attached to the sustaining of performances before an audience of others. In Presentation of Self Goffman, because of the conceptual nature of this incipient work, introduced a great number of terms and concepts into the collective sociological consciousness, and today such Goffmanisms as "teams," "region behavior," "impression management," "front" and "backstage" have become an entrenched part of the sociological vernacular.

---

<sup>10</sup> However, it should be noted that between the years 1951 and 1959 Goffman was quite active professionally, having published thirteen articles including such notable pieces as "On Cooling the Mark Out" (Goffman 1952), "On Face-Work" (Goffman 1955), "The Nature of Deference and Demeanor" (Goffman 1956a), and "Embarrassment and Social Organization" (Goffman 1956b), the latter three of which were republished in Goffman's Interaction Ritual.

2) Asylums: A 1961 book release. This is primarily a descriptive ethnographic study of ward behavior in a mental institution, although one of the book's most important theoretical statements emerges from Goffman's superb generalized discussion of the characteristics and properties of so-called "total institutions."<sup>11</sup> A total institution is one which is characterized by encompassment and ubiquity, that is, the total encircling and control of physical space such that all life contained within its borders and boundaries is wholly subject to its rules, regulations, and so forth. As Goffman (1961a:4) explains, the total character of the total institution "is symbolized by the barrier to social intercourse with the outside and to departure that is often built right into the physical plant, such as locked doors, high walls, barbed wire, cliffs, water, forests, or moors."

3) Stigma: A 1963 release which delineated a conceptual scheme for the in-depth analysis of "stigma," the situation wherein individuals find themselves excluded from free participation in social life as a result of being disqualified, for whatever reason, from full social

---

<sup>11</sup> As Hillery (1982:85) has pointed out, Goffman's formulation of total institution is compatible with Parsons' theory of organizations, especially in terms of his notion that an organization is ostensibly one which is oriented toward the attainment of a specific goal.

acceptance.

All societies, Goffman contends, establish means by which individuals are categorized, i.e., that complement of attributes obtainable within or by an individual which are deemed as "normal" and "natural" (Goffman 1963b:2). A stigma, then, is an attribute which carries deeply discrediting connotations, and this attachment to the individual serves to define (in most instances) both societal identification of the individual as well as the individual's own self-identification. Through acknowledgement of the deficiency of self which the stigma unveils to the stigmatized, the stigmatized individual can then proceed to internalize those attributes which are proclaimed as normal, thereby embarking upon or identifying with a "moral career," that is, learning to cope with the consequences of possessing a particular stigma. This finally, then, allows for the theoretical conceptualization Goffman had been working towards all along, namely, the tying together of stigma with perceived forms of deviance. That is, situations occasionally arise--because (and Goffman closely follows Parsons here) no social system is in perfect equilibrium at all times. Because of this, the socialization process is now and again subject to breakdown, an example being the improper transmission of cultural norms and values to the recipient of the

socialization (in this case, the stigmatized individual does not successfully internalize his or her own stigma) whereby the individual's self-identity is incompatible or at odds with the stigmatized social identity. The ramifications of this type of social systems breakdown or dysfunction might then lead the individual into certain forms of deviance (at least as viewed from society's perspective).

4) Interaction Ritual: A 1967 compilation of previously published articles (as mentioned in note 11 above) as well as one newly published article, the important and influential "Where the Action Is."<sup>12</sup> It is worthwhile here to engage in a somewhat extended discussion of this article because its theme is reflected in much of the focused research interest prevalent among social psychologists and phenomenologists of the time.

Goffman elucidates some of the features or structural components of games of chance which are generally taken for granted as given or which are otherwise overlooked entirely, and from this discussion parallels are drawn to

---

<sup>12</sup> I describe this article as influential because according to this study's findings (the bulk of which will be discussed in the results section) among those authors citing Interaction Ritual, an overwhelming number did so with specific mention of or concentration upon "Where the Action Is."

the situation of human social action, particularly with regards to the epistemological or theoretical status of the means-end schema. For example, Goffman suggests that whereas in real life actions and their eventual outcomes have a discernable temporal dimension, that is, the final outcomes of choices of actions are decided sometimes decades later, in games of chance the periods (or phases, these being "squaring off," determination," disclosure," and "settlement" [Goffman 1967:155]) are decided almost instantaneously, that is, once the bet has been placed the "outcome is determined and payoff awarded all in the same breath of experience" (Goffman 1967:156).

Goffman then addresses the criteria by which actors assess risk and potential gains connected with any course of action, that is, in terms of game theory, the participant's cost/benefit considerations with regard to playing any particular game of chance. But since in real life there are too many variables to take into account and since oftentimes ends of action lie in the distant future, social actors playing the game of life rely less on purely utilitarian cost/benefit probabilities and instead make decisions based on "subjective probability," a very loose consideration or assessment of the situation based on either past experience, advice, or even personal hunch. "Consequentiality," then, is the term used to describe the

far-flung ramifications which derive from the placing of a bet and the subsequent connected potential outcomes. That is, the bettor playing the game of life must attempt to assess the long-term consequences of any specific possible move.

But even with the most carefully calculated, the most meticulously planned course of action, the human experience is such that in certain instances situations and events may conspire to produce an outcome far removed from the original intent of the action. "Fatefulness," then, is the term used to describe the mechanisms by which chance occurrences infiltrate life. Goffman (1967:169) delineates three ways in which managed social activity can degenerate into fits of fatefulness: 1) dumb luck, that is, the "adventitious linking of events" which can creep into the picture beyond the control or manageability of the participants; 2) the vulnerability of the body: As Goffman (1967:167) so correctly elucidates, "A body is a piece of consequential equipment, and its owner is always putting it on the line."<sup>13</sup> The importance of this observation is obvious: no matter how safe an actor feels in his or her situation or station in life, the fact remains that for the

---

<sup>13</sup> See Parsons' (1978) similar line of thinking in his "A Paradigm of the Human Condition."

person to properly live that moment of life, that is, to partake in any moment of living, the actor must be physically present, in the flesh, in order to carry out that slice or strip of living. Thus, humans are always susceptible to accidents, bodily harm, etc., during the routine living of a life; 3) the norms which regulate behavior in social gatherings are such that the need to maintain appropriate social demeanor is tacitly incumbent upon all social participants. Even very slight slippages or deteriorations of demeanor maintenance, especially if someone makes a consistent showing of the unfortunate offensive trait (i.e., the inability to suppress in public normal physiological processes which are considered to be properly contained within the boundaries of self and thus not open to public consumption or perusal, e.g., belching, burping, passing gas, and so forth) can bring discredit to the actor. Thus, the need to maintain face and proper demeanor in situational engagements, and the potential consequences for failing to uphold the tacit conventions of social gatherings, are the risks which actors take in playing the game of life.

Goffman's point in all of this is to suggest that humans attempt to manage their affairs--both their time on and time off (this time off, known as "killing time," possesses an ontological status apart from other social activity but

nevertheless has very little effect on the life course, i.e., low "consequentiality" [Goffman 1967:162-63])--such that fatefulness can be avoided. "Action," then, as Goffman construes the term here, is a function of modern industrial society, that is, action (e.g., games of chance, sports) is voluntarily manufactured and foisted upon the general public (oftentimes being coordinated by commercial enterprises such as casinos, sporting goods manufacturers, etc.) to break up the monotony of life, to shake up the routine of living and of uneventful moments which are calculably managed to create as little consequentially problematic situations or outcomes as possible, these management episodes again functioning to minimize the eventfulness or fatefulness of life moments (Goffman 1967:175).

Goffman (1967:185) defines action as those "activities that are consequential, problematic, and undertaken for what is felt to be their own sake." This definition then allows Goffman to tie together all the various strands of this long (121 pages) but extremely provocative, fascinating article: these gaming events are engaged in as ends in themselves to provide actors with manageable situations which simulate the exhilaration of such real-life episodes without having to take the connected risks. Participants engaged in such events--be it gambling,

mountain climbing, sky-diving, or other types of physically demanding team or individual sports--report that certain affective states are aroused, the situations provided by various forms of "action" offering its participants "excitement without cost."<sup>14</sup>

5) Frame Analysis: An exhaustive 1974 work in which Goffman attempts to provide a definitive treatment of the organization of experience. This book is much too long (586 pages) to get into great detail here, but briefly, Goffman takes on the huge task of specifying the ways in which actors engaged in interaction make sense of the situation. Further, it is argued that any one framework chosen to discuss exactly what transpires during any singular engagement is necessarily limited or constrained by any one particular view of that action. For example, as Goffman discusses on page 10, what makes a party enjoyable for one

---

<sup>14</sup> Granted, we could say that there is definite costs or risks involved in engaging in such activities as sky-diving and so forth. But this gets back to the notion of subjective probability; that is, each activity has its own degree of "action" wherein participants can use their own subjective probabilities as guidelines to assess exactly which activities would be appropriate to engage in for the purpose of producing specific levels of excitement while at the same time maintaining acceptable levels of manageability. That is, each participant has an internal subjectivity which perceives the levels of risk or "action" by which decisions are made concerning which activities to engage in and, summarily, where the line is to be drawn pertaining to perceived overly risky ventures.

individual may be the very same thing that makes the party unbearable for another. Thus the calling is to provide a rigorous conceptual analysis of "frame," that is, the view from which social actors make sense of or impute subjective meaning into interaction episodes.

"Frame analysis" is the term Goffman chooses to describe the study of the organization of experience as witnessed through the subjective framing of events. Primary frameworks are important in that they allow the observer of action to make sense of a situation which otherwise might seem meaningless; the primary framework provides a convention by which experience is organized around a specified locus of understanding so that actors do not have to constantly "reinvent the wheel" each time a new or modified form of activity presents itself.

Goffman (1974:43-44) then moves into a discussion of "keying," the ways in which events already definable within a primary framework are transformed into another framework, such as, for example, the simple processes of language translation or music transcription, or even the transformation of the production of a play from its original time frame to a more contemporary setting (a la Jack Dudley's transformative work pertaining to Moliere's "Tartuffe" [Dudley et al. 1990]).

### 4.3 George Homans

George Homans is somewhat unique among the four theorists being discussed here because unlike Garfinkel, Goffman, and Parsons, Homans never attained the Ph.D. degree. Nevertheless, the veracity of Homans' intellectual depth became apparent while still a graduate student at Harvard. Commencing in the fall term of 1932-33 Harvard physiologist Lawrence J. Henderson began offering a series of seminars on the sociology of Vilfredo Pareto,<sup>15</sup> and from the insights gained through these lectures Homans would eventually have published (in 1934 at age 24) his first book (co-authored with Charles P. Curtis), entitled An Introduction to Pareto: His Sociology (Homans 1984:106-

---

<sup>15</sup> Henderson, born in Lynn, Massachusetts in 1878, received the M.D. degree from Harvard Medical School in 1902 and did postgraduate work at the University of Strasbourg. He never actually practiced medicine, however, choosing instead various intellectual pursuits such as medical research, philosophy and sociology (Barber 1970). It was Henderson's zeal for Vilfredo Pareto which eventually brought Pareto to the attention of Homans (Homans 1984:89-90). Further, one should note that these informal seminars became the setting for the formation of the infamous Pareto Circle, its better known participants being Henderson, Homans, Joseph Schumpeter, Crane Brinton, Elton Mayo, Talcott Parsons, Robert K. Merton, and even Pitirim Sorokin on occasion (Homans 1984:105); "infamous" in that Gouldner (1970a) attempts to link through "guilt by association" Parsons' alleged conservatism back to the generally conservative membership of the Pareto Circle.

7).<sup>16</sup>

Suffice it to say that this early excursion into the literary field kept Homans from pursuing his Ph.D. within the orthodox academic framework; upon beginning graduate work at Harvard Homans was made a junior fellow in the Society of Fellows, an organization which helped a select few scholars pursue their own academic interests by providing a modest stipend, living quarters, funding for special research projects, and so forth. In effect, the society was established to enable promising young scholars the chance to pursue research interests apart from those of traditional degree-seeking programs. As Homans (1984:120) explains, "The only rule was that a Junior Fellow might not be a candidate for the Ph.D. degree, for it was the special purpose of the society to allow young scholars to escape the stultifying requirements of the doctorate."

Homans thus embarked on a wide and diverse range of ventures, studying psychology with George Elton Mayo at the

---

<sup>16</sup> For the record, the Pareto work was not actually Homans' first book publication, because in 1930 the Massachusetts Bay Tercentenary Commission authorized the publication of Homans' Massachusetts on the Sea (co-authored with Samuel Morison), a work focusing primarily on "New England schooners and other sailing ships" (Homans 1984:107).

Harvard Graduate School of Business Administration<sup>17</sup> (and, importantly, being exposed along the way to the functionalism of social anthropologists such as Bronislaw Malinowski and A. Radcliffe-Brown); and working with Harvard historian Charles McIlwain, the result of which led Homans into the study of English constitutional history, culminating finally in the publication in 1941 of his book English Villagers of the Thirteenth Century.

Two twists of fate--one bad, one good--eventually led to Homans' gaining a professorship without benefit of the Ph.D. First, because Homans was a member of the Navy ROTC during his undergraduate days at Harvard (1928-32), he was eventually called to active duty in 1941. This necessitated, of course, the immediate resignation of his Junior Fellowship. Homans ended up serving for four years, returning upon completion of his military obligations to Harvard during the 1945-46 academic year.

Almost immediately upon his return Homans was awarded the position of associate professor, gaining tenure more or less on the strength of his various published books and

---

<sup>17</sup> Henderson played a prominent role in leading Homans to Mayo; that is, at the Business School Henderson and Mayo worked somewhat as a team, with Mayo heading the Department of Industrial Research (their primary concern being the human problems of work), and Henderson heading the Fatigue Laboratory (their concern being the physiological problems associated with work) (Homans 1984:135).

articles. This fortuitous turn of events came about because at the time Harvard was just launching a new department, conceived largely through the efforts of Talcott Parsons, which was eventually to become the Department of Social Relations. Parsons, along with other members of the advisory panel, decided to award two new tenured positions for the fledgling department, one going of course to Homans, the other, to Samuel A. Stouffer.

These series of events take us up to 1950 and the publication of Homans' first major work of sociological theory, The Human Group. The works discussed below represent the five most highly cited of Homans works as documented in the SSCI (and as reflected in Table 4.1), with one exception. Homans' English Villagers was in fact as heavily cited as the two articles (to be discussed below) which were eventually chosen for inclusion in the study, however, it was decided that this book has little or no relevance to sociological concerns. Rather, it was ostensibly a book on the history of English villages, not sociology per se. Thus, these five works are:

1) The Human Group: This 1950 book was Homans' most heavily cited work up until the release of Social Behavior: Its Elementary Forms in 1961. In fact, Human Group and Social Behavior together account for an overwhelming

percentage of total cites to his works.<sup>18</sup>

Reflective of his earlier training in anthropological functionalism, Homans provides field studies of five concrete, closely observed small groups, with the conceptual scheme revolving around the delineation of three classes of variables, namely, interaction, sentiment, and activity.<sup>19</sup>

It was Homans' contention that, as far as things appeared to him in the late 1940s, the social sciences had never adequately conceptualized or addressed the most basic, fundamental phenomena of the human experience, namely, the human group. His definition of "group" (Homans 1950:1) still stands as a fundamental building block in sociological theorizing, and many of those citing The Human Group do so with specific mention to this definition.

The usefulness of Homans' theory is that he provides straightforward, workable, and thus testable postulates and

<sup>18</sup> That is, the other three works included in this study--these being the two articles "Social Behavior as Exchange" and "Bringing Men Back In," along with The Nature of Social Science--contributed relatively little compared to the large number of cites garnered by Human Group and Social Behavior.

<sup>19</sup> These variables were originally posited, in somewhat different form, in one of Homans' earlier works, English Villagers of the Thirteenth Century. The primary difference was that in English Villagers the variable "activity" was referred to as "function" (Homans 1984:315).

hypotheses about group life from which further research may proceed. That is, by taking the three elements of behavior--sentiment, activity, and interaction--Homans shows how any component of group interaction is explainable in terms of some combination of these variables. Homans' entire analysis follows closely the classical deductive-nomological rules of scientific explanation, and in this sense his analysis is most certainly functional, as he attempts here to present merely a narrow group of statements of fact concerning observations of activity within small groups.<sup>20</sup>

2) "Social Behavior as Exchange": An important 1958 AJS article. As Mullins (1973:105) has suggested, the emergence of two books in 1950, Robert F. Bales' Interaction Process Analysis and Homans' The Human Group, represented the core theoretical works from which the exchange perspective would eventually attain paradigmatic status.

Much of the research which evolved out of these

---

<sup>20</sup> For an excellent discussion of the logic of functional analysis see Hempel (1965); his description of the deductive-nomological system of explanation (which Homans is here attempting to emulate) is as follows: the explanatory system contains 1) "explicans" or premises, these being a) statements of fact and/or b) general laws; and 2) the "explicandum," i.e. the conclusion which follows logically from the explicans (Hempel 1965:299). Of course, at this point in his program Homans had not yet considered the general laws from which the observed phenomena (or statements of fact) could be deduced.

inaugural efforts involved experimental or field studies of small groups, with researchers observing and attempting to take note of only a small range of variables (in order to ensure the applicability of rigid statistical analysis). This phase of progression within exchange theory--known as the "cluster stage" according to Mullins (1973:115)--ran from 1950 until approximately 1958, its major architects being Bales, Homans, Kurt Lewin, Paul Hare, Leon Festinger, Muzafer Sherif, and a handful of others.

But by 1958 the exchange perspective was already growing tired, its major intellectual efforts producing within the research environment little more than imitations of the types of psychological techniques (experimental design, small group observation) which had already been established years earlier. Thus, in light of this stagnation, it was only fitting that Homans would attempt a revitalization and renewal of the exchange perspective; this attempt was Homans' influential 1958 article "Social Behavior as Exchange."<sup>2 1</sup>

Homans' purpose in the paper was to address head-on some

---

<sup>2 1</sup> Many observers agree that "Social Behavior as Exchange" can be considered influential and even a citation classic. For example, one may witness an affirmation of this sentiment in James Coleman's (1986:1324) description of the article as "...Homans's (1958) classic paper on social exchange in small groups."

of the problems plaguing small group research. He did this by delineating three broad areas of concern: 1) a more concerted effort needed to be directed toward linking experimentally-derived results with those of quasi-anthropological field work. That is, it was Homans' contention that the propositions derived from experimental research had not yet been demonstrated to be consistently amenable to real life; 2) these propositions, however tenuous their status may be in light of the inadequate linkages between experimental and field researches, nevertheless need to be pulled together into some kind of fundamental, general statement. That is, researchers need to step back and aim for the articulation of only a small range of concrete propositions about group life which would be consistently derivable in any particular research setting. Homans' (1958:597) most important observation, then, is that the first aim of science is to provide a derivable set of propositions--however limited--pertaining to the phenomena under investigation; 3) those propositions which are derived from small group research and observation should eventually be shown to be derivable from a set of still more general propositions, that is, from empirical propositions which exist independently of and beyond the area of concern circumscribed by small group research. Homans goes on to suggest that this more general set of

empirical propositions would more than likely turn out to be the set of extant propositions of behavioral psychology.

3) Social Behavior: Its Elementary Forms: The 1961 book which represents Homans' most heavily cited work. As Homans (1961:8-16) explains, Social Behavior was written to be by and large an extension of the work which began in The Human Group, with at least one important difference. That is, the analysis presented in The Human Group was deductive in that the primary aim was simply to observe various small groups and, with no theoretical preconditions or assumptions in mind, then attempt to formulate propositions concerning what types of activities could be observed taking place within these groups. Homans' initial concern, then, was not to delineate why the observed patterns of behavior took place, only to establish that they do take place, in some consistent, definable pattern.

In contrast, Homans in Social Behavior takes the deduced and observed correlates from The Human Group in an attempt to arrive, inductively, at explanations of why the observed phenomena were happening. He attempts to link the general empirical propositions to an already extant set of propositions; as Homans (1961:10) explains, "The process of borrowing or inventing the more general propositions I call induction...." As he foreshadowed in his 1958 article, the more general set of propositions turned out (for Homans) to

be those contained within behavioral psychology, specifically the natural behaviorism of B.F. Skinner.

As I have drawn fairly heavily from Mullins' (1973) observations of the various theories and theory groups in sociology, one of my initial inclinations was to hold as accurate Mullins' suggestion that Social Behavior represented a huge disappointment within the exchange field because of Homans' decision to embrace Skinnerian behaviorism. As Mullins (1973:120) notes:

...Homans' Social Behavior: Its Elementary Forms (1961) was a great disappointment to those who had anticipated a synthesis [of sociology, psychology, and anthropology]. The author [instead] retreated to a psychological reductionist position in which he tried, unsuccessfully, to demonstrate that all sociological phenomena necessarily follow from psychological laws (in particular, those of behaviorist psychologist B.F. Skinner). The psychological position was unacceptable to most social psychologists, and the deduction itself, dubious. In short, the [promised] connection between field observation and experimental observation, applied work in group dynamics and therapy, and theoretical work in group processes, was just not made.

From these observations I anticipated that Social Behavior in particular and Homans' theoretical fortunes in general would not fare well in terms of my measure of influence, that is, as measured by the amount of citations which each author's works received. However, the results of my data analysis, which will be discussed more fully below, do not support Mullins' contention; in fact, as it turns out, Social Behavior was the most heavily cited work in the

entire study, more so even than Parsons' The Social System!

4) "Bringing Men Back In": Homans' 1964 ASA Presidential Address (published in ASR). Among Homans' published works, this article finished in fourth place, just behind "Social Behavior as Exchange," according to the cite counts provided by the SSCI.<sup>22</sup>

Coming on the heels of the first wave of Parsonian critiques, Homans' purpose was to present a repudiation of his earlier functionalist inclinations.<sup>23</sup> The critique of functionalism he offered up was strong and to the point, and as Homans (1964a:809) states, "For a whole generation it [functionalism] has been the dominant, indeed the only distinct, school of sociological thought. I think it has run its course, done its work, and now positively gets in the way of our understanding social phenomena."

This was an important work because through his critique of functionalism Homans was attempting to put greater and greater distance between the functionalist school and

---

<sup>22</sup> Surprisingly, Homans' (1962) book Sentiments and Activities did not garner as many cites as either of the two articles mentioned here, and thus was not included in the study.

<sup>23</sup> This article also served to answer Kingsley Davis's (1959) Presidential Address wherein he announced that sociological analysis and functional analysis were one and the same.

exchange theory. Much of the blurring of the boundaries between the two perspectives had been accomplished through the collaborative efforts of Robert Bales, a social psychologist primarily involved in small group research, and Talcott Parsons, especially in their successful volumes Family, Socialization and Interaction Process and Working Papers in the Theory of Action. As Bershad (1973:115) has noted, these collaborations can largely be understood as merely one component of Parsons' broad attempt (which began in 1945 with the formation of Harvard's Department of Social Relations) to forge a unified theory of social science. However, Homans would have none of this; instead, he gives notice that functionalism as a theoretical perspective never adequately met the requirements of scientific deduction. That is, as Homans (1964a:818) claims, functionalism has been largely descriptive, not explanatory.

5) The Nature of Social Science: A 1967 book which lays out Homans' ideas concerning, as the title implies, the nature of social scientific theory. The book is largely an extension of the arguments first raised in The Human Group and on through to the critiques of functionalism which were articulated in "Bringing Men Back In."

Homans again stresses the two main purposes which any theory purporting to call itself scientific attempts to

serve, these being discovery and explanation. "A discovery," suggests Homans (1967:8), "takes the form of a statement of a relationship between properties of nature." It is a simple statement of correlation, e.g., the more individuals interact with one another the more likely it is that these individuals like each other. This says something concrete about the tendency of persons to exhibit toward others certain specific frequencies of interaction, and although this observation would appear obvious and mundane, it nevertheless falls into the category of "discovery."<sup>24</sup>

Explanation, on the other hand, is the "...process of showing how empirical findings follow from, can be deduced from, general propositions under particular given conditions" (Homans 1967:79). And, as Homans again stresses, the general propositions of all social sciences are psychological, that is, all explanation reduces down to the behavior of individuals in society.

#### 4.4 Talcott Parsons

Of the four theorists being discussed here, Talcott

---

<sup>24</sup> This proposition concerning the tendency toward interaction can be stated more rigorously following Homans' (1961:54) second human exchange proposition: "The more often within a given period of time a man's activity rewards the activity of another, the more often the other will emit the activity."

Parsons has by far the largest corpus of published works, encompassing approximately 19 books and some 230 articles (Parsons 1978). But even in the face of such prodigious numbers one is able to isolate, upon consulting the citation rates as documented in the SSCI, a specific period in Parsons' career within which very high levels of citations to his works were attained.

This time frame, reflective of an extremely productive period for Parsons, spans the years from 1951 through 1960. The numbers, according to SSCI, indicate that of Parsons' five most cited works, four--The Social System and Toward a General Theory of Action (1951), Family, Socialization, and Interaction Process (1955), and Structure and Process in Modern Societies (1960)--were released during this period.<sup>25</sup>

Before reviewing the five books chosen for inclusion, however, a brief biographical sketch of Parsons is in order, especially in light of Gouldner's own biographical

---

<sup>25</sup> As reflected in Table 4.1, the other publication chosen for inclusion in this study, The Structure of Social Action, was released in 1937. It should also be mentioned that two other later works--1966's Societies: Evolutionary and Comparative Perspectives and 1973's The American University--were in close competition with Structure and Process in Modern Societies for fifth place among Parsons' most heavily cited works, with Structure and Process just pulling ahead of the other two in the final count.

account of Parsons.<sup>26</sup>

It seems safe to say that Parsons, born in 1902 in Colorado Springs, Colorado, experienced a solidly middle class upbringing. His father, Edward Parsons, a Congregational minister who was an active participant in the Social Gospel movement (Hamilton 1983:31), was the patriarch of a religious family whose moral base of life was ostensibly that of ascetic Protestantism. In addition, the elder Parsons served as president of Marietta College (a small Ohio college), and as Rocher (1975) suggests, because of these circumstances Parsons lived during this period of his life in an atmosphere of American midwest provincialism.

Parsons attended Amherst College in Massachusetts as an undergraduate from 1920-24, studying primarily biology with early aspirations of becoming a physician (Parsons 1970:826). But by his junior year, Parsons became, thanks to the influence of the unorthodox "institutional economist" Walter Hamilton, a convert of the social sciences. After graduating from Amherst in 1924, and with help from an uncle who provided financing for a year of study abroad, Parsons attended the London School of

---

<sup>26</sup> For a very fine and thorough biographical and intellectual account of Parsons through the publication of his The Social System in 1951, see Wearne (1989).

Economics, there being exposed to the writings of Hobhouse, Ginsberg, and Malinowski (Martindale 1982:62).

After only one year in London, Parsons accepted in the latter months of 1925 an exchange fellowship to Germany, and was assigned, with very little personal voice in the matter, to Heidelberg University (Parsons 1970:827). The appointment to Heidelberg was an extremely significant event which helped guide Parsons' intellectual development, for it was there that Parsons became exposed for the first time to the writings of Max Weber. Weber, who had died in Munich just five years earlier, was at the time a virtual unknown among American sociologists. Nevertheless, his influence at Heidelberg was, as one might expect, enormous.

Although not originally a degree candidate, Parsons (Ibid.) soon learned that the German Dr. Phil. degree would be attainable "...with only three semesters' credit, oral examinations, and a dissertation." Parsons thus embarked on the doctorate, with his dissertation, "The Concept of Capitalism in Recent German Literature," being written under the general direction and supervision of Edgar Salin.<sup>27</sup>

---

<sup>27</sup> Parsons experienced a mishap of monumental proportions during the writing of his thesis. While working on the thesis at Amherst Parsons would routinely send completed chapters to his »Doctorvater«, Edgar Salin, back at Heidelberg. After consultation with another committee member, Arnold Bergstraesser, the chapters were approved,

Parsons spent only the 1926 academic year at Heidelberg, returning to Amherst in 1927 as an instructor in economics while simultaneously working toward his degree in absentia. This time spent at Amherst allowed Parsons to think through and refine the major themes of his dissertation, these being 1) the nature of capitalism as a socioeconomic system (with concentration on selected German perspectives such as those provided by Karl Marx, Lugo Brentano, and Werner Sombart), and, of course, 2) the work of Max Weber as a social theorist (Parsons Ibid.).

From this point things began happening very quickly for Parsons: the head of the economics department at Amherst was so impressed with Parsons' work that by the autumn of 1927 a job had been arranged for him at Harvard University (Hamilton 1983:35); in early 1928 Parsons received his doctorate from Heidelberg (Martindale 1982:62); later that same year Parsons had his first article published: this article, "Capitalism in Recent German Literature: Sombart

---

with Bergstraesser finally mailing the documents back to Parsons at Amherst. But the chapters never arrived, apparently having been lost in the mail. So Parsons was promoted to Dr.Phil. on the basis of only the last, third chapter. The final title of the dissertation was »Der Kapitalismus bei Sombart und Max Weber. Inaugural-Dissertation zur Erlangung der Doktorwuerde einer hohen philosophischen Fakultaet der Ruperto-Carola-Universitaet zu Heidelberg«. (I am grateful to Harald Wenzel at Heidelberg University for providing me this information.)

and Weber, I," along with the companion installment released in early 1929, were taken almost verbatim from the third chapter of Parsons' dissertation. Then in 1930 Parsons released an English translation of Weber's The Protestant Ethic and the Spirit of Capitalism.

As we have seen, even though his original appointment at Harvard was in economics, in 1930 Pitirim Sorokin, head of the newly created (in 1929) sociology department, recruited Parsons as an instructor for the department (Sorokin 1963). However, Parsons continued pursuing his economics interests, releasing articles about Marshall, Malthus and Pareto during the years 1931-34. By 1934 Parsons had "discovered" Emile Durkheim,<sup>28</sup> discussing, for example, in his 1934 and 1935 articles "Sociological Elements in Economic Thought, I & II" Durkheim's revelations concerning the paradoxical relationship between on the one hand orthodox utilitarian economic theory and its assumption of progressive "societal happiness," contrasted against Durkheim's own observation that one particular negative indicator or measure of this alleged "happiness" in modern industrial society, namely, suicide rates, were not ebbing

---

<sup>28</sup> By "discovered" I mean that it was not until this time that Durkheim became a central figure in Parsons' theoretical scheme. However, as Wearne (1989:32) has reported, Parsons became aware of Durkheim's writings as early as 1922 while an undergraduate student at Amherst.

at all; in fact, they appeared to be rising.<sup>29</sup>

Parsons continued this theme in his 1935 paper "The Place of Ultimate Values in Sociological Theory," wherein he began moving toward a formulation of a "voluntaristic theory of action" vis-a-vis the attempt to synthesize the thoughts of Marshall, Pareto, Durkheim and Weber. This article was the precursor to Parsons' first major work, The Structure of Social Action. This book, along with the other four works chosen for inclusion in this study, will be discussed below.

1) The Structure of Social Action: As mentioned previously, this is Parsons' first book. As Parsons (1970:829-30) states in his autobiography, his thesis of the convergence of thought among four European writers--the avowed purpose of which was, of course, the construction of a theoretical framework by which Parsons' ideas concerning the modern socioeconomic order, capitalism and free enterprise could be given substantive or empirical

---

<sup>29</sup> Durkheim's critique of utilitarianism began in his Division of Labor in Society and extended into a later work, Suicide. Another key concept which Parsons worked into his early economic writings was Durkheim's treatment of the institutional framework of property and contract as contrasted against orthodox theories of the dynamics of economic activity per se (Parsons 1970:829). Durkheim's sociological analysis, then, articulated well with Weber's analysis of capitalism and Marshall's notion of free enterprise, with this complementarity in turn allowing for the linking of economic with sociological analysis.

legitimation or support--was to a great extent an incipient work in the sociology of science. To rephrase, Parsons' "analytical realism" was as much as anything an effort to formulate not only a unique conception of the nature and conditions of empirical knowledge, but also of the role of theory itself in that empirical knowledge. As he had been exposed to the writings of Kant at Heidelberg, Parsons came to appreciate the study of subjective states of mind and their role in contributing to the study of motives and meaning in human action (i.e., the problem of *Verstehen*).

Upon returning to the United States after his stint at Heidelberg Parsons was instantly struck by what he perceived to be several unacceptable strands of thought operating within the American academic milieu; these being what he described as "rampant behaviorism" and an overt hostility toward the notion that the interpretation of subjective states of mind had scientific validity (Parsons 1970:830). Besides these dominant intellectual traditions, Parsons also had to contend with empiricism and the continuing effort to extent the tenets of logical positivism into the social sciences.

As Bershady (1973) has discussed, although Parsons railed against utilitarianism, positivism and atomism, he was not arguing against these various approaches for the purpose of completely rejecting one analytical approach in

favor of another. Parsons was concerned, rather, with retaining the various approaches so that a selection between and from competing perspectives was possible; by attacking the problem in this manner Parsons believed he could stay true to Max Weber's own opinions concerning the cognitive validity of selection among available factual resources and information. For Parsons, then, the conceptual scheme which one chooses represents (as long as it corresponds with empirical fact) the most important aspect of theory building. In fact, Parsons had always taken to heart a dictum passed down from one of his great intellectual mentors, Lawrence J. Henderson, who stated that "A fact is a statement about experience in terms of a conceptual scheme" (Parsons 1970:830).<sup>30</sup>

2) and 3) Toward a General Theory of Action, the 1951 edited (with Edward A. Shils) volume which served as a theoretical companion to Parsons' other 1951 book, The Social System. These books are being treated together because their twin release was calculated to literally saturate and overwhelm the discipline, that is, the

---

<sup>30</sup> Parsons was, of course, also influenced greatly by A. N. Whitehead, whose concept of the "fallacy of misplaced concreteness" (Parsons 1937:476-77) further served to instill in Parsons the conviction that his convergence thesis and thus his analytical scheme was every bit as empirical or scientific as traditionally envisioned hard or "concrete" empiricism or positivism.

attempted forging of a cognitive consensus whose rallying points were ostensibly systems theory and functional analysis.

Fourteen years passed between the release of The Structure of Social Action and the 1951 twin release of the volumes being discussed here, and as might be expected a great many things happened in Parsons' life--both professional and private--which contributed to his move toward systems theory. One theme worth elaborating upon was Parsons' continuing interest in the "rationality problem" which had played a part in his analysis of modern industrial society as put forth in Structure. Of particular interest to Parsons was the status of the professions as a phenomena in modern society (Wearne 1989:123-127), especially in light of the various alternatives put forth in the context of the capitalism/socialism debate. What Parsons found intriguing was that no matter from which ideological position the problem was approached, both capitalism and socialism had their own versions of the "rational pursuit of self-interest," these being, as Parsons (1970:834) describes, the capitalist version whose utilitarian tradition emphasizes the interest of the individual in the satisfaction of wants, compared against the socialist version which emphasizes the interest of the collectivity (as articulated by Hobbes and Austin) in

securing the satisfaction of the public interest.

Thus, spurred on by his earlier interests in biology and medicine, Parsons' study of the "learned professions" naturally focused upon the medical profession. Henderson, who had played a prominent role in the trajectory of George Homans' professional career, also greatly influenced Parsons' own development; because he was both a physiologist and a great admirer of Pareto, Henderson had combined these interests in a 1935 paper entitled "Physician and Patient as a Social System." Thanks to Henderson, the idea that the professional complex might actually be seen as constituting a system (in the Paretan sense) of role relationships (physician and patient) led Parsons toward the formulation of the "sick role" and of systems theory in general.

Parsons then became involved in several participant-observation research projects, spending time, for example, with internists at the Massachusetts General Hospital. A further stint at the Boston Psychoanalytic Institute exposed Parsons to psychoanalysis<sup>3 1</sup> and, thanks to Elton Mayo, who had advised Parsons that in light of his

---

<sup>3 1</sup> Parsons actually entered, in 1946, formal psychoanalytical training at the Boston institution as a "Class C" candidate; however, because he did not have a medical degree, he never actually practiced psychoanalysis (Parsons 1970:840).

interests he would need to become more familiar with current and classical psychoanalytic theory, Parsons had an important encounter with the writings of Sigmund Freud.

So what began as an interest in the social situation of psychoanalysis per se (i.e., the doctor/patient relationship) evolved for Parsons into the study or explication of the theory of socialization (Parsons 1970:838). Parsons saw an immediate correspondence, for example, between the institutionalization of cultural norms at the social system level (i.e., role relationships) on the one hand, and the internalization of societal norms and values as a feature of the structure of personality as a system, on the other (Parsons 1951).

The key element which serves to tie together this brief discussion of the events leading up to the production of the books in question and the volumes themselves is Parsons' "pattern variable" scheme. This scheme, which plays a central part in the theoretical approaches employed in both Toward a General Theory and Social System, was originally formulated as an interpretation of the professions. In essence, Parsons had felt that the capitalism-socialism dichotomy served as an inadequate distinction--since they did not possess altogether antithetical views of rationality (that is, the distinction was made only along one dimension, namely, individual

versus collectivity)--for an analysis of the professions. The next logical step, then, was an attempt to expand upon Toennies' dichotomous societal types, namely Gemeinschaft and Gesellschaft. The role of "self-interest" was still of paramount interest for Parsons; thus, for example, the role of physician was described as "disinterested," and this professional orientation became in effect one side of the "affectivity vs. affective neutrality" pattern variable.<sup>32</sup> As the physician is supposedly interested primarily in the welfare of the patient, this is perceived upon first glance, then, as a Gemeinschaft category (Parsons 1970).

But somewhat of a paradox comes into focus when one considers that the universalistic character of cognitive rationality and the scientific component of modern medicine would seem to suggest that this complex of factors would have to be classified as Gesellschaft, not Gemeinschaft. What this meant to Parsons was that neither end of the Toennies dichotomy would suffice in articulating a thorough

---

<sup>32</sup> There were a total of five pattern variables; in addition to the one noted here the four others are 2) diffuseness vs. specificity: covers the range or scope of obligations in an interaction situation; 3) universalism vs. particularism: evaluative standards which apply in interaction situations; 4) achievement vs. ascription: judging an individual in terms of general performance criteria versus forms of inherited, non-performative standards (e.g. age, sex, race); 5) self vs. collectivity (Parsons 1951).

conceptualization of self-interest (as seen as imbedded within a matrix of collective sentiments).

Thanks to much of the interdisciplinary "theoretical stocktaking" which had been occurring since the creation, in 1946, of the Harvard Department of Social Relations, Parsons had been over the years in contact with such thinkers as Edward Shils and the psychologist Edward Tolman; these discussions centered around the elaboration and further systemization of Parsons' pattern variable scheme, the results of which was the volume Toward a General Theory of Action.<sup>33</sup>

We see, then, in light of the above discussion, that Parsons' companion volume The Social System attempted a further generalization and systematization of the pattern variable scheme, including an analysis of how the new scheme relates to the nature of the social system at the

---

<sup>33</sup> It should be noted that as far as the counting of cites to this volume is concerned, I counted only cites which either specifically referenced Part 1 or Part 2 (which were authored or co-authored by Parsons) or which merely provided a general cite to the volume [such as, for example, "Parsons and Shils (eds.) 1951. Toward a General Theory of Action.] In other words, for purposes of measuring Parsons' influence per se, cites to the volume were not counted whose sole authors were any of the various authors constituting Parts 3 and 4. This was necessitated in large part due to the fairly large number of cites Tolman's "A Psychological Model" (in Part 3) received, cites which for the most part were independent of or did not consider or discuss Parsons' contributions to the volume.

macrosocial level. The pattern variables thus represented the main theoretical framework for the analysis of the social system, and through this theoretical perspective Parsons was able to formulate and clarify the relations between social systems themselves, on the one hand, and personality and cultural systems, on the other (Parsons 1970:844).

4) Family, Socialization, and Interaction Process: The 1955 Parsons and Bales edited volume. Since the pattern variable scheme had taken such a central position in Parsons' analytical scheme, it was only natural that in light of the collaborative atmosphere engendered within the Department of Social Relations, Parsons would continue the attempt to extend and modify the scheme as inputs from outside sources dictated. These collaborations produced important modifications of the pattern variable scheme beginning in the 1953 Parsons/Bales/Shils volume, Working Papers in the Theory of Action. The major revision involved the deletion of the fifth, self-collectivity variable; this immediately led Parsons to suggest that his scheme coincided sharply with the four-factor formulation which Bales had produced in his Interaction Process Analysis. Out of this emerged the AGIL scheme, an exhaustive theory for the analysis of systems of action which specifies four functional requisites necessary for the survival of any

system, these being "adaptation" (A), "goal attainment" (G), "integration" (I), and "latent pattern maintenance" (or "tension management") (L).

Since the socialization process had been a continuing focal point in Parsons' theoretical work, and with the insights gained from Bales and his work on small groups, it was only natural that Parsons would turn to the analysis of the family, as it represented both a ready-made small group as well as the obvious starting point in the socialization process.

Parsons' (1970:844-45) own words best describe the primary purpose of the Family volume: "Its main theme was that the nuclear family, emergent in modern industrial societies, could be treated as a small group and differentiated according to the four-function [AGIL] paradigm, on the axes of generation and sex, in ways closely analogous to the pattern of differentiation of many of the small experimental groups with which Bales and his associates had been working."

This was an important volume because it represented Parsons' most fully articulated application of the still fledgling AGIL scheme; as such the volume generated much interest and in turn was appropriated, largely for theoretical purposes, by a wide range of researchers working primarily within the fields of marriage and

family.<sup>34</sup>

Before leaving this discussion, however, it should be noted that Peter Blau (1956) has made an extremely cogent point in his review of the Family volume. That is, regarding their cross-cultural analysis of 56 societies, Parsons et al. proudly proclaimed that 46 of these societies do indeed reflect nuclear family differentiation along the dimensions of instrumental and expressive roles, and by all rights these figures should serve to confirm their hypothesis. However, the very fact that ten of these societies do not exhibit an instrumental-expressive role differentiation seems to suggest that such a differentiation is not a necessary condition of the family's existence. This then raises the question that perhaps the authors' overriding concern with explicating

---

<sup>34</sup> This fact is nowhere more apparent than in Chafetz's negative assessment of Parsons' general theoretical program. That is, as Chafetz is working from a feminist epistemological position she is, not surprisingly, wont to make the point that much of the gender role biases of medical science have crept, unfortunately, into behavioral sciences such as psychology and sociology. The attempt, then, had been to pass off these stereotypical gender roles as "scientific truths," and as Chafetz (1978:102) continues, "From World War II until the late 1960s, sociology was dominated by the theoretical approach known as functionalism, which is associated primarily with Talcott Parsons. Perhaps nowhere was the influence of this orientation greater than in the field of marriage and the family, in which it remained dominant long after the rest of the discipline increasingly questioned it."

the universal conditions or functional requisites of systems of action has blinded them to addressing the more fundamental question concerning the possible variations between the relevant social structures themselves. What Blau (1956:489) was intimating, of course, is that Parsons had very possibly not adequately formulated conceptions of the social structures or processes of development (including their boundaries) of the various systems in question, namely personality, culture and social systems.

5) Structure and Process in Modern Societies: A 1960 compilation of ten essays covering the period from 1953 through 1960. These essays represent the important extension and modification of Parsonian systems theory which began with Working Papers, continued on through the volumes Family and Economy and Society, and which culminated in 1958, a highly productive year for Parsons in which eight articles were published, five of which were included in the Structure and Process volume being discussed here.

The book is organized around four major themes: 1) the analysis of formal organizations; 2) social structure and economic development; 3) structure and process in political systems; and 4) the structural setting of some social functions (specifically, those of community, medical education, and religious organization). Collectively the

works represent Parsons' thoughts on the nature of modern industrial society, and especially how the various subsystems (e.g. economic, political) are related to one another and the place of administrative organization in such interrelations (Udy 1960).<sup>35</sup>

It should also be noted that two articles in particular from this volume, namely "The Distribution of Power in American Society"<sup>36</sup> and "Social Strains in America," attracted the ire of Gouldner. That is, Gouldner's (1970a:286-338) chapter 8, "Parsons on Power and Wealth," focused on these articles as evidence of Parsons' political and ideological conservatism. But in his zeal to prove conclusively Parsons' bourgeois inclinations, his elitist

<sup>35</sup> Although Udy's review of the volume is largely positive, Clark (1960) believes that although it was a noble effort to apply "some theoretical perspectives of sociology" in areas not normally thought to be subjects amenable to sociological analysis, he suggests nevertheless that Parsons' over-reliance on Weber's institutional theory gives the reader the feeling that Parsons had not really given serious consideration to the pertinent large body of literature--particularly in the area of economic development--which had arisen since Weber's day.

<sup>36</sup> This particular article served as Parsons' critique of C. Wright Mills's The Power Elite; the simple fact that Parsons was critical of several features of Mills's analysis was evidence enough for Gouldner of Parsons' elitism and conservatism. (Parsons' main lines of criticism, by the way, were Mill's "zero-sum" conception of power, his view of power as inherently "evil," and that those possessing power could be characterized as embodying a "higher immorality.")

bias, and so forth, Gouldner opens himself up to some glaring mistakes in interpretation. My purpose in discussing these points here is twofold: One, I want to stress again that for the most part Gouldner's analysis is sound, and that his attempt to come to grips with the place and role of Parsonian theory in what he perceives as the inexorable, rapidly approaching coming crisis in sociology is admirable and worthy of investigation in its own right. Second, however, because we all must carry our own theoretical predilections and biases into any scholarly work, Gouldner's analysis occasionally makes painfully apparent the precarious nature of such an ideological critique, because at times reasoned judgment gives way to faulty and occluded analysis. One example (although there are several from which to choose from chapter 8) will serve to make my point.

In analyzing Parsons' conception of power, Gouldner (1970a:317-20) suggests that his formulation is essentially compatible with the development of the Welfare State; that is, in light of the continuing loss of centralized power of the business elites, Parsons sees an expanding role for the government toward greater control of the formerly nearly unregulated free economy. As Gouldner (1970a:317) quotes Parsons, "We must have a stronger government than we have traditionally been accustomed to, and we must come to trust

it more fully."

But, suggests Gouldner, the key here is that Parsons, although calling for the strengthening of the central government, is not simultaneously calling for a concomitant expansion of electoral power and rights. This implies for Gouldner, then, that Parsons is at least tacitly attempting to legitimate those ruling elites--whom at the time were largely wealthy Republicans--already in power; that is, Parsons is calling for the strengthening of the ruling elites rather than the general polity, that is. the average American citizen-voter. What this country needs according to Parsons, at least as Gouldner sees it, is increased governance by "Harvard-type men."

And although Parsons states that the changed situation places new demands on a far-reaching change in American social structure, this change is not really anything new at all, according to Gouldner. This change, in fact, merely "entails acceptance of and accommodation to the traditional power of business as well as a movement in the direction of a more diversified power elite" (Gouldner 1970a:318). When all is said and done, then, Parsons envisioned a newly politically empowered business elite, one which would have "veto power" in the face of the increased autonomy of the government. That is, the veto power of business elites is important in checking the continuing move toward

differentiation of power.

But Gouldner has quite literally missed the entire point of Parsons' paper. That is, although Gouldner is concentrating here on Parsons' "Social Strains in America" in making his argument, nowhere does he acknowledge that Parsons' discussion revolves around the phenomena of McCarthyism. Parsons is, in fact, arguing against the conservative, neofascist implications of McCarthyism; he is saying that the emergence of such a phenomena has uncovered some deeply rooted structural strains in modern American society. Parsons is saying that we as a nation, with the realization of the increasingly important role which the United States has played in world politics since the middle 30s, have not handled well this new, "greatly enhanced level of national political responsibility" (Parsons 1960:226). The strain of the time is symptomatic of a perplexed, not altogether well articulated national response to the twin rise and threats of Communism and Nazism. With national interests concentrated largely upon the military aspects of these two ideological/political manifestations an "obsessiveness" set into the American psyche which, in face of the ideal negative symbols which Communism and Nazism provided, led to the elevating to positions of primacy questions of American "loyalty" against such perceived dangers.

The slowly evolving attitude-structure which emerged out of this, the "changed situation" which Parsons is addressing here, then, is McCarthyism. When Parsons speaks of the need to regain "our national self-confidence" and to take active steps to cope with the changed situation, he is directing his attention specifically to this particular crisis of confidence, to the baffled, perplexed reaction to Communism which led a nation, as symbolized by Joe McCarthy, to seek relief by "hunting scapegoats" (Parsons 1960:246). But nowhere does Gouldner acknowledge this fact (nor does he even mention McCarthy); and nowhere does he mention Parsons' strong criticism of McCarthyism (presumably because it would damage his claim of Parsons' conservatism). It is an egregious omission on the part of Gouldner, one which serves exceedingly well his overtly stated ideological ends: the attempt to prove unequivocally the conservatism of Parsonian functionalism.

## V. INTERPRETATION OF FINDINGS

This study began with the signal hypothesis that several postulates put forth by Alvin Gouldner in his Coming Crisis of Western Sociology are worthy of empirical testing. In particular, we have been concerned primarily with Gouldner's assertion that, as he saw things in 1970, the discipline of sociology would soon witness the decline and loss of hegemony of its dominant theoretical perspective, namely, Parsonian structural functionalism. In addition to this hypothesized decline in Parsons' theoretical fortunes, Gouldner suggested that several competing theories would emerge relative to the decline of functionalism, thereby forging within the discipline a new theoretical polycentrism.

The key factor in Gouldner's analysis, then, is his assertion that Parsons' "influence," once the dominant intellectual force in Western sociology, would for a variety of reasons wane. As discussed above, the operationalization of "influence," that is, the way we have decided to measure this phenomena, is by way of citation analysis. Further, it was decided that the data (i.e., the total cite counts to each author's work) would be drawn

from three prominent sociological journals: two core macro-oriented journals (American Sociological Review and American Journal of Sociology), and one social psychology journal (Social Psychology Quarterly). The investigation covers the citing years 1960 through 1989; the data introduced into the study represent cites to five selected works from each theorist as extracted from the above mentioned journals. Our theory suggests that these cite totals, both the absolute totals and the ways in which the numbers vary over the years, give a good indication of the levels of influence each theorist has enjoyed over the years in question. This evidence of influence can then be used to either confirm or disconfirm this particular component of Gouldner's hypothesized crisis in sociology which we have defined as problematic.

Our primary working hypothesis, then, has been that Gouldner was correct about the decline in Parsons' influence over the years. Further, it is suggested that our data will allow us to isolate the year or years in which the decline, if any, becomes apparent. Also, by simultaneously analyzing the cite patterns to the works of Homans, Goffman, and Garfinkel over the same period we are in a position to test empirically Gouldner's hypothesized move toward theoretical polycentrism. That is, the data should provide a schematic for assessing the relative

strengths of the various theory groups and paradigms in competition within the discipline, these being functionalism, exchange theory, dramaturgy, and ethnomethodology. According to Gouldner, the domain assumptions of ethnomethodology and dramaturgy in particular should theoretically resonate well with the newly emergent sentiment structure of sociology. In light of this, the data should provide a confirmation or disconfirmation of this hypothesis as well.

I will begin the data analysis with a broad overview of the findings vis-a-vis the explication of the raw cumulative citation counts. I will then break down the data into specific parameters of interest: First, brief summaries of the findings as they relate to each journal will be provided. Second, a review of the findings pertaining to each theorist's (Parsons, Homans, Goffman, and Garfinkel) work will be elaborated. Along the way I will refer to various graphs and tables which will help provide visual insights to and illuminations of the data. Finally, an attempt will be made to explain what the findings mean as relating to and in terms of Gouldner's own observations and predictions.

### 5.1 Overview

This section provides a presentation of the cumulative

citation totals to each author across the three chosen journals. Again, it should be emphasized that from the beginning one of this study's primary methodological goals was to measure influence as fully as possible across both the macro and micro realms. Since we have already shown that two of the highest impact sociological journals, namely American Sociological Review and American Journal of Sociology, are by all rights considered macro-oriented, the next step was to include the highest impact micro-oriented sociological journal as well. As was discussed in chapter III, Social Psychology Quarterly turned out to be the best candidate to fulfill this last requirement, that is, to bring the fullest coverage to our measurement of influence by augmenting the core macro journals ASR and AJS with the micro journal SPQ.

The choice of SPQ from among the long list of sociology's other specialty journals should not be seen, however, as an egregious and indefensible example of selection bias. For validation of this point, I return once again to the precedence which Mark Oromaner (1979) has established in regard to the selection (and justification for same) of non-core sociological journals in diachronous citation studies of the sort we are herein engaged. That is to say, when we attempt to forge a methodology by which a citation analysis is employed to measure influence we must,

in order to control for the growth of the literature, necessarily limit the number of journals from which to cull our citation data. Granted, this move imposes in its own right an immediate but unavoidable selection bias onto the study. But, on the other hand, it does not signal the invalidity of the study. As we have discussed in chapter one, any scientific theory necessarily introduces to some degree a "selection bias" inasmuch as a conceptual scheme is imposed onto reality so that the phenomenon of interest can be broken down or sliced into manageable components. Thus, we realize that the best we can do is give justifications for selecting the journals which are ultimately chosen for inclusion.

To reiterate, it appears safe to say that when we are dealing with the topic of sociological theory any citation analysis so defined must, as described above, limit the journal universe for purposes of controlling for literature growth. At the very minimum, then, ASR and AJS would surely have to be included in a study of general sociological theory which concerns itself with the measurement of influence of any particular theorists of interest. Besides ASR and AJS, Oromaner (1977, 1979) has described the journal Social Forces (SF) as well as being a core sociological journal. In other words, it would appear safe to choose any or all three of these publications as

representative of core, macro-oriented sociological journals.

But we should not of course be limited to only these three core journals when conducting a citation analysis. As intimated above, Oromaner has shown that it is valid to choose other journals which serve the purposes of the particular research problem. So when Oromaner (1979) attempted, for example, to gauge via citation analysis how Parsons' works had been utilized within the sociological community during the years 1936-50, he chose from the publication universe four specific sociological journals. Three of those chosen were, as one might expect, the three core journals, namely, ASR, AJS, and SF. However, the fourth journal included in his study, namely Sociology and Social Research, is a so-called "specialty" journal chosen to act as a corrective against the regional biases represented by the three previously mentioned journals.<sup>1</sup>

The important point to remember here is that Oromaner introduced a non-core journal into his study for the purpose of balancing or counteracting a particular bias present in the core journals. Additionally, he showed that one can indeed combine data from all chosen journals

---

<sup>1</sup> Please see my discussion in chapter three for an elaboration of Oromaner's (1979) study.

particular to that study so that cumulative comparisons and totals may be made across journals. Following Oromaner's example, I too have chosen a non-core sociological journal to offset a particular bias present in the core journals ASR and AJS. That is, to offset the macro-orientedness of ASR and AJS I have chosen the micro-oriented journal Social Psychology Quarterly.

As far as the cumulative data are concerned, the 20 works (5 from each theorist) included in the study were cited a grand total of 1,196 times in the three selected journals over our thirty year citing interval (1960-89). Of these, Talcott Parsons garnered 455 cites (or 38% of the total), George Homans 377 (31.5%), Erving Goffman 272 (22.7%), and Harold Garfinkel 92 (or 7.7% of the total).<sup>2</sup>

---

<sup>2</sup> These totals may not seem impressive, i.e. over the 30 years of the study the average number of citations per year for each author was as follows: Parsons 15.12; Homans 12.56; Goffman 9.07; and Garfinkel 3.07. By comparison, Cole and Cole (1987:349) have reported that as of 1970 data the average number of citations to any cited scientist (that is, to their entire corpus) was slightly less than 10 per year. At first glance, however, the Cole and Cole estimate appeared to be much too high, especially in light of Braxton and Bayer's (1986:35) findings which report that of all cited articles published in Science Citation Index (SCI) journals, more than half were cited only once over the next decade. Also, of all articles cited in SCI journals, only about 8% are cited 10 or more times (Braxton and Bayer 1986:36). However, these data only refer to the probability or frequency of any single work being sighted. My above totals are concerned with the cumulative cites which the authors of interest have received in this study's selected journals. So in this light the Cole and Cole and Braxton and Bayer findings are not incompatible; the former

When broken down into five year intervals, the data clearly indicate Parsons' domination during the first citing period (1960-64). (See Table 5.1 below.) This finding seems to confirm Gouldner's suggestion that Parsons was indeed a major figure, was heavily cited and thus influential during this period.

Table 5.1. Total Cites to Five Key Works of Each Author in Three Selected Journals, by 5-Year Intervals

	60-64	65-69	70-74	75-79	80-84	85-89	Totals
Parsons	123	73	89	76	46	48	455
Homans	60	82	79	72	38	46	377
Goffman	27	42	40	67	40	56	272
Garfinkel	1	14	14	22	11	30	92
Totals	211	211	222	237	135	180	1196
%	17.6	17.6	18.6	19.8	11.3	15.1	

One should contrast immediately Parsons' domination of the first five year period with what the data reflect for

is talking about total cites to an author's corpus of work, the latter, to the probability of any one work being cited. Both works seem to verify the fact that since our data indicate that Parsons, Homans and Goffman averaged around 10 or more citations per year to their five chosen works, this is actually quite an impressive accomplishment.

the period 1965-69, for it is here that the first indications of the decline of Parsons' theoretical hegemony becomes apparent. In the first period Parsons accounted for 58.3% of all cites; in the second period this dropped to 34.6%, with Homans actually pulling slightly ahead in total cites. We also see a rise in cites to the works of Goffman and Garfinkel, although both still lag relatively far behind the other two theorists.

By the third period (1970-74) Parsons regained a very slight lead on Homans, garnering 40.1% of total cites compared to Homans' 35.6%, with Goffman and Garfinkel still lagging behind. The fourth period (1975-79) provided the highest number of total cites (237) to all theorist's works; this was due largely to the fact that the latest published work included in this study--Goffman's Frame Analysis--was released during the preceding five year period (in 1974). Thus, this period represents, besides the combined effects of the other 19 works, the initial burst of citation activity which accompanied the release of the then very new Frame Analysis. Consequently, although Parsons still retained a slight lead, Goffman enjoyed a surge in cites, gaining a total of 67 (28.3%), compared against Parsons' 76 (32.1%) and Homans' 72 (30.4%). This period also comes closest to what Gouldner would refer to as "theoretical polycentrism," because we see that these

three theorists are very close to one another in our measure of influence.<sup>3</sup>

One should also notice that the period 1980-84 was very hard on the four theorists, as they garnered all told only 135 cites. In fact, as the graph in Figure 5.1 indicates, there was a precipitous decline of all four theorists from the late 70s through the early 80s, with only a modest upturn after 1984 returning the theorists to fairly respectable levels of appropriation.

There are at least two reasons, I believe, for this drop in influence. One, we must realize that obsolescence plays at least a part in the simultaneous decline in rates of citation for the four theorists. When we keep in mind that the most recent publication (Erving Goffman's Frame Analysis) was released in 1974, and also considering that references to monographs in social science journals have a

---

<sup>3</sup> Granted, it appears at least initially that the last period (1985-89) would seem a better candidate for the label of theoretical polycentrism since Garfinkel finally rises to a level comparable to the other three. But I believe by the period 1980-84 and beyond all four theorists had passed their citation utility or effectiveness due in large part to literature obsolescence. The great majority of the cites garnered in these last two periods are peripheral and/or neutral cites in which the theorists and their work are viewed merely as citation classics or other such modes of veneration. In other words, by 1980 all four theorists, judging by the nature of the cites, had fallen far out of being considered as integral factors with regard to the various research fronts.

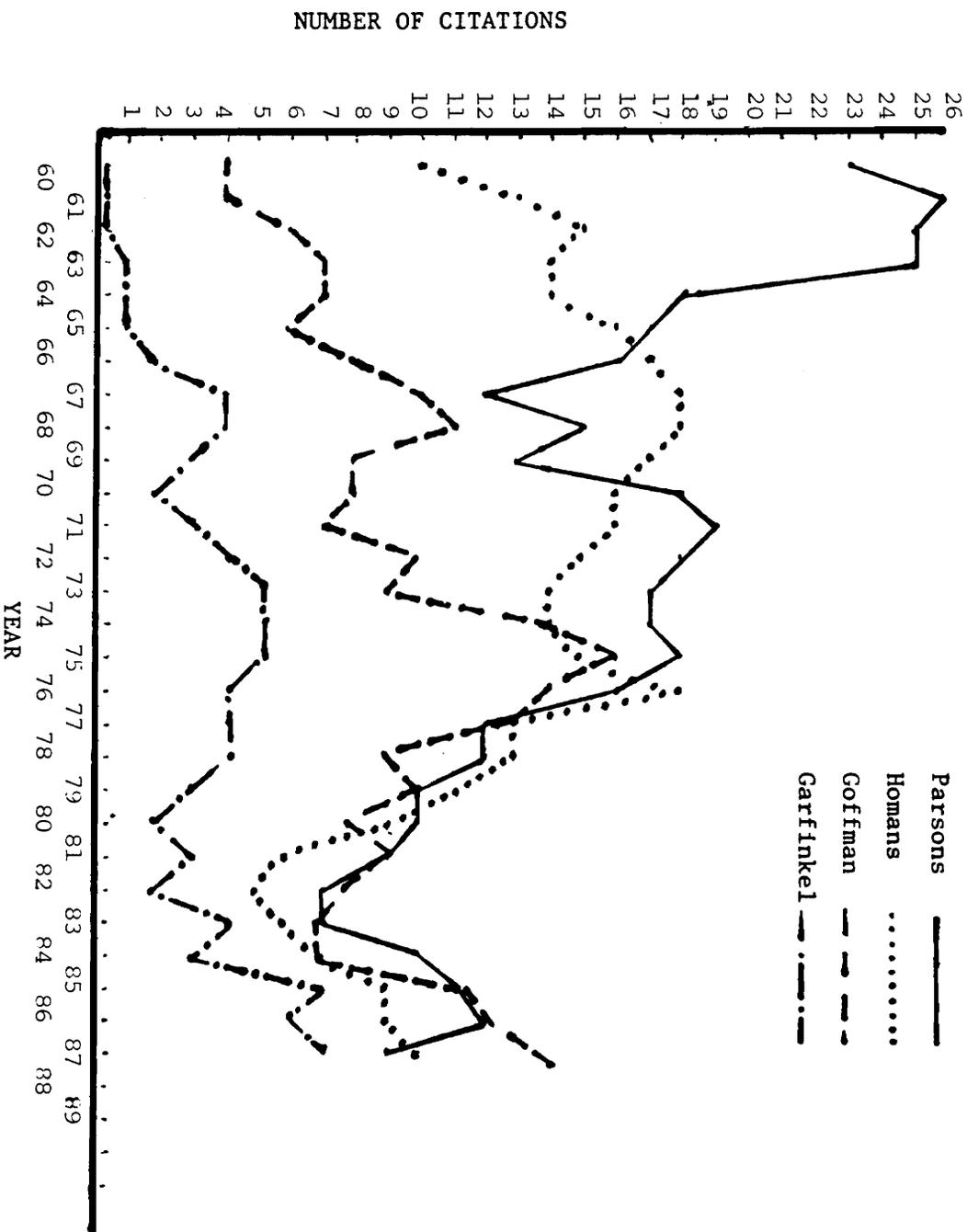


Figure 5.1. Total Citations in Three Selected Journals to Each Author's Chosen Works, 1960-89

half-life of approximately ten years (e.g. Line 1979; Oromaner 1986, 1977; Brittain and Line 1973; Avramescu 1979; Aversa 1984; King et al. 1981), we observe that the majority of the works being cited after 1977 had already passed the prime citation years or period of "initial burst," as Price's terminology suggests. Two, as Oromaner's (1986) study indicates, beginning in 1978 it appears that major sociological journals such as AJS, ASR, and Social Forces started moving toward the publication of more empirically-driven and less overtly theoretical articles. As Oromaner (1986:34) states, "...sociology as a discipline is increasingly rewarding [as measured by citations] empirical and numerical data studies." This is important because Parsons, Homans, Goffman and Garfinkel are first and foremost sociological theorists; as my data indicate, they tend to be cited overwhelmingly, especially in the later years (after 1970) in terms of the types of contribution each has made to their respective theoretical programs.

Also worth noting, as Peritz (1983) has discussed, is the fact that methodological papers and monographs tend to be more highly cited in sociological journals than either theoretical or empirical works. Now, corroborating the insights provided by Mullins (1973) in his discussion of how theory groups come together, flourish, and eventually

decline in the discipline of sociology, one of the most important factors in the early and successful building of a theoretical school or tradition is the formalization of a new or novel methodological technique which serves to identify the unique stamp of the particular theorist and/or theory group. Thus, when Parsons released The Social System and the companion volume Toward a General Theory of Action in 1951, Parsons and the structural-functional school effectively produced strong program statements which brought together theoretical orientations and, most importantly, augmented these with methodological techniques which served to provide guidelines by which research could be carried out at the research front. Thus in these early stages (in Parsons' case, 1960 through about 1965) citations to his works were largely methodological as opposed to merely theoretical; that is, they were not citing Parsons as merely a theoretical classic, they were also utilizing his works within clearly defined methodological frameworks.\*

These same patterns of citation tendency apply to the

---

\* For example, Gutman (1960) cites Parsons' Family, Socialization, and Interaction Process as contributing substantially to the empirical study of population; Dubin's (1960) extension of the Parsonian pattern-variables helped bring greater quantification to the postulates; Bock (1963) suggests that The Social System contributes new insights into social change via functional analysis; and so forth.

other theorists as well: in the early years, besides being cited among theoretically-oriented works, the authors tend as well to be in the forefront of discussions pertaining to the implementation of the theories within the research environment. As the literature ages, the citations change from reflecting the immediate needs of those on the research front toward the obligatory citing of the authors in either theoretical or empirical papers as "classic thinkers" in the field.

Finally, I should say that, judging by the trend of articles in the journals included here, there seemed to be a "silent revolt" against theory (which would be in keeping with Oromaner's [1986] work) during the years from the late 70s through the early 80s. Combined with the general obsolescence trends each theorist was experiencing during this period, one sees why the citations dropped so precipitously during the period.

Now, granted, as figure 5.1 indicates, since the graphs reflect the combined effects of each author's works, we do not get a truly clear picture of the appropriate decay curves. This is why I have included, in figure 5.2, graphs of the most heavily cited works from each theorist. Whereas in figure 5.1 the x-axis represents year of citation from 1960-89, in figure 5.2 the x-axis indicates number of years since release of the book. The y-axis indicates, of course,

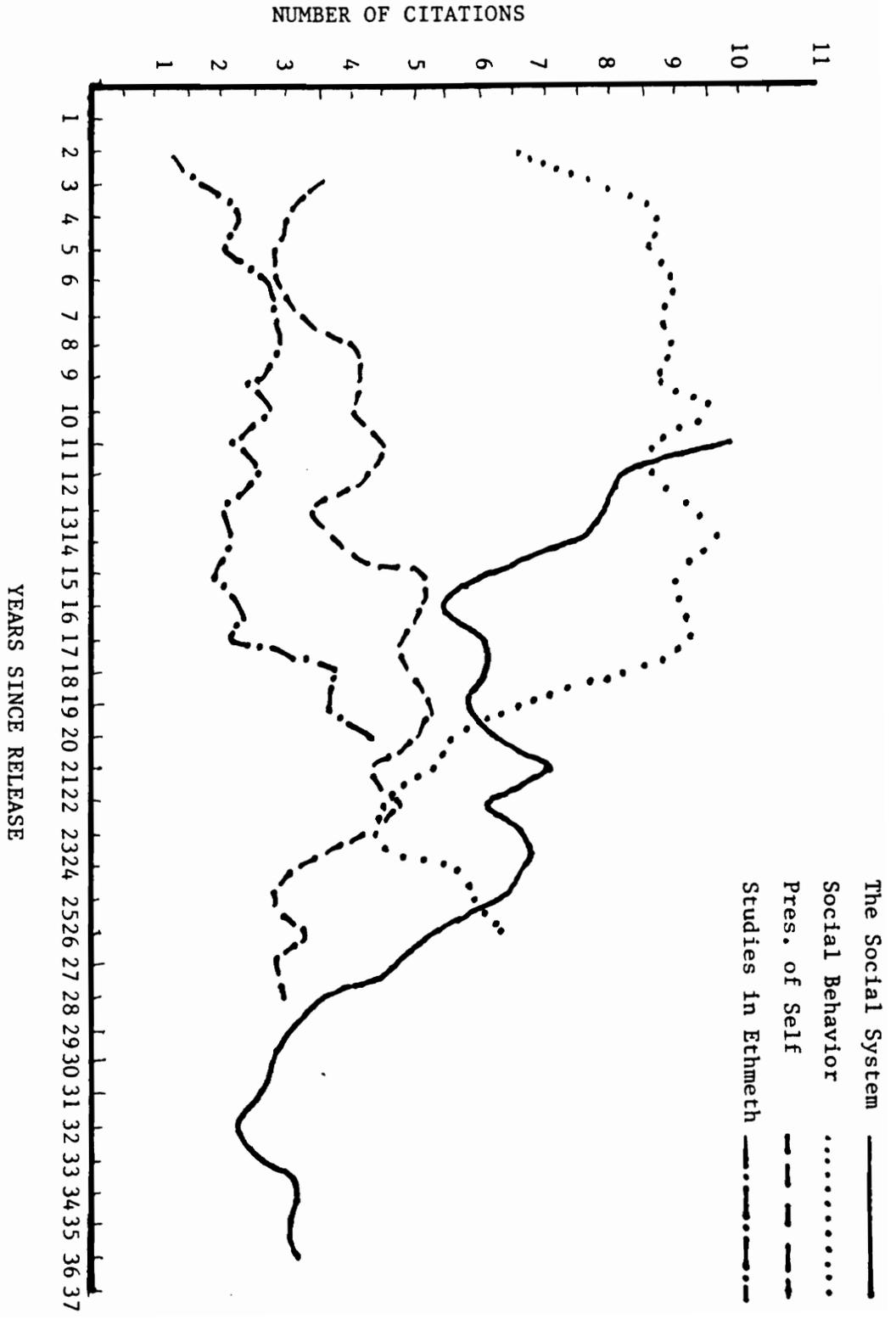


Figure 5.2. Authors' Single Most Cited Work in Three Selected Journals, by Years Since Release

citations to each work. Also, it should be noted that when we are dealing with combined works (as in the previous graph) the cumulative data allow for more data points which in turn requires less "smoothing" of the data. Thus, in figure 5.1 I used three year rolling averages; here in figure 5.2 it was necessary, since we are dealing with only single books, to use 5-year rolling averages for purposes of smoothing the data.

Note that for Parsons The Social System was his most cited work; likewise for Homans it was Social Behavior: Its Elementary Forms; Goffman The Presentation of Self; and for Garfinkel Studies in Ethnomethodology. Note that here we are given a much better indication of the type of decay patterns which we would generally anticipate. The Social System is especially typical: since it was released in 1951, we are missing the first ten years or so of its citation history. As we pick it up 11 years after its release we see that it is beginning its descent (which is typical of a ten-year half life), and then gradually dissipates over the years. Notice also that had we tracked the work from zero years after release, that is, from year of release, it would have undoubtedly arched well above the levels it had attained by its eleventh year. Oromaner's (1979) work indicates that indeed such was the case, i.e. from 1951 through 1960 (our missing years) levels of

citation were quite high for The Social System.

George Homans' Social Behavior was this study's most heavily cited work. Notice that from year 2 through year 17 (that is, since release date) the book maintained fairly high citation levels. This is because Homans' theory of distributive justice and his emphasis on small groups was highly amenable to the types of experimental research being conducted in Social Psychology Quarterly. Also, Homans' theories are easily axiomatized and converted into mathematical language, thereby allowing his theoretical formulations to be appropriated by empirical researchers as well.

Erving Goffman's Presentation of Self is possibly the most puzzling curve of the four treated here. It does not really reflect the classic pattern of initial burst of citing activity and then gradual fall off after ten years or so. Rather, it is a fairly flat curve which indicates that the work did not generate (at least in comparison to Parsons and Homans) as much interest among authors publishing within these particular sociology journals as might have been expected.<sup>5</sup> This could very well be

---

<sup>5</sup> Avramescu (1979: 298) has offered graphical representation of the four ways in which journal citations typically are manifested. Goffman's curve falls between Avramescu's "basic recognized work" (which is the classic decay curve which we usually see), and his "scarcely reflected work," which rises slightly after release and

reflective of the multidisciplinary nature of Goffman's work. If we compare our numbers to the general SSCI data on Presentation of Self reflected in table 4.1, we see that the work did indeed generate good numbers (300+ cites) in the social science literature in general, which indicates that the work was apparently cited greatly beyond sociology proper.<sup>6</sup>

Finally, we see that Garfinkel's Studies in Ethnomethodology is the least cited of the four most cited

---

maintains a somewhat low level throughout the duration of its citation life. Also, although Goffman's curve does not look overly impressive in comparison to those of Parsons or Homans, it should be kept in mind that we are dealing here with theorists of "superstar" status. That is, Goffman's and even Garfinkel's levels of attainment are, in comparison to how the average social scientist is cited, actually quite high.

<sup>6</sup> Actually, there is some evidence, besides the SSCI data, to support the conviction that Goffman has been heavily cited outside of the confines of the sociological literature in general and our three journals in particular. For example, in his analysis of the 100 books most cited by social scientists from 1969-77, Garfield (1980b) found that Goffman's Presentation of Self, with 612 total cites, finished sixth among those books cited by social psychologists and motivation theorists. (By comparison, Homans' Social Behavior finished third with 674 cites.) Among sociologists, Goffman's Asylums finished third by garnering 895 cites; compare Parsons' Social System, which finished fourth with 751 total cites. Finally, in terms of the 100 most-cited social science authors (again from 1969-77), Garfield (1980c) reports that within sociology Parsons finished first with 5,600 total cites, Goffman was second with 3,473, and, for comparison, Merton finished third with 3,030 total cites.

works (but still cited relatively heavily in comparison to the average theorist). Released in 1967, the work remained at lower levels than even Goffman's work at comparable periods after its release; however, it should be noticed that after its seventeenth year of release the book began making a bit of a move and nearly approached the levels which Goffman and Homans attained in the same relative period. Even at its lower overall level, this is still quite an accomplishment. As Price (1976) has discussed, it is very rare for a work's citation totals to rise so long after its release.

Also, when I cautioned in an earlier section that Garfinkel may not be adequately represented since four of his five works are articles (because the rate of obsolescence for articles is much higher than that for monographs), I alluded that as it turned out, this mixing of various types of literature is not problematic for purposes of our study. And this is why: as illustrated here, even Garfinkel's most heavily cited work--Studies in Ethnomethodology--still falls well below the other four theorists' levels of attainment. Thus the combining of differential obsolescence rates, i.e. by combining citation data from monographs and articles, does not adversely affect our understanding of the processes at work with regard to these particular theorists.

## 5.2 American Sociological Review

As discussed earlier, the American Sociological Review (ASR), is consistently one of the top three ranked sociological journals by Impact Factor. This journal, along with American Journal of Sociology, represent what we might call, following Oromaner (1981), the core, macro-oriented journals of "establishment sociology," with ASR representing the Eastern establishment, and AJS representing the Chicago school orientation.

As Wilner (1985) has shown, ASR has published an average of 50 articles per year since 1957.<sup>7</sup> Since they publish six issues per year, this comes to about 8.3 articles per issue. These numbers are changing somewhat: from 1957-75 ASR published 966 articles, an average of 8.47 per issue; from 1976-82 345 articles were published, which averages out to about 8.2 articles per issue. Since 1982 approximately 8 articles have been published per issue.

---

<sup>7</sup> As Line and Roberts (1976) have shown, ASR's average of 50 articles per year is fairly typical of social science journals in general. For example, as of 1976 the Check List of Social Science Serials (CLOSSS) indicates that the average social science journal produces about 47 articles a year. Also, ASR and AJS are somewhat unique in that they publish six times yearly: as Line and Roberts (1976:132) have indicated, the most common frequencies of publication are quarterly (28%), annually (20%), and monthly (15%). Bi-monthly publications, such as ASR and AJS, account for only about 10% of all serial titles.

Since the page numbers have remained relatively constant over the years, what this implies is that articles are getting somewhat longer, thereby reducing slightly the total number of articles being published per issue.<sup>8</sup>

Table 5.2 (p. 175) provides a breakdown of the total citations to each author's works by journal.<sup>9</sup> Much of Gouldner's analysis hinges on his suggestion that Talcott Parsons was, at least through the sixties, the one theorist most closely associated with or representative of establishment or academic sociology. Thus we expected to find a good showing by Parsons in ASR; this in fact turned out to be the case. Parsons' Social System was the most frequently cited work among the twenty being tracked here, and his 236 cites accounted for over 47% of ASR cites to

---

<sup>8</sup> This last average (8 articles per issue) requires some explanation. Since the early 80s research notes, which are shorter articles reporting on direct empirical findings produced from the pertinent research project, began proliferating somewhat in ASR, averaging about three per issue through the 80s. Thus, when combined with articles the numbers show that, for example, from 1985-89 there were 311 total articles and notes, which comes to about 10.4 articles per issue. This is up somewhat from Wilner's estimate simply because she does not include research notes in her totals as I have done.

<sup>9</sup> Unless otherwise specified, the data cited in the remainder of this chapter will come from Table 5.2.

Table 5.2. Total Citations to 5 Key Works of Each Author by Selected Citing Journals, 1960-89

Author	Work	ASR	AJS	SPQ	Total (%)
Parsons	Social System	90	58	21	169
	Struc Soc Action	49	44	3	96
	Family, Soc, and Int	32	28	22	82
	Tow Gen Theory	39	20	8	67
	Struc and Process	26	14	1	41
	Total	236	164	55	455 (38.0)
Homans	Soc Behav:Elem Forms	74	49	94	217
	Human Group	42	35	35	112
	SB as Exchange	8	6	6	20
	Bringing Men Back In	9	7	3	19
	Nature of Soc Science	5	4	-	9
	Total	138	101	138	377 (31.5)
Goffman	Presentation of Self	31	31	55	117
	Asylums	25	24	8	57
	Interaction Ritual	13	12	14	39
	Stigma	13	13	12	38
	Frame Analysis	7	7	7	21
	Total	89	87	96	272 (22.7)
Garfinkel	Studies in Ethmeth	25	21	14	60
	Degradation Ceremonies	4	7	3	14
	Routine Grounds	3	1	4	8
	Formal Structures	4	1	1	6
	Experiments with Trust	2	1	1	4
	Total	38	31	23	92 (7.7)
Grand Total (%)		501 (41.9)	383 (32.0)	312 (26.1)	1196 (100.0)

all authors' works.<sup>10</sup>

Parsons' biggest year by far was 1963, a year in which his five chosen works were cited 26 times.<sup>11</sup> There are several reasons for this pinnacle of achievement: 1) as discussed in a previous section, the first wave of Parsonian critiques (and polemics against functionalism in general) which arose in the late 50s served to generate much discussion over the next several years. Especially salient here is the appearance of certain key articles, e.g. Kingsley Davis's 1959 ASA Presidential Address on the place of functionalism within sociology, along with Richard Dubin's controversial extension of Parsons' pattern-variable scheme in his 1960 paper. That is to say, a frenzy of articles concerning functionalism in general and Parsons (both for and against) in particular, occupied a great part of the discussion during this period. This period, from 1960-63, seemed to be one of theory's brightest moments within the discipline, as article after article were oriented overwhelmingly toward theory as opposed to empirical or methodological studies (Oromaner 1986). In

---

<sup>10</sup> As indicated in Table 5.2, of the 1196 total citations to all authors' works, ASR accounted for 501 of these, or 41.9% of the total.

<sup>11</sup> Compare, for example, that the second highest ASR total was George Homans' 17 cites in 1973.

fact, the volume 28, number 1 issue in 1963 was dedicated to a symposium on functional analysis, with such theory luminaries as W. Moore, P. Slater, S.M. Lipset, R. Bendix, B. Johnson, S. Nagi, H. Turk, J. Scott, E. Bittner, P. Berger contributing to the discussion.

One might also suggest that this orientation toward theory and particularly functional analysis was facilitated by Neil Smelser, who happened to be ASR editor from 1963-65. But although Smelser was editor during Parsons' most successful year, the subsequent years' totals do not indicate such a bias or orientation toward theoretical papers. That is, in 1964 Parsons' citations dropped to only 8; and in 1965 the total rose once again to 14. I believe that because there tends to be a fairly quick turnover of ASR editors (i.e., most serve on the average only three years) there is not enough time spent in editorship residence within which to establish any type of conclusive patterns of exclusionary or facilitory editorial practices.<sup>12</sup>

George Homans was the second most cited author in ASR,

---

<sup>12</sup> Remember, the article I referenced earlier on possible editor effects on journal citation patterns (Sievert and Haughwout 1989) concentrated on journal editors who averaged approximately ten years in the position. Obviously, in cases such as this where tenure is quite long and uninterrupted, hypotheses concerning possible editor effects would be more appropriate.

collecting 138 citations (about 28% of the total). His Social Behavior: Its Elementary Forms finished just behind Parsons' Social System as the second most frequently cited work.

Homans did not really start accumulating big numbers (that is, in terms of creeping closer to some of the numbers Parsons was putting up in the early sixties) until the 1970s. In fact, it seems that the 1970 article by R. Maris, "The Logical Adequacy of Homans' Social Theory," served to push discussions of Homans into the next level. Immediate reactions to this work in 1971 by G. Swanson, in 1972 by P. Singlemann, and a whole flurry of activity in 1973 seemed to indicate that Homans was enjoying some of the highest levels of attention he had ever attained. This concentration of attention seemed to entail at least two dimensions: 1) the continuing loss of interest of Parsonian theory left somewhat of a void in focused theoretical discussion. This, then, seems to vindicate Gouldner's suggestion that the crisis of sociology would be as much as anything the uncertainty which would arise in the face of the dissolution of Parsons' hegemonic theoretical domination. Also, we could see this as a "period of interregnum" as writers such as Wiley (1979) and Tiryakian (1979) have suggested. Although Parsons' influence appeared to have peaked in 1963, the dissolution of the Parsons

project, and the concomitant rise of Homans and Goffman's theoretical projects, seemed to have coalesced in the early seventies. This is supported as well by Wiley (1979:70), who suggested that "...[b]y the end of the sixties it was clear that, although no vote had been taken at the ASA business meeting, functionalism was no longer king. Something else was happening."

The "something else" that was happening, according to Wiley, was certainly not the sixties euphoria surrounding what was expected to be an emergent wave of critical and conflict perspectives; instead, methodological and empirical programs seemed to have emerged in strength, their locus of support being the new computer technology which had served to successfully usher in a new age of quantitative sociology. This is why, I believe, Homans' theories became so attractive: his postulates concerning exchange and small group theory were highly amenable to mathematization and quantification, and additionally were readily applicable to experimental research being conducted by a wide range of sociologists and social psychologists of the time.

Erving Goffman collected 89 of the 501 ASR cites (17.8%), with his Presentation of Self leading the way with 31 citations. Goffman's biggest year was 1976, wherein his works garnered 11 cites.

It is safe to say, I believe, that even though Goffman is a sociologist by trade his works were not heavily discussed within the halls of academic sociology, at least in comparison to the totals that are available elsewhere. I say this because anyone who references the SSCI (as reflected in Table 4.1) notices instantly that in terms of the social sciences in general his numbers are very high, which by our definition of influence means that his works are appropriated by a wide range of social scientists not necessarily circumscribed by the boundaries of sociology or sociologically-oriented journals. Thus in 1976, Goffman's year of most frequent cites (in ASR), citing authors such as J. Gusfield, G. Tuchman, and E. Gerson were wont to discuss other not necessarily sociologically-oriented dimensions of Goffman's work, such as his frequent guest editorships of the journal Semiotica, his contributions to other similar journals such as Language in Society, and the like. The strange thing is that by 1982, a year in which Goffman attained the presidency of the ASA, he was hardly discussed in ASR at all, garnering only 16 citations between the years 1980-88. Perhaps Posner (1978, 1980) was right when she suggested that Goffman's work has never seriously been considered or discussed within establishment sociology. The numbers as reflected in ASR seem to indicate as much.

If we can consider Goffman as being largely ignored by establishment sociology, what does this say about the numbers pertaining to Harold Garfinkel?

Garfinkel garnered only 38 citations, or about 7.6% of all ASR cites. Granted, he is at somewhat of a disadvantage since this investigation's methodology imposed a decision rule which resulted in the choosing of four articles and only one book among the five works being tracked. Granted also, Garfinkel's one book, Studies in Ethnomethodology, the one source from which it was expected that he would be appropriated with the same general or comparable frequency of a Parsons or Homans, was not released until 1967.

One should notice as well that almost no one outside of Garfinkel's immediate theory group--I include ethnomethodologists, phenomenologists, and the like--cited him in ASR. In fact, it is easier to document a few of the exceptions by way of noting those rare cites by non-ethnomethodologists: thus, in 1965 Tiryakian cited "Routine Grounds"; In 1975 L. Coser cited Studies; in 1987 J. Turner cited Studies, as did C. Camic. That is it, literally!

We see, then, that much of what Gouldner had to say about academic sociology seems to be embodied in the citation patterns as reflected in ASR. Parsons was heavily cited early on, and subsequently suffered a precipitous decline which lasted through the early 80s. Of course,

Gouldner could not have foreseen the brief Parsonian revival which occurred beginning in the late 70s and which found fullest articulation in 1986-89, largely as a result of renewed discussions of Parsons which had been generated by the works of such writers as J. Alexander,<sup>13</sup> C. Camic, R. Munch, F. Bourricaud, N. Luhmann, to name but a few.

Likewise, George Homans eventually rose to a position by 1970 of equal footing with Parsons, as sociology seemed at that time to have entered a period of interregnum. This decline of Parsonian functionalism also brought into relief the works of Erving Goffman, although he never attained the levels of Homans (because, we must point out, Homans had been tied into the "Eastern establishment" via his affiliation with Harvard University and his earlier functionalist inclinations).

Finally, we see that Harold Garfinkel was, in comparison to the other three theorists, treated as a virtual outsider in the pages of ASR.

---

<sup>13</sup> Although Alexander's (1983) four volume Theoretical Logic in Sociology generated a fair amount of renewed discussion of Parsons within sociology, his 1978 ASR article, "Formal and Substantive Voluntarism in the Work of Talcott Parsons," seemed to be the single article that blazed a path for extended discussions of his works at the research front, i.e. within the pages of journals. In fact, it appears that the emergence of only a handful of well-placed individuals--e.g. Alexander, Munch, Camic, and Luhmann--can substantially effect the ways in which a theorist is discussed.

### 5.3 American Journal of Sociology

Like ASR, AJS is a bi-monthly sociological journal which publishes an average of 45 articles per year.<sup>14</sup> Table 5.2 provides summaries of how each theorist's works were appropriated by AJS authors.

One should note immediately that AJS accounted for only 383 cites compared to 501 for ASR. Some of this difference might be explainable in the fact that ASR averages slightly more articles per issue than AJS, although since the total page numbers of the two journals are very similar (which means of course that AJS articles tend to be longer than those in ASR), this is not altogether likely. We might suggest, alternatively, that a main reason for the differential is that AJS has been oriented much more toward empirical as opposed to the type of theoretical controversies which tend to be internal to the discipline itself. Thus, although both AJS and ASR could rightly be considered sociology's two macro-oriented, core journals,

---

<sup>14</sup> However, there has been some divergence from this average of late. Since the late 70s articles in AJS have tended to be much longer than previous issues or those in ASR. For example, from 1985-89 there were only 200 articles in AJS, which comes to only about 6.67 articles per issue. Please note also that since review symposiums, which tend to run as long as regular articles, play a large part in AJS's dissemination of knowledge at the research front, they have been included as well in these totals.

ASR is more or less "internalist" while AJS could be considered in this same light "externalist."

The empirical orientation of AJS can be observed especially during the years 1961-72, a period of 12 years in which the journal was edited by Peter Blau and C. Arnold Anderson.<sup>15</sup> Consequently during this period the orientation was overwhelmingly toward articles dealing with organizational theory in one form or another. And although Parsons has done a fair amount of work in this particular field, he is not known for his work on organizations per se. Likewise for Homans, Goffman, and Garfinkel.

However, given that gross citations to the four theorists are down somewhat from the totals accumulated in ASR, the percentage of total citations as distributed among the theorists in AJS are somewhat similar to ASR's numbers. That is, e.g. Parsons accounted for 47.1% of all citations in ASR, and 42.8% (164 out of 383) in AJS. Similar patterns follow as well for Homans, Goffman, and Garfinkel.

The Social System, ASR's most frequently cited work, also took the top spot in AJS. Whereas in ASR Parsons' most productive (in terms of total number of citations) year was 1963, in AJS it turned out to be 1961. Something worth

---

<sup>15</sup> As Mullins (1973:54) has reported, both Anderson and Blau spent a good part of their respective careers at Chicago.

noting, and which might be further testament to the "internal" nature of ASR compared against AJS, is that AJS articles appearing after 1985 provide little evidence of the Parsonian revival which was so prevalent in ASR. Even more interesting, the one brief flurry of Parsonian studies appearing in AJS actually began before the ASR revival. Specifically, these AJS articles are as follows: in 1977, T. Burger, "Talcott Parsons, the Problem of Order in Society, and the Program of an Analytical Sociology"; in 1978, R. Stephen Turner, "Toward a Redefinition of Action Theory: Paying the Cognitive Element Its Due"; in 1979, C. Camic, "The Utilitarians Revisited"; in 1980, D. Zaret, "From Weber to Parsons and Schutz: The Eclipse of History in Modern Social Theory"; in 1981, R. Munch, "Talcott Parsons and the Theory of Action I: The Structure of the Kantian Core"; in 1982, the second part of Munch's article, "Talcott Parsons and the Theory of Action II: The Continuity of the Development"; in 1984, L. Mayhew, "In Defense of Modernity: Talcott Parsons and the Utilitarian Tradition" and D. Sciulli, "Talcott Parsons's Analytical Critique of Marxism's Concept of Alienation"; and finally, in 1989, C. Camic, "Structure after 50 Years: the Anatomy of a Charter."

Now this may seem like quite a bit of activity, but it was virtually all that was going on within the pages of AJS

pertaining to Parsons during the 80s.

George Homans' AJS numbers as they relate to his ASR totals are similar to those of Parsons', that is, lower gross totals but similar percentages. Homans' biggest year in ASR was 1973, whereas in AJS it was 1967 (and again, we see this tendency of AJS to precede or anticipate certain patterns which eventually follow in ASR.) It is easy to see why 1967 was a good year for Homans: citing authors chose overwhelmingly to note Homans' brief treatment of organizational behavior as discussed primarily in his Social Behavior. Remember, AJS's organizational orientation was strong during the combined tenures of Blau and Anderson. For example, citing authors during this period mentioned the following reasons for citing Homans: exchange theory in didactic relationships (R. Priest and J. Sawyer); suggestion that interpersonal obligations are stronger than institutional ties (B. Schwartz); the demands of the external environment bring about cooperation among group members in order to facilitate meeting specific challenges (R. Gordon); consumption of social information is processed at the micro-group level, thereby eventually being translated into collective decision making (A. Etzioni); and internal elements of action operate to constrain structural variability (D. Sweetser).

When one refers to Table 5.2 one notices that there is

an uncanny parallel in the total number of cites to each of Erving Goffman's works when comparing ASR and AJS. Of course, since AJS produced fewer total cites Goffman's AJS percentage (of total cites) is slightly higher.

Other than this, there is nothing remarkable about the way Goffman's works were appropriated in AJS; as such the patterns are extremely similar to those occurring in ASR.

In this light the same could be said of Harold Garfinkel. As Table 5.2 indicates, his AJS totals and percentages are quite similar to ASR. Again, Garfinkel was very rarely cited; in fact, he was not cited until 1969 (by E. Schur), and through 1972 Studies in Ethnomethodology had only been cited twice!

#### 5.4 Social Psychology Quarterly

Quite possibly the most controversial methodological decision in this study was, as discussed earlier, the inclusion of the journal Sociometry/Social Psychology Quarterly (hereafter SPQ).<sup>16</sup> This was done, once again, in an attempt to counterbalance the macro-orientation of the two core sociological journals mentioned above with a micro-oriented journal. After checking the Impact Factors

---

<sup>16</sup> This journal defines itself, not surprisingly, as "a journal of research in social psychology."

for the various journals (as provided in SSCI) it was discovered that SPQ was consistently the highest ranking social psychological journal not published by the American Psychological Association.

Unlike the two previous journals, SPQ is a quarterly publication. Each issue tend to have fewer pages than AJS and ASR, but conversely the journal averages about 2 to 3 more articles per issue than the others. Thus, the SPQ articles tend to be much shorter than their counterparts. This is simply indicative of the function of the articles in SPQ: they are largely oriented toward disseminating results of small group (usually experimental) research conducted in a controlled or laboratory setting. In this light, SPQ articles are similar, in both content (i.e., empirically-oriented) and length, to ASR research notes. Consequently there is not a great deal of lengthy and involved theoretical discussion occurring in their pages. Most authors, for example, assume that the reader is familiar with the theoretical debates and pertinent issues lurking behind the research problem. This makes for concise, short articles.

The results of this move of including a micro-oriented journal were anticipated and did in fact come to fruition. Specifically, we anticipated that Talcott Parsons would not fare well in SPQ, and as it turned out this did indeed

happen.<sup>17</sup> Of the 312 total SPQ citations, Parsons received only 55 (17.6%). The volume Family, Socialization, and Interaction Process was Parsons' one saving grace in that Robert Bales, a small groups researcher, was associated with the work. Bales of course was and has been one of the biggest names in the field, as his work was routinely referenced along with other SPQ staples such as Seeman, Hare, Festinger, Aronson, Borgatta, Sherif, and on occasion R. Turner and even R.K. Merton.<sup>18</sup>

The only SPQ article I found which took the time to discuss Parsons' contribution to small groups and social psychology was J. House's 1977 article wherein he explains the importance of Parsons as social psychologist (especially in terms of his socialization theory). However, articles such as this one were truly the exception, as

---

<sup>17</sup> This move was not made because we wanted to diminish Parsons' numbers in the face of Gouldner's theory (which of course suggested that Parsons' theoretical program was heading toward entropy). That is, there was no ulterior motive to select those particular journals which would serve to help "prove" Gouldner's distinctively negative reading of Parsons. Again, the particular journals chosen for this study were selected merely to cover as completely as possible the micro- and macro-theoretical realms.

<sup>18</sup> Interestingly enough, Paul Hare's definitive bibliography of small group research which appeared in SPQ's pages in 1972 did not list Family, Socialization, and Interaction Process; instead he referenced Parsons and Bales' Working Papers in the Theory of Action.

Parsons simply did not generate much interest among regular SPQ contributors.

Talcott Parsons theoretical fortunes in SPQ were nearly inversely proportional to those of George Homans. Homans did quite well here, and this was to be expected.

Homans had a superb run of cites from 1968-72, having been cited 41 times during this period. Earlier, in 1964, R. Emerson articulated the simple fact that Homans' postulates concerning small group tendencies are easily translatable and thus testable with regard to a wide range of social psychological experiments. In fact, Emerson's own string of power-dependence relational experiments (many of whose results were reported in SPQ) appropriated Homans' Social Behavior as the theoretical foundation. Again this points to the fact (which is overtly stated in Herbert Simon's quantitative testing of Homans' earlier Human Group postulates) that Homans' work attempted to facilitate the linking of theory with experimental and field design, which is of course the forte of experimental social psychology. It should also be noted that Parsons' theories never really made this successful transition to research-oriented work, and that this suggestion appears vindicated in light of his poor showing in SPQ.

Erving Goffman's fortunes also improved somewhat in SPQ. His 96 cites were more than he received in either AJS or

ASR. This was due largely to the good showing of Presentation of Self, which garnered 55 cites and finished second only to Homans' Social Behavior.<sup>19</sup>

Goffman's last three years were especially strong in SPQ, and this of course is graphically represented in the upturn in total citations which his works received as illustrated in figure 5.1. Much of this late occurring rise in fortunes is directly traceable to a 1987 SPQ special issue (volume 50, number 2) on the syntactical study of speech interaction, titled "Language and Social Interaction." As will be discussed briefly below, Garfinkel's numbers were also enhanced by this issue.

Unlike Homans and Goffman, Garfinkel was not helped a great deal by SPQ. That is to say, as Table 5.1 indicates, Garfinkel's citations accounted for 7.6% of ASR's total, 8.1% of AJS's total, and 7.4% of SPQ's total. One reason for this is surely, as Mullins (1973) has explained, the fact that ethnomethodology's methodology was overtly antagonistic toward the type of empirically-driven, social

---

<sup>19</sup> Interestingly enough, Asylums was not appropriated nearly as frequently as expected. But after reconsidering Goffman's orientation in that work, one realizes that Asylums is by and large a work of pure sociological theory, and most quantitatively-oriented researchers publishing in SPQ must have certainly concluded that Goffman had not formulated enough concrete, testable propositions by which experimental or field research could be guided.

psychological research which was the bellwether of SPQ. In other words, for a variety of reasons, whether in the macro- arenas of AJS or ASR or the micro-oriented SPQ, Garfinkel was always the proverbial "odd man out."

Ethnomethodology as a method always tended to slip through the cracks from the perspective of the discipline at large because it had always felt the need to burn its theoretical bridges as carried on by Garfinkel and Cicourel. Starting more or less within a functionalist tradition, Garfinkel quickly put distance between himself and functionalism by rejecting the assumption that social actors share common symbols, norms and values, thereby rejecting the notion of society as a stable system.

Later, as Garfinkel was establishing ethnomethodology on the West Coast with the help of Cicourel and Sacks, he soon realized how isolated and seemingly hermetically sealed the ethnomethodological perspective had become within the discipline. In the face of this, for example, Garfinkel went so far as to allow his ASA membership to lapse in 1967, apparently feeling that it would be of little consequence (Mullins 1973:198). As mentioned earlier, because ethnomethodology's earlier attacks were largely directed toward quantitative methodologists, and with the combined move away from structural-functionalism, Garfinkel found himself increasingly closed off with only a

relatively small cluster of adherents with which to work.<sup>20</sup>

Thus, effectively sealed off from the rest of the discipline, ethnomethodology progressed and new students were trained within an atmosphere whereby "the essence of the tradition [was] contained in the group's work and nowhere else" (Mullins 1973:195).

---

<sup>20</sup> One should also note that James Coleman's (1968) highly negative review of Studies in Ethnomethodology was a counterattack against Garfinkel's earlier attacks on practitioners of quantitative method.

## VI. CONCLUSIONS

### 6.1 Critique of the Study

This work has gone to some length in discussing and testing several key propositions which were central to Alvin Gouldner's hypothesized coming crisis of sociology. The attempt was made to measure intellectual influence by way of citation analysis. Ideally, it was felt that this measure of influence would be easily interpreted such that fairly straightforward decisions could be made regarding the confirmation or disconfirmation of Gouldner's thesis.

There are two major components to Gouldner's thesis which were felt to be amenable to empirical testing. One, we interpreted Gouldner to suggest that the once dominant theories of Talcott Parsons, the leading representative of the structural functional school, would soon decline to the point that Parsons would no longer enjoy the hegemonic tradition which he had been instrumental in forging during the middle forties and which he had extended, with the help of a large coterie of adherents, through the sixties. Two, concomitant to this decline of Parsonian functionalism there would be a rise of competing theories--specifically,

those of George Homans, Erving Goffman, and Harold Garfinkel--which would vie for the theoretical limelight. Gouldner further suggests that at some future unspecified point a theoretical polycentrism would emerge within the discipline, a polycentrism which would not represent the end of the crisis but merely a phase in which a period of interregnum would dominate. Since Gouldner could see no single theoretical program stepping in to reclaim Parsons' lost hegemonic domination (because all of the then-current competing perspectives were, in Gouldner's opinion, unacceptably ahistorical), he felt that eventually the discipline would go through a major transformation by which a new program, namely reflexive sociology, would emerge as the unifying theoretical perspective. Gouldner realized that this transformation would not come easily nor would it become manifest anytime in the near or foreseeable future, for it would first require a transformation of the internal sentiment structure of the entire society.

Thus, ultimately Gouldner's coming crisis of sociology is as much as anything the coming crisis of Western society (Collins 1981). Gouldner believes that the transformation could come by way of a more enlightened, better informed, and overtly self-reflexive program of social theory. But in fact, although Gouldner would like to see the discipline held intact during the transition toward the new reflexive

sociology, others have suggested that the ideology of sociology itself is threatened in Gouldner's formulation. Thus the coming crisis of sociology becomes, as intimated above, the coming crisis of Western society. As Shaw (1971:109), who himself is attempting to go beyond Gouldner, has stated, "The answer is not Gouldner's assertion that the sociologist must become self-aware, but to overcome 'sociology' in the development of the revolutionary self-consciousness of the working class."

Thus, when we look back reflectively to the beginning of the theoretical enterprise we see that we have shards and threads of thought from which to begin. It is never an easy thing conceptualizing all in one fell swoop what is required in an investigation of this sort. But begin we must, and it is necessarily the case that even when we delude ourselves into thinking that we are off and running we realize eventually, somewhere down the line, that our legs are in reality as shaky as those of a newborn calf.

But this should not be seen as a setback or as somehow a signal of the inherent inferiority of the intellectual project; no, it should be seen instead as the coming face-to-face with a good healthy dose of reality. Grandiose plans more often than not reduce to the bare essentials, that is, they reduce to that which is needed to get the job done and little more. I say this, of course, in light of my

earlier discussion (in chapter III) wherein I suggested that a content analysis of the citations, even perhaps utilizing some kind of scale by which to assign weights to different types of citation be they confirmatory, negational, peripheral, central, or so forth, would be desirable in order to further establish the kind of "influence" that each theorist had exerted on the various citing authors. But I quickly learned that this proposition had some major drawbacks, due mainly to the fact that any attempt to categorize or dichotomize such things as citations is by and large a subjective one. For example, if we attempt to pin the label of "positive" or "negative" on a citation we run into certain ambiguities, e.g. what about the cases (and there were quite a few of them) where an author positively cites a particular work then later turns right around and critiques that same work for some other (possibly unrelated) reason? Similar problems were encountered along the dimension of central/peripheral, that is, there were simply too many cases which were ambiguous.<sup>1</sup>

---

<sup>1</sup> Of course, I stated earlier in the text that in the later years (1975 and beyond) the citations to each theorist tended to become more and more peripheral as opposed to central in the context of the citing authors' discussions. This is because as obsolescence began setting in, i.e. as the literature aged, more and more writers citing these works tended to do so in the spirit of veneration or, following Gilbert (1977), citing authors mentioned these works primarily as "classics," thus appropriating the work ostensibly for purposes of

It is not a simple matter to draw an arbitrary line defining what constitutes a peripheral as opposed to central cite. Granted the two extremes are rather straightforward; but the middle 40% or so of the cites are not so routinely classifiable.

Thus, this investigation ended up providing only limited qualitative analyses of the citations vis-a-vis brief discussions concerning such things as which citing author was citing which author and why. Unfortunately, I was unable to produce a set procedure through which a systematic analysis of these cites, free from the excess baggage of weights, scales, and the like, might have been possible. If such a systematic analysis of the cites had been conceptualized in the formative stages of this study this might have perhaps reduced or completely eliminated the need--the force of which guided the original methodological decision rules--to classify the citation as being this or that type.<sup>2</sup>

---

persuasion.

<sup>2</sup> For example, the primary reason for not including all authors' works in favor of choosing only five selected works was that it was assumed that a full blown qualitative analysis would be conducted on each citing article. Since my original attempt, then, was to go deeper into the form and nature of these cites through such an analysis, this methodological decision necessarily precluded utilizing the on-line citation analysis in favor of a manual one.

As I have already intimated above, it is not an easy thing to look back upon one's work with a critical eye in an attempt to discern exactly what the major weaknesses, drawbacks, or shortcomings of the analysis entail. I have already discussed, for example, how original methodological commitments impinge upon the totality of the work and its findings. In keeping with the spirit of this endeavor it should be noted once again that our attempt to measure intellectual influence has relied very heavily, obviously, on one particular approach, namely citation analysis. We have seen that certain writers, e.g. MacRoberts and MacRoberts (1986), have raised concerns that because the tools of citation analysis (e.g. Garfield's SSCI) have become so readily available, researchers might be tempted to rely on this one dimension of influence to the detriment of investigating other possible correlative measures or approaches. They are correct to an extent: intellectual influence is a highly complex process, one which, as shown in earlier sections, comes in many guises and which possesses several dimensions.

Harriet Zuckerman (1987) has summarized these concerns in her paper, "Citation Analysis and the Complex Problem of Intellectual Influence." Again, as she (1987:329) states, "...it is useful to recognize that from its emergence, citation analysis has been just one, hardly the sole

procedure, used to trace intellectual influence flowing from designated works in science and scholarship by designated authors" (my emphasis).

Thus we recognize immediately that another major limitations of this investigation has been its reliance on citation analysis as by and large the sole measure of influence. By relying on citation analysis in this fashion we have left virtually untapped other important and valid ways of measuring intellectual influence. In particular, non-bibliometric methods such as organizational (e.g., Mullins 1973; Turner and Turner 1990) and field studies, the study of the informal channels of scientific communication, textual analyses, or even historical analyses aimed at reconstructing the influence of lesser known researchers (Lawani 1987), are available and could have conceivably been employed in this study as an alternative to citation analysis.

Another dimension of influence which this study does not address is the role of monographs (books) in scientific communication and intellectual impact or influence. That is, although our analysis offers a glimpse into the structure of citations in certain sociological journals, it says nothing about how and in what fashion book authors are citing other authors. The main reason for choosing journals as the source for our citation data rather than books was

one of pragmatics: As Line and Roberts (1976) have reported, journals function to target specific audiences with specific research agendas or interests. On the other hand, books are not so easily narrowly definable and thus are not always clearly categorizable with respect to their targeted audience. For example, Eugene Garfield has shown that to a relatively manageable extent social science journals can be identified and grouped together for purposes of tracking how and to what extent cited authors are being appropriated by citing authors. However, imagine the difficulties one might face in attempting to round up the many books and monographs which could in one way or another be classified or labeled as social scientific. Obviously, although the task could be accomplished, this is a much messier prospect than the fairly straightforward citation studies available in the journal literature.

Finally, the question must be asked, is it valid to attempt to produce a quantified test of a theory such as Gouldner's coming crisis? The answer, I believe, is "yes," with at least one important caveat. This caveat is of course a huge one: we can attempt to empirically test a theory just so long as we understand that the particular conceptual scheme which we choose to impose on the problem of interest serves in its own right to delimit the extent and the depth of our conclusions.

For example, just as I am here attempting to test certain aspects of Gouldner's crisis thesis, Gouldner himself was involved in Coming Crisis with analyzing and discussing, according to his criteria and definitions, certain dimensions of academic sociology in general and Parsonian functionalist theory in particular. Roscoe Hinkle (1971:498) succinctly articulated this fact when, in reference to Coming Crisis, he suggested that

However illuminating and stimulating Gouldner's interpretation of the origins of Parsonian structural functionalism may be, it is only one case study of the emergence of one sociologist's theory. It can only serve to illustrate--and not confirm or disconfirm--Gouldner's generalized explanatory thesis that social theories... arise in association with a personal view of reality and a distinctive set of sentiments....

In this same light, this work can claim only to be a case study of Gouldner's crisis thesis. The confirmation or disconfirmation of Gouldner's general theory must thus be limited only to the context within which the problem was originally stated and defined. Thus, the citation analysis offered here, and the conclusions about intellectual influence which have thereby been drawn, must be similarly perceived.

## 6.2 Strengths of the Study

But limited though it is, we believe that this study still has in its unidimensional approach something to offer

concerning the understanding of the cognitive structure of sociology from 1960 to the present. Because (as stated above) we are primarily concerned with testing only a relatively narrow range of concerns, namely Gouldner's hypothesized coming crisis of sociology, we are not tasked with producing an overarching or all-inclusive explanation covering all possible ranges and dimensions of the problem. As van Fraassen (1989) has suggested, like problems demand like solutions. So, for example, when we approach the coming crisis thesis we have decided, understandably, to focus upon one dimension of the problem by looking at the works of Parsons, Goffman, Homans and Garfinkel. This is surely not all there is to Gouldner's coming crisis, but it is what we have defined as problematic and worthy of investigation for our purposes, in this particular work. The limitations imposed by any narrowly defined set of problems require that the methodological approach be symmetrical to the demands of the study. Thus, in order to measure intellectual influence we have offered here citation analysis as a surrogate to a more elaborate, multidimensional analysis.

Hence, what we are reduced to in the end are the gross citation totals and breakdowns of these totals as represented in the various tables and graphs. But as it turns out I believe the numbers are in themselves somewhat

illuminating concerning this elusive measure of influence which we have been grappling with over the course of this study. My interpretation of these numbers as presented in chapter V and again here constitute, I believe, as good an explanation of the goings-on within the discipline from 1960 to the present as the limitations of such a study allow.<sup>3</sup>

What can be said, then, about this investigation and how it relates to Gouldner's thesis? First of all, the findings seem to indicate that many of his readings of the situation as relating to Parsons were correct. We found good evidence to support the contention that Parsons was dominant early on and that there existed what we might call a "theoretical monocentrism" wherein one dominant theoretical perspective, namely structural functionalism, ruled the theoretical roost. Tracking the five chosen works over time indicated that by 1963 Parsons' dominance had come to an end. In fact, George Homans' 1964 ASA presidential address,

---

<sup>3</sup> By limitations I mean, i.e. if there had been more time and if the mandate for such an elaborate investigation (such as that provided by a doctoral dissertation) had been in force, we could have extended this work into a full-blown co-citation analysis which might have provided conclusive documentation pertaining to, say, the cognitive structure and the types of communicative links which exist between those scientists who share similar research interests vis-a-vis the citation record. This might have revealed an interesting and possibly important dimension of our notion of influence.

"Bringing Men Back In," wherein he officially announced the death of functionalism's hegemonic domination, seems to coincide exceedingly well with Parsons' fall.

We also witness some evidence that the hypothesized rise of competing theories did in fact occur. When we refer to Figure 5.1, for example, we see that Homans and Goffman's theoretical fortunes began to rise somewhat commencing in the early sixties. By 1967, the combined effect of Homans' five works had attained equal footing with Parsons' position. Goffman, lagging somewhat behind, never really approached the levels of either Parsons or Homans until around 1976, at which time all theorists (but to a lesser extent Garfinkel) seemed to begin a simultaneous precipitous decline.

This precipitous decline occurred, I believe, for at least two reasons. First, most assuredly the decline represents at least partially the simple aging of the literature. By 1976 all theorist's key works (with the exception of Goffman's Frame Analysis) were ten years of age or older. When we consider that literature decay rates within the social sciences begin on the average around ten years after publication, we see that most works were at the time prime candidates for obsolescence. Second, it is possible, although much harder to substantiate, that a "silent revolt" against theory, as was discussed earlier,

manifested itself within the discipline as represented in the three journals included here.

One area where Gouldner was apparently wrong was in his judgment that Harold Garfinkel's ethnomethodological project would rise to a level comparable to those of Parsons, Homans or Goffman. Writing as he was in 1970, Gouldner's view of the rise of ethnomethodology was skewed somewhat in that Garfinkel himself was riding the crest of the ethnomethodological wave resulting from the successful release of his Studies in Ethnomethodology in 1967. This was, as Mullins (1973:196-198) has noted, a good time for ethnomethodology; in fact, he describes the years 1966-71 as ethnomethodology's "cluster stage": we see the simultaneous emergence and efflorescence of the program through the efforts of Aaron Cicourel at Santa Barbara; of Garfinkel at UCLA, himself in turn producing students such as Don Zimmerman, Howard Schwartz, D. Lawrence Wieder, and David Sudnow; and of Harvey Sacks at Irvine.

But the high hopes which grew out of the cluster stage never really were translated into sustained activity. Much of this is surely due to Garfinkel's own incomprehensibility which was routinely displayed in his various papers and monographs. Also, there was little transference of knowledge from teacher to student beyond the first generation of ethnomethodologists. The internal

squabbings and splits within the perspective never allowed ethnomethodology to rally as one unified force to counteract charges from establishment sociology that ethnomethodology's methodological approach, that being participant observation of the routine activities of individuals (including a special emphasis on conversational analysis), was little more than methodological "faddism" (Mullins 1973:204). In the end, Mullins suggests that ethnomethodology never really made the necessary successful transition to the specialty stage, that is, the dispersal of key working members of the discipline away from the originating zone (i.e., the West Coast). As Mullins (1973:205) concludes, "There is no longer a cluster, and one cannot again develop. If the transition to specialty or discipline is not completed, ethnomethodology will gradually disappear, and its insights will be parceled out to new groups of sociologists." Although the contemporary situation for ethnomethodologists is not nearly so dire as Mullins warned against (especially in light of Garfinkel's late upturn as illustrated in figures 5.1 and 5.2), we could say that the truth about ethnomethodology lies somewhere between Gouldner's 1970 optimism and Mullins' later observations.

### 6.3 Coming Crisis in Retrospect

Another thing to consider pertaining to any final judgment of the validity of Gouldner's assertions is this: granted, there is good evidence to support the claim that events unfolded as Gouldner predicted. But might one not suggest that the "crisis" was already in full bloom, and that Gouldner's analysis is not so much a prediction of things to come but rather a statement of how things are (or rather "were" as Gouldner was writing in the late 60s)?<sup>4</sup>

This is a valid question, especially when we consider once again the graph of figure 5.1 which reflects our total measure of influence as seen through the combined effects of all four authors' works. Specifically, the graph indicates, and there is good reason to believe, that Parsons' influence peaked in 1963, and rapidly fell off beginning in 1964. This of course is a full six years

---

<sup>4</sup> In answer to this question, Randall Collins (1981:317) has offered the following astute observations: "The shortcomings of The Coming Crisis of Western Sociology are contained in its title. 'Western sociology' is too broad; it is primarily about functionalist sociology, not about empirical research, neo-Marxist and neo-Weberian conflict theory, phenomenological sociology, or even the European idealist-historicist tradition on which Gouldner himself draws here. And the 'coming crisis' is too late: Functionalist sociology has been in crisis for at least a decade, during which the vital front of intellectual advances has passed to the phenomenologists, the historical-comparatives, and the conflict theorists. A more literal title might have been 'The Crisis of Functionalist Sociology.'" For a very similar line of thought see Giddens (1987).

before the publication of crisis. Also by 1970 we see that Homans, according to our measure, had more or less "caught up" with Parsons' level of influence.

Before we pass judgment on this issue, however, we should be fair to Gouldner by pointing out that he stated repeatedly during his analysis that he believed that Parsons had already reached his peak of prominence within sociology and was beginning a free fall toward entropy. A few examples will suffice to make this point: "Parsons' system is undergoing a kind of entropy and is taking a declining place in the professional attention of academic sociologists" (159); "In the United States, where I believe Parsons' influence has reached its apogee, his work retains a considerable audience, and its standpoint still commands considerable respect" (168).<sup>5</sup>

Figure 5.1 further suggests that Gouldner's hypothesized "theoretical polycentrism" came to pass beginning in 1976. This polycentrism is defined only for the four theorists Gouldner is focusing on, these being of course Parsons,

---

<sup>5</sup> Gouldner is right on both counts: as figure 5.1 indicates, Parsons' influence had indeed reached its apogee and was on the decline as Gouldner wrote in 1970. Further, even in the face of this decline Parsons was still being heavily discussed and cited, as Garfield (1980c) has shown.

Homans, Goffman and Garfinkel.<sup>6</sup>

As was shown above in Collins' (1981) assessment of the overstated and perhaps misleading nature of Gouldner's title, if there is any valid criticism to be made of Gouldner's own analysis perhaps we could suggest along with Merton (1976b) that he overestimated the extent to which sociology was in "crisis" as the situation was perceived in the late sixties. Merton, in fact, speaks of the natural state of affairs in sociology as being in "chronic crisis," or rather, the sentiment that sociology has always throughout its history been typified as being in an unstable or transitional state of oscillating between extreme optimism and extreme pessimism.<sup>7</sup>

---

<sup>6</sup> Gouldner (1970a:444) suggests that besides this convergence among the "neo-functionalism" perspectives, the move toward polycentrism would also include elements from Marxian sociology, although he gives no specifics as to the latter's representatives. In any event, the new polycentrism is seen by Gouldner as tension filled: that is, between the ideological ramifications of Marxism competing with the instrumentalist orientations of, for example, exchange theory in particular and middle range theories amenable to Welfare State manipulation and policy decision-making in general.

<sup>7</sup> On another axis, for example, Wolf Lepenies (1988) has documented how sociology has struggled to come to grips with the question of where it fits in between the extremes of the literary disciplines on the one hand and science on the other. That is, the eternal question for sociology has been: do we see our practitioners as imbued with a cold rationality which seeks to comprehend the laws of modern society by means of measurement and computation, the theories of course modeled more or less after the axiomatic

Extreme optimism is, after all, the starting point of the discipline. For example, the visions of St. Simon and Comte posited the emergence of a modern world order based on a new, positive secular religion which the scientific study of society (i.e., sociology) would ideally represent.

As Merton correctly notes, sociology has a rich history of such visions of optimism, as we have seen in Comte and St. Simon, and in impending crisis: e.g. in 1956 Georges Gurvitch's announced "crisis of sociological explanation"; Sorokin's vision of crisis as embodied in his Fads and Foibles in Modern Sociology; and of course George Homans' "Bringing Men Back In." Whether it is phrased in the language of Gurvitch's dialectical sociology, Sorokin's integralist sociology, or even Gouldner's reflexive sociology, the crisis sentiment of cognitive revolt within the discipline is generally the same. The continuities are that the signs and symptoms of crisis are always couched in terms of a change and clash of doctrines which is summarily

---

language of deductive-nomological explanation? Or are we closer to literary scholars whose intuition sees farther (supposedly) than the analyses offered by coldly calculating positivistic science and whose ability to address the important problems of humanity is to be preferred to the products of other disciplines that misunderstand themselves as being the embodiment of a natural science of society (Lepenies 1988:13)? This pair of ambivalences are at work, suggests Gouldner, in contemporary sociology and as such contribute to the sense of crisis within the discipline.

accompanied by deepened tensions and conflict among (seemingly) disparate practitioners of the trade, especially in terms of the sentiment that the dominant paradigm is no longer sufficient or able to handle or address the problems it should ideally be capable of handling. Gouldner's discussion of the emergence of a new sentiment structure among the young allowed him to map these larger sociohistorical events (such as the campus revolts and uprisings of the 60s) into the situation within sociology, thereby connecting sociology's sense of cognitive crisis to the crisis of confidence within the establishment in general. Merton (1976b:12-13) sums up this entire line of thought quite nicely when he said that

The social and cognitive processes within the collectivity of sociologists thus interact with developments in the environing society to produce variability in appraisals of the state of the art. When historical demands coalesce in both cognitive and social domains, as they appear to have done in the late 1960s, they generate an acute sense, in place of the acceptably or even agreeably chronic sense, of less than adequate performance in developing cognitively powerful and socially applicable paradigms. Such historical times of trouble transform chronic sociological aches into acute sociological pains. It is then that observant doctors are apt to advance their diagnosis that sociology is in deep crisis.

Even with this suggestion that Gouldner overestimated the "crisis" in Western sociology, it is certainly not a damning indictment against the worthiness and utility of Gouldner's book. We may all be afforded the luxury of

overstating a position for the sake of presenting a theoretical position or for the sake of attempting to shed new light or producing a novel way of approaching problems of interest within sociology's cognitive realm. This process of reflecting upon, dissecting, and attempting to define and expose the powers and forces that drive a discipline in one direction or another is a continually evolving one. Gouldner was during his time witness to a nearly unprecedented occurrence within the discipline of sociology: the dissolution of a highly refined, technically proficient theoretical perspective. It is valid reason enough to inquire into the reasons for such a dissolution; the conclusions (whether wholly or only partially accurate) which Gouldner reached in attempting to define and describe the forces at work which contributed to the entropy of Talcott Parsons' project themselves contributed substantially to the advancement of knowledge within the discipline.

#### 6.4 James Coleman and Rational Choice Theory

Finally, something should be said about the present status of sociological theory as it relates to the general concerns voiced by Gouldner. As was discussed above, Gouldner was primarily interested in the dissolution of a hegemonic tradition and the ways in which a crisis would

manifest itself during the period of uncertainty, or what Wiley (1979) described as the period of interregnum.<sup>8</sup> Although no comparable situation exists within sociology today, we may approach Gouldner's problem from the opposite direction by hypothesizing that right now, as we currently speak, there may very well be emerging a new hegemonic tradition which promises to approach the levels which Talcott Parsons and his structural-functional school attained in the 1950s.

I am speaking of course of James Coleman and the school which has loosely been described as rational choice theory.<sup>9</sup> It appears that Coleman and rational choice theory

---

<sup>8</sup> Just as Gouldner's crisis was couched in terms of the dissolution of sociology's cognitive consensus, recent thinkers such as Turner (1989) have, in light of the emergence of the pervasive theoretical polycentrism which has replaced functionalism's earlier monocentric hegemony, spoken of the "disintegration" of American sociology. The point is that many writers seem to be wishing for a new unifying theoretical perspective or metatheory--such as perhaps rational choice theory--to emerge, thereby serving to counteract the increasingly factionalizing tendencies which appear rampant in contemporary sociology.

<sup>9</sup> Although Coleman had already been fairly prominent within sociology since the early sixties, Gouldner discussed him only in passing in Coming Crisis. Specifically, in his discussion of the emergence of academic sociology within the Soviet Union, Gouldner (1970a:460) suggested that the works of certain mathematically oriented sociologists like Coleman and Harrison White were circa 1970 beginning to be brought to the attention of Eastern European sociologists. Gouldner concluded that this particular trend promised to have some lasting consequences on the development of sociology within

have fulfilled all of the preliminary requirements necessary for the forging of a hegemonic tradition (or at the very least, a viable, distinctive school within sociology): (1) Coleman himself fills the role of charismatic (or perhaps intellectual) leader. He is well placed and has superb institutional backing in Chicago, certainly one of the elite centers of sociological thought. Also, Coleman has recently scaled the personal heights within the discipline, having been elected ASA president for the academic year 1990-91. (2) Coleman's just released book, Foundations of Social Theory, represents a solid program statement around which adherents to rational choice theory within sociology may align themselves. He has suggested, for example, that he has indeed discovered the key transformation which helps to explain how individual choices translate into collective action, thus apparently solving an age-old theoretical and ontological debate, namely the micro-macro split. (3) Coinciding with the release of the book Coleman has had the great fortune of launching his own journal Rationality and Society, an important communicative link for those researchers working in the front lines of rational choice theory. (4) Observers from outside the discipline are tending more and more

---

certain European and Communist countries.

(especially since the release of Foundations) to suggest that Coleman represents the state of the art within sociology.<sup>10</sup> They see this (move toward rational choice theory) as simply a rational, pragmatic move toward greater systematization of a discipline (sociology) that is in desperate need (or so the sentiment goes) of establishing a definitive cognitive base and orientation.

This move or attempt to bring greater systemization and theoretical unity to sociology by borrowing already existing, well established bodies of thought from other disciplines has a history worth noting here, especially in light of the parallels between Parsons' hegemonic tradition and Coleman's current program. As such, I am willing to suggest the following: Talcott Parsons' primary goal over his long, fifty year career was to establish not only within sociology but amongst all the social sciences a unified, general theory of human action. To accomplish this goal Parsons believed that certain highly systematized theoretical systems, specifically those of the biological

---

<sup>10</sup> For example, see David Warsh's review wherein he proclaims with a straight face that the publication of Coleman's Foundations of Social Theory is "probably the most important thing to happen in sociology in 50 years" ("Modern Thinkers Merge Sociology, Economics to Explain Today's World," in The Washington Post, Wed., Aug. 15, 1990, p. D3).

sciences<sup>11</sup> and the writings of key French functionalist anthropologists (especially Radcliffe-Brown, Malinowski and Durkheim), would provide the most fruitful model for such an undertaking.

Coleman, on the other hand, has turned for his unifying project to economics and the longstanding tradition of utilitarian and rational choice theory. Parsons of course had considered economics as a possible model upon which to build sociology's general theory, but as we have seen he eventually came to reject this perspective, opting for a voluntaristic and normatively-oriented theory of action.<sup>12</sup>

---

<sup>11</sup> From biology Parsons was heavily influenced by Walter B. Cannon's Wisdom of the Body, a work which allowed him to formulate a more rigorous organic analogy by speaking of action systems as containing internal environments similar to the situation of living organisms. This of course took its fullest form in Parsons' turn toward cybernetics (beginning in the early sixties) and culminating in his last great work, "A Paradigm of the Human Condition."

<sup>12</sup> Gouldner (1970a) speaks of "theoretical amnesia," that is, the tendency within a discipline toward the rejection of entire schools of thought only to see them recycled years later in updated or somewhat modified form. In connection with this sentiment, Mark Gould (1989:649) makes a trenchant observation about what's happening in contemporary sociology: "In our current situation, where economists are seeking to apply utilitarian models to everything from crime and immigration to the selling of children, where utilitarian rational-choice models seem once again to be making inroads into the heart of sociology, and where methodological empiricism is stronger than ever, it is well to reread Parsons's fifty year-old demolition of these very same points of view."

Coleman (1990a) believes that rational choice theory, as it turns out, does provide a sufficiently compelling model by which to systematize sociological theory. The reasoning is that the concept of rationality plays a central role in any study of human social action. Parsons believed that any attempt to apply this type of economistic/utilitarian approach would have to operate within a particularly problematic and hence unacceptable bundle of assumptions, these being: (1) atomism--stress on the individual act; (2) rationality--stress on the choice of efficient means; (3) empiricism--stress on the concreteness or reality of individual acts; and (4) randomness of ends--stress on the not necessarily shared, idiosyncratic nature of the goals human actors pursue (Bershady 1973:42).

Coleman believes, in contradistinction, that these assumptions when taken together do not render his sociological theory of action untenable. Whereas Parsons was never quite successful in explaining how or by what mechanisms rationality at the individual level could translate to rationality at the social system level,<sup>13</sup> Coleman is very willing indeed to offer such a

---

<sup>13</sup> This is of course Coleman's contention. For a more sympathetic discussion of Parsons' status as an empirical researcher, see Lidz (1986).

formulation.<sup>14</sup>

Regardless of this seemingly eternal debate, the parallels between the two projects which I alluded to earlier have to do with the way both approaches have identified their own respective "positive heuristics" (in Lakatos's [1978] terminology), that is, those concepts, problems or definitions which are identified as important and which help guide research and further development of the theory. For example, Parsons' structural functionalist theory was primarily concerned with the problem of social order, that is, the question of how it is that societies cohere rather than being torn apart at the seams. As we have seen throughout the course of this study, many of Parsons' critics chose to polemicize against this

---

<sup>14</sup> Coleman (1990b) does a nice job of delineating the continuities and divergences between his recent writings and Parsons' earlier formulations. For example, he believes Parsons was right in emphasizing a subjective view of social institutions, for this method allows the linking of the micro and the macro vis-a-vis the realization that commonly held ultimate values lead through rational paths to certain common goals or ends (Coleman 1990b:334). Unfortunately, according to Coleman, Parsons never carried through with the promising direction of his earlier (pre-1951) work and eventually fell into the "functionalist fallacy" by assuming that "needs" at the individual level would become manifest as action at the collectivity or institutional level. As Coleman (1990b:338) states, "Parsons abandoned his attempt to found social theory on a theory of rational action: he reverted to classification schemes that were no less sterile in his hands than in the hands of those he criticized in this [Parsons 1990] essay."

particular emphasis of his research project, claiming that Parsons' theories could never adequately deal with or explain, for example, social conflict or change in light of the overweening emphasis on norms and values which were seen by Parsons as contributing to societal equilibrium and order.

But in a similar vein, just as van Fraassen (1989) has noted, rational choice theory has its own version of emphasizing one concept to the exclusion of considering other questions or problems. That is, by its very nature rational choice theory emphasizes "success"; more to the point, and reduced to a fundamental statement or equation, rational choice theory assumes that rationality = success.

So what we have produced in discussing the Parsons and Coleman projects in this light are two sets of dichotomies: Parsonian functionalism being divided along the axes of order or consensus versus disequilibrium or conflict; and Colemanian rational choice theory along the axes of success versus failure. It is my contention that these two approaches are similar in their respective positive heuristics in that they emphasize order (Parsons) and success (Coleman) over conflict and failure.

Contrary to much of the interest in conflict theory or other species of thought which attempt to focus on society's "underdogs" or those segments of society which

are perceived as failures in the grand scheme of things, it does not appear within the contemporary sociological milieu that these approaches (such as, e.g., Collins 1981) are now bearing or will be bearing in the foreseeable future significant fruit. The main reason this is so, I believe, is that by their very nature episodes of failure lay in disembodied limbo, that is, they never are realized and thus no system is produced by which an organized analysis may be undertaken. One could presumably gather a whole range of data through survey research or the like, asking respondents, for example, to elaborate on certain recently failed undertakings, asking their help in identifying the specific goals which were attempting to be attained, the unsuccessful means chosen in the attempt to reach the stated goal, and so forth. But one can already see at this point the wide range of variables which would undoubtedly be introduced, thus leading to the potential for rapid diffusion of focus which would more than likely occur in a study of this sort. In short, completed and ordered systems of human activity seem much more amenable to the kind of research problems which sociology has attempted to claim for its own domain of interest. Until such time that a well thought out and fully articulated theory of failure or

disorder is achieved,<sup>15</sup> the legacy of sociology and the types of hegemonic traditions which will emerge and continue to flourish within its disciplinary boundaries appear to be those projects akin to the efforts of a Talcott Parsons or a James Coleman.

---

<sup>15</sup> Goffman has, in my estimation, come as close as any sociological theorist to formulating a successful and comprehensive theory of failure. We may see this, e.g. in his paper "On Cooling the Mark Out: Some Aspects of Adaptation to Failure," as well as in his monographs Asylums and Stigma. However, no one has yet extended, elaborated, modified, or applied Goffman's seminal works toward the realization of such a project. Perhaps in due time someone will be able, upon recognizing the wisdom and artistry of Goffman's penetrating anthropological vision, to apply this grand vision toward the production of a meaningful (and, just as importantly, believable) structural theory of conflict, disorder and failure.

## BIBLIOGRAPHY

- Adriaansens, Hans P.M. 1989. "Talcott Parsons and Beyond: Recollections of An Outsider." Theory, Culture & Society, 6:613-621.
- Alexander, Jeffrey C. 1978. "Formal and Substantive Voluntarism in the Work of Talcott Parsons: a Theoretical and Ideological Reinterpretation." American Sociological Review, 43:177-198.
- \_\_\_\_\_. 1983. The Modern Reconstruction of Classical Thought: Talcott Parsons, vol. 4 of his Theoretical Logic in Sociology. Berkeley: University of California Press.
- \_\_\_\_\_. 1988. "The New Theoretical Movement." Pp. 77-101 in Smelser (1988a).
- Alt, John. 1981. "Alvin W. Gouldner (1920-1980)." Telos, 47:198-203.
- Aversa, Elizabeth Smith. 1985. "Citation Patterns of Highly Cited Papers and Their Relationship to Literature Aging: A Study of the Working Literature." Scientometrics, 7 (3-6):383-389.
- Avramescu, Aurel. 1979. "Actuality and Obsolescence of Scientific Literature." Journal of the American Society for Information Science, 30 (5):296-303.
- Bain, R. 1962. "The Most Important Sociologists?" American Sociological Review, 27:746-748.
- Bales, Robert F. 1950. Interaction Process Analysis: A Method for the Study of Small Groups. Reading, Ma.: Addison-Wesley.
- Barber, Bernard. 1970. "L.J. Henderson: An Introduction." Pp. 1-53 in L.J. Henderson on the Social System, ed. Bernard Barber. Chicago: University of Chicago Press.
- Bayer, Alan E. and John Folger. 1966. "Some Correlates of a Citation Measure of Productivity in Science." Sociology of Education, 39 (4):381-390.

- Bayer, Alan E., John C. Smart and Gerald W. McLaughlin. 1990. "Mapping Intellectual Structure of a Scientific Subfield Through Author Cocitation." Journal of the American Society for Information Science, 41 (6):444-452.
- Bendix, Reinhard. 1970. Embattled Reason. New York: Oxford University Press.
- Berger, Bennett M. 1970. "Book Review: Coming Crisis." Social Problems, 18 (2):275-280.
- Bernstein, Richard J. 1972. "Critique of Gouldner's The Coming Crisis of Western Sociology." Sociological Inquiry, 42 (1):65-76.
- Bershady, Harold. J. 1973. Ideology and Social Knowledge. New York: Wiley.
- Black, Max (ed.) 1961. The Social Theories of Talcott Parsons. Prentice-Hall.
- Blau, Peter M. 1956. "Book Review: Family, Socialization, and Interaction Process." American Journal of Sociology, 61 (5):488-489.
- Bock, Kenneth E. 1963. "Evolution, Function, and Change." American Sociological Review, 28 (2):229-237.
- Bourricaud, Francois. 1981. The Sociology of Talcott Parsons. Chicago: University of Chicago Press.
- Braxton, John M. and Alan E. Bayer. 1986. "Assessing Faculty Scholarly Performance." Pp. 25-42 in Measuring Faculty Research Performance, ed. J.W. Creswell. San Francisco: Jossey-Bass.
- Brittain, J. Michael and Maurice B. Line. 1973. "Sources of Citations and References for Analysis Purposes: A Comparative Assessment." Journal of Documentation, 29 (1):72-80.
- Brown, J.S. and B.G. Gilmartin. 1969. "Sociology Today: Lacunae, Emphases, and Surfeits." The American Sociologist, 4 (4):273-286.
- Bryant, Christopher G.A. 1983. "Who Now Reads Parsons?" Sociological Review, 31:337-349.

- Buxton, William. 1985. Talcott Parsons and the Capitalist Nation-State. Toronto: University of Toronto Press.
- Camic, Charles. 1989. "Structure after 50 Years: The Anatomy of a Charter." American Journal of Sociology, 95 (1):38-107.
- Chafetz, Janet Saltzman. 1978. Masculine, Feminine, or Human? Itasca, Il.: Peacock.
- Chubin, Daryl E. and Soumyo D. Moitra. 1975. "Content Analysis of References: Adjunct or Alternative to Citation Counting?" Social Studies of Science, 5:423-441.
- Clark, S.D. 1960. "Review of Structure and Process in Modern Societies." American Sociological Review, 25 (6):971-972.
- Cole, S. and J.R. Cole. 1968. "Visibility and the Structural Bases of Awareness of Scientific Research." American Sociological Review, 33:397-413.
- \_\_\_\_\_. 1972. "The Ortega Hypothesis." Science, 178:368-375.
- \_\_\_\_\_. 1973. Social Stratification in Science. Chicago: University of Chicago Press.
- \_\_\_\_\_. 1987. "Testing the Ortega Hypothesis: Milestone or Millstone?" Scientometrics, 12 (5-6):345-353.
- Coleman, James S. 1968. "Review Symposium: Studies in Ethnomethodology." American Sociological Review, 33 (1):126-130.
- \_\_\_\_\_. 1986. "Social Theory, Social Research, and a Theory of Action." American Journal of Sociology, 91 (6):1309-1335.
- \_\_\_\_\_. 1990a. Foundations of Social Theory. Cambridge, Ma.: Harvard University Press.
- \_\_\_\_\_. 1990b. "Commentary: Social Institutions and Social Theory." American Sociological Review, 55 (3):333-339.

- Collins, Randall. 1981. Sociology Since Midcentury: Essays in Theory Cumulation. New York: Academic Press.
- Colvard, Richard. 1990. "Gouldner is Always Worth Reading." American Sociologist, 20 (4):381-384.
- Crane, D. 1972. Invisible Colleges: Diffusion of Knowledge in Scientific Communities. Chicago: University of Chicago Press.
- Cronin, Blaise. 1981. "The Need for a Theory of Citing." Journal of Documentation, 37 (1):16-24.
- Dahlstrom, Edmund. 1972. "Review of Gouldner's Coming Crisis." Acta Sociologica, 15 (4):373-374.
- Dahrendorf, Ralf. 1958. "Out of Utopia: Toward a Reorientation of Sociological Analysis." American Journal of Sociology, 64:115-127.
- \_\_\_\_\_. 1959. Class and Class Conflict in Industrial Society. Stanford, Ca.: Stanford University Press.
- \_\_\_\_\_. 1988. The Modern Social Conflict. New York: Weidenfeld & Nicolson.
- Davis, Kingsley. 1959. "The Myth of Functional Analysis as a Special Method in Sociology and Anthropology." American Sociological Review, 24:757-773.
- Deutsch, Steven E. 1971. "Review Symposium: Coming Crisis." American Sociological Review, 36 (2):321-326.
- Dubin, Robert. 1960. "Parsons' Actor: Continuities in Social Theory." American Sociological Review, 25 (4):457-466.
- Dudley, Charles J., Gregory W. Justice and David R. Lux. 1990. "Notes from a Production of Tartuffe: Solutions to Micro/Macro Analysis in the Art of Sociology." Paper presented at the annual meeting of the Society for Literature and Science (SLS), Portland, Or., October 1990.
- Flacks, Richard. 1972. "Notes on the 'Crisis of Sociology.'" Social Policy, 2 (6):4, 6-12.
- \_\_\_\_\_. 1990. "Gouldner's Prophetic Voice." American Sociologist, 20 (4):353-356.

- Friedrichs, Robert W. 1970. A Sociology of Sociology. New York: Free Press.
- \_\_\_\_\_. 1971. "Friedrichs on Gouldner: The Case for a Plurality of Sociologies of Sociology." LSU Journal of Sociology, 2 (1):100-107.
- \_\_\_\_\_. 1972. "Dialectic Sociology: Toward a Resolution of the Current 'Crisis' in Western Sociology." British Journal of Sociology, 23 (3):263-274.
- Fuhrman, Ellsworth R. 1984. "Alvin Gouldner and the Sociology of Knowledge: Three Significant Problem Shifts." The Sociological Quarterly, 25:287-300.
- \_\_\_\_\_. 1990. "Reflexivity and Alvin Gouldner: The Coming Crisis in 1990." American Sociologist, 20 (4):357-361.
- Fuhrman, Ellsworth and William Snizek. 1990. "Neither Proscience nor Antiscience: Metasociology as Dialogue." Sociological Forum, 5 (1):17-35.
- Garfield, Eugene. 1980a. Essays of an Information Scientist, vol. 3, 1977-1978. Philadelphia: ISI Press.
- \_\_\_\_\_. 1980b. "The 100 Books Most Cited by Social Scientists, 1969-1977." Pp. 621-632 in Garfield (1980a).
- \_\_\_\_\_. 1980c. "The 100 Most-Cited SSCI Authors, 1969-1977." Pp. 633-639 in Garfield (1980a).
- \_\_\_\_\_. (ed.) 1989. SSCI Journal Citation Reports, 1988 Annual, vol. 6. Philadelphia: ISI Press.
- Garfinkel, Harold. 1949. "Research Note on Inter- and Intra-Racial Homicides." Social Forces, 27 (4):369-381.
- \_\_\_\_\_. 1956. "Conditions of Successful Degradation Ceremonies." American Journal of Sociology, 61 (5):420-424.
- \_\_\_\_\_. 1963. "A Conception of, and Experiments with, 'Trust' as a Condition of Stable Concerted Actions." Pp. 187-238 in Motivation and Social Interaction, ed. O.J. Harvey. New York: Ronald Press.
- \_\_\_\_\_. 1964. "Studies of the Routine Grounds of Everyday Activities." Social Forces, 11 (3):225-250.

- \_\_\_\_\_. 1967. Studies in Ethnomethodology. Englewood Cliffs, N.J.: Prentice-Hall.
- \_\_\_\_\_. 1988. "Evidence for Locally Produced, Naturally Accountable Phenomena of Order", Logic, Reason, Meaning, Method, etc., in and as of the Essential Quiddity of Immortal Ordinary Society, (I of IV): An Announcement of Studies." Sociological Theory, 6 (1):103-109.
- Garfinkel, Harold and Harvey Sacks. 1970. "On Formal Structures of Practical Actions." Pp. 337-366 in Theoretical Sociology, eds. John C. McKinney and Edward A. Tiryakian. New York: Appleton-Century-Crofts.
- Garfinkel, Harold, Michael Lynch and Eric Livingston. 1981. "The Work of a Discovering Science Construed with Materials from the Optically Discovered Pulsar." Philosophy of the Social Sciences, 11:131-158.
- Giddens, Anthony. 1987. "Alvin Gouldner and the Intellectuals." Pp. 253-274 in his Social Theory and Modern Sociology. Stanford, Ca.: Stanford University Press.
- Gilbert, G. Nigel. 1977. "Referencing as Persuasion." Social Studies of Science, 7:113-122.
- Goffman, Erving. 1952. "On Cooling the Mark Out: Some Aspects of Adaptation to Failure." Psychiatry: Journal for the Study of Interpersonal Processes, 15 (4):451-463.
- \_\_\_\_\_. 1955. "On Face-work: An Analysis of Ritual Elements in Social Interaction." Psychiatry: Journal for the Study of Interpersonal Processes, 18 (3):213-231.
- \_\_\_\_\_. 1956a. "The Nature of Defence and Demeanor." American Anthropologist, 58 (3):473-502.
- \_\_\_\_\_. 1956b. "Embarrassment and Social Organization." American Journal of Sociology, 62 (3):264-271.
- \_\_\_\_\_. 1959. The Presentation of Self in Everyday Life. Garden City, N.Y.: Doubleday Anchor.
- \_\_\_\_\_. 1961a. Asylums: Essays on the Social Situation of Mental Patients and Other Inmates. Garden City, N.Y.: Doubleday Anchor.

- \_\_\_\_\_. 1961b. Encounters: Two Studies in the Sociology of Interaction. Indianapolis, In.: Bobbs-Merrill.
- \_\_\_\_\_. 1963a. Behavior in Public Places. New York: Free Press.
- \_\_\_\_\_. 1963b. Stigma: Notes on the Management of Spoiled Identity. Englewood Cliffs, N.J.: Prentice-Hall.
- \_\_\_\_\_. 1967. Interaction Ritual: Essays on Face-to-Face Behavior. Garden City, N.Y.: Doubleday Anchor.
- \_\_\_\_\_. 1969. Strategic Interaction. Philadelphia: University of Pennsylvania Press.
- \_\_\_\_\_. 1971. Relations in Public: Microstudies of the Public Order. New York: Basic Books.
- \_\_\_\_\_. 1974. Frame Analysis: An Essay on the Organization of Experience. Cambridge, Ma.: Harvard University Press.
- \_\_\_\_\_. 1983. "The Interaction Order." American Sociological Review, 48:1-17.
- Gould, Mark. 1989. "Voluntarism versus Utilitarianism: A Critique of Camic's History of Ideas." Theory, Culture & Society, 6:637-654.
- Gouldner, Alvin W. 1955. "Metaphysical Pathos and the Theory of Bureaucracy." American Political Science Review, XLIX (2):496-507.
- \_\_\_\_\_. 1970a. The Coming Crisis of Western Sociology. New York: Basic Books.
- \_\_\_\_\_. 1970b. "Toward the Radical Reconstruction of Sociology." Social Policy, 1:18-25.
- \_\_\_\_\_. 1971. "Sociology Today Does Not Need a Karl Marx or an Isaac Newton; It Needs a V.I. Lenin." Psychology Today, 5:53-57, 96, 97.
- \_\_\_\_\_. 1972. "The Politics of the Mind." Social Policy, 2 (6):5, 13-21, 54-58.
- \_\_\_\_\_. 1973a. For Sociology. New York: Basic Books.

- \_\_\_\_\_. 1973b. "On the Quality of Discourse Among Some Sociologists." American Journal of Sociology, 79 (1):152-157.
- \_\_\_\_\_. 1976. The Dialectic of Ideology and Technology. New York: Oxford University Press.
- \_\_\_\_\_. 1979. "Talcott Parsons, 1902-1979." Theory and Society, 8:299-301.
- Gouldner, Alvin W. and Helen P. Gouldner. 1963. Modern Sociology. New York: Harcourt, Brace & World.
- Gouldner, Alvin W. and J. Timothy Sprehe. 1965. "The Study of Man, 4: Sociologists Look at Themselves." Trans-Action, 2:42-44.
- Grathoff, Richard. 1978. The Theory of Social Action: The Correspondence of Alfred Schutz and Talcott Parsons. Bloomington, In.: Indiana University Press.
- Griffith, Belver C., Patricia N. Servi, Anita L. Anker, and M. Carl Drott. 1979. "The Aging of Scientific Literature: A Citation Analysis." Journal of Documentation, 35 (3):179-196.
- Gutman, Robert. 1960. "In Defense of Population Theory." American Sociological Review, 25 (3):325-333.
- Hacking, Ian. Representing and Intervening. Cambridge: Cambridge University Press.
- Hagan, Robert A. and Ted. R. Vaughan. 1990. "The Legacy of The Coming Crisis: Gouldner's Contribution to Social Theory." American Sociologist, 20 (4):373-376.
- Hagstrom, Warren O. 1965. The Scientific Community. New York: Basic Books.
- Hamilton, Peter. 1983. Talcott Parsons. London: Horwood/Tavistock.
- Hempel, Carl G. 1965. Aspects of Scientific Explanation. New York: Free Press.
- Henderson, Lawrence J. 1935. "Physician and Patient as a Social System." New England Journal of Medicine, 212:819-823.

- Heritage, J.C. 1971. "Book Review: Coming Crisis." The Sociological Review, 19 (3):454-457.
- Hillery, George A., Jr. 1982. A Research Odyssey. New Brunswick: Transaction Books.
- Hinkle, Roscoe C. 1971. "On the Psychosociology of Knowledge: A Case of Disciplinary Omphaloskepsis." Social Forces, 49 (3):497-499.
- Hirst, Paul Q. 1972. "Recent Tendencies in Sociological Theory." Economy and Society, 1 (1):216-227.
- Homans, George C. 1941. English Villagers of the Thirteenth Century. Cambridge, Ma.: Harvard University Press.
- \_\_\_\_\_. 1950. The Human Group. New York: Harcourt and Brace.
- \_\_\_\_\_. 1958. "Social Behavior as Exchange." American Journal of Sociology, 63 (6):597-606.
- \_\_\_\_\_. 1961. Social Behavior: Its Elementary Forms. New York: Harcourt, Brace & World.
- \_\_\_\_\_. 1962. Sentiments and Activities. New York: Free Press.
- \_\_\_\_\_. 1964a. "Bringing Men Back In." American Sociological Review, 29 (5):809-818.
- \_\_\_\_\_. 1964b. "Commentary." Sociological Inquiry, (Spring): 225.
- \_\_\_\_\_. 1967. The Nature of Social Science. New York: Harcourt, Brace & World.
- \_\_\_\_\_. 1971. "Commentary." Pp.363-379 in Turk and Simpson (1971a).
- \_\_\_\_\_. 1984. Coming To My Senses: The Autobiography of A Sociologist. New Brunswick: Transaction Books.
- \_\_\_\_\_. 1986. "Fifty Years of Sociology." Annual Review of Sociology, 12:xiii-xxx.
- Homans, George C. and Samuel E. Morison. 1930. Massachusetts on the Sea. Boston: Massachusetts Tercentenary Commission.

- Homans, George C. and Charles P. Curtis, Jr. 1934. An Introduction to Pareto: His Sociology. New York: Knopf.
- Huaco, George A. 1986. "Ideology and General Theory: The Case of Sociological Functionalism." Comparative Studies in Society and History, 28 (1):34-54.
- Jacoby, Russell. 1987. The Last Intellectuals. New York: Basic Books.
- Johnson, Miriam. 1990. "Gouldner Twenty Years Later." American Sociologist, 20 (4):377-380.
- Johnston, Barry. 1986. "Sorokin and Parsons at Harvard: Institutional Conflict and the Origin of a Hegemonic Tradition." Journal of the History of the Behavioral Sciences, 22:107-127.
- Kaplan, Leo S. 1971. "Looking Backward." Transaction, 8 (11):64, 66-70.
- King, Donald W., Dennis D. McDonald and Nancy K. Roderer. 1981. Scientific Journals in the United States. Stroudsburg, Pa.: Hutchinson Ross Publishing.
- Kolb, William L. 1961. "Images of Man and the Sociology of Religion." Journal for the Scientific Study of Religion, 1:5-22.
- \_\_\_\_\_. 1962. "Rejoinder to Comment of Talcott Parsons." Journal for the Scientific Study of Religion, 1:214-217.
- Kuhn, Thomas S. 1962. The Structure of Scientific Revolutions. Chicago: University of Chicago Press.
- Lakatos, Imre. 1978. Philosophical Papers, 2 vols., eds. J. Worrall and G. Currie. London: Cambridge University Press.
- Lawani, Stephen M. 1987. "The Ortega Hypothesis, Individual Differences, and Cumulative Advantage." Scientometrics, 12 (5-6):321-323.
- Lawani, Stephen M. and Alan E. Bayer. 1983. "Validity of Citation Criteria for Assessing the Influence of Scientific Publications: New Evidence with Peer Assessment." Journal of the American Society for Information Science, 34 (1):59-66.

- Lepenies, Wolf. 1988. Between Literature and Science: the Rise of Sociology. Cambridge: Cambridge University Press.
- Levesque-Lopman, Louise. 1990. "Seeing Our Seeing: Gouldner's Reflexive Sociology from a Feminist Phenomenological Perspective." American Sociologist, 20 (4):362-372.
- Lidz, Victor M. 1986. "Parsons and Empirical Sociology." Pp. 141-182 in The Nationalization of the Social Sciences, eds. Samuel Z. Klausner and Victor M. Lidz. Philadelphia: University of Pennsylvania Press.
- Lindstrom, Frederick B. 1972. "Sociology Today, Yesterday and Tomorrow." Rocky Mountain Social Science Journal, 9 (1):143-147.
- Line, Maurice B. 1979. "The Influence of the Type of Sources Used on the Results of Citation Analyses." Journal of Documentation, 35 (4):265-284.
- Line, Maurice and Stephen Roberts. 1976. "The Size, Growth, and Composition of Social Science Literature." International Social Science Journal, 28 (1):122-159.
- Line, Maurice B. and A. Sandison. 1974. "'Obsolescence' and Changes in the Use of Literature with Time." Journal of Documentation, 30 (3):283-350.
- Lipset, Seymour Martin and Everett Carll Ladd, Jr. 1972. "The Politics of American Sociologists." American Journal of Sociology, 78 (1):67-104.
- \_\_\_\_\_. 1973. "Gouldner and the 'Politics of American Sociologists.'" American Journal of Sociology, 78 (6):1485-1493.
- Lotka, Alfred J. 1926. "The Frequency Distribution of Scientific Productivity." Journal of the Washington Academy of Sciences, 16:317-323.
- Loubser, J.J., R.C. Baum, A. Effrat and V. Lidz (eds.) 1976. Explorations in General Theory in Social Science, 2 vols. New York: Free Press.
- Luhmann, Niklas. 1982. The Differentiation of Society. New York: Columbia University Press.

- MacRoberts, Michael H. and Barbara R. MacRoberts. 1986. "Quantitative Measures of Communication in Science: A Study of the Formal Level." Social Studies of Science, 16:151-172.
- \_\_\_\_\_. 1987. "Measurement in the Face of Universal Uncertainty: A Reply to Stigler." Social Studies of Science, 17:334-336.
- Mannheim, Karl. 1936. Ideology and Utopia. New York: Harvest Books.
- Martindale, Don. 1982. Personality and Milieu: The Shaping of Social Science Culture. Houston: Cap and Gown Press.
- Merton, Robert K. 1968. "The Matthew Effect in Science." Science, 159 (3810):56-63.
- \_\_\_\_\_. 1976a. Sociological Ambivalence and Other Essays. New York: Free Press.
- \_\_\_\_\_. 1976b. "Structural Analysis in Sociology." Pp. 109-144 in Merton (1976a).
- \_\_\_\_\_. 1982. "Alvin W. Gouldner: Genesis & Growth of a Friendship." Theory and Society, 11:915-938.
- \_\_\_\_\_. 1987. "Three Fragments from a Sociologist's Notebooks." Annual Review of Sociology, 13:1-28.
- \_\_\_\_\_. 1988. "The Matthew Effect in Science, II: Cumulative Advantage and the Symbolism of Intellectual Property." ISIS, 79:606-623.
- Merton, Robert K. and Elinor Barber. 1976. "Sociological Ambivalence." Pp. 3-31 in Merton (1976a).
- Miller, S.M. 1981. "Alvin W. Gouldner: The Sociologist as Outlaw." Social Policy, 12 (1):2-6.
- Mills, C. Wright. 1959. The Sociological Imagination. New York: Oxford.
- Moore, Barrington, Jr. 1958. Political Power and Social Theory. Cambridge, Ma.: Harvard University Press.
- Moravcsik, M.J. 1973. "Measures of Scientific Growth." Research Policy, 2:266-275.

- Moravcsik, M.J. and P. Murugesan. 1975. "Some Results on the Function and Quality of Citations." Social Studies of Science, 5:86-92.
- Motylev, V.M. 1989. "The Main Problems of Studying Literature Aging." Scientometrics, 15 (1-2):97-109.
- Mullins, Nicholas C. 1973. Theories and Theory Groups in Contemporary American Sociology. New York: Harper and Row.
- Munch, Richard. 1987. Theory of Action: A New Synthesis Going Beyond Parsons. London: Routledge & Kegan Paul.
- O'Neill, John. 1972. "The New Sociology and the Advent of Alvin W. Gouldner." Canadian Review of Sociology and Anthropology, 9 (2):167-175.
- \_\_\_\_\_. 1989. The Communicative Body. Evanston, Il.: Northwestern University Press.
- Oromaner, M.J. 1969. "The Audience as a Determinant of the Most Important Sociologists." American Sociologist, 4:332-335.
- \_\_\_\_\_. 1977. "The Career of Sociological Literature: A Diachronous Study." Social Studies of Science, 7:126-132.
- \_\_\_\_\_. 1979. "The American Sociological Community and the Works of Talcott Parsons: 1936-1950." The Journal of the History of Sociology, 1 (2):76-92.
- \_\_\_\_\_. 1980. "Erving Goffman and the Academic Community." Philosophy of the Social Sciences, 10 (3):287-291.
- \_\_\_\_\_. 1981. "Cognitive Consensus in Recent Mainstream American Sociology: An Empirical Analysis." Scientometrics, 3 (2):73-84.
- \_\_\_\_\_. 1985. "The Ortega Hypothesis and Influential Articles in American Sociology." Scientometrics, 7 (1-2):3-10.
- \_\_\_\_\_. 1986. "The Diffusion of Core Publications in American Sociology: A Replication." International Journal of Information Management, 6:29-35.

- \_\_\_\_\_. 1987. "Ortega, Obliteration and Policy Consequences." Scientometrics, 12 (5-6):317-319.
- Parsons, Talcott. 1928. "'Capitalism' in Recent German Literature: Sombart and Weber, I." Journal of Political Economy, 36:641-661.
- \_\_\_\_\_. 1929. "'Capitalism' in Recent German Literature: Sombart and Weber, II." Journal of Political Economy, 37:31-51.
- \_\_\_\_\_. 1931. "Wants and Activities in Marshall." Quarterly Journal of Economics, 46:101-140.
- \_\_\_\_\_. 1932. "Economics and Sociology: Marshall in Relation to the Thought of His Time." Quarterly Journal of Economics, 46:316-347.
- \_\_\_\_\_. 1933a. "Malthus." Encyclopedia of the Social Sciences, 10:68-69.
- \_\_\_\_\_. 1933b. "Pareto." Encyclopedia of the Social Sciences, 11:576-578.
- \_\_\_\_\_. 1934. "Sociological Elements in Economic Thought, I." Quarterly Journal of Economics, 49:414-453.
- \_\_\_\_\_. 1935a. "Sociological Elements in Economic Thought, II." Quarterly Journal of Economics, 49:645-667.
- \_\_\_\_\_. 1935b. "The Place of Ultimate Values in Sociological Theory." International Journal of Ethics, 45:282-316.
- \_\_\_\_\_. 1937. The Structure of Social Action. Glencoe, Il.: Free Press.
- \_\_\_\_\_. 1951. The Social System. New York: Free Press.
- \_\_\_\_\_. 1960. Structure and Process in Modern Society. New York: Free Press.
- \_\_\_\_\_. 1966. Societies: Evolutionary and Comparative Perspectives. Englewood Cliffs, N.J.: Prentice-Hall.
- \_\_\_\_\_. 1970. "On Building Social System Theory: A Personal History." Daedalus, 99 (4):826-881.

- \_\_\_\_\_. 1971. "Commentary." Pp. 380-399 in Turk and Simpson (1971a).
- \_\_\_\_\_. 1978. Action Theory and the Human Condition. New York: Free Press.
- \_\_\_\_\_. 1990 [previously unpublished manuscript from 1934]. "Prolegomena to a Theory of Social Institutions." American Sociological Review, 55 (3):319-333.
- Parsons, Talcott and Edward A. Shils (eds.) 1951. Toward a General Theory of Action. Cambridge, Ma.: Harvard University Press.
- Parsons, Talcott, Robert F. Bales and Edward Shils. 1953. Working Papers in the Theory of Action. New York: Free Press.
- Parsons, Talcott and Robert F. Bales. 1955. Family, Socialization, and Interaction Process. New York: Free Press.
- Parsons, Talcott and Neil J. Smelser. 1956. Economy and Society. New York: Free Press.
- Parsons, Talcott and Gerald M. Platt. 1973. The American University. Cambridge, Ma.: Harvard University Press.
- Peritz, B.C. 1983. "Are Methodological Papers More Cited than Theoretical or Empirical Ones? The Case of Sociology." Scientometrics, 5 (4):211-218.
- Peterson, Richard A. 1971. "Review Symposium: Coming Crisis." American Sociological Review, 36 (2):326-328.
- Porter, Alan L. 1977. "Citation Analysis: Queries and Caveats." Social Studies of Science, 7:257-267.
- Posner, Judith. 1978. "Erving Goffman: His Presentation of Self." Philosophy of the Social Sciences, 8:67-78.
- \_\_\_\_\_. 1980. "Rebuttal: Oromaner Paper." Philosophy of the Social Sciences, 10 (3):293-294.
- Price, Derek de Solla. 1976. "A General Theory of Bibliometric and Other Cumulative Advantage Processes." Journal of the American Society for Information Science, 27 (5/6):292-306.

- \_\_\_\_\_. 1986. Little Science, Big Science...and Beyond. New York: Columbia University Press.
- Proust, Joelle. 1989. Questions of Form: Logic and the Analytic Proposition from Kant to Carnap, tr. by Anastasios Albert Brenner. Minneapolis: University of Minnesota Press.
- Rex, John. 1981. Social Conflict: A Conceptual and Theoretical Analysis. London: Longman.
- Reynolds, Larry and Janice Reynolds (eds.) 1970. The Sociology of Sociology. New York: David McKay Company.
- Rhoads, John K. 1972. "On Gouldner's Crisis of Western Sociology." American Journal of Sociology, 78 (1):136-154.
- \_\_\_\_\_. 1973. "Rhoads Replies to Gouldner." American Journal of Sociology, 78 (6):1493-1496.
- Ritzer, George. 1970. "Book Review: The Coming Crisis of Western Sociology." Kansas Journal of Sociology, 6 (4):213-216.
- Roach, Jack L. 1971. "Book Review: Coming Crisis." Social Science Quarterly, 52 (2):422.
- Robertson, Roland and Bryan S. Turner. 1989. "Talcott Parsons and Modern Social Theory - An Appreciation." Theory, Culture & Society, 6:539-558.
- Rocher, Guy. 1975. Talcott Parsons and American Sociology, tr. by Barbara and Stephen Mennell. London: Nelson.
- Rogers, Mary F. 1980. "Goffman on Power, Hierarchy, and Status." Pp. 100-133 in The View from Goffman, ed. Jason Ditton. New York: St. Martin's Press.
- Savage, Stephen P. 1981. The Theories of Talcott Parsons. New York: St. Martin's Press.
- Sciulli, David and Dean Gerstein. 1985. "Social Theory and Talcott Parsons in the 1980s." Annual Review of Sociology, 11:369-387.
- Shaw, Martin. 1971. "The Coming Crisis of Radical Sociology." New Left Review, 70:101-109.

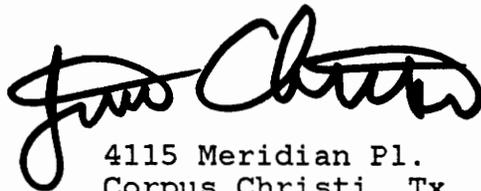
- Sica, Alan. 1983. "Parsons, Jr." American Journal of Sociology, 89 (1):200-219.
- \_\_\_\_\_. 1989. "Handbooks Past and Present." Contemporary Sociology, 18 (4):504-508.
- Sievert, MaryEllen and Mary Haughwout. 1989. "An Editor's Influence on Citation Patterns: A Case Study of Elementary School Journal." Journal of the American Society for Information Science, 40 (5):334-341.
- Simpson, Richard L. 1971. "System and Humanism in Social Science." Science, 172:661-664.
- Smelser, Neil J. (ed.) 1988a. Handbook of Sociology. Newbury Park, Ca.: Sage.
- \_\_\_\_\_. 1988b. "Introduction." Pp. 9-19 in Smelser (1988a).
- \_\_\_\_\_. 1988c. "Social Structure." Pp. 103-129 in Smelser (1988a).
- Smith, Gregory W.H. and Frances Chaput Waksler. 1989. "The Published Works of Erving Goffman." Human Studies, 12:173-176.
- Smith, Linda C. 1981. "Citation Analysis." Library Trends, 30:83-106.
- Snizek, William E., Ellsworth R. Fuhrman and Michael K. Miller. (eds.) 1979. Contemporary Issues in Theory and Research. Westport, Conn.: Greenwood Press.
- Sorokin, Pitirim A. 1963. A Long Journey. New Haven, Ct.: College and University Press.
- Stigler, Stephen M. 1987. "Precise Measurement in the Face of Error: A Comment on MacRoberts and MacRoberts." Social Studies of Science, 17:332-334.
- Swanson, Guy E. 1968. "Review Symposium: Studies in Ethnomethodology." American Sociological Review, 33 (1):122-124.
- \_\_\_\_\_. 1971. "Review Symposium: Coming Crisis." American Sociological Review, 36 (2):317-321.

- Swatos, William H., Jr. 1980. "Five More Years: Introductory Text Citations Revisited." Southern Sociologist, 11 (2):15-18.
- Tiryakian, Edward A. 1979. "The Significance of Schools in the Development of Sociology." Pp. 211-233 in Snizek et al. (1979).
- Toby, Jackson. 1972. "Gouldner's Misleading Reading of the Theories of Talcott Parsons." Contemporary Sociology, 1 (1):109-110.
- Touraine, Alain. 1972. "Review Symposium: Coming Crisis." American Journal of Sociology, 77 (2):317-323.
- Turk, Herman and Richard L. Simpson (eds.) 1971a. Institutions and Social Exchange: The Sociologies of Talcott Parsons & George C. Homans. Indianapolis: Bobbs-Merrill.
- \_\_\_\_\_. 1971b. "Perspectives on Homans, Parsons, and Current Theory." Pp. 3-20 in Turk and Simpson (1971a).
- Turner, Jonathan H. 1989. "The Disintegration of American Sociology." Sociological Perspectives, 32 (4):419-433.
- Turner, Stephen P. and Jonathan H. Turner. 1990. The Impossible Science: An Institutional Analysis of American Sociology. Newbury Park, Ca.: Sage.
- Udy, Stanley H., Jr. 1960. "Review of Structure and Process in Modern Societies." American Journal of Sociology, 66 (1):96.
- Urry, John. 1972. "More Notes on Sociology's Coming Crisis." British Journal of Sociology, 23 (2):246-248.
- van Fraassen, Bas C. 1989. Laws and Symmetry. Oxford: Clarendon Press.
- Wallace, Anthony F.C. 1968. "Review Symposium: Studies in Ethnomethodology." American Sociological Review, 33 (1):124-126.
- Wax, Murray L. 1970. "Some Errors of Gouldner's History." Kansas Journal of Sociology, 6 (4):217-219.

- Wearne, Bruce C. 1989. The Theory and Scholarship of Talcott Parsons to 1951. Cambridge: Cambridge University Press.
- Wiley, Norbert. 1979. "The Rise and Fall of Dominating Theories in American Sociology." Pp. 47-79 in Snizek et al. (1979).
- Wilkins, James. 1968. "Review of Studies in Ethnomethodology." American Journal of Sociology, 73 (5):642-643.
- Wilner, Patricia. 1985. "The Main Drift of Sociology Between 1936 and 1982." History of Sociology, 2:1-20.
- Wrong, Dennis. 1959. "The Failure of American Sociology." Commentary, (Nov.):375-380.
- \_\_\_\_\_. 1961. "The Oversocialized Conception of Man in Modern Sociology." American Sociological Review, 26:183-193.
- Young, T.R. 1971. "The Politics of Sociology: Gouldner, Goffman, and Garfinkel." American Sociologist, 6 (4):276-281.
- Zeitlin, Maurice. 1972. "Review Symposium: Coming Crisis." American Journal of Sociology, 77 (2):312-317.
- Ziman, John M. 1968. Public Knowledge. Cambridge, Ma.: Cambridge University Press.
- Zuckerman, Harriet. 1977. Scientific Elite: Nobel Laureates in the United States. New York: Free Press.
- \_\_\_\_\_. 1987. "Citation Analysis and the Complex Problem of Intellectual Influence." Scientometrics, 12 (5-6):329-338.

## VITA

James Chriss was born in Kalamata, Greece on May 12, 1955. He attended the University of Texas at Austin from 1973-76, majoring in mathematics. After a tour of active duty in the U.S. Navy he returned in 1987 to complete his bachelor's degree in sociology/math, graduating from Texas A&I University, Kingsville in December, 1988. He began graduate studies in January, 1989 at Virginia Polytechnic Institute and State University, receiving his master's degree in sociology in December, 1990.



4115 Meridian Pl.  
Corpus Christi, Tx. 78411