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THE CHICAGO SCHOOL: A METASCIENTIFIC STUDY

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In memory of my dad
Aveley Walter Harvey
whose encouragement
made this possible.
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ABSTRACT

The value of the concept of 'school' in metascience is examined with reference to the 'Chicago School' of sociology during the period from 1900 to 1952. The notion of school is widely used by sociologists in accounting for developments within their discipline. However, the use of schools as a framework for documenting and interpreting the history of sociology tends to obscure the complexity and variety of intellectual development.

Five myths about the 'Chicago School' are identified: that its members were social ameliorists; that they were primarily ethnographers; that they exhibited little concern with theory; that they were heavily reliant on a framework provided by the social psychological perspective of George Herbert Mead; and that they were an insular group with little direct involvement in, or long term impact on, the development of sociology in the United States.

It is argued that sociological work at the University of Chicago was an integral part of American sociology throughout the period under study and that the 'Chicago School' did not display a distinct set of theoretical and methodological ideas. What was common to the members of the sociology department of the University of Chicago was in large part also typical of American sociology as a whole.

In the light of this empirical study, the potential of the metascientific models proposed by Mullins (1973) and Tiryakian (1979a, 1979b) is examined in detail both in terms of their theoretical underpinnings and their approach to the case study material. 'Doubt is cast on the value of these models and the implications of this for a 'schools' or 'unit approach' to metascience are considered. It is suggested that a schools approach which concentrates on the knowledge transformative processes within a school rather than on identifying schools with a distinct set of ideas might be a more profitable way of developing a theory of the production of sociological knowledge and would be less likely to perpetrate myths.
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CHAPTER ONE

METASCIENCE AND THE CONCEPT OF 'SCHOOL'
IN THE SOCIOLOGY OF KNOWLEDGE
1.1 Metascience.

The usage of the term metascience in this thesis reflects that outlined by Radnitzky (1973). It is research into science as a developer of knowledge. Etymologically, it is something coming 'after' science, or 'about' science. Science is here taken to refer to any empirically grounded area of enquiry, through which theoretical statements about the nature of the world (physical, natural or social) are made. This position is synonymous with such terms as 'Wissenschaft', 'scienza' and 'nauka'. The metascientific enquiry in this thesis is specifically oriented to the production of scientific knowledge, from innovation to legitimation.

In general, the analysis of the process of the production of scientific knowledge has tended to be empirically grounded through reference to detailed case studies. These either involve participant observation or, more usually, historical reconstructions. In either case, there is a tendency towards post hoc reconstruction directed towards a specific metascientific perspective.

1.2 The Nature of 'Schools' in the Sociology of Knowledge

In order to assist in unravelling complex interrelations of ideas, research practice and personnel in all branches of the sciences and the humanities, historians and sociologists of the different disciplines have tended to develop loose categorisations of people to which the label 'school' is
frequently applied.

Such references to 'schools' in the sociology and history of sociology, however, (e.g. the Chicago School, the Frankfurt School) imply more than particular institutional affiliation. It implies that within that institution there is an accepted way of working particularly in respect of a given discipline. Thus there is the Chicago School of sociology, of philosophy, of economics and of law. These are not simply departments within the University but assumed groupings of co-operating or like-minded practitioners. Sometimes this is delimited to a group of practitioners sharing the same theoretical presuppositions. This notion of school is also extended to national boundaries, so that commentators refer to a Russian school of sociology or Polish sociology, for example. At its most general, a school may have no physical referents of importance, and rather reflect a 'school of thought' in the sense of a group of theorists sharing the same philosophy, or of an identifiable theoretical or philosophical perspective to which significant figures in the history may be attached, or a 'general theoretic orientation' (Merton 1968a), a 'tradition' or 'paradigm' as in Marxism or functionalism. Szacki (1975) also suggested that schools can be constructed in terms of typologies reflecting their content. This profusion of usages leads to a variety of overlapping schools and an ambiguity as to the precise nature of any given school. The concept should not, then, be taken for granted, its genesis and usage are important areas of investigation.

The criteria for demarcation of 'schools' is, thus, somewhat
vague, and essentially, nothing more than a convenience device, or convention, for grouping certain people together to show similarities in style, approach, epistemology, theoretical concerns, or substantive interests. [1]

This unsystematic approach to the unit of knowledge production tends to lead to a view of the progress of knowledge as the progress either of 'great ideas' being developed and expanded, or as the work of 'great people', who can be identified as major contributors and slotted into various 'camps'. This general approach tends, too, to be self-perpetuating. The 'schools' or groups identified remain as the focal point of the analysis of knowledge production and the supposed concerns of such groupings becomes fossilised and mythologised. In short, the history and sociology of knowledge lean heavily on secondary accounts and preconceptions of the concerns of pre-identified central groups.

The confusion of usage of the term school [2] has had repercussions on the development of the concept for purposes of metascientific analysis. As more or less any grouping, of people or ideas, can be said to constitute a school in one sense or another then the term has been of little specific analytical value. Thus, school has been a synonym for groups of collaborators, but, for the most part, lacking in consistency of usage and failing to provide a framework for assessing the relevance and impact of the social milieu on the production of scientific knowledge.

Doubts about the utility of the notion of schools in the history, philosophy and sociology of knowledge and of sociology in
particular are confronted directly by two relatively recent advocates of the schools approach. Mullins (1973) and Tiryakian (1979a, 1979b) in their critiques of conventional 'great man' and 'great ideas' approaches to the history of scientific disciplines, both attempt to refine and systematise the notion of school as a metascientific construct in light of the developments in the philosophy of science which stemmed from the publication of Kuhn's (1962b, 1970) thesis of the paradigm nature of knowledge and, to a lesser extent, from Lakatos' methodology of scientific research programmes (Lakatos, 1970, 1975). Mullins and Tiryakian also draw on the work of Price (1963) and its particular exposition by Crane (1972). The notion of scientific community offered by Mullins and Tiryakian will be examined to see how the historicism of the 'great ideas' and 'great persons' approaches is confronted by a 'unit' approach. First, however, the paradigmatic and research programme theses and the idea of invisible college, which underpin the approaches developed by Mullins and Tiryakian will be examined.

1.3 Kuhnian Paradigms

Kuhn's paradigm thesis is well known, if somewhat abused in the social sciences (Harvey, 1982; Eckberg & Hill, 1979; Martins, 1972). Kuhn's model evolved from a critique of Popperian falsificationism. While both Kuhn and Popper were concerned with the logical structure of the products of scientific research, with the concurrence between history of science and theories of scientific development and were opposed to the empiricism of
classical positivism, disavowed inductivism and rejected the idea that science progresses through accretion in a simple cumulative manner, they, nonetheless, differed substantially. Kuhn considered that Popper characterised science in terms that apply only to its revolutionary moments; that Popper ignored 'normal' science. By concentrating on overthrows of theory, Popper misconstrued the usual practice of science. He ignored the normal puzzle solving which of necessity accepts the theory (or theories) as 'raison d'être' for the puzzle. Contingent upon this is the process of growing crisis which sparks off the revolution in thought. Popper ignored this too, regarding the context of discovery as the province of 'psychologism'. Furthermore, Kuhn argued, it is 'normal science' which 'most clearly distinguishes science from other enterprises' rather than the occasional extraordinary scientific exploits which of necessity involve a return to philosophical debate.

Kuhn also rejected the whole notion of testability. He considered that science does not 'progress' through the testing and disposal of theories, which is so central to Popper's falsificationism thesis. Fundamentally, Kuhn raised the old problem of the theoretical context of observation and suggested that, although Popper did not admit conclusive disproof of theories through observation, his adherence to his falsificationist position (however sophisticated it may appear) was still essentially naive [4] because he failed to confront the problems of the theory laden nature of observation. To devise 'tests' of a theory, Kuhn pointed out, would require going beyond that theory in order to
conceptualise such tests. It requires a theory beyond a theory to frame hypotheses and tests of the original theory. To simply test from within is to develop the puzzles of 'normal' science (which may be resolved from within the prevailing theoretical context, or 'paradigm', or be added to the list of anomalies). The framing of real tests, (ie. ones going beyond the theoretical framework) requires a psychological shift, or gestalt switch on the part of the scientist. It requires a new way of seeing. Such shifts are traumatic and not part of 'normal' science. Science does not progress through constant tests of theory, rather through an accumulation of anomalies as the result of normal puzzle solving activities of scientists. Popper demanded that science is marked by the falsifiability of its theories. Kuhn, on the other hand, suggested that science is marked by puzzle solving, the elaboration of theories constrained by a paradigm.

In his 'Structure of Scientific Revolutions' (1962b) Kuhn expounded the basic principles of his thesis of the production of scientific knowledge. His subsequent contributions to the sociology and philosophy of knowledge have largely been amendments of this position in response to critiques. His 'replies' are largely contained in his two contributions to Lakatos and Musgrave (1970), and the Appendix to the second edition of 'Structures of Scientific Revolutions' (Kuhn 1970).

Kuhn's model is one which presupposes that science 'grows' through shifts in the basic conceptions operated by the scientific community. Such conceptions are located within a scientific paradigm. A single dominant paradigm emerges from the
plethora of theories which marks the pre-paradigmatic or non-scientific stage of a discipline. The concept of paradigm is, then, central to Kuhn's thesis about the growth and development of science. It provides a framework for understanding the nature of change and stasis in science. It embodies certain fundamental views of science and scientific development crucial to the model of change advanced by Kuhn. Yet, despite its centrality Kuhn has been somewhat vague about the concept of paradigm and it can be seen to have several meanings (Masterman, 1970). This has led to a confusion in the analysis of Kuhn's model and also a variety of interpretations of Kuhn's thesis.

Kuhn (1970) attempted a clarification of his concept of paradigm. For him, a paradigm is a framework which constrains scientific activity. This framework embodies two types of guiding principles, metaphysical and exemplary. The metaphysical sense of paradigm involves a view that the scientist works within a taken-for-granted context, one in which certain principles of science and aspects of the discipline in which the scientist is located are unquestioningly self-evident. The paradigm, thus embodies a subliminal metaphysical core which is quite incontrovertible but which is rarely explicit. It is so much taken for granted that it is inconceivable that it be challenged, indeed, such challenge would involve philosophic speculation well beyond the normal reflective activity of the practising scientist.

The second sense, that of exemplar, effectively makes the paradigm empirically determinable. It gives the practising
The prevailing conceptualisation or paradigm of science (or of a scientific discipline) is the framework within which scientists operate in order to elaborate problems. The work done by scientists within a paradigm is 'normal science'. Most scientists spend most of their time solving puzzles which are thrown up by the paradigm. This 'normal' activity relates to how the scientist is expected to pursue his or her work and present it for judgement and acclaim by peers. A paradigm contains theories that need refining. Normal science, puzzle solving, working out the details of theories within a paradigm, is, then, what usually occupies the attention of scientists. As such, normal science seems to be an attempt to 'force nature into a preformed and relatively inflexible box' which is supplied by the paradigm.

'No part of the aim of normal science is to call forth new sorts of phenomena; indeed those that will not fit the box are often not seen at all. Nor do scientists normally aim to invent new theories, and they are often intolerant of those invented by others [5]. Instead, normal-scientific research is directed to the articulation of these phenomena and theories that the paradigm already supplies.' (Kuhn, 1970, p. 24).
Normal science may lead to the restructuring of a theory and frequently to a readjustment of the theory but it does not lead to the guiding principles of scientific work embodied in the paradigm being challenged. These guiding principles are not only 'metaphysical' constraints but reflect the established and accepted practice of doing science and the admissibility of evidence; particularly the prescription not to pursue idle speculation tangential to well-corroborated theories. There is no code of practice, as such, nor any list of immutable propositions, but the scientific community lays down its principles through its pedagogic function, by its very transmission of scientific knowledge. Its emphasis on exemplary theories and processes, its reification of certain 'affirmative' experiments, of the experimental method and of particular rules of reasoning, are central to the establishment of paradigm principles through pedagogy. This is reinforced by the exposure given to scientific work through professional journals. As such, then, a paradigm is a set of guiding principles constraining scientific work. A paradigm thus provides a framework and plenty of scope within which to pursue the 'normal' practice of science. It is, according to Kuhn, the paradigm adherence to normal science which distinguishes scientific from non-scientific activity. Without it, practitioners cannot escape philosophical debate and are unable to undertake the work of puzzle solving.

Nonetheless, this process of puzzle solving within normal science produces anomalies. An anomaly for Kuhn is an
'observational' result which the theory (or theories) within a paradigm is unable to account for but which has been produced within the paradigm. Normally, anomalies are shelved, eventually adjustments are made to theories within a particular paradigm in order to account for anomalies. However, the process of adjustment is limited. Ultimately, irresolvable anomalies appear and 'ad hoc' amendments are made. As anomalies accrue the effect on the paradigm becomes increasingly damaging. As more anomalous observations become 'well corroborated'; as ad hoc adjustments and auxiliary hypotheses begin to contradict one another; as theories become self-contradictory in order to account for anomalies without transcending the paradigm, then the scientist, and the scientific community as a whole are forced to rethink the guiding principles of the paradigm. The piling up of anomalies (and Kuhn implies that this is an exponential development) causes a kind of anomie amongst scientists practicing within a particular paradigm. Slowly anomalies shift from the periphery of concern of the scientific community to the centre of the stage. Eventually the resolution of anomalies becomes the subject matter of the discipline. This leads to a crisis of confidence in the paradigm.

For Kuhn, shifts in knowledge come about as a result of the inability of the prevailing conceptualisation to deal with anomalies. A revolution takes place and a new paradigm replaces the old one. Such shifts are relatively rare and constitute revolutions in theory. Such revolutions embody a fundamental shift of ideas and are not evident in the usual practice of science.
The new paradigm is characterised, eventually, by its ability to explain all that the old paradigm could, plus some of the anomalies. This is Kuhn's conception of 'progress' in science. The new paradigm embodies totally new theories and is quite incommensurable with the previous paradigm. The work of normal science, then, is to develop, articulate and specify the theory or theories embodied in the new paradigm. Once again, anomalies will appear leading to a further crisis and, eventually, a paradigmatic shift. Thus the cycle goes on indefinitely.

Paradigm shifts are not rational affairs directed by the subject matter of science but require a reconceptualisation on the part of the scientist, which Kuhn likened to a 'Gestalt switch'. The scientist working in the area no longer sees the the field of study in the same light as it had once appeared, a revolutionary shift is not a logical outcome but the result of persuasion. This is not a smooth adaptation of prior theories. A crisis is characterised by a disunity within the paradigm; by a large number of alternative theoretical conjectures, initially adjustments to established theory and subsequently attempts to reformulate a theory within a paradigm. Ultimately a single new paradigm will emerge and normal puzzle solving re-commence. Not all scientists will be 'converted' to the new paradigm, but the conservative ones will gradually die out and the new paradigm will become the scientific orthodoxy.

The development of knowledge, for Kuhn, then, is consensual. Kuhn makes this clearer in his replies to critics where he emphasised the role of the 'scientific community' in his model. He
considered that his original model underdeveloped the social interactive aspect of science, and a concentration on this element helped overcome one empirical objection raised generally against his account; namely, the implied congruence of scientific communities with subject matter. Kuhn (1970) suggested that scientific communities exist at various levels, from science to speciality, and that paradigms are shared by members of such communities. In short, that paradigms are community based.

1.4 Lakatos' Methodology of Scientific Research Programmes

While Kuhn's paradigm thesis is well known, Lakatos' thesis of scientific research programmes is less well known and comparatively little discussed. However, it represents the most sophisticated presentation of the falsificationist approach which Kuhn had called into question [6]. Lakatos took account of Kuhn's objections to Popper's theory and offered an historically more 'accurate'-model than Popper's. However, Lakatos embraced neither the paradigmatic mechanism nor Kuhn's ostensive irrationalism. The central element of Lakatos' approach is the scientific research programme whose history is rationally reconstructed. Lakatos focused on the internal historiography of science, taking account of external factors only as they constitute rational elements in scientific decisions. These he then subsumed under the rubric of 'internal and rational'.

Lakatos posited a view of science as community based practitioners developing research programmes. In Lakatos' scheme,
scientists engaged in the practice of any science are operating within a research programme. A scientific research programme is characterised by a 'hard core' which is adopted by convention and a 'protective belt' of assumptions. The hard core is the 'unfalsifiable central tenets', the core assumptions upon which all work within the programme is based. The protective belt comprises the surrounding set of assumptions which are, ultimately, subject to negotiation and amendment in the light of work carried out in the programme, or as a result of discoveries made elsewhere which conflict with the operating principles of the programme. The research programme is, in itself, a constructive enterprise, seeking to discover novel phenomena and develop the theoretical framework through a sophistication of the protective belt.

The methodological rules which outline the direction of a research programme are of two sorts. The negative heuristic informs the members of which paths of research to avoid and the positive heuristic informs them of the paths to pursue. The negative heuristic involves the articulation of this hard core and the deflection of research interests away from it. The negative heuristic protects the 'hard core' by demanding ingenuity in the construction of auxiliary hypotheses which form a 'protective belt' around the core. All research activity aimed at the core must be directed to the belt of auxiliary hypotheses. This protective belt comes under scrutiny and is adjusted (or even replaced) to maintain the integrity of the core. The scientist, having accepted a conventional 'universal theory',
constructs a programme that seeks to supplement the irrefutable hard core so that it may explain and predict real phenomena. This 'positive heuristic' defines problems, outlines the construction of a belt of auxiliary hypotheses, foresees anomalies and turns them victoriously into examples, all according to a preconceived plan. The scientist lists anomalies, but as long as his research programme sustains its momentum he may freely put them aside. It is primarily the positive heuristic of his programme, not the anomalies, which dictate [sic] the choice of his problems' (Lakatos, 1975, pp. 9-10.).

The success of a research programme, then, depends on these changes in the protective belt. Changes in the protective belt are not spontaneous, rather they are the result of 'a long term research policy which anticipates ... refutations. This research policy, or order of research, is set out - in more or less detail - in the positive heuristic of the research programme.' (Lakatos, 1970, p. 135).

Thus, argued Lakatos, the positive heuristic allows the researcher to make sense of the research sphere, principally by anticipating problematic areas and projecting likely resolutions. The positive heuristic saves the scientist from becoming confused by the anomalies, tackling the problem by setting out in a programmatic way an ever more complex and comprehensive model simulating reality. The scientist is thus concerned to build up the model following instructions which are laid down in the positive part of the programme. Effectively, then, the scientist ignores the available data in as much as counter examples are shelved. As Lakatos noted, 'If a scientist (or mathematician) has a positive heuristic, he refuses to be drawn into observation. He will 'lie down on his couch and forget about the data'... Occasionally, of course, he will ask Nature a shrewd question: he will then be encouraged by Nature's yes, but not discouraged by its no.' (Lakatos, 1970, footnote 1 to page 135). [7]
It is essential to think of developments in research programmes in terms of models (of increasing sophistication). A model is a set of initial conditions that one knows is bound to be replaced, the positive heuristic provides the key to foreseeing, more or less, how these initial conditions will be superceded.

Research programmes are launched enthusiastically and, in the initial stages, persevered with despite difficulties and evident falsifications. Anomalies at this stage are ignored. The programme becomes established and while a forward momentum persists, usually thanks to successful research results, the programme will continue to develop, and gradually anomalies will be overcome. Ignoring anomalies is not an irrational act by the programmatic researcher, provided new developments are being made.

While a programme continues to develop its positive heuristic successfully, without compromising its hard core, and is similarly able to account for discoveries that are external to the programme, it is said to be a progressive programme. When it ceases to produce novel phenomena and has to content itself with amendments to its protective belt of assumptions in the light of external developments, it is stagnating and when such external developments fundamentally challenge the hard core without the programme answering through novel discoveries, then it is said to be a degenerating research programme.

According to Lakatos, scientists are acting rationally in adhering to a programme while it is progressive irrespective of anomalies. However, once the momentum falters and the programme begins to stagnate practitioners will begin to move off, either
developing entirely new interests or gravitating towards a programme that exhibits a progressive problemshift. It is the replacing of one programme by another with more empirical and theoretical content that constitutes the rational development of scientific knowledge. Thus anomalies only have significance once a programme ceases to be progressive.

In short, then, Lakatos is arguing that scientists work out a programme and that falsifiability operates only when the momentum of the programme diminishes. The negative heuristic specifies the 'hard core' and is irrefutable by dint of a methodological decision of the research workers, thus providing a secure and stable basis from which to work. The positive heuristic consists of a 'partially articulated set of suggestions and hints' on how to develop the research, that is; how to 'sophisticate the protective belt'. A programme is progressive while such sophistication continues.

Lakatos argued that science is a rational enterprise. The development of science, he asserted, can be reconstructed objectively. The focus of attention of this objective rational reconstruction is to be 'problemshifts' in scientific theories. Lakatos saw a progressive problemshift as one where a problemshift is both theoretically and empirically progressive. A series of theories will be theoretically progressive if each new theory has some excess empirical content over its predecessor. Excess empirical content means that the new theory predicts some novel, hitherto unexpected fact. Similarly an empirically progressive problemshift is characterised by the corroboration of
excess empirical content, that is, each new theory leads to the actual discovery of a new fact.

Progress is measured by the degree to which a problemshift is progressive, that is, by the degree to which a series of theories leads to the discovery of novel facts. A theory in the series is 'falsified' when it is superceded by a theory with a higher corroborated content. If a research programme progressively explains more than competing research programmes then, for Lakatos, the competitor is 'superceded' and may be eliminated, or at least shelved.

Lakatos asserted that the history of science clearly shows the persistence of 'refuted' (in Popperian terms) research programmes. Some of these refuted programmes emerge as genuinely progressive, and no programme would take off at all if the Popperian falsificationist principle were rigidly adhered to. Lakatos places a premium on the theoretical autonomy of science. Any mediation by 'fact' is supplementary. But while the strong positive heuristic of a research programme is reflected by a heavy emphasis on theoretical development prior to experimentation, the very progressive nature of a research programme is exemplified by the theory's ability to be confirmed by observational test.

Lakatos suggested that scientists operate within more than one programme and are able to find clues in one that provide for development in another. This facilitates transition, provides a content for the synthesis of programmes (such as Maxwell's electro-magnetic theory) and underplays revolutionary moments in
the history of science.

The growth of science is thus characterised by this serial nature of theoretical development. Indeed Lakatos maintained that the most important series of theories for the history of science are those which are characterised by some continuity of personnel as well as ideas. Such a continuity evolves from a 'genuine research programme adumbrated at the start'. For Lakatos, a scientific research programme must be programmatic. It should map out future programmes of research and to do this it must be internally coherent and provide a context for the discovery of novel phenomena. This situation he sees as obtaining only in the natural and pure sciences and not in the social disciplines. Freudianism and Marxism do not fulfill the latter criteria and sociology is not programmatic, according to Lakatos.

1.5 Invisible Colleges

Price (1961, 1963, 1965a) and later Crane (1965, 1969, 1972) have elaborated the idea of invisible college as the basic metascientific unit. Price (1963) argued that invisible colleges arise as a pragmatic response to the growth of science from 'little science' to 'big science'. Essentially, the rapid development of science, the escalating education of, and generation of scientists, the massive cost of research, the spiralling numbers of publications and the continual splitting of sciences into specialist areas means that a researcher, if determined to 'progress', needs to become involved in one specific area. However this does not mean that science is
comprised of non-communicating closed shops. According to Price research scientists tend to 'congregate' in communicating groups with an upper limit of around one hundred members. These groups Price called 'invisible colleges'. An invisible college is characterised by an unofficial network which gives

'each man status in the form of approbation from his peers, they confer prestige, and, above all, they effectively solve a communication crisis by reducing a large group to a small select one of the maximum size that can be handled by interpersonal relationships....

These groups devise mechanisms for day-to-day communication. There is an elaborate apparatus for sending out not merely reprints of publications but preprints and pre-preprints of work in progress and results about to be achieved.... In addition ways and means are being found for physical juxtaposition of the members. They seem to have mastered the art of attracting invitations from centers where they can work along with several members of the group for a short time. This done, they move on to the next centre and other members. Then they return to home base, but always their allegiance is to the group rather than to the institution which supports them, unless it happens to be a station on such a circuit. For each group there exists a sort of communicating circuit of institutions, research centers, and summer schools, giving them the opportunity to meet piecemeal, so that over an interval of a few years everybody who is anybody has worked with everybody else in the same category.' (Price, 1963, p. 85)

Crane elaborated Price's notion of invisible college, and related it more directly to the social context of scientific work. She linked it closer to Kuhn's thesis arguing that Kuhn's developmental model is closer to empirical evidence than conventionalist accounts [8]. Kuhn, in return, used the notion of invisible college when he revised his paradigm thesis at the end of the 1960s.

Crane's empirical investigation supported Price's idea of the exponential growth of science and showed that those disciplines with little or no interpersonal relationships between researchers were characterised by linear rather than exponential growth
curves. Crane argued that

'the logistic growth of scientific knowledge is the result of the exploitation of intellectual innovations by a particular type of social community' (Crane, 1972, p. 2).

Such a community is what she calls an invisible college.

The essential feature of the invisible college is, according to Crane (1972, p. 67), that contact between researchers, within the framework of such networks, contributes to the cumulative growth of knowledge. Such interpersonal contact is the basis for the interaction of research sub-groups. The invisible college is, at one level, the network of such interrelated research sub-groups, but at another level invisible colleges connect a research area to other research areas, through the interaction network of influential (and usually highly productive) research area leaders. As Crane noted

'Analysis of the social organisation of research areas has shown that a few scientists in each area played very important roles in recruiting and influencing other members. This may suggest that consensus concerning a paradigm for an area may emerge from a small group of scientists who then transmit it to many others. (Crane, 1972, p. 67).

The role of leadership in these networks or invisible colleges is important for Crane. These leaders are intermediaries who are fundamental for the cross-fertilization of ideas from one research area to another. Such leaders are, ideally, flexible, communicative and concerned with a wider perspective, and quite different from the dogmatic leader of a 'school'.

Indeed Crane saw invisible colleges as distinct from schools, the latter being of less importance in the growth of scientific knowledge than the former. Schools, she implied, are a product of
a less well defined paradigm (or possibly of no paradigm in the Kuhnian sense at all). She suggested that areas where 'paradigms are not so evident' (social science, humanities and technology) are characterised by lengthy theoretical conflicts. On the basis of research, (notably by Krantz, 1971a, 1971b) Crane argued that Skinnerian psychologists, for example, 'suggest a group that is closed to external influences and in this sense has some of the characteristics of a 'school''.

A school is characterised by the uncritical acceptance on the part of disciples of a leader's idea system (Krantz, 1971a). It rejects external influence and validation of its work. By creating a journal of its own, such a group can 'by-pass the criticism of referees from other areas' (Crane, 1972, p. 87). Crane saw schools as fragmenting scientific knowledge thereby inhibiting its growth. She likened schools to religious sects, constructing a new faith and intolerant of critique and deviationism. She noted,

'Schools have similarities to religious sects the latter break away from the church and build separate organizations, emphasising aspects of doctrine or policy that they believe have been ignored or misrepresented by the church. The religious sect is a relatively closed system that resists external influences rather than attempting to adopt them. Members who deviate from orthodox views on any issue are quickly expelled (see for example Coser 1954, Johnson 1964, Yinger 1957).' (Crane, 1972, footnote to p. 87). [9]

The school, unlike the invisible college, is thus seen as a small unit of detached researchers without the benefit of cross-fertilization of ideas to promote innovation within the research programme. Determined to safeguard its theoretical stance a school will actively reject alternative conceptualisations from
within its own discipline. Crane implied that such a state of affairs will, at worst, lead nowhere, or, at best, advance will be slow and inefficient as research confined to schools will lose momentum. She maintained that it is essential for interaction between groups if research is to lead to cumulative growth.

'Social and cognitive influences flow across research areas at all stages of their growth... this openness to external influence plays an essential role in the process of innovation in scientific communities.' (Crane, 1972, p. 99)

In short, Crane maintained that her own empirical work, plus available evidence from other studies, showed that research areas are not closed communities unreceptive to external ideas. On the contrary invisible colleges, unlike schools, exist to promote cross-fertilization of ideas, and they can not be easily boundaried. Indeed, invisible colleges are able to embrace, and possibly encourage, interdisciplinary study and peripheral or hybrid work on the boundaries of disciplines or research areas. Citing Back (1962), Crane suggested that when a research area abandons non-directive searching for new ideas its level of innovation declines.

This is not, however, meant to imply that invisible colleges are simply loose associations of similarly motivated workers. Invisible colleges are more than an ad hoc grouping, they have an autonomy grounded in the prevailing paradigm which constrains innovation, while not denying inputs of ideas from parallel realms. Invisible colleges, according to Crane, set norms of research orientation, of social interaction, of citation practice and of information utilisation. Invisible colleges are at the core of the social structure of science, they act to constrain
scientific work within a manageable framework whilst providing a forum for innovation and critique.

Crane summed up the role of the community in the development of scientific knowledge as follows:

'Research areas seem to have tendencies toward both a high degree of specialization and toward receptivity to external ideas.... The existence of a 'core' of journals in the literature and of scientists in the research area provides a kind of repetition in scientific communication insuring that certain ideas will be repeated and emphasized sufficiently so that the scientists who are interested in these problems will be sure of receiving at least some of the currently important messages and therefore continue to do research on these problems. The exchange of ideas between members of different research areas is important in generating new lines of inquiry and in producing some integration of the findings from diverse areas. Some degree of closure is necessary in order to permit scientific knowledge to become cumulative and grow, while their ability to assimilate knowledge from other research areas prevents the activities of scientific communities from becoming completely subjective and dogmatic.' (Crane 1972, p. 114)

1.6 Networks as the Unit of Metascientific Analysis

As suggested above, two relatively recent attempts have been made to re-present the history and sociology of sociology in terms of communicating units. Mullins (1973) adopted a broad approach which adapted the invisible college thesis. Tiryakian (1979a, 1979b) placed the emphasis squarely on the more restricted notion of school, this is considered below.

In an attempt to develop the sociology of sociology, Mullins offered an historical assessment of the development of some contemporary tendencies in sociology. He did this by focussing his attention on theory groups. Mullins (1973) suggested that the history of sociology has been written in one of four ways, as
history of core concepts (Nisbet, 1966), as an intellectual history (Bendix, 1966; Mitzman, 1970), as biography (Demerath & Peterson, 1967; Martindale, 1960) and as schools of sociological thought (Faris, 1967). The first two reflect an emphasis on ideas, the last two on persons. The biography and core concepts approaches being directed at an individual level and the intellectual history and schools approaches at a system level. Mullins was critical of all. Core concepts approaches fail, he maintained, because they emphasise current ideas or concepts rather than historically specific meanings and attempts to recreate the cumulative development of theories. The biographical approach, like the 'great man' theory of political history, entirely ignores the social mileu, presenting history as an individual endeavour. The intellectual history, for Mullins, was nothing more than a collection of biographies which fail to adequately establish 'traditions' nor assess their origins or developmental processes. This is what Mullins attempted to do by linking intellectual history with the fourth and potentially most potent approach from his point of view, that of schools.

However, as it stands, the writing of the history of sociology in terms of schools is inadequate because

'It is largely the product of a socially and psychologically informed philosophy of science which emphasises the importance of early training and the acquisition of paradigms.' (Mullins, 1973, p. 11)

Mullins was not criticising this approach for its use of a paradigmatic model nor for its concentration on early training, rather he was concerned that the 'paradigm' is taken-for-granted and not examined and that the processes by which theories are
generated, indeed the very content of theories, are ignored in favour of interpersonal relations.

'The school approach is widely used by genealogists of science who wish to discover a disciplines roots. Its strength is on the social context of theory. Its weakness is that the actual product of science, the theories themselves, are not discussed. Moreover, this approach usually fails to explain how the same training (e.g. by Talcott Parsons at Harvard during the early 1950s) can produce such different persons as Harold Garfinkel (an ethnomethodologist) and Robert Bellah (a structural functionalist).’ (Mullins, 1973, p. 11).

Mullins contended that the four approaches to writing the history of the social sciences are nothing more than classification systems inadequate to explain the rise and fall of different theoretical systems. He suggested that the network approach he proposed overcame the problems of conventionalism.

Mullins (1973) elaborated an approach to analysing the development of scientific communities which he hoped would lead to an understanding of the way in which different kinds of theory come to be written. Taking up Price’s cue, Mullins, like Crane, developed a thesis which located an amended view of invisible college within a Kuhnian paradigmatic framework. Mullins’ model, which he saw as generalisable to all science, began with his analysis of the phage group of microbiologists (Mullins, 1968) in which he attempted to demonstrate that they were an integral part of the development of molecular biology and that the speciality itself developed within a paradigmatic framework emerging from social interaction among scientists. On the basis of this enquiry he posited a network approach involving a multi-stage model. In his later work (1973) he attempted to apply this model to American sociology, asking why social theory came to be
so variable and how some social theories became established and why all eventually die out.

Mullins' approach involved the construction of a four stage model, each stage being marked by empirically demonstrable social and intellectual characteristics. The model involved a progression from a 'paradigm group' (later relabelled the 'normal stage') through a network stage, then a cluster stage to a speciality (or discipline).

Central to the stages of development was the communication structure, for, in accord with Medvedev, Mullins maintained that critical appraisal of any work is primarily through 'verbal, direct and immediate discussion in a circle of understanding colleagues' (Medvedev, 1971, pp. 133-134) and that such appraisal is at the root of the development of knowledge.

The stages outlined by Mullins were each characterised by various types of communication structures and the pattern of such types is indicative of the stages of the social interaction. These component communication structures are, first, communication (i.e. serious discussion about current research, unrestricted by institutional, status or disciplinary boundaries), second, co-authorship (i.e. two scientists jointly reporting research results), third, apprenticeship (students trained and sponsored by a teacher), fourth, colleagueship (two scientists working together in the same laboratory).

Stage one, paradigm development, occurs, for Mullins (interpreting Kuhn, 1962b, 1969), when a group of persons
experience a 'gestalt shift'. The subsequent research of the persons involved utilises this new perspective, and such a shift is justifiable only if success in problem solving can be established and new problems pointed out. Further, success, Mullins argued, may eventually enable the new approach to 'establish itself as normal science in Kuhn's sense'. Then the puzzle solving activity, characteristic of normal science, may begin.

The emergence of a paradigm (albeit somewhat loosely defined) is located within the paradigm group. This group is the minimal form of scientific group.

'Itits members have no necessary social connections. Kuhn indicates that any useful paradigm must, by definition, be the possession of some social group which is using it.... The minimal requirement of such an entity is two or more established scientists who have shifted from one viewpoint to another (Gestalt switch), and who might or might not be in communication with one another. A paradigm group is thus a set of individuals, all of whom have moved into a similar cognitive situation with respect to the same, or similar, problems.' (Mullins, 1973, pp. 54-55)

This normal stage is, in Mullin's view, somewhat directionless, almost anomic.

'The normal stage is characterized by a low degree of organization both within the literature and within social relationships... no groups of students are being formed. The commitments of persons involved in normal areas are generally short in duration and constitute only one of several commitments, each to different areas. Hence little co-ordinated effort is made to solve any particular problem.' (Mullins, 1973, p. 21)

Despite Mullin's contention, his normal stage does not appear to be the same as Kuhn's 'normal' process of science because it is not a puzzle solving stage and appears to preface the emergence of a paradigm in which puzzle solving could take place. In fact, Mullins sees the normal stage as an almost dormant period just
waiting to be shaken up by the emergence of a coherent group. For Kuhn, the normal stage is the active process of puzzle-solving which reaches crisis proportions as anomalies build up. Normal science is not an amorphic stage waiting for the onslaught of a coherent group. Mullins' normal stage is thus closer to Kuhn's pre-paradigmatic stage.

The second stage, the emergence of a network incorporates a limited idea of invisible colleges [10]. At this stage Mullins sees a tendency for small group co-operative research sustained by the publication of successful findings. The communication network consists of pairs and triads of scientists engaged in regular communication over a period of time. These patterns, however, lack constancy and changes have an imperceptible effect on the science (i.e. the broad area within which the network is operating). Changes in the network and its functioning are dependent upon personal contacts made by scientists (there being little conscious idea that a scientist is part of a specific network and less that it may be a nascent speciality).

Mullins argued that the network stage emerges from the normal stage when a group of 'likeminded' researchers gather round a particular intellectual product and then begin to evolve 'overlapping' invisible colleges, work together and recruit one another. Without some research breakthrough at this stage no further development is likely. Alluding to the invisible college model, Mullins suggested that

'In a group of scientists writing on the same very specific problem area, some of them might have all their contacts within the group, others might have their contacts within and without the group, while others who are clearly working
on the same problem as these scientists already mentioned, might not be connected with any of the other groups. These contacts might include any of the activities from communication through co-authorship and colleagueship to apprenticeship. We should note that the communication network structure shows two changes from the paradigm group structure (1) increased connection among scientists who are working in the area, and (2) a corresponding decrease in disconnected or independent persons. (Mullins, 1973, pp. 58-59).

The initial network (invisible college) is augmented by the adoption of students and a general 'thickening' of the research group. The later stages of a network come to resemble a 'school' when successful 'network' groups make an explicit agreement regarding the style and content of work to be done, which appears in a programme statement.

The third stage, the development of a cluster is a more self conscious stage than the previous ones. A cluster is formed when scientists become aware of and concerned about the communication patterns and begin to delineate 'problem groups'. Such groups are scientists who are working on common problems and have developed from the earlier pairs and triads through recombinations. This development, Mullins suggested, is encumbent upon the existence of favourable conditions such as, leadership, supporting institutions, substantial research problems and good luck. These clusters are often identified by name by the members and by scientists outside their boundaries. This is indicative of the more stable nature of the clusters than of the pairs and triads that constituted them. Clusters start to generate a distinct culture and draw support and recruit students.

'Communication becomes even more rigorous. Clusters of students and colleagues form around the key figures in a group in one or a few institutions. Students are important because only a few scientists in a field ever have any
graduate students, and those who do usually perform most of the research and publish frequently as well. (See Price, 1963)."

Mullins suggested that a cluster normally includes three or more Ph.Ds who reinforce each other's interests along with some graduate students and that this stage constitutes the first real institutionalisation of the research area. It is more stable and less informal than the network stage, and 'outside' contacts tend to be narrower, limited to people with similar research interests, especially amongst students. Coauthorship becomes important and a large amount of research is carried out and the group gradually reveals its detachment from the prevailing pseudo-paradigm. Even at this cluster stage, though, personnel changes rapidly and relations are seldom long lived.

The final stage, the emergence of a speciality is in effect the institutionalisation of the cluster, its work and ideas. Prior to becoming a speciality, the cluster is still vulnerable for it has not established formal structures and procedures and relies on the informal connections of coauthorship and communication. These are dependent on individual contacts, rather than institutional organisations; even though they are self-conscious. The speciality emerges when the cluster develops, through an institutional base (or bases), a 'regular process for training and recruitment into roles which are institutionally defined as belonging to that speciality'.

The transition from cluster to speciality stage is an exponential progression. Students become successful in their own right, the new orientation, as it blossoms, cannot be contained as a cluster...
at one institution. The successful cluster becomes dispersed.

'We might hypothesize that the foremost factor in determining whether a coherent group develops at some location in the general science structure is: Are the scientists involved proceeding only empirically (simply moving from one research problem to another, without benefit of broad, theoretical guidance) or, alternatively are they carrying out a new theoretical orientation and being guided by it?'. (Mullins, 1973, p. 27).

In short, then, success depends upon whether the coherent group has a research programme. Most groups are not successful, according to Mullins, they do not have any effect on the course of science.

While a programme seems to be essential in Mullins analysis it is not apparently enough. There are necessarily other external factors which must exist in order to provide a context for the working through of the programme. These factors include a centre for the training of students and the carrying out of a research programme which necessitates the creation of an environment for close work equipped with 'intellectual materials'. A social organisational leader is also important, someone to orchestrate the advance of the programme. The availability of these factors, which includes the access to cash, is external to the research programme and thus to the intellectual development of science. Mullins did not bother to attempt to analyse how such resources become distributed to practising scientists.

The speciality leads to impersonalisation, the penalty for success is lack of community spirit. Members of a speciality will be aware of (at least some) of the work done by other members.

'They may share a paradigm and a set of judgements about what general work should be done in the field, although the details of those ideas might differ .... The speciality's
problems might be described by Kuhn's concept of puzzle-solving which is the normal activity of science. Kuhn describes puzzle-solving as having the following characteristics: an assumed solution, rules which limit the acceptable solutions, and rules which limit the means for arriving at those solutions'. (Mullins, 1973, pp. 74-75).

Mullins approach emphasised the scientific community and saw success for a group in terms of the ability to generate an approach to a discipline, or more likely a sub-discipline on the margins of existing disciplines, which spreads beyond the confines of the promoting group. The initiation is specific, and the 'school' as a historically specific object is, effectively, encompassed in the middle phases of the network model, while the final (and relatively rare) phase is indicative of the generalisation of the school in a theoretical orientation at some level.

1.7 The School As The Unit Of Metascientific Analysis

Tiryakian (1979a, 1979b) has inaugurated a revival of the specific concern with the 'school' as the focus for metascientific enquiry. He initially adopted a Kuhnian frame of reference, but subsequently amended it to incorporate elements of Lakatos' (1970) methodology of scientific research programmes.

Tiryakian argued that the schools approach to the development of sociology provided a framework consistent with historical evidence and one reflecting the uneven growth of sociology. He argued that the growth of sociology is not characterised by cumulative growth of theory based on empirical evidence; on the contrary it is uneven and discontinuous, and to a large measure, is a series of episodes, of periodic infusions of theory, brought
about by the work of a small number of 'major schools'. Tiryakian, like Crane (1972), saw his view as related to the Kuhnian perspective of the growth of science while those who posit cumulative theoretical growth reflect pre-Kuhnian theories of scientific development. Tiryakian's contention was that his 'unit' approach was neither a 'great man' approach nor a 'great ideas' approach, but one which looks at the institutions in which both operate. It thus lies between the two and is a 'middle range theory'. [11]

On the basis of investigation of two schools Tiryakian has built up an ideal typification of the concept of school as scientific community (Tiryakian 1979a). Schools vary in size and tend to grow from a core of less than a dozen to around three dozen, with admission to membership being ad hoc and largely dependent on the decision of the founder-leader. It is similar in its formative stage to a religious sect, [12] providing its members with a sense of mission, ostensibly in the form of taking on the 'conservatism' of the prevailing views of the profession. In turn, the new school is excluded from the mainstream and develops its own organs of diffusion. The leader tends to be charismatic and imports a basic concept of reality, or at least how to approach reality, to which the followers are committed and which they validate through empirical study. (The leader must, incidentally, have a commitment to teaching students). The basic conception becomes the core of the 'revolutionary paradigm'.

The school gradually becomes more institutionally visible and its core ideas are disseminated in more formal ways (through
As it does so it grows, its ideas become popularised and it is less dependent on face to face interaction (especially leader to pupil) as means of disseminating ideas (or of teaching recruits). The charisma of the school becomes institutionalised or routinised and its ideas become part of the standard conceptions of the discipline. The 'paradigm' becomes depersonalised and its conceptions become utilised by new generations of sociologists unaware of their specific socio-intellectual origins. As the community becomes the orthodox party (and not all do) the school faces paradoxes of institutionalisation. Effectively, the core conceptions run the risk of being watered down. Specifically, Tiryakian suggested,

'an element of the paradigm may get lost, namely that which is a covert dimension, its presuppositions.' (Tiryakian 1979a, p. 218).

A populariser of the school is frequently necessary and is important to the survival and influence of the school. The roles in the school are as follows: leader, converts, students (lieutenants who are the agents of institutionalisation), auxiliaries (journal editors etc), patron. [13]

In view of Crane's analysis and Mullins' reservations about the conventionalism of a 'schools' approach, Tiryakian appears to be taking a regressive step in his determination to establish schools as the unit of metascientific analysis. Crane has suggested that the school is indicative of a lack of cogent development of scientific knowledge. What then are Tiryakian's reasons for adopting the school as unit of analysis for sociology, and what does the adoption of that unit of analysis
add to an understanding of the growth of scientific knowledge?

Tiryakian's approach is explicitly Kuhnian, based upon Kuhn's remarks in his postscript to the second edition of 'Structure of Scientific Revolutions' (Kuhn, 1970). However, he sees Kuhn's idea of scientific community as an assemblage of 'practitioners of a scientific speciality' as too vague and too broad for sociology. The notion of school provides a less generalised community. Thus school is idealised by Tiryakian as a group of intellectuals comprising a small community whose origins and formative period can be localised in time and place. For Tiryakian, schools in science are similar to schools of art, constituted by an interactive group clustered around a founder-leader, as he imagined that Surrealism was around Breton.

'Without further specification it would appear as if the notion of 'community' entertained by Kuhn is that of a group of scientists having as a common denominator a professional specialization in the division of labor of their discipline, who show a common intellectual or cognitive orientation to the subject matter of their field (including a common language or jargon), and who may or may not be aware of the presence and attitudes of members of that community via various communication networks (specialized journals or newsletters, telephone and computer conversations, etc.). This is essentially an assemblage of widely scattered individuals who come together only occasionally, such as at national or international meetings, externally funded conferences and the like.' (Tiryakian, 1979a, p. 212)

Tiryakian suggested that such a typification of a scientific community is an impersonal one and that for the examination of the growth of scientific knowledge a smaller grouping, the school, is more appropriate.

Another reason for Tiryakian's insistence on the primacy of schools as the unit of analysis is his contention that the development of science is effected by methodological innovation.

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Such innovation, he argued, is community located. Schools do not contribute to scientific development by generating new theoretical models nor by new empirical observation but instead contribute through methodological innovation which Tiryakian saw as 'fuelling the development of the discipline above all else' [14].

The scientific community, redefined as a school by Tiryakian, is, then, characterised by a close adherence to the ideas of a leading figure, from which the school's paradigm emerges. The leader provides a core of assumptions which underpins the work of the school. These core assumptions, central to the 'paradigm', are embodied in what Tiryakian called the 'presuppositions' of the school. This is of central importance. One of the intellectual activities of those engaged in a school analysis of the development of sociology seems to be to determine what these 'presuppositions' are and to demonstrate their persistence throughout the school's history. (Faught, 1980).

1.8 Conclusion

The concept of school is very widely used in attempts to understand the history and contemporary structure of sociology. For the most part it is used informally, without definition, to refer to groups of sociologists sharing a certain perspective and perhaps a particular institutional location. However, there have been some attempts to develop the concept and make it more rigorous. Building on the work of Kuhn, Lakatos, Price and Crane, Mullins and Tiryakian have each presented accounts of sociology
in terms of units or schools. Moreover, both have used the 'Chicago School' as a key example in their work. In this thesis an in depth investigation of the 'Chicago School' will be presented both to provide a sounder basis for understanding the development of sociological practice in Chicago in the first half of the twentieth century and to provide a basis for the appraisal of the value of the concept of school in metascience.
NOTES TO CHAPTER ONE

1. The term conventionalist when applied to a sociologist of knowledge, historian of science etc., is taken here to imply a view which essentially relies upon constructing schema of theoretical/substantive divisions within a discipline and allocating people to them. The schema are grounded in conventional wisdom. Epistemologically, this conventionalist approach is grounded in a view of the development of scientific knowledge which accepts the fallibility of the empirical base of knowledge and thus of the impossibility of establishing proven knowledge. The impossibility of conclusive falsification results in some hypotheses being granted the status of conventions 'statements which are neither true or false but which operate as methodological rules so that other hypotheses can be tested empirically' (Halfpenny, 1982, p. 103).

2. The term school has multiple meanings. These may be broken down into three conceptual groups. First is the common sense meaning of school as an institution of learning, usually for children. This meaning is of no concern for metascience. Second, those meanings which are simply convenient nominalist shorthand for a group of academics acting in some co-operative, coincidental or other common manner, usually incorporating the same basic theoretical or conceptual presuppositions. Third, those specific applications of the above to the theory of research. These constitute analytic definitions.

Of the second group of definitions, the following may be identified as administrative (as distinct from purely academic) divisions within an institution. The school as a sub-group of a department, or a complete department within a faculty. Or the school as a grouping of departments (either a whole faculty, or a cross-departmental part of a faculty, or a cross faculty grouping). There is no uniform definition of school at this administrative level.

Academically, the school, although not an 'official' administrative unit may be an even more complex grouping within an institution. Thus a 'school' may be
a. a small informal sub-group within a department
b. a formalised subgroup of a department, identified as such for academic purposes
c. a complete department
d. an informal grouping of sub-groups across departments
e. a formal interdisciplinary grouping of only some members of the participating departments
f. a formal interdepartmental grouping (possibly independent of any faculty structure)  
g. an interfaculty grouping or entire college
h. an autonomous (research) organisation attached to a college
i. some form of grouping of like minded academics from various institutions, which may or may not have an institutional focal point.
This latter definition, although vague, may be sharpened up by suggesting the possible forms it may take. Such a grouping is of two key types, a contemporaneous network or a crosstemporal/spatial ensemble based on convergence of ideas. The contemporaneous network is delineated by space and time in various ways.

i. a co-operating group of interacting researchers
ii. a group of communicating researchers
iii. a group of contemporaries adopting similar ideas (basic presuppositions, core theories, subject areas or a combination of these) who attend conferences, and/or read publications of the group, thus keeping abreast of developments.
iv. a 'frail' network, usually based on a tentative link such as nationality, preferred methodology or theoretical orientation.

The congruent ideas school is a much looser grouping which is not constrained by time or space and is usually designated by adherence to a development of particular theoretic positions. (See, for example, the use of the term 'School' by Sorokin, 1928).

The distinction between academic and administrative definitions is to some extent arbitrary as most groupings, whatever their primary function will be involved in and affected by decisions relating to their non-specific realm. Indeed, some organisational frameworks deliberately attempt to incorporate both elements and these may be labelled as 'schools' as for example in the case of a 'School of Education' attached to a British university.

3. The analysis in this thesis involves both historical reconstruction and the analysis of the process of historical reconstruction. It enquires into historical events and the writing of such history. In order to keep a clear distinction between different meanings of the terms history and historiography (which fortunately have overlapping meanings in general usage), the following uses will be adhered to.

History, is what historians do, i.e. the process of writing history through an interpretation of past events. Historiography will be restricted to the second of its dictionary definitions (Oxford English Dictionary, 1976) namely the study of history-writing.

In this thesis a distinction is made between three perspectives on history. Historicism involves a view of history as 'fact', ignoring the role of the interpretive subject/historian. Historicism accepts the interpretive role of the subject/historian but aims at a reconstruction of the history, in the sense that, given a cultural product such as a text, the historicist attempts to reconstruct the meanings of the author. This reconstruction is usually pitched at, what is hoped to be, an objective level. Finally, the historicalist approach, while accepting the intrinsic interpretative nature of history, maintains that it is impossible to achieve transhistorical objectivity. Historical understanding is seen as rooted in the mediation of past and present traditions.
4. The term 'sophisticated' has emerged in the philosophy of science debate to refer to approaches which take a particular philosophy of science and, in the light of critique, develop it to accommodate historical evidence. This is particularly the case in relation to falsificationism, where Lakatos adopted Popper's model, and without destroying the central core of Popper's evolving notion of falsificationism, radically transformed it as a guiding principle for scientific research.

Lakatos (1970) argued that Kuhn fails to distinguish the two strands of falsificationism. Kuhn is correct, he argued, in attacking naive falsificationism but does not appreciate the subtleties of the sophisticated version, and hence its potential. However, it is doubtful whether Popper appreciated Lakatos' developments of sophisticated falsificationism or altogether agreed with them. Besides, even the most sophisticated version of falsificationism is unable, ultimately, to confront the fallibilism of observation.


6. Lakatos' Methodology of Scientific Research programmes is generally regarded as probably the most 'sophisticated' internalist account of scientific change. It balances a critique of falsificationism with a retention of the rationalist spirit of that model. Lakatos dispenses with simple (or naive) falsificationism, in which their is an assumption that a demonstration of a falsified scientific proposition is all that is needed to eliminate it as far as the rational scientist is concerned.

7. Newton's work provides an example of the positive heuristic of research programmes. Lakatos claims that the subsequent developments in Newton's programme were all forseeable at the time Newton developed his first naive model.

8. Crane does not use these terms. Rather, she compares Kuhnian paradigm approaches with what she calls a 'random development model' and with a simple inductivist cumulative model.


10. The Price-Crane invisible college is in effect a collapse of these four stages into two. The invisible college thus conceals the stages of the development of new specialities (including those which do not survive) and the differences within a wider area of study. Nonetheless, invisible college analysis reflects those elements characteristic of stage three of Mullin's model.

11. The desirability of 'middle range theory' is not self-evident, it is a personal preference, with no established metascientific credibility. Further, theories of the middle range are of a specific type and serve a specific metatheoretical
function. As Merton (1945) has suggested, they are testable theoretical structures, related to a specific area of (social) scientific concern. They are distinct from narrow theoretical formulations which have no locus in general sociological theory, nor are they, at the other extreme, general theoretical formulations. Tiryakian’s view of theories of the middle range does not, then, coincide with Merton's.

12. Note the identity of analogy with Crane (1972). Tiryakian does not acknowledge this earlier work.

13. This set of roles was anticipated by a more complex analysis by Radnitzky (1969, 1973). Again Tiryakian makes no reference to this earlier work.

14. It is not clear from Tiryakian whether such methodological innovation can only be enacted within the arena of the school and whether all developments in science are at root methodological.
CHAPTER TWO

THE 'CHICAGO SCHOOL'
2.1 Introduction

The suggestion that there was a 'Chicago School' of sociology implies that the administrative unit, the Department of Sociology at the University of Chicago, constituted a unified 'school of thought' in some form or other. Either it involved a particular philosophical perspective, a preferred approach to sociological work, the development of a particular sociological perspective both theoretically and substantively or a combination of all or some of these. To be labelled a 'school' also implies that whatever serves to identify the practice of the members of the school is somehow distinct from practice elsewhere. An analysis of the sociological work done by the Chicagoans will involve an assessment of the sense in which they may be construed as a 'school'.

2.2 Chicago as a Distinct School

There are innumerable statements in the sociological literature that suggest that the 'Chicago School' was an integrated unit, (Faris, 1967; Carey, 1975; Ritzer, 1978; Tiryakian, 1979a; Fish, 1977; Foucault, 1981) and in some way constituted a school.

While denying that the Chicagoans used the term, Cavan suggested that

'In a sense there was a School of Sociology at Chicago, given form by the Park and Burgess Textbook and by the Polish Peasant. Add to this the demand for empirical work focussed on the 'untouched' city of Chicago.' (Cavan, 1983, p. 411).

Thomas (1983a, p. 390) claimed that

'The Chicago School may be considered a "school" rather than
a solidarity group committed to a particular point of view, in that it represented a vertically bonded network of practitioners located in and identified with a specific institution, all of whom shared near identical beliefs and ideas.'

There are, however, problems with this 'definition'. The identification of the beliefs held in common is problematic. Such an approach assumes a community of ideas or beliefs within the department which were distinct from those held generally within the discipline.

Conversely, an integrated sub-group of practitioners from within the department boundaried by a particular temporal frame could, superficially, be seen to constitute a school, but this is to some extent circular, in that those who fulfil the criteria become the school and the rest are non-school members. The result is the specification of a variety of 'Chicago Schools' in order to fit certain preconceptions. The 'Chicago School', therefore, takes on a variety of forms.

2.3 The Designations of the Chicago School

While some commentators distinguish between a Chicago 'style', adopted outside Chicago as well as inside, and a 'group' of Chicago based sociologists, (Thomas, 1983a, p. 387) and others see the 'school' as institutionally located (Faris, 1967), still other commentators use the term 'Chicago School' all embraceingly. The term in this sense reflects a 'network' with its centre at Chicago (Mullins, 1973).

The narrowest definitions are provided by Blumer (1972) [1] and Anderson (1983). Blumer suggested that Park, Burgess and Faris
made up the 'Chicago School' while Anderson included Small along with this triumvirate. For Anderson, this simple definition involved nothing outside the institutional framework. There is no particular 'Chicago core' evident in the work of the four faculty he designates either theoretically, empirically or epistemologically. Blumer, in limiting the 'Chicago School' to three people, merely suggested that Ogburn had a very different intellectual stance to Park. However, he accepted that Faris and Park also had differences. The exclusion criteria, for Blumer, seems to have been the nature of sociological generalisations. In this Ogburn, as a statistician, was more concerned with 'so-called objectivity' than 'dealing with the whole process whereby action came into being' (Blumer, 1972, p. 13). If the Chicagoans are to be seen as a group of co-operating sociologists which may be designated a 'school', then the group must exclude Ogburn if it includes Park.

When I went to the University of Chicago in 1927, September, Professor and Mrs. Park gave a large party in the first part of November to which neither I nor my wife was invited. I was sensitive on this point. Next I was told that repeatedly by various persons that Park spent a good deal of time in his classes belittling statistics and pointing out their limitations. I was invited to the University in part to teach statistics since none had ever been taught in sociology and none was then taught in any other social science. Perhaps I displayed too much missionary zeal for Park, who questioned whether there was any need of teaching statistics, so I was told. Next, one day he came in my office with a hand full of books and asked me to review them for the American Journal of Sociology, and then proceeded to tell me how to do it and what was expected of me. I took the books but never reviewed them. Though Park was twenty years older than I, I had been a full professor at Columbia for ten years, and was quite intellectually mature.... I never forgave Park, which is a trait very marked in me, not to forgive or forget a slight. I wish I were different and had not been so sensitive in regard to Park.... So I never saw Park except at meetings or greeted him as he passed. Oh yes, he did invite me once with all the department to his house.
and I went. I think Park was a great teacher for the few. (Ogburn journal, 4th & 5th April, 1955)

However, to simply construct a 'Chicago School' that excludes Ogburn is to ignore the influence that Ogburn had on the sociological work of the department, not least upon Burgess, with whom he worked quite closely. Furthermore, a Park-led 'Chicago School' would also have to exclude Faris, another enormously influential figure (Cavan, 1972; Blumer, 1972; Faris, 1967), but certainly not a follower of Park's. In short, if there was a 'Chicago School' of sociology it cannot simply be construed in terms of a sub group of the Chicagoans revolving around Park.

Becker (1979a) has suggested that while there might not have been a community of sociologists with common interests, there was, however, some things that the majority of them had in common and that one of these was contact with Park.

'They all essentially looked at things the way the old man had looked at it, and he had a very comprehensive view. I mean, his notion of research included making ecological studies, making maps of spatial distribution of social phenomena, as well as the most detailed kind of ethnographic research. That was all part of it. And most of the people who studied with him, down to the later descendants, you know, in the second and third generation, like me, took that view.' (Becker, 1979a, p. 5)

Furthermore, these sociologists also 'did an enormous amount of research in Chicago' and the research was cumulative. However, this was not simply the sociologists but spread far beyond with political science, for example, 'tightly linked into the same kind of thing'.

A number of commentators have, then, gone further than a limited designation of personnel to imply an approach to sociology deriving from the work done in the Department of Sociology at
Chicago. This has been developed in a variety of ways with the focus of attention being on a 'generational' view, on the work done in the 'Golden Era', on the development of Chicago symbolic interactionism, on the later work of the so-called 'New School' or on a retrospective selective reconstruction of an urban studies heritage evident in the 'Chicago revival'. These will be briefly assessed below.

2.3.1 The 'Four Generation' Approach

The Chicagoans are often referred to in terms of generations, (Tiryakian, 1979a; Becker, 1979a). Three and sometimes four generations are alluded to. The first generation consists of the tenured staff and their students up to 1914, principally Small, Henderson, Thomas and Vincent. These are seen as the founders of a 'Chicago Approach' in the sense of promoting empirical enquiry and concentrating attention on the city of Chicago (Diner, 1980; Dibble, 1972). The second generation usually refers to the 'Golden Era' particularly to Park and Burgess, to Ellsworth Faris and, in some accounts, to William Ogburn. These four staff members and their students are seen as developing the embryonic concerns of the first generation (Faris, 1967; Carey 1975). Out of these came the third generation, principally graduates who, often after a short absence, returned to Chicago and became tenured. Notable here are Blumer, Wirth, Hughes and Stouffer. The fourth generation again tended to be students of the third generation, but often developed much of their sociology away from Chicago, thus following up the precedents begun in the third generation. Becker, Strauss, Goffman and Janowitz are among the
most clearly identified members of the fourth generation Chicagoans.

In effect this approach tends to divide up the personnel into a relatively simple temporal sequence, identifying the dominant characters at each phase of the Department's history. The implication being that some kind of core approach was retained and gradually developed over time by each successive generation. The result is that the 'Chicago School' is discussed in terms of phases such as the 'early period' (Hunter, 1983) or the 'late Chicago School' or 'late symbolic interactionism'. The concentration on different phases of the department's history has led to different people and ideas being referred to by the single term the 'Chicago School'.

2.3.2 The Golden Era

Many discussions of the 'Chicago School' really only see the school as operative in terms of the work carried out in the 1920s and early 1930s, particularly that guided by Park and Burgess. Thus the school is seen essentially in terms of the second generation, and this is referred to as the 'Golden Era'.

Faris (1967), Matthews (1977), and Raushenbush (1979) all discuss the Chicago School in effect in terms of the 'Golden Era', the latter two placing Park very much at the centre of the 'School'. Philpott (1978), and Cavan (1983) similarly construed the school in terms of this limited period. Philpott identified Park, Burgess and Wirth as the 'pillars of the Chicago school of sociology', while Cavan suggested that Shaw was indicative of
the 'School' because he, more than anyone, developed the 'life history' form of the 'highly regarded' case study at Chicago. Both Madge (1963), in his discussion of the origins of scientific sociology, and Hunter (1983) also saw the 'Chicago School' in terms of this 'Golden Era' and both took Zorbaugh's work (1929) as indicative of the Chicago approach. At this stage, Hunter suggested, interactionism was not central rather the urban community studies, on which the Chicagoans concentrated and which were concerned with 'external' concepts such as 'natural area' and 'natural forces', were more important.

2.3.3 The Chicago School of Symbolic Interactionism

Meltzer, Petras and Reynolds (1975) and Carabana (1978) argued that the 'Chicago School' was manifested in Blumerian symbolic interactionism, and that this was but one variety of symbolic interactionism. Littlejohn (1977) also referred to the 'Chicago School' as led by Herbert Blumer and at variance with the 'Iowa School' led by Kuhn.

Snodgrass (1983), too, linked the 'Chicago School' to symbolic interactionism which he dated back to the 1920s. In so doing he adopted an exclusive approach and, Shaw, for example is not included on the grounds that he is insufficiently interactionist.

2.3.4 The New Chicago School

Recently, there have been claims that a new 'Chicago School' emerged in the 1950s and flourished in the 1960s and 1970s. In the main, this new school was not based at Chicago.
Lapperriere (1982), for example, argued that the 'New Chicago School' of sociology arose in the United States in the 1950s. It attempted to break the hold of 'quantitative' sociologists on the discipline, which had coincided with the theoretical sterility of sociology. She argued that the 'New Chicago School' aimed to develop a systematic, open and empirical approach to theory construction. This allowed them to take into account the richness of social reality while adopting rigorous sociological method. The 'New Chicago School', she argued, was characterised by a more systematic and wider approach than that exhibited by other qualitative sociologists.

In effect, this designation of a 'New Chicago School' is reflected in the accounts of those commentators who talk of the 'late Chicago School' or of the labelling theory of the 'Chicago School'. This is primarily in terms of the work of Becker, Geer, Strauss, the later work of Hughes and his students and the emergence of the 'dramaturgical approach' found in Goffmann, Duncan and Burke (Littlejohn, 1977; Dotter, 1980). In short, this idea of a new school is an attempt to disengage the 'fourth generation' Chicagoans from their earlier heritage.

2.3.5 The Revival - Urban Life

A late 1960s revival of the Chicago School of Urban Sociology was heralded by Gerald Suttles (1968) 'The Social Order of the Slum'. In the preface Janowitz noted that

'By the end of the 1950's, it would have appeared to the intellectual historian that the Chicago school of urban sociology had exhausted itself. Even at the University of Chicago, the intensive and humanistically oriented study of
the social worlds of the metropolis had come to an end. The older figures had disappeared one by one, and a new generation of sociologists were interested in quantitative methodology and systematic theory. A few disciples of the traditional approach carried on in the shadows of the university or were scattered through the country. But intellectual traditions are transmitted and transformed as much by the intrinsic vitality of their content as by the institutions of academic life. A mere decade later the themes of a reconstructed urban sociology are once again at the center of social science thinking. The complexity of social behavior in the urban setting and the rise of concern with policy issues has meant that urban sociologists have come to focus on a particular social grouping or on a specific social institution, such as the family, the juvenile gang, the slum school, or newly emerging community organizations. Nevertheless, in the reconstruction of urban sociology, the community study remains a basic vehicle for holistic and comprehensive understanding of the metropolitan condition. (Janowitz, 1968, p. vii)

The revival of the 'Chicago School' heralded by Janowitz's comments, was put into more formal practice, albeit along different lines, by the establishment of the journal 'Urban Life', following the short lived endeavours of a group of 'Chicago Irregulars' [2], in 1969 with the aim of 'reviving an ethnographic tradition' and encouraging works of 'Chicago informed urban ethnography' (Thomas, 1983a, p. 391). Matza, (1969) and Lofland (1980) are major works of these neo-Chicagoans.

Arguably, by 'recreating' a 'Chicago urban ethnography' these irregulars are doing no more than providing a heritage and legitimation for their work. Thus possibly 'Urban Life' presents nothing more substantial, by way of an elucidation of the 'Chicago Approach', than a picture of Chicago which fits in with its own requirements as a vehicle for ethnographic urban researchers.
The revival is, however, indicative of a view of the 'Chicago School' as something other than a group of people in an historical setting, rather it is indicative of a spirit of enquiry focussing on the urban environment. Indeed, in his comments on Suttles' study, Janowitz noted that the research was a

'powerful expression of the contemporary effort to maintain a continuity in the tradition of the urban community study and to contribute to an urban sociology based on a more precise methodological base and a sounder theoretical frame of reference.... He proceeds with an empirical orientation reflective of the Chicago school when social anthropology had not yet separated from sociology.... Suttles joins a tradition that emphasizes the contributions of the sociologist to policy and professional practice.' (Janowitz, 1968, pp. vii-ix)

2.4. The Lack of a Definitive Referent

So, although the term 'Chicago School' has been used to refer to the work of the Chicagoans it has been taken to mean different things by different commentators. This confusion of referents is not alleviated by seeking out early references to the Chicago School to use as a benchmark. There are virtually none in the literature up to 1940, a date by which some commentators regard the 'Chicago School' to have been in decline for some time. The earliest use is supposedly in Bernard (1930, p.133), but this is merely indicative rather than in any way definitive. Notably, Bernard does not refer to a 'Chicago School' of sociology during his engagement with the Chicagoans at the time of the 'coup' in the American Sociological Society in 1935 (Lengermann, 1979).

An article by White (1936), promisingly entitled 'The Chicago School' turns out to be a reference to Chicago University itself and the article a review of Hutchins (1936) collected speeches.
Key text books such as Chapin (1930) 'Field Work and Social Research', Hiller (1933) 'Principles of Sociology' and Young (1949) 'Sociology' make no reference to the 'Chicago School'. Both Hiller and Young discuss the work of the Chicagoans. In Hiller's book Park, Burgess, Thomas, and Cooley get far more references than any one else (except Sumner) and Young has an extensive discussion of urban sociology which includes the works of the Chicagoans among others, but neither separate the 'Chicago School' from other American sociologists. Similarly, R. D. McKenzie's contribution to the President's Research Committee on Social Trends (1933) about developments in Metropolitan Communities makes no reference to a 'Chicago School' although it discusses in some detail the various analyses of the city of Chicago undertaken through the University of Chicago.

It seems, then, unlikely that there was any recognition of a 'Chicago School' before 1935 and that any references to it up to the 1960s were unsystematic, vague and devoid of the implications that have become associated with it over the last quarter of a century.

The assumption among historians and sociologists of sociology, however, is that there was a 'Chicago School' of sociology and that it had a considerable bearing on the development of American sociology during the first half of the twentieth century. As has been shown above, the exact nature of the 'school' and the impact it had are not clearly defined. At this point an elaboration of the sociologists in the Department at Chicago up to 1950 is appropriate.
2.5 A Brief Chronology of the Department of Sociology at the University of Chicago

When Albion Small was appointed head professor of the Department of Sociology and Anthropology at the University of Chicago in 1892 it was the first sociology department to be established in a university anywhere in the world. Along with Small in the Department in its first year were six other people. Charles Henderson was associate professor of social science, later to become head of the Department of Ecclesiastical Sociology (1906) subsequently renamed Practical Sociology (1913). Marion Talbot was assistant professor in sanitary science; in 1903 she moved to the newly created Department of Household Administration. E. W. Bemis who was an associate lecturer in political economy was a member of the extension staff in sociology. W.I. Thomas was a fellow in the Department. He received his doctorate in 1896, was slowly promoted to a professorship (1910) and stayed on in the Department until forced to resign in 1918. In addition were two anthropologists, Frederick Starr, assistant professor and curator of the museum who remained in the department until he retired in 1923 and G. H. West, docent for three years.

Small remained head of the department and professor until 1925 during which time approximately thirty people appeared in the annual Official Publications of the University of Chicago as sociology teaching staff along with another three anthropologists. (Full details are in Appendix 1). At no time, however, did the sociology group consist of more than half a dozen people. Notable amongst the appointments made during Small's headship were that of G. Vincent, a graduate student from
1893 who co-authored a sociology text book with Small (Small and Vincent, 1894), became professor and Dean of the faculty of arts, literature and science in 1907 before leaving to become President of the University of Minnesota and later President of the Rockefeller Foundation in 1917 (Diner, 1980).

Another graduate student, E. W. Burgess, who received his doctorate in 1913 was appointed in 1917 as an assistant professor and remained in the Department until 1951 and, following his retirement in 1952, remained active as an emeritus professor into the 1960s. His long term involvement with the Department covering some fifty years made him an important figure in its development.

An equally important appointment initiated by Thomas (Raushenbush, 1979; Matthews, 1977) was that of Robert Park in 1914. Initially appointed as a professorial lecturer, Park became a full professor in 1923. He is regarded by a number of commentators (Faris, 1967; Matthews, 1977; Coser, 1971) as the prime force behind the rapid development of empirical study in the Department during the 1920s. Park retired in 1935, and although he remained professor emeritus until his death in 1944, his energies were directed to the Tuskegee Institute to whom he was attached after 1935.

Another long term appointment was made under Small, that of S. W. Bedford, who was associated with the Department over a twenty year period until his resignation in 1925. His principal interest had been urban sociology and according to Diner (1980) his teaching actually gave an impetus to the area which is most usually associated with the 'Chicago School'. However, he
published very little and did no empirical research and perhaps for this reason is not often regarded as a significant figure in the development of sociology within the Department. Faris (1967, p. 32) makes but one reference to Bedford referring to him as an instructor. Bedford was, in fact, an associate professor when he resigned [3].

Apart from Ira Howerth and Anne Marion McLean who taught in the extension division, other members of the department, excluding fellows, recorded in the Official Publications up to 1925 were C. Zeublin, A. F. Bentley, J. H. Raymond, H. Woodhead, G. Taylor, M. S. Handman, C. Rainwater, F. Znaniecki, F. N. House and E. N. Simpson. In addition Edith Abbott taught part time in the department as a lecturer in methods of social investigation until 1920 while also assistant director of the training school for social workers, The Chicago School of Civics and Philanthropy (from 1908). With the formation of the School of Social Science Administration in 1920 she left the Department of Sociology and Anthropology.

One other major appointment was made during Small's leadership. Following Thomas' departure, Ellsworth Faris was appointed professor in 1920 and became Head of Department in 1926, a position he retained until his own retirement in 1939. During this period (1926 - 1939) the Department of Sociology and Anthropology split into two separate departments (1929) and the Sociology department finally appointed its own quantitative expert, W. F. Ogburn in 1927.
Chicago graduates from this period who were to become professors and exercise some considerable influence on the department were Louis Wirth, Herbert Blumer, Samuel Stouffer and Everett Hughes. Wirth was granted the doctorate in 1926 and, after two years at Tulane returned to Chicago in 1931 and became a full professor in 1940 following Ogburn's own promotion to Head of Department. Blumer gained his doctorate in 1928 and remained at Chicago. Following Mead's death in 1931, Blumer took over the teaching of the social psychology course that had been offered by Mead. Blumer's own promotion to full professor was not until 1947, at which time Ernest Burgess had taken over the headship of the department. Stouffer, who had been an instructor for two years following the award of the doctorate in 1930, was reappointed at Chicago in 1935 as a full professor. Hughes, a graduate student to 1928 left to teach at McGill University in Canada before returning to take up the appointment of assistant professor in 1938. He was eventually appointed to full professor in 1949 and became head of the department for three years in 1954 before retiring in 1961.

Hughes was succeeded as head of department by Philip Hauser, who was awarded a doctorate in 1938. He had been an instructor in the Department for five years prior to that and was re-appointed in 1947, in the first year of Burgess' headship, as a full professor.

Burgess' headship also saw the inauguration of more varied developments in the department and the growth of a number of associated staff involved in a variety of projects including the
National Opinion Research Center (whose directors were C. W. Hart (1954) and P. Rossi (1962) and whose senior study directors included S. A. Star (1954), E. S. Marks (1954), J. Elinson (1955), E. Shanas (1958), L. Kriesberg (1960), J. Feldman (1961) and J. W. Johnstone (1962)); the Industrial Relations Centre (for whom C. Nelson was Director of programme evaluation, 1955); the Chicago Community Inventory; the Population Research and Training Centre (E. Kattagawa, 1956 and O. D. Duncan, 1959); the Farm Study Centre (E. Litwak, 1954); and Community Studies Inc., of Kansas City, Miss., (with whom Howard S. Becker was associated).

Another thirty people were also employed in a lecturing capacity in the sociology department from 1926 to 1954. Of these, the following spent five years or more lecturing in the Department: E. H. Sutherland (1930-34), M. M. Davis (1932-37), E. S. Johnson (1933-41), C. Shaw (1935-1957), J. D. Lohman (1940-1956), E. A. Shils (1940-7 and 1957 onwards), W. F. Whyte (1944-48), L. Goodman (from 1950), D. Horton (1948-57), D. G. Moore (1950-55), N. Foot (1952-56) and W. Bradbury (1952-58). (Full details are in Appendix 1).

In addition to the resident staff, the Department invited eminent sociologists from other institutions to teach, especially during the Summer quarter (a system relatively unique to the University of Chicago). Among the twenty five different sociologists listed in the Official Publications who provided such courses were E. A. Ross of Leyland Stanford University (1895), Lester Ward of the Smithsonian Institute (1896) Talcott Parsons of Harvard (1937) and Paul Lazarsfeld of Columbia (1949).
2.6 The Diversity of Chicago Sociology

Arguably, the diverse activities and interests of the Chicagoans inhibited any possible development of a 'school', and interestingly, the term 'Chicago School' was rarely used by the Chicagoans themselves and tends to have been applied retrospectively. The question remains, is such an application indicative of the practice of the Chicagoans?

As early as 1911, Small noted that

'There is quite as much difference of opinion in matters of detail between members of our sociological staff as will be found between representatives of different institutions.' (Small, 1911, p. 634)

Janowitz (1966) stated that the Chicagoans were not a school.

"It is a disputable question whether there was a distinct or unified Chicago approach to sociology ... the Chicago school contained theoretical viewpoints and substantive interests which were extremely variegated."

The brochure of the Department of Sociology at Chicago for 1981-82, as in previous years said,

'The department has never been dominated by a single individual or by a single school of thought.'

Cavan (1983) stated in her review of the period 1918 to 1933, of what she says has now become known as the 'Golden Era' of Chicago sociology, that she can not ever recall hearing the term 'Chicago School' during the 1920s and quotes Everett Hughes, who was a graduate student at Chicago from 1923 to 1927 as saying

'I don't remember where or when I first heard of the Chicago School. That phrase was invented by others, not the Chicago people. I suppose there was some sense in the term, but it implies more consensus than existed.' (Cavan, 1983, p. 408).

And in 1969 Hughes indicated that he still disliked talking of a 'Chicago School' or 'any other kind of school' (Hughes, 1980b, p. 58)
The Bulletin of the Society for Social Research [5] makes no references to a 'Chicago School', or a 'Chicago Approach', or to a specifically 'Chicago Sociology'. Nor does Wirth (1947), in his review of the history of sociology (1915-1947), make any references to a 'Chicago School' or a particularly unique practice located at Chicago university.

Indeed as Becker (1979a) recalled, Wirth did not recognise a 'Chicago School'.

'When I was a graduate student at Chicago, one of the people who was really considered to be a leader in a 'Chicago School of Sociology' was Louis Wirth. And Louis Wirth used to say that he was constantly amazed at being told that he was part of the Chicago School of Sociology, because he couldn't imagine what he had in common with all those other people.' (Becker, 1979a, p.3)

While it seems that some outsiders did regard sociology at Chicago as representing a distinct school, it is easy to overemphasise the amount of outside consensus about the distinctiveness of Chicago sociology. For example, even by 1940, by which time the notion of a Chicago School had appeared in print and may have existed informally, Wilson of Harvard in writing to Burgess to recommend William F. Whyte to Chicago made no mention of a 'Chicago School' nor an exclusive style of sociology at Chicago. Wilson's (1940) recommendation was made primarily for administrative reasons, and secondarily because of the sort of work undertaken in the field by Whyte. Wilson saw both Chicago and Columbia universities as alternatives for the kind of empirical work Whyte wanted to undertake.

'I have suggested to him that as far as I know the best place in this country for him to work out a doctorate might
be Chicago with you and I have further suggested that he might well look into the situation at Columbia where there must be people who would be interested in the sort of thing he is interested in - possibly Lynd.' (Wilson, 1940)

Wilson saw Columbia and Chicago as offering the same opportunities for Whyte, and not as antithetical institutions. Such a view reflects Coleman's (1980) understanding of the situation, at least prior to 1945. This is counter to the usual view that places the two institutions at polar extremes within the history of American sociology.

2.7 The 'Chicago School' and the Society for Social Research

It is in 1939, that probably the first attempt to specify the 'Chicago School' can be found. This is in Park's (1939) retrospective account of the development of the Society for Social Research.

Park refers to the 'Chicago School' in a manner that indicates that there was an approach to sociology practiced at Chicago during his association with the department, but also that such a practice was not particularly clear cut, except that it was grounded in Thomas' conception of sociology. This single reference to the 'Chicago School', made in a paper of 1939 is one of Park's earliest references to such a 'school', a term he rarely used. Park appears, in the use of 'Chicago School', to be picking up a term that he has heard elsewhere and fitting it to the Society for Social Research.

Park recalled that the Society for Social Research was organised in the Fall of 1921, and that its aim was to bring together
interested and competent researchers (students and staff).

'Research in the social sciences at Chicago began before the organization of the Society for Social Research. However, the particular type of research that has been identified with the "Chicago School" has found in this Society, in its Institute, and its publications an effective organ of expression. The Society was originally organized to stimulate a wider interest and a more intelligent co-operation among faculty and students in a program of studies that focused investigation on the local community.' (Park, 1939, p. 1) [6]

Park is here referring to what others have identified as the 'Chicago School', and he sets it clearly in the compass of the Society for Social Research. In effect, he sees the Society for Social Research as the institutional manifestation of a general approach to sociology at Chicago, which was based on what, in retrospect, Park considered to be the unique contribution of Thomas.

'Long before the founding of the Society Thomas had planned ... the Polish Peasant.... Much of Thomas' work previous to that time seems to have been in the nature of a preparation for the more elaborate and systematic investigation undertaken later. In fact it is in those earlier writings of his that we will find the first positive expression of a point of view which has found a consistent expression in most, if not all, of the subsequent published studies of the students and instructors in sociology at Chicago.' (Park, 1939, p. 2)

Park emphasised the empirical, anti-moralistic, disinterested and sociological aspect of the work, noting in particular the 'natural history' of society. This approach, Park suggested, was rooted in

'a tradition at the University that the city was, or at any rate should be, a natural laboratory for the study of sociological problems. This suggested possibilities to Thomas and at his suggestion we started out to make the city the center and focus of all our studies. It was at Thomas' suggestion that I wrote the article on "The City" (1914) [Park, 1915] which was eventually expanded to make a book under the same title. [Park & Burgess, 1925]' (Park, 1939 p. 3).
For Park, the success of the Society for Social Research was bound up with the new approach signified by Thomas's pioneering work and suggestions, thus they found themselves working in 'a virgin field because of our new frame of reference for approaching problems. There was to be sure an extensive literature on the subject of the city in existence but no one had up to that time it seemed regarded the city as a natural phenomenon ... as an inevitable product of ecological, economic and other processes.... Of course not all the studies carried on in the Department of Sociology were concerned exclusively with the city as a natural phenomenon. Yet it was remarkable to what extent the conditions which urban life imposes seemed to have entered into and complicated every problem which students sought to study. This became increasingly so as students became aware of these conditions and to take them into account when studying their problems. On the other hand the specific problems investigated, to the extent that they were conditioned or complicated by the urban environment, inevitably tended to throw some light on the nature of the urban community itself'. (Park, 1939 p. 4).

Nonetheless the implication is that Park saw Chicago type research as empirical and integrally effected by the study of city environments, which reflected his self image as a 'city man'. Park suggested that the aim of the studies was not just for the information they threw up but as an indicator of the nature of society and the social order generally. Crucial to the sociological enterprise was the attempt, pioneered by Thomas, to integrate the subjective aspect of society with an objective assessment, the relationship of attitudes and values.

These comments of Park's were made five years after he retired from Chicago, and were clearly influenced by how he saw other people interpreting the Chicago style. He reconfirmed the focus of attention on the urban environment and indicated that the Society for Social Research was instrumental in pioneering empirical research. However, this does not mean that Park saw...
Chicago as a distinct and isolated school. Rather, he saw the department as intrinsic to the development of empirical sociological investigation in the United States.

So, while Chicago up to 1920 may have been pioneering, it soon became established as integral to the discipline. Only in retrospect did it become relabeled a 'school' with the accompanying overtones of unique, united sociological practice.

An article about the Summer institute on the front page of the Bulletin of the Society for Social Research for June, 1929, sums up the intellectual climate of the Society.

'The Summer institute has become one of the most interesting and valuable events of the year for sociologists and students of sociology at the University of Chicago and neighboring schools. Its purpose is to serve as a clearing house for current research projects. Here students and faculty members bring their hypotheses, data, and conclusions and submit them to the shafts of friendly criticism from some 75 or 100 fellow research workers.'

This view of the Society as a clearing house is perhaps most indicative of the nature of the 'Chicago School'.

2.8 The Myths Of The 'Chicago School'

While the extent to which the Chicagoans saw themselves as a 'school' is unclear, others, as has been suggested, have claimed to have identified the 'school' and its characteristics. Moreover, this designation is taken to imply certain conceptions about Chicago sociology which have taken on the character of myths, emblems of a distinctive sociological approach. The prevalent taken-for-granted views of a 'Chicago School' raise metascientific questions. The designation of a 'Chicago School'
is not independent of a view of the activities, approach and impact of the school. In effect, certain preconceptions are amplified by the designation of the work of the Chicagoans as reflecting a school. So, the designation of the 'Chicago School' and the myths of the 'School' are interdependent. What is taken to constitute the 'School' is influenced by what commentators take as characteristic of its work, while the myths about the school are amplified by specific widespread designations, (Harvey, 1981, 1983; Lofland, 1983).

To some extent, then, any definition of a 'Chicago School' or of 'Chicago Sociology', can be seen as an arbitrary exercise. In assessing the sociological practice at Chicago, recourse will be made to primary sources in order to assess secondary accounts. The problem of dealing with such an extensive case study has been made manageable by focussing on the various designations and the accompanying myths of the 'Chicago School'. The procedure has been to identify what is seen to be 'essentially Chicagoan', what are the key elements of the various myths about 'Chicago Sociology', where they came from and how credible they are [7].

As will be shown, myths [8] of the 'Chicago School' are varied and often conflict. This is because they do not all derive from the same source nor are they directed to the same ends. However, five myths, may be identified which surround the 'Chicago School' and emerge from various accounts of sociological practice at, or informed by, Chicago.

These five myths are:
(1) that Chicago sociologists were primarily social ameliorists, sympathising with Progressive or liberal ideas and concerned to resolve social problems.

(2) that Chicago sociology was dogmatically qualitative and had no interest in quantitative techniques of social research and, indeed, were openly hostile toward them.

(3) that Chicago sociology had no strong theoretical orientation and its work, in the main, constituted a descriptive exercise. Such theories as it did produce were little more than ideal type models (notably the 'concentric zone' thesis) with little explanatory power.

(4) that Chicago sociology is closely associated with symbolic interactionism and dominated by the epistemological perspective of G. H. Mead.

(5) that the 'Chicago School' dominated American sociology until the mid-1930s and then went into decline and became isolated mainly because it retained an old fashioned, unscientific, approach to sociology.

The following five chapters examine each of these myths in detail and chapter eight involves an assessment of the role the notion of school has played in helping generate such myths. The constructs of the 'Chicago School' adopted by both Tiryakian and Mullins will, incidentally, be reviewed in the light of this analysis, following the critique of the myths. The final chapter will suggest an alternative interpretation of the historical data.
to that encapsulated in the myths of the 'Chicago School'. 
NOTES TO CHAPTER TWO

1. In 1972 James Carey interviewed a number of ex-graduates who had been at Chicago in the 1920s. The transcripts of these tape recorded interviews are lodged in the Special Collections Department of the Regenstein Library, University of Chicago.

2. According to Lofland (1980, pp. 251-252), The 'Chicago Irregulars' were a group 'born in the living room of Sherri Cavan's San Francisco home on April 11, 1969, when Sherri Cavan, John Irwin, John Lofland, Sheldon Messinger, Chet Winton, Jacqueline Wiseman, and I met and agreed that a "mutually supportive association of sociologists and others interested in the study of natural settings, everyday life, everyday worlds, social worlds, urban lifestyles, scenes, and the like" was in order. It died in late 1969 or early 1970 when the energies required to keep it going simply ran out. In between it turned out three newsletters (mailed to a continually growing list), held several seminars, started an archives (long defunct), and, most memorably, organized the Blumer-Hughes talk [of 1st September 1969]!' Sheldon Messinger, introducing the Blumer-Hughes talk added that the 'Chicago School Irregulars' had 'had the strong feeling that there is a substantial group of people in sociology for whom the Chicago School is still a very viable institution, notwithstanding the spread of its members away from Chicago to Berkeley and Brandeis, to name two places.... The group is devoted to keeping the Chicago School tradition alive. Many of the people in it do what is nowadays called ethnography - in the old days it was called nosing around. Others, who aren't themselves doing ethnography, are reading about it, talking about it, and trying to keep up the standards established many years ago by people at Chicago.' (Messinger, 1980b, p. 254)

During the talk, Hughes was disparaging about attempts to preserve a tradition but told the group 'go ahead and be a Chicago School if you like.' (Hughes, 1980b, p. 277)

3. Blumer (1980b) and Hughes (1980b) recalled their time at Chicago in a talk in 1969. They made jocular references to 'someone who taught urban sociology', but were unable to recall Bedford by name. Bulmer (1984) pointed out that Bedford was forced to resign as he did not fulfil the criteria for a member of staff Small required of him.
4. Bulmer points out that Sutherland was a research professor in the Division of Social Science from 1930-1935 and that Shils' original appointment was on the Committee on Social Thought. (Correspondence 1.4.1985). The information in the text derives from the Official Publications of the University of Chicago.

5. The Department of Sociology and Anthropology inaugurated a Society for Social Research in 1921. It produced a Bulletin in 1926 and continued to do so two or three times a year throughout the period of this study. The circulation list included current and past Chicago graduates. Membership was open to all social researchers (graduates and staff), election to the society was fairly straightforward and new members were constantly being added. (See Appendix 3 for details of the Society for Social Research including the Constitution and a membership list). By 1926 there were around one hundred and fifty members. Subscription, payable annually, was a nominal $1. Each year, from 1923, Summer Institutes were held which lasted about three days and included a substantial proportion of invited visiting speakers, some of whom were members. The format of the regular weekly meetings changed over the years, but generally they were addressed by graduates, staff, or outside speakers on matters of research practice, findings or philosophy (see Appendix 3). The society served to keep members informed of current research ideas and work in progress and also functioned as an informal network with contacts around the country. Hughes (1980b) noted that while he was away in Canada he kept in constant touch with the University through the Society for Social Research. The society also performed one other major function, that of arranging discounts on text books and research monographs.


7. Notwithstanding Denzin (1984, p. 1431) who, in a review of Lewis & Smith (1981), argues against any myths surrounding the 'Chicago School'. Whether Denzin is simply referring to the myth Lewis and Smith project around the role of Mead or whether he is suggesting that the 'Chicago School' is not characterised by myth at all, is not clear. However, the substantive point is that in analysing myths the historian must avoid the construction of the myth.

8. Myth does not refer here to the original anthropological sense of 'fabulous narration' a sense in which it is still commonly widely used. Nor does it refer to a 'distorted' thesis about the origins of humanity. In short, myth does not mean either fable or legend. Nor is myth used simply to mean a deliberately false account or belief. It is used in the sociological sense of a pervasive taken-for-granted account. This reflects, for example, the work done in analysing the mythical element of media messages. Thus myth is used in the sense of generalised connotation (Barthes 1967, 1974; Centre for Contemporary Cultural Studies, 1978; Larrain, 1979). This raises questions about the relationship between myth and ideology, the nature of ideology and the relationship between ideology and knowledge. These
important questions, however, go beyond the scope of this analysis of the role of 'school' in the production of scientific knowledge and the particular account of the 'Chicago School'
CHAPTER THREE

CHICAGOANS AS AMELIORISTS
3.1. The Myth

The idea that Chicago sociology was primarily aimed at social amelioration can be found in the writings of a number of commentators. Friedrichs (1970, p. 73), for example, characterises the leaders of the 'Chicago School' as 'prophetic seers dedicated to the progressive amelioration of social ills', and Madge, (1963, p. 125) talks of the 'Chicago School's' 'faith in human betterment'.

The view of Chicago sociology as reformist is primarily aimed at the early years of the 'Chicago School'. It does not appear to apply to the later generations as Gouldner (1973) has pointed out. The post-1950s work of the Chicagoans is seen by Gouldner as determinedly devoid of moral judgements and reformist motivations. Indeed, the stance adopted by Becker and others in the 1950s was one that categorically demanded an absence of moralising and the adoption of a perspective which engaged the perspective of the subject group.

In which case, the designation of 'Chicago School' sociology as reformist in its early years implies that a 'volte face' occurred at some later date. When this is supposed to have occurred and for what reasons, is not so clear, indeed, most commentators do not address themselves to this change of attitude, except to suppose that the emergence of symbolic interactionism from Chicago made such a shift inevitable. Gouldner (1970), however, had already suggested that the work of the 'Chicago School' was motivated by a desire for academic status and, with one eye on financial expediency, a disinclination to criticise vested
interests when social conditions changed in the 1920s.

Irrespective of any concern with assessing any shift of per- spective in the 'Chicago Tradition', a number of commentators see clear indicators of the reformism in the work of the Chicagoans, (Berger and Berger, 1976, p. 48; Brake, 1980, p. 30,) and the concern of the Chicagoans to apply their knowledge to social problems.

'The concluding chapters of Zorbaugh's study, as did most analyses of the early Chicago social scientists (Hunter, 1980b), turned to the issue of applying his knowledge and insights to ameliorative social reform.' (Hunter, 1983, p. 473).

Interestingly both Hunter (1983) and Madge (1963) take Zorbaugh (1929) as a paradigm case. Hunter cited Castells (1977) and Molotch (1976) and noted the apparent paradox of ameliorative concerns and the determistic models of 'natural area' and 'natural forces'. The only resolution of this paradox offered by Hunter, and this incidentally, is that the Chicagoans were interested in consciousness raising to effect changes in these external 'forces'. Thus he noted that the new social politics to which Zorbaugh appealed was a

'welding of voluntary, citywide philanthropic foundations and charities and of the community survey movement, which provided the social science data base for specific reforms.' (Hunter, 1983, p. 473).

Tiryakian (1979a) also linked the Chicagoans to 'civic reform'. Members of the school at either the doctoral or post-doctoral stage, he noted, were able to find employment in the various civic and municipal agencies which abounded in Chicago and were thus able to help 'guide the development of the rapidly growing metropolis'. These commentators tend to overemphasise the policy.
orientation of the work undertaken and considerably underplay the avowed anti-reformist attitude at Chicago.

Richard Lapiere, in his retrospective account of the development of American sociology, located the apparent ameliorist approach at Chicago in a wider context. He wrote:

'As you no doubt learned in your first course on the history of sociology, American sociologists of the first two decades of this century were — with some few exceptions, of which Cooley is the only one who comes to mind — just moralistic reformers in scientists' clothing. What you may not know, or at least not fully appreciate, is that well into the 1930s the status of sociology, and hence of sociologists was abominable, both within and outside the academic community. The public image of the sociologist was that of a blue-nosed reformer, ever ready to pronounce moral judgements, and against all pleasurable forms of social conduct. In the universities, sociology was generally thought of as an uneasy mixture of social philosophy and social work.' (Lapiere; 1964)

Lapiere further suggested that it was only through the work of individuals using quantitative methods and located in isolated, mainly 'one man' departments, that sociology threw off this image. Yet this contradicts what the Chicagoans believed themselves to be doing. The Chicagoans, in accord with other pioneers in the field, were concerned to establish a scientific approach to society devoid of reformist interests.

The contention in this thesis, then, is that no sudden change occurred because Chicago sociology was never committed to amelioration. Rather, it consistently adopted an attitude of 'detached' sociological enquiry, reflecting changes throughout the discipline.

Cavan argued that there was no change from reformism to moral detachment. From the beginning Albion Small advocated and,
through a distancing of the sociological work of the department from social work and the religious concerns of the University, achieved an objective approach.

'The conflicting interests of human betterment and objective studies were apparent from the beginning. The purpose of sociology was not humanitarianism or social reform, but an understanding of human behaviour.... Small had not only set the stage for an objective research approach ... but had extended the influence of the department far beyond'. (Cavan, 1983, 9. 409)

A closer analysis of the research conducted at Chicago will indicate the extent to which reformist tendencies were evident. This analysis, as will be shown, suggests that the 'Chicago School' orientation reflected changes in United States sociology as a whole of which it was an integral part.

3.2 Small and Henderson and Christian Reform

Cavan (1983, p. 407) calls the 'Golden Era' (1918-1933) the period of

'transition between the founding period and the establishment of objective research.'

This founding period, during which time Small and Henderson were most influential, was, arguably, directed towards reformist ends. (Matthews, 1977, p. 93). Henderson saw sociology as interwoven with Christian reform and under the auspices of his role as University chaplain published an article in which he wrote that 'God had providentially placed the social sciences at the disposal of reformers', (Henderson, 1899). Henderson had had a long experience of practical philanthropy before moving to the University. He also developed close ties with Jane Addams' 'Hull House' and Graham Taylor's 'Chicago Commons' and, impressed by
the social surveys done by these organisations, promoted empirical work at the University. He argued that first hand observation and intimate experience of daily life as experienced by the 'poor, the socially deviant and the distraught' was essential as a basis for Christian reform.

Although apparently strongly oriented towards reform, even at this stage, the encouragement of first hand investigation was directed towards an understanding of problems rather than merely expunging them. Indeed, Small argued forcibly for empirical research and encouraged first hand investigation and, while he believed that sociology must be essentially Christian, 'distrusted the preachers of the Social Gospel' (Matthews, 1977, p. 95). Barnhardt (1972) recalled that Small was not at all keen on his attending Matthews course on 'The New Testament and Social Problems' in the Divinity School, nor a course on social work with Breckenridge.

Empirical investigation was forthcoming in the studies encouraged by Henderson and Talbot. Although much of this early enquiry was motivated by 'reformist' concerns they were not, however, simplistic ameliorative undertakings. For example, MacLean (1910) researched women in the labour force with a view to examining the role of trade unions. For her, trade unions represented a rational theory of industrial betterment in which the employee rather than the employer was the motivating force.

Inskeep (1977) has argued that an examination of the 'Chicago School' between 1895 and 1920 reveals that Small and Henderson attempted to reconcile the debate between the naturalist
sociologists and the Christian reformers. They refused, on the one hand, to confine sociology to the New Testament scriptures, nor, on the other, would sacrifice spiritual answers entirely. Post Darwinian naturalism and New Testament spiritualism both held self-evident natural truths for Small and Henderson and they were not prepared to limit their insights to a unidimensional approach.

However, by the 1920s, many of Carey's interviewees indicated that the department tended to ignore religion, in the sense that there was no attempt to develop an attitude to sociology or social problems which ostensibly reflected religious concerns. The bitter struggle between evolutionism and fundamentalism had been resolved in the former's favour and Chicago adopted an evolutionary approach, and with it a kind of 'religious indifference'.

The Department had been established at a time when sociology and Christian reform were seen as compatible and its faculty composition was influenced by this perceived relationship. As the new century unfurled, sociology and reform were less closely allied. Indeed, at Chicago, a separate department of Ecclesiastical Sociology was established in 1904, later to become the Department of Practical Sociology (1913). Henderson was professor of the department and it is notable that after his death in 1915, the course structure in the Sociology department, with whom he had still been closely linked, was revised. Courses which linked sociology with religion and reform were dropped or began to fade out. Thus, Small's own course on the 'Ethics of Sociology' was
never actually taught, and Burgess's 'The Causes and Prevention of Poverty' ran only rarely in the next decade. 'Problems and Methods of Church Expansion', 'Contemporary Charities' and 'Family Rehabilitation' rapidly disappeared and, like 'Church and Society', were not taught by mainstream sociology staff. The later reorganisation of courses in 1924 saw a pronounced distancing of sociology from social reformism. (See Appendix 4).

In the last resort, Dibble (1972) argued that Small saw sociology as an objective science in its own right, and not just as an extension of moral philosophy although ethical considerations pervaded his sociology. In arguing for the founding of the American Journal of Sociology, Small noted that

> 'every silly and mischievous doctrine which agitators advertize, claims sociology as its sponsor. A scientific journal of Sociology could be of practical social service in discrediting pseudo-sociology and in forcing social doctrinaires back to accredited facts and principles.'

(Small, 1895) [1]

For Small, sociology was a scientific and ethical discipline oriented towards reform based on sound knowledge (Dibble 1972). Ethical considerations, for Small, provided the basis for decisions about what areas of enquiry were suitable for sociology, while enquiry should proceed scientifically. The canons of science advocated by Small were value neutrality, objectivity, and theoretical analysis. Value neutrality meant non-partisanship, objectivity meant rooting assertions in empirical evidence rather than conjecture, and he saw science as necessitating inductive theorising not just the collection of 'raw facts' (Small and Vincent, 1894). He saw science proceeding through the correlation of facts, through a procedure that would
allow for the grasping of meaning embedded in facts.

Small maintained that the worth of scholarly research lay in its objective, non-partisan perspective. The more a work took into account diverse views the more objective Small saw it to be. He expected sociological research to take into account the wider social milieu, yet be empirically grounded, as he noted in a letter to Harper in 1891,

'I would never grant the doctorate to men of the microscope alone, but would insist that they shall have acquired a sharp sense of relation of what their microscope discovers, to the laws of society as a whole.' (Small, 1891)

3.3 Thomas and the Demand for Pure Research

Thomas became prominent at Chicago during the first decade of the century. His was an important role in the transformation of sociology into an 'objective empirical' discipline. He reasserted Small's contention that understanding of the social world must precede social action and that objective research was the goal of sociologists. He extended this to the point of shelving Small's ethical concerns as criteria for such objective analysis thereby reflecting the prevailing attitude towards social science. Objectivity, in the sense of impartiality, had become a major concern and directive for social research (Bernard and Bernard, 1943; Schwendinger and Schwendinger, 1974; Furner, 1975).

In his review of the origins of the Society for Social Research at the University of Chicago, Park (1939) suggested that Thomas provided the basis for work done at Chicago, and that this
involved distancing sociology from reformist concerns.

'It is in the work of W.I. Thomas, I believe, that the present tradition in research at Chicago was established. .... His 'Source Book for Social Origins' (1909) ... introduced a point of view from which society, with its codes, conventions, and social programs, was regarded as a natural phenomenon, the result of purely natural processes. From this point of view society ceased to be a body of legal conventions or moral ideas which sociologists were seeking to criticize...

He wanted to see, to know, and to report, disinterestedly and without respect to anyone's politics or program, the world of men and things as he experienced it.' (Park, 1939, pp.1-2)

Thomas was, then, instrumental in the advocacy of direct, first hand, experience of the social world. Hayner (1972) attributed the development of detached empiricism at Chicago to Thomas, and Park too traces it back to Thomas.

'Interest and curiosity about human beings ... found expression in his [Thomas's] course, Social Attitudes, and indirectly in most of the studies in racial psychology that have been made at Chicago since that time. The purpose of these studies have been to enlarge our acquaintance with the subjective aspects of life and they have found expression in studies like that of Thrasher's studies of the gang and Shaw's life histories of delinquent boys.' (Park, 1939, p. 2-3).

Thomas saw himself as a scientist of society, and sociology as providing the laws by which change could take place. These laws were not seen by Thomas as completely deterministic but merely as providing the frame within which action would be constrained. These laws would provide the basis for social control, the principal objective of sociological enquiry. Empirical analysis of concrete historical situations in the manner of the objective sciences would permit the discovery of these laws.

'By following the example of the physical sciences and accumulating the largest possible amount of secure and varied information and establishing general and particular laws which we can draw on to meet any crisis as it arises, shall we be able to secure a control in the social world comparable to that obtained in the natural world, and to
To achieve this, Thomas argued that sociology should concern itself with 'pure' research, and should not worry about direct social utility. Drawing parallels with the physical sciences he argued that research in the social sciences should proceed irrespective of practical applicability. Like pioneers in physics, problems should be seen initially as ends in themselves. Sociologists should not be dependent on practice, particularistic reform should not inform sociological endeavours. He argued that

'if we recognise that social reform is to be reached through the study of behavior, and that its technique is to consist in the creation of attitudes appropriate to desired values, then I suggest that the most essential attitude at the present moment is a public attitude of hospitality toward all forms of research in the social world. (Thomas, 1917, p. 188)

This pure research approach, while not the only consideration underpinning research in the department, became established among the Chicago sociologists and, indeed, within the social sciences at Chicago in general. Bedford's resignation, for example, after twenty five years in the department, following Small's retirement, was probably due to his lack of interest in 'pure sociology' and too great a social work orientation (Barnhardt, 1972).

The Ogg Report (1928) on the extent and nature of research in the humanities undertaken at American Universities recommended that social scientific research should be more directed to 'pure learning' rather than to direct application. Only a handful of universities, the report noted, of which Chicago and Columbia were the most advanced, were adopting such an approach. In 1925 a
Chicago faculty committee had submitted a report [2] stressing knowledge for its own sake rather than applied knowledge with practical applications, and that graduate courses be organised to foster such research.

3.4 Park's Anti-reformism

Park endorsed Thomas' perspective (Blumer, 1980b) and reflected the development of American sociology in general. Park had moved from a concern to expose social ills, to doing something about them, to beginning to understand the processes which bring about social problems. He had reached the threshold of the last stage when he moved to Chicago, to a department that he saw as clearly espousing a theoretical rather than an ameliorist perspective.

Looking back, Park (1939, p.2) noted that he had always been of the opinion that sociology as a mere 'science of social welfare' would be ineffective as a contributor to social welfare because it would be limited to practical affairs and lacking a fundamental theoretical base.

'The conception of sociology which made it a science of welfare would have limited research to practical problems, as they were conceived by various social agencies. In so doing it tended to discourage that intellectual interest and natural curiosity which had been so largely responsible for the growth of science in other fields (social anthropology for instance) which have not been dominated by practical and ethnocentric interests to the same extent that is true of sociology in the United States. It was in his Social Origins that Thomas seems definitely to have broken with the American tradition that identified sociology with social politics and limited social research to so-called 'social problems'. (Park, 1939, p.2).

Park indicated that 'Social Origins' served to shift the focus of attention of sociology and to revive a scientific interest in
social processes and turn sociology away from social reform and 'social welfare'. Park's analysis denies the assumption sometimes made, that empiricism at Chicago was a function of reform initiatives. For him, empirically grounded study served to reveal the 'real world' of social relations, and illustrate the essentially limited and misconstrued world view of 'do-gooder' reformers.

Park identified with Thomas' view of sociology and he was a vociferous supporter of the dictum of non-alignment, and is reported (Coser, 1971) to have been displeased by students who espoused reformist sentiments.

'Park was just vicious in his attack on social workers and reformers and do-gooders. They were lower than dirt. The thing to do was get out and know what life was like. His students got this attitude quite readily from Park and Thomas also.' (Cottrell, 1972)

Apparently, Park commented that Chicago had suffered more at the hands of 'lady reformers' than from gangsterism, and Blumer (1972) recalled that there was hostility between Park and Breckenridge. Sociology students, while able to take courses in other departments, were discouraged from taking social work courses. This antagonism towards social work, Blumer suggested, was about 'science'.

'Park was decisive in his view that in order for reforms to have reasonable chances of being successful, as well as being meritricious that they should be grounded upon a scientific knowledge of human society and settings in which the reform efforts were to be undertaken.' (Blumer, 1972, p.8)

In the wake of the race riots of 1919 Park advised the black co-director of the Commission of investigation, Johnson (associate executive secretary), to study race relations with the same
detachment that a biologist might adopt in dissecting a potato bug (Turner, 1967, p. xvi). Johnson apparently adopted a dispassionate approach, and was sometimes attacked for being a 'calm student' rather than an 'active reformer' (Bracey, et al, 1973, p. 15). Park actually maintained a distance from the Commission investigating the Chicago Race Riots, although asked for advice on the nomination of the executive secretary. As he was the foremost authority in Chicago on race relations, this distancing seems peculiar. Bulmer (1981b) implies that as the Commission was 'clearly the idea of reform minded civic leaders', Park wanted little directly to do with it, and contented himself with advising Johnson from a distance.

In his own involvement, in 1924, in a survey of 'Chinese, Japanese and British Indian' residents on the Pacific Coast, (Park, 1926) sponsored by the Institute for Social and Religious Research, Park insisted on a disinterested survey while the sponsors wanted to use the survey as a means of educating the public. Park argued for a study which took popular views about ethnic minorities at face value and then attempted to understand why and how such opinions emerged (Matthews, 1977, p. 114).

Carey's interviewees, reflecting on the reformist element of sociology at the time were unanimous in suggesting that sociology and social reform were distinct.

'We were committed sociologists, we didn't think of it being opposed to social reform or even really consider social reform as such.' (Faris, R., 1972)

'By the time I got to Chicago I pretty well made the kind of transition that I think a lot of people made from a kind of religious motivated interest to do something about the ills of society to a scientific orientation. What is it that
makes the thing tick? And by doping that out we can solve problems. I think that was the orientation shift that took place there. (Cottrell, 1972)

'The idea as I saw it was that sociology was not an applied science, its function was not to bring about changes in society but describe society accurately and presumably, then, if you wanted to become a society changer you could do so by stepping out of the description role and going into the action role.' (Dollard, 1972)

The impression emerges that Park fought vehemently against reformist crusades and, indeed, he saw the work of Thomas as pioneering a theoretical approach to the science of society. He was at times somewhat aggressive in making this view quite clear to his students, especially those who espoused reformist sentiments. However, Park did not represent a complete break with the past or with a prevailing tendency in American sociology. Rather, he tended to reflect prevailing views on the complex relationship between reform and sociology. Thomas provided the impetus for moving away from amelioration altogether and Park argued for sociological work that was detached from an amelioration-reform continuum and shifted along a policy-pure research continuum. Most of his own research and that of his students was located along that continuum, informed by policy problems but conducted with varying degrees of 'detachment' from policy implications. The sociological analysis of social problems figured largely in their work but Park insisted that any commentary on 'social ills' be pitched at a holistic level. The improvement of society was, at the time, seen as a major task of sociology and in this respect Park was no different.

'Though the outsider would never have suspected it, he embarked upon his career with a passion for social reform and ended it with the same—goal to improve the human lot. But he was never a 'for God-saker', as he used to refer to the crusaders who ignored reality. His interest in the
solution of problems of human interrelationships was chastened by the recognition of the facts of life and the nature of social change. He was a disciplined humanitarian.' (Wirth, 1944, p. 3)

As a journalist Park had been a 'muckraker' particularly towards the Congo with the avowed aim of exposing the appalling situation in that country. His later work with Washington, as 'detached' as it was, was also geared to getting a better deal for blacks (notably through promoting self-help). In the field of race relations in particular, though, despite his 'sympathy for the downtrodden negro' (Cottrell, 1972), Park retained a 'big picture' perspective. The 'reformist' ideals he engaged in this realm were always holistic and radically opposed to piecemeal adjustment. Park saw racial equality as synonymous with democracy, and despite casual observations and perspectives which may now seem to be somewhat racist, his endeavour was directed towards the ending of prejudice. This is reflected in the obituary article written by Horace Cayton, in which he noted that while Park was economically conservative, or even reactionary, he had an altogether different view of race relations. Cayton referred to a letter written by Park.

'Democracy is not something that some people in the country have and others not have, not something to be shared and divided like a pie - some getting a small piece and some getting a large piece. Democracy is an integral thing. If any part of the country has it, they all have it. If any part of the country doesn't have it, the rest of the country doesn't have it. The Negro, therefore, in fighting for democracy for himself, is simply fighting the battle for our democracy... I think the liberals realize now that the Negro's cause must in the long run win. The only thing is, they don't want it to win too soon and they don't want the change to be so rapid as to result in the disorders that we have had. Personally I don't agree with these liberals. In fact I've never been a liberal. If conflicts arise as a result of efforts to get their place it will be because the white people started them. These conflicts will probably occur and are more or less inevitable but conditions will be
better after they are over. In any case, this is my conviction.' (Park, 1944)

While Small had increasingly argued for the development of sociology independent of ameliorist concerns, he had been constrained by ethical considerations. Thomas had pushed Small's scientific concern away from its ethical constraints by arguing for 'pure' research as fundamental for the understanding of social problems and as a basis for sound policy decisions. This suited Park who increasingly advocated the practice of a disinterested sociology.

'I think, like all young newspapermen, he thought that the power of the press was something and he could really get things done in the world by exposing it, and so on. I think that he just sort of found out that that wasn't the case and retired into a scientific attitude. The thing to do is to understand these processes and when we understand them then we will be able to control intelligently and rationally. That was the general mind set I got from him. So reformism was in ill-repute. (Cottrell, 1972)

Nonetheless, Park was not opposed to social action per se. What he was opposed to was piecemeal and theoretically uninformed social action. For him, social action needed to be rooted in a sound understanding of social processes. Park, like Thomas, saw the pure dimension as preceding the policy dimension of research.

'Even Park would say that once we know what is going on you can use this knowledge to support some kind of rational social order. And Burgess was much more explicit. In fact Burgess was always somewhat suspect among his more 'objective scientific colleagues' as being a little bit too much motivated by wanting to save the family or cure delinquency or help social workers do their jobs better because he always had a very practical flair, everything he did he was really very atheoretical and eclectic. (Cottrell, 1972)

Park, then, wanted a clear distinction between social science and reformism, not out of any notion that sociologists had no role to play in reform, but because reform would be served better if
social science developed independently of reformist constraints and perspectives.

3.5 Burgess and Action Research

It would seem that Burgess was more concerned with 'action research' than some of the other Chicagoans, particularly with respect to 'rehabilitation' which led on to his later interest in prediction studies.

'Burgess had had a certain amount of reformism in his background...' (Cottrell, 1972)

'I worked with Burgess and, as Faris so rightly points out, he did retain his humanitarian interest.' (Hayner, 1972)

Nonetheless, this was not generally communicated to the students to the extent that reformist concerns were uppermost in their research. Of the forty two theses examined in detail, only three (7%) directed some attention towards reformism, while twelve explicitly disassociated themselves from reformist concerns and the remaining twenty seven had nothing to say on the subject.

Many of the social problems that prompted study by graduate students in Chicago from 1915 to 1930 under the guidance of Burgess were initiated by reformist concerns, for example, Cressey's study of 'Taxi Dance Halls' (Cressey, 1929), and the work of Shaw (1930). [3] Much of the funding of such research, especially in the period up to 1924 (at which time the Laura Spelman Rockefeller Fund began to support social science research at Chicago) came from private or ameliorist sources. Anderson (1983) recalled that the money for his study of Hobohemia (1923) came privately from Dr. W. A. Evans, and Shaw's studies were initially prompted by the interest of the Chicago Women's Club
(Burgess, 1925). This was later incorporated under the auspices of the Chicago Juvenile Protective Agency with which Burgess had close ties and which also supported the work of Cressey. Indeed, Burgess had close ties with a number of external ameliorist agencies.

'Burgess had a lot to do with what was going on in various agencies in the city and he could place students.' (Cottrell, 1972)

'Burgess was the big fixer. He knew everybody. Wirth had a few connections. But I don't think Park bothered much with that.' (Faris, R., 1972)

'Burgess' idea behind the Chicago Area Project was to muster community resources and getting leaders of groups to work together in small councils.... The Chicago Area Project was never central to the Department's activity - it was a function of Burgess's humanitarian interests.' (Blumer, 1972)

Burgess also had contacts with national social work bodies, for example he addressed the National Conference of Social Work meeting at Des Moines in 1926 on the 'The Contribution to Family Case Work by the Sociologist' (Bulletin of the Society for Social Research, June 1927) [4].

Burgess, then, somewhat uncharacteristically among the sociologists, sought out contacts with social agencies in the city. Nonetheless, despite undertaking research linked to such agencies to a greater degree than did his colleagues, he remained a social scientist first, and social reformer second; and insisted that his students adopt a similar distancing from reform. The approach to reformist crusades by the Chicago sociologists is clearly illustrated in the study of taxi-dance hall undertaken by Cressey and supervised by Burgess.
3.5.1 The Taxi-Dance Hall as an Example of 'Big Picture' Reformism.

Cressey's study of the taxi-dance hall phenomenon was carried out during the latter part of the twenties. Taxi-dance halls were establishments where male patrons paid for dances with female dancers, a commission being paid to the owner of the hall. The popular view of the time associated such establishments with vice.

In the Introduction to the 'Taxi Dance Hall' (Cressey, 1932) Burgess notes the reformist zeal which underlay the enquiry into the halls.

'The campaign against the taxi-dance hall has run true to the traditional American pattern of reform, namely, reaction to the external aspects of a situation without any real understanding of the social forces underlying its origin and growth.'

Indeed, in the preface to the same book, J.F. Binford, Executive Director of the Juvenile Protective Association for whom Cressey had worked as a case-worker and special investigator in 1925 on the then unknown phenomenon of the taxi dance hall, hoped that more such work of a community type could be undertaken so that both 'intelligent regulation and control' and the 'substitution of wholesome acceptable social centers' would become possible. She had noted that

'Though our interpretation of the taxi-dance hall may not coincide entirely with Mr. Cressey's, this possibility does not make us less appreciative of the great contribution he has made.' (Binford, 1931)

Indeed, it was never Burgess' intention to encourage Cressey into producing an ameliorative tract, rather, as he indicated in the introduction, the research was more an enquiry into the
phenomenon than framed as a solution to it.

'The present study, undertaken under the assumption that knowledge should precede action, had a threefold purpose. The first object of the enquiry was to give an unbiased and intimate picture of the social world of the typical taxi-dance hall....
The second purpose of the study was to trace the natural history of the taxi-dance hall as an urban institution, to discover those conditions in city life favourable to its rise and development, and to analyse its function in terms of the basic wishes and needs of its male patrons. The third object of the study was to present as impartially as possible the present kinds of control operating to maintain order, to create codes of conduct, and to enforce standards whether on the part of managers, instructresses, patrons, police, social workers, or the press. (Burgess, 1932)

The study was oriented towards a clear statement of what this institution was, how it came to arise in the city and how it operated. There was no assumption of social pathology (although this obviously sparked off the study through the Juvenile Protective Association), and the internal control mechanisms of the halls were considered as well as the external control exercised by policing agencies. Unlike the later studies of deviancy conducted in the 1950s and 1960s, there was no agonising over whose side the researcher should be on, whose point of view is valid. The work, although attempting a detachment uncharacteristic of social reformers, nonetheless maintained the 'moral standards' of middle class America and was unsympathetic towards the uncontrolled and 'unwholesome forms of sexual stimulation' provided by (mainly) 'foreign managers unacquainted with American standards' who 'give the public what it wants' at the risk of promoting promiscuity with its consequent risks of vice and exploitation.

Nonetheless, the wider social implications, the 'Big Picture'
that encompasses the taxi-dance hall, was considered in preparing the book. The taxi-dance hall was not viewed as an isolated pathological institution, rather it was seen as a product of the extensive changes in American leisure activities over half a century such as the growth of professional baseball and university American football, of the emergence of professional boxing championship bouts as national events, of the rising numbers of automobiles 'which now average almost one to a family', of the increase in radio ownership, of the emergence of the speakeasy and the 'blind pig' in place of the neighbourhood saloon, and of the increase in cinemas (to 20000 establishments), beauty contests, dance marathons, and the arrival of dance palaces, roadhouses and night clubs.

It was this perspective which located the taxi-dance hall as a profit making enterprise catering for the 'stimulation of individual emotion' with little or no 'function for social integration'. The taxi-dance halls, like all public dance halls, were designed to exploit promiscuous situations which was where the problem of control of such institutions stemmed from. Burgess regarded as superficial any approach that failed to relate the social life of young people to the wider perspective. The futility of some ameliorative solutions to the problem, the high handed approach of some institutionally based social reformers and the animosities that developed, were highlighted in the book. The early proprietors of these dance halls, most of them 'recent immigrants lacking a more objective point of view'
'came to think of the "public" in terms of "prohibitionists," "priests," "preachers," "puritans," "women reformers" - these being almost the only ones of the larger American public with whom they were brought into vital contact. Since "religious fanaticism" and "hypocrisy" were thought to be securely in the saddle, the only possible adjustment for the proprietor was to protect himself ... from the present-day reformer. (Cressey, 1932, p. 208).

Indeed, as Burgess noted, these very crusades against the taxi-dance hall were an integral part of the problem

'since they are as chronic as the condition which they periodically seek to reform. But, as Mr. Cressey insists, the problem should be worked out experimentally and constructively in the light of the facts and in the interest of all the human values so clearly revealed in his study.' (Burgess, 1932).

In conclusion, and with reference to the social reformist motives of the study's auspices, and following an exposure of misconceptions about the halls, Cressey repeated that the taxi-dance hall developed naturally in the urban environment and should be considered as a problem of the modern city just like

'the problem of crime, of vice, and of family disorganization, we find in the taxi-dance hall the same forces which operate in all city life.... Toward misconduct such as is associated with the taxi-dance hall it would be easy to advocate some form of repression. But a policy involving repression alone would never be wholly successful. It does not get at the heart of the problem, for the problem is as big as the city itself.' (Cressey, 1932, p. 287)

However, despite this liberal scientism, Cressey finished by suggesting that the needs of the taxi-dance hall patron should be met more 'wholesomely', following a thorough investigation, but in the interim they should be condoned conditionally as they 'serve the legitimate interests of men whose needs are not met elsewhere'. (Cressey, 1932, p. 291).

The taxi dance hall study reflects the concerns of many of the sociologists at Chicago in the 1920s. A spirit of scientific
enquiry was dominant, but a sense of social responsibility still lay at the heart of most of the interests of the graduates.

'A lot of my friends all seemed to have been challenged by having rejected a former kind of religio-moral orientation ... and we all still, underneath, we wanted to do some good, we wanted to make the city better, solve the problems of the family, do something about crime and so on... If you could get at these problems by a scientific objective rational approach analysing and working out.. you could avoid seeing the possibility that you might be a communist... We were rebelling [through] this detached objective rational scientific way....' (Cottrell, 1972)

Some of Burgess's students were very much involved in 'action' research (Shaw) and, later, prediction studies, (Cottrell and Wallin) and Burgess himself announced with pride that his earlier work on parole (Burgess & Tibbits, 1928) had been adopted by

'The Illinois Board of Pardons and Parole in 1933 and has been in use ever since.' (Burgess, 1941).

Nonetheless, it is questionable whether even Burgess ever adopted a wholly applied perspective. As time went on, Burgess shifted more and more towards a theoretical sociological perspective and gradually loosened his connections with the social workers

'Social workers weren't on good terms with sociology, they disliked Burgess very strongly. She [Breckenridge] liked my father. He had a knack with getting along with women. I don't know what Burgess had done to offend them. Maybe Shaw did, Shaw was Burgess's protege... I think it was Breckenridge showing a fairly good statistical relationship between broken homes and delinquency. Shaw made a study where he just showed how defective that was. I think that infuriated her.... There was no real conflict between social work and sociology, I think we just ignored each other.' (Faris, R., 1972)

Certainly, by the 1940s, the emphasis in the department had shifted irrevocably away from reformism and any such concerns were expunged from Burgess's proposal to the Rockefeller Foundation in 1941. In it he argued for the necessity of basic research rather than policy research on 'transitory matters'. By
this time, if not earlier, Burgess' primary concern as a sociologist was to extend the frontiers of scientific knowledge (Burgess, 1941).

3.6 The Local Community Research Committee and Reformism at Chicago

Nonetheless, in the period up to the mid-1930s Burgess was, as suggested, amenable to the interests of social agencies. In addition to his contacts in city based social organisations, Burgess, it seems, also had reasonably friendly relations with the social workers in the university. He encouraged Hayner and Guy Johnson, (Hayner 1972; Johnson, G., 1972), for example, to take courses in case work in the School of Social Science Administration in the early 1920s despite a certain distancing of the sociologists and social workers.

'There wasn't much friendly contact between the social work school and the department of sociology. Park was always very nasty in his comments about the social work profession. Burgess, they got along better with Burgess because Burgess did work with social agencies and was sympathetic to their problems and did think that investigation should throw light on what you could do about immediate practical problems in the family and delinquency and that sort of thing... Only Burgess had a real interest in the Chicago Area Project.' (Cottrell, 1972)

These ties were integrally related to Burgess's involvement in the Local Community Research Committee the work of which was, to a large extent, tied up with the concerns of city based social agencies as is evident from the list of those organisations contributing matched funds to the research of the Local Community Research Committee, (see Appendix 2). Burgess, because of his social welfare contacts, alone of the sociologists, was heavily involved in the Local Community Research Committee during its
formative years. An assessment of the work published by social researchers funded by this committee shows a heavy practical, reformist orientation.

Of the forty eight books or contributions to books published under the auspices of the Local Community Research Committee from 1923 to 1929, thirteen were on social welfare, nine on sociology, eight on politics, eight on business studies, three on geography, two on economics, one each on the census and on history. Of the forty one journal articles published under the auspices of the Local Community Research Committee, ten were sociology articles, eight of these published by Burgess and two by Frazier. Thurstone, the psychologist, published thirteen articles and the publishing locations of the total article output clearly indicates that sociology was a peripheral rather than a central concern of the Committee. Even the three articles published in the American Journal of Sociology were by non-sociologists. Of research in progress in 1929, only seventeen (22%) of the seventy eight projects were sociological which was less than the economics (29%) and welfare projects (23%). Of this sociological work in progress, about half was M.A. theses and most of the rest was quantitatively oriented work undertaken or inspired by Ogburn, (see Appendix 2).

The non-quantitative sociological projects carried out in conjunction with the Local Community Research Committee seem to be notably 'social reformist' at least in their initial conception if not their final presentation; Reckless, (Ph.D, 1925) on vice, Cressey (MA 1929) on taxi-dance halls, Hayner
Arguably, the Local Community Research Committee was the motor that generated reformist oriented work at Chicago and may thus be identified as the source of the 'ameliorist' myth. It does appear to have played a substantial role in social science research at Chicago in the rapidly expanding period of the 1920s, and thus more credence could be attributed to a version of the reformist myth which stressed the role of this organization. A closer examination of the impact of the Local Community Research Committee is therefore necessary.

The Local Community Research Committee was constituted in 1923. An interdepartmental initiative involving Small, Merriam (political science) and Marshall (political economy) requested support for a research organisation from the trustees. In February, Small urged an interdepartmental meeting which also included history, social service administration and philosophy to develop co-operative social science research. Stimulated by the 'inspiring, but unhappily brief, leadership of Dr. E.D. Burton, president of the University of Chicago, and by the dynamic personality of A.W. Small, dean of the Graduate School of Arts, many of the fundamental issues of urbanism, of maladjustment, of the growth and interaction of institutions, of personality were realized more sharply than ever before to spread themselves at our feet for inspection and analysis, and even for diagnosis and prescription.' (Smith & White, 1929, p.20)

This stimulation was made into a material possibility when the
Laura Spellman Rockefeller Memorial provided funding (initially $21000, in 1923) for the work.

The executive committee of the Local Community Research Committee through its six year life were H. A. Millis (Economics), C. E. Merriam (Political Science), M. W. Jernegan (History), T. V. Smith (Philosophy) E. Abbott (Social Service Administration), E. W. Burgess (Sociology) and L. D. White (also Politics but who was executive secretary from 1926). The role of the Local Community Research Committee was to have been the planning and administration of a research programme. In the event, the initiative for research proposals usually came from individuals or departments rather than through the initiative of the committee. Thus the committee tended to operate as an approving body for the requests for funds from the Laura Spellman Rockefeller Memorial Foundation. Though it also attempted to monitor research in progress through subcommittees.

For two main reasons, the work of the Local Community Research Committee was oriented towards policy research and the solving of particular problems. First, the matched funding procedure that accounted for about half the total funding of the projects under the Local Community Research Committee (Bulmer, 1980, p. 78), in some cases, placed the initiative for research in the hands of the community, who in effect made requests for services, sometimes to the detriment of less immediately applied research (Smith & White, 1929). Second, funding came from the Laura Spellman Rockefeller Memorial Foundation and, only through the intervention of Ruml as the newly appointed director of the fund,
was such support forthcoming. The Memorial Foundation had been set up to support research into social welfare, notably of women and children. Ruml persuaded the trustees that a shift to social science would be beneficial on the grounds that the development of the social sciences would allow for the solution of social problems. Ruml indicated that the substantive problems to which such funded social science research should be oriented would be related to children, the aged, immigrants, leisure and recreation, poverty, and neighbourhood relationships. (Bulmer, 1980, p. 72). Thus, the sphere of research was, at least initially, somewhat limited.

Bulmer's account of the development of the Local Community Research Committee suggested that the organisation was of major importance for the development of social scientific research at Chicago in the 1920s and beyond, and furthermore that it served as a prototype for social science research organisations in universities throughout the United States [6]. Moreover, in many respects, the guidelines drafted and redrafted by the Social Science Research Council between 1930 and 1950 never shifted from the core intention of the Local Community Research Committee, namely to encourage, administer and monitor co-operative research in the social sciences.

The Local Community Research Committee was, Bulmer has suggested, an important element in the development of sociology at Chicago.

'The programme of research which the Research Committee supported in its first year was primarily concentrated on the research of members of the departments of sociology and political science. In sociology a variety of projects on the local community received support, including detailed maps of urban growth, mapping of local community areas, studies of
the distribution of juvenile delinquency, juvenile gangs, family disorganisation and divorce, and detailed investigations of homeless men, hotel life, rooming-house keepers and the Lower North Side. The pioneering research of Park and Burgess and their students including Frederick Thrasher, Ernest and Ruth Mowrer, Nels Anderson, Norman Hayner and Harvey Zorbaugh rested from 1923 onwards on the support provided by the Memorial' (Bulmer, 1980, p. 75)

But just how important was the Local Community Research Committee? To what extent and in what ways did it affect the development of sociology at Chicago? Did it, through the lure of research monies, lead sociology away from the blossoming pure approach prescribed by Thomas to concerns with social reform?

The Local Community Research Committee had both positive and negative consequences for 'pure research'. It did serve to establish the principle of co-operative social science research. It served as an embryonic organisation for the administration of such research. It encouraged a greater concern with methodology, and particularly provided opportunities to develop quantitative techniques (Bulmer, 1981). It encouraged, if further encouragement was needed, empirical research in the social sciences. More importantly it enabled this research by attracting funds.

On the other hand, the nature of the funds attracted and the problem solving focus provided avenues for ameliorist-reformist research, possibly at the expense of 'objective scientific' research. It, in no way, provided an academic forum. It had occasional discussion meetings to review progress, at which time the only satisfaction voiced was with its administrative role. It seemed not able to fulfil a consultancy, stimulatory, monitoring or, in any sense, an academic role. Even its organisation seemed
To be ad hoc, Cottrell (1972) recalled that the organisation was loose and informal with decisions often being made by telephone. The Local Community Research Committee, at least as far as the sociologists at Chicago were concerned, was quite remote from their research even if it did supply the money to enable the research to be carried out. Cressey made a single reference to the Local Community Research Committee in the preface to 'Taxi Dance Halls', and that to express gratitude to the Local Community Research Committee and the Department of Sociology for making it possible for him to pursue the study through his appointment as a research assistant.

In some respects, then, the Local Community Research Committee acted as an umbrella under which a varied and essentially unconnected series of research studies in the social sciences were located. The list of research so covered and the discussion of it (in, for example, Smith and White, 1929) is suggestive of far more cohesion and organisational ethos than there appears to have been. Shaw, for example, considered himself to be working for the Juvenile Protective Agency, and Cressey saw his auspices similarly although operating as a relatively autonomous researcher. Thrasher, Zorbaugh and Reckless were appointed as research assistants but were concerned with producing a doctoral thesis in sociology, and other than the fact that their funding came via the Local Community Research Committee indicated no acceptance of, or allegiance to, the particular aims of the Local Community Research Committee.

The success of the Local Community Research Committee, in
practice, was the attraction of research commissions and funds from outside, usually civic and 'caring' agencies. The result was a concentration on policy oriented research, rather than the more 'objective scientific' research encouraged and fostered through the Social Science Research Council in the post 1930 period.

It was the coincidence of the desire to develop interdisciplinary or co-operative social research and the availability of Laura Spellman Rockefeller Memorial money that made the Local Community Research Committee an important organisation. The key functions of the Local Community Research Committee were the attraction of research monies, the development of funding application skills, and the presentation of a positive image of the efficacy of the research carried out at the University. This had long term impact on the Rockefeller Foundation which took up support of the social sciences in the 1930s. The Local Community Research Committee did not really generate interdisciplinary research with the possible exception of the development of quantitative techniques. The co-operative work was limited and was effectively a composite of distinct individualistic ventures. Essentially, 'departments were unwilling to subordinate their interests to those of the Research Committee' (Bulmer, 1980, p. 102). (Co-operative ventures only really emerged, in a limited way, after 1930, although the Social Science Research Council was still discussing ways of developing this kind of research a decade later). The Local Community Research Committee, then, served essentially to establish the principle of co-ordination and co-operation in attracting money. Its existence served to attract more funds over a longer period than would probably have been the
case had the departments involved applied for such funding on an individual basis.

The role of the Local Community Research Committee seems to have been primarily, if not exclusively, administrative and directed towards fund raising. As far as the sociologists were concerned, this source of funds did not generate research which either occupied all their research time, nor, where they were involved, did they get absorbed into reformist concerns at the expense of the development of sociological theory.

Park, notably, seems to have remained detached from the Local Community Research Committee. The publication of the 'City' (Park and Burgess, 1925) in the sociological series under the auspices of the Local Community Research Committee did not stem from any direct involvement in the Committee by Park. The subject of the book was clearly within the province of the Local Community Research Committee and the involvement of that organisation seems merely to have been to halve the cost of publication of the book for Park and Burgess. The two men contributed a total of $1200 and this was matched out of the research fund administered by the Local Community Research Committee. In no way, then, can the publication of 'The City' be seen as an endorsement, by Park, of the role of the Local Community Research Committee in sociological research at Chicago. Park's other publication listed as assisted by the committee was far from significant in his output. It was an article contributed to a book on social science research edited by Wilson Gee.

Ogburn became involved with the Committee after 1927, encouraged
by the Committee's sponsorship of the quantitatively oriented
Thurstone and Schultz as Research Professors. It was only after
1929, when the Local Community Research Committee was terminated
and reborn into the the Social Science Research Committee at
Chicago in accord with the growing role of the Social Science
Research Council nationally, that sociologists became more
involved, and Faris, Wirth and Ogburn in addition to Burgess
served on the Social Science Research Committee between 1929 and
1940.

Bulmer (1980, p. 109) has asked whether the dedication of the
Social Science Research Building at Chicago which marked the end
of the Local Community Research Committee represented the
'apogee' of early Chicago social science research? He suggested
that it put the seal of approval upon what Chicago social
scientists were trying to do but that, in the following years,
'the early spirit was more difficult to recreate'. The
implication of this suggestion is that the 'Golden Era' of
Chicago sociology was one dominated by predominantly atheoretical
concerns with the city of Chicago, with solutions to
sociological problems besetting that city, and, in close co-
operation with reforming agencies. White also emphasised the
extent of the service the Local Community Research Committee
offered to civic and social agencies in Chicago. Since 1923, and
indeed, long before via the work of individuals, the Local
Community Research Committee had

'rendered notable service in solving political, economic and
social problems of the city and state'. (Smith & White,
1929, p.33).
The answer to the question, 'did the Local Community Research Committee, through its funding attraction and management role, inhibit sociological research and encourage ameliorist concerns?' must be no. Amongst the sociologists, it was really only the work of Burgess and some of his students that may have been affected, but on balance, as illustrated above, Burgess and most of his students still undertook scientific rather than ameliorist research and they could not be described as ameliorists, or even as reformers, in the first instance.

Despite its importance in generating funds and promoting 'social' as opposed to 'sociological' research, the Local Community Research Committee did not create an institutional environment which fostered a reformist rather than a theoretical climate within which the sociologists had to work. Nor did it foster any great amount of inter-disciplinary study (despite the interest in other social sciences amongst the sociologists) or team research in sociology. The majority of work done in the sociology department was individual, occasionally two members of staff would co-operate on some research, more frequently staff and students would be jointly involved. The work done under the auspices of the Local Community Research Committee was no different. While some of the sociologists took advantage of the funding possibilities when opportunities afforded themselves, the majority of work done under the auspices of the Committee was located in other departments, (see Appendix 2), notably the School of Social Service Administration.
3.7 The Society for Social Research

The Local Community Research Committee was a base for getting funds, but not a forum for ideas or analysis of work. In the Chicago context this was done through the Society for Social Research. If any organisation can be said to be representative of the 'Chicago School' then it is the Society for Social Research, rather than the Local Community Research Committee. Curiously ignored, as Bulmer (1983) documents [7], the society was clearly the major academic clearing house for sociological and other social scientific research at Chicago for thirty years. Bulmer has suggested that it underpinned the research. Perhaps one should go further and argue that it provided the institutional focus for the development of the sociological work done at Chicago. Through the annual Institutes, it kept Chicago in touch with research in the discipline and others could see the developments at Chicago.

The Society for Social Research was a crucial forum for sociological and to some extent social scientific research, and its orientation was firmly 'scientific'; social reform was kept at arms length. On one occasion, for example, the regular meeting of the Society was addressed by Louis Brownlow on municipal administration. He suggested that government leaders would welcome help from social researchers. Faris responded by distancing the sociologist from the administrator, pointing out the dangers in attempting to force social scientific results upon practitioners. Radcliffe-Brown, when addressing the Society in 1932, specifically argued that social science should do away with 'generalizations of the practical man' and thus reflected Ogburn's earlier
stand in which he 'deplored the obsession with practical ends as diverting attention from more fundamental studies and emphasized the need for more exact knowledge in the social sciences as well as withholding researches until they have matured and been made thorough.' (Bulletin of the Society for Social Research, June 1930, p. 3).

The meetings of the Society between 1924 and 1935 do not indicate any real concern with reformism. Of the one hundred and thirty six meetings on which information was available, only 16 (12%) were directed to reformist or welfare concerns. Of these, twelve were given in the period 1924-26 and none of the addresses after 1933 could be construed as concerned with welfare or reform issues. Fifty per cent of all addresses that were presented to the Society which had dealt with reform were presented by external, non-academic speakers, including all those after 1930. Only two sociology students addressed the Society about reform concerns and none of the sociology faculty did. The real concern of the Society was with the development of an 'objective' methodology and the consideration of the nature of scientific sociology, (see Appendix 3).

3.8 The Intrinsic Reformism of 'Chicago Sociology'

Commentators disagree as to the reformist motivations of the 'Chicago School', at least in the 'Golden Era'. For the most part, the Chicagoans of the time (Carey's interviewees, Anderson 1983, Cavan, 1983) see the 'Chicago School' as scientific and non-reformist, as having new ideas while contemporary non-Chicagoans (Lapiere, 1964) [8] and some more recent commentators, (Madge, 1963, Hunter, 1983) see it as moralistic, reformist, non-objective/scientific and old fashioned.
This apparent contradiction probably has two sources. The first is a piling up of secondary accounts that take for granted the reformist concerns of the early Chicagoans. This, as has been illustrated, misrepresents the endeavours of the Chicagoans, since at least 1910. On the other hand, the designation of Chicago as reformist because it lacked objectivity has some credibility, in as much as the conceptualisation of what constitutes scientific sociological research has shifted considerably in the United States. This dynamic will be addressed in chapter five.

A more telling critique which suggests that the Chicagoans were reformist despite themselves is advanced by Kuklick (1980) who maintained that the 'Chicago School' developed a form of 'reform Darwinism'. Irrespective of any ostensive reform concerns, the approach had direct policy effects. For example, following their research into 'natural areas' of Chicago, property valuation became addressed in terms of the race and character of the mortgagee thus encouraging the institutionalisation of racial discrimination. Similarly, the movement of population through housing areas has become a self-fulfilling prophecy with further discriminatory results, including the subsidising of the middle classes. Similarly, Greeley (1977), points to the 'Chicago School's' disorganisation thesis as responsible for augmenting nativists' reactions to immigrants. While the racial inferiority of non-Anglo Saxons was being promoted by, for example, the Dillingham Commission, the disorganisation thesis indicated that such immigrants were also culturally inferior. This provided a rationalisation for the Americanization movements of the early
decades of the century, to which the race relations cycle theory addressed itself.

Philpott (1978) reflected these sentiments too in addressing the way in which vested interests conspired to keep Chicago's slum dwellers in their run down neighbourhoods. Chicago sociologists, he intimated, naively contributed to this conspiracy through the provision of legitimating criteria. Prominent amongst these was the idea that ethnic groups all passed through a ghetto stage before the ethnic groups naturally disintegrated. Ghettos, Philpott argued, only ever really existed for the black population. For the rest the ghetto was a 'state of mind' rather than a geographic concentration. Ethnic identity could be maintained without the burden of becoming ghetto inmates with the economic oppression that involved. The Chicagoans, through their 'middle class oriented studies' of the colourful areas of the city, inadvertently provided grist to the segregation mill.

Carabana (1978) had also developed a similar line of critique in respect of symbolic interactionism arguing that it does not provide for a critical consciousness that can perceive of contradictory expectations. It represents, he claimed, classic individualist contractualism, reducing personality to instincts. It is essentially 'reformist American individualism' providing a rationale for commercialisation.

These criticisms, which have the benefit of hindsight, do suggest that the Chicagoans' apoliticism (Carey, 1975) probably unwittingly served conservative reformist ends. The Chicagoans,
along with the majority of sociologists at the time, imagined that empirically based, disinterested research was fundamental to scientific sociological enquiry. Changes in the nature of the concepts of science and objectivity, the incorporation of reliability and validity into a notion of social science, and recent critiques of the ideology of science, have made this conceptualisation inadequate.

There is little evidence to suggest that the Chicagoans provided the basis for a critical methodology (Thomas, 1983b), despite Bodemann's (1978) suggestion that the early 'Chicago School' provided a major step towards praxis oriented fieldwork, notably in Hughes' research, where intervention is encouraged and the emancipatory role of observational research is emphasised.

3.9 Conclusion

As the nature of the concept of 'objectivity' changed and as a more 'scientistic' approach to sociology emerged, so American sociology became more remote from social reformism, although by no means remote from political considerations, (see chapter seven). This is reflected in changes of attitude towards amelioration and reform. These two concepts took on divergent connotations during the first fifty years of the twentieth century as applied to social science. While the earlier concerns of sociology were highly influenced by Christian ethics and the desire to improve the lot of humanity, this ethical utopianism lost its momentum when confronted by the radical disavowal of religious concerns in Spencer's so-called 'Social Darwinism'.
While ameliorists still probably formed the majority of American sociologists by 1910 (Matthews 1977), there was a gradual shift towards a more 'disinterested' approach which reflected the concerns of laissez-faire capitalism. The movement towards an 'objective' social science increasingly displaced ostensive ethical considerations in the wake of the first world war and the distinction between amelioration (guided by personal ethical values and a sense of Christian mission) and social reform (guided by concerns with social control) became clearer (Furner, 1975; Rucker, 1969; Carey, 1975; Dibble, 1972). Charitable works, the essence of amelioration in the nineteenth century, were seen as incidental to the sociological endeavour, and while amelioration, as practiced by various organisations and analysed by academic departments, attempted to move beyond the administration of charity, there was still a strong sense in which amelioration was seen as 'doing good', as alleviating hardship. It was still integrally linked with voluntary organisations and unable to overcome the taint of amateurism, of partisanship, of interference. The 'revelatory' nature of the presentation of ameliorative enquiry, usually through the assemblage of 'facts' derived from statistical surveys designed to illustrate the extent of deprivation, and the ad hoc approach to social problems detached ameliorative work from either theoretical concerns or an integrated social policy.

Sociology began its detachment from this charitable orientation by becoming more involved in social surveys (Burgess, 1916). Sociology provided a wider theoretical focus for amelioratively motivated enquiry towards social policy, the 'revelatory facts'
were put into a wider context. This reform oriented work differed from the amelioration studies in that it attempted to seek causes in terms of generalisable social phenomena rather than in terms of particular situations. Reform was thus linked to social/sociological theory, and ethical judgements, partisanship and personal advocacy drifted more into the background. Analysis was aimed at a societal level, with a view to suggesting action.

A bi-polar development of social action emerged, at one extreme, social policy research took the form of 'action research', analysing problems, suggesting courses of action and assessing them. At the other, amelioration, guided by moral sensibility, lacked any requirement for social processural analysis. The two poles 'action research' and 'amelioration' were 'extremes' of a continuum. In the period up to 1940 much sociological research in the United States was oriented towards this continuum, although there was always a third, pure research, dimension which became more pronounced as the concern to establish objective theoretical sociology proceeded throughout the century (Ogg, 1928; Blumer, 1954).

Sociology divorced itself from amelioration by the mid century in two ways. First, policy research had become the particular application of sociological enquiry, at the reformist rather than ameliorist end of the continuum. Second, enquiry was directed towards an understanding of social phenomena which tended to divorce sociology from preconceived ethical considerations and thus separated it from any moral or value position underpinning ameliorative concerns. This shift was made easier by the adoption
of the 'disinterested' laissez-faire attitude in the stead of religiously based morals.

This is reflected in the 'Chicago School' which, Farberman (1979) suggested, adopted an objective pure science approach because it came to reflect the 'disinterested' laissez-faire doctrine of the 1920s. The point is, however, that Chicago sociologists revised their research practice, their methodological presuppositions and theoretical orientations to accommodate these changes over time.

Whatever reform orientation, motivation or consideration the Chicago sociologists espoused was mediated by an overriding concern to develop an 'objective science' of sociology. This dated back to Small, was carried through by Thomas and developed by Park and Burgess and their students. Whatever reform intent these latter students may have had was, for the most part, subverted by a broader sociological concern. This is especially noticeable in the potentially partisan studies which Park encouraged his students to undertake. He considered that developing a detached attitude to an area in which the researcher had some familiarity would provide insights that went beyond the mask of surface appearance. The work of Hayner (1923), Frazier (1931), Horak (1920), Wirth (1926), Young (1924) and even Thrasher (1926) (of whom Madge (1963 p. 110) noted 'was committed to a certain point of view in relation to gangs, which he found morally odious and difficult to view objectively.'), are testimonies to the effectiveness of the approach and the adherence to objectivist demands. The Chicagoans were not primarily
proponents of social reform. They were, rather, first and foremost detached enquirers into the social world, reflecting the growing 'scientific' concerns of the profession. They were, in no way, simply reformers dressed in scientist's clothing.
NOTES TO CHAPTER THREE

1. Small was concerned to disassociate sociology from socialism. He had actually been attacked by the newspapers for his socialist views and even went to the lengths of duplicating his lectures and handing them out to students (and then reading them) to cover himself from accusations of teaching left wing radical views. Small argued that there were different types of socialism and himself sympathised with the progressive movement.

2. Report of the Commission on the Graduate Schools of the University of Chicago, 26.10.1925

3. Not all the theses and dissertations supervised by Burgess were examined. Besides those supervised by Burgess in the sample of theses, the work of Burgess' most 'famous' students such as Shaw and Cressey, who did not submit Ph.D.s was also considered.

4. This meeting was also addressed by other members of the Society for Social Research including Stuart A. Queen 'Non-statistical studies of social work', and A.F. Kuhlman 'An Evaluation of Recent Crime Surveys'.

5. It is notable that most of this work was Masters dissertation research and possibly the orientation of the Local Community Research Committee inhibited a theoretical development that would have made it amenable to doctoral research.

6. This honour is claimed by the Society for Social Research. In the minutes of the Society there is reference to a meeting of 14th May 1925 to consider future policies and plans made necessary by the overlap with the 'group on local community research'. The minutes note

'the research society, however, has been and is engaged on some projects that make it essential to the Department of Sociology. Organised about 1920, it has developed and extended the interests in social research among graduate students and others. Out of its activities came the project for local community research that has assumed considerable importance in the last few years. It has interested itself in the problems and research methods of other social sciences than sociology. And persons from these fields are among its members. One of the main activities of the research society has been organization and direction of annual institutes for social research.... The institutes and other activities of the society have been influential in formation of research organisations in Southern California and North Carolina. There also seems to be a field for the society to co-operate with the national research council in a compilation of current research problems and a comparative analysis of different studies. On these studies may hinge the future of appropriations for research in the social sciences. A few years ago the secretary of the research society compiled a classified index to the American Journal of Sociology.' (Minutes of the Society for Social Research,
May 14th, 1925

7. Bulmer's (1983) descriptive account of the Society for Social Research concentrates on the Society in the 1930s and does not take his analysis into the 1950s. He tends to understate the role of the Society for Social Research, despite pointing to its importance, in as much as his account does not emphasise that the Society was, for a quarter of a century, the key forum for importing external ideas to sociology. This was done through the speakers at regular meetings, the institutes and the books from non-Chicagoans which were on the lists of texts available to members at discount prices. The Society was central to the development of a 'balanced' and healthy approach to sociological enquiry through the theory and method debates and exposure of research work to scrutiny.

8. One might suggest that this misrepresentation was deliberately fostered as Lapiere's way of legitimating his own uncritical adoption of statistical studies in sociology in the 1930s, which of course, he went on to critically reconsider. This could well be indicative of how an 'anti-Chicago myth' is developed.
CHAPTER FOUR

CHICAGOANS AS ETHNOGRAPHERS
4.1. The Myth

One of the most common views of Chicago sociology is to see it as embodying a qualitative approach to sociology and thus at variance with a 'quantitative tradition' embodied in the approach adopted at Columbia. (Berger and Berger, 1976, p.48; Glaser and Strauss, 1967, p.vii).

Chicago is portrayed as rooted in 'qualitative sociology' (Kuklick, 1980, p.207; Wiley, 1979, p. 56; Tiryakian, 1979a, p.227; Deutscher, 1973, p. 325). Some commentators suggest that map drawing, life history collection and even walking (Bell, 1977, p.52) were the major preoccupations of the Chicago sociologists (at least until 1930); while others imply that the Chicagoans were principally, if not exclusively, participant observers (Bogdan and Taylor, 1975; Rock, 1979; Meltzer, et al, 1975; and Madge, 1963, p. 89).

This methodological aspect of the myth of the 'Chicago School' is the most enduring and specific. It is popularly held that Park instituted a programme of research which led to the adoption of participant observation and that this approach was the main one employed by the Chicagoans. Pusic (1973) wrote of a 'new sociological approach' known as the 'Chicago School' which presupposes the greater role of the individual and reliance on direct researcher-subject contacts.

The idea of Chicago as the bulwark of ethnography is usually supported through reference to the work of three people at Chicago, namely Thomas, Park and Blumer. Their position vis a vis
ethnography and quantitative sociology will be examined below, along with that of other significant figures in the history of Chicago. The approaches adopted at Chicago will be put into the context of American sociology as a whole in order to assess the extent to which Chicago sociologists offered something methodologically unique.

4.2 Methodological Concerns Of The 'Chicago School'

The Chicagoans did not use participant observation to the exclusion of other methods. To suppose that they did is misleading on two fronts. First, the Chicagoans, when undertaking 'ethnographic' work, tended to use a 'case study' approach, including the collection of life histories, rather than relying solely upon direct participation. Secondly, the Chicagoans were not simply 'qualitative sociologists', they were heavily involved in developing quantitative techniques.

4.2.1. The nature of ethnography

Part of the problem in assessing the methodological tendencies of the Chicago sociologists lies in the confusion over the use of terms. Ethnography has emerged as a term preferred to 'qualitative approach' but is no clearer in its delimitation of methodic practice or methodological tendency. Hammersley and Atkinson (1983, p.1) pointed to the diversity of usage of the term 'ethnography'.

'There is disagreement as to whether ethnography's distinctive feature is the elicitation of cultural knowledge (Spradley, 1980), the detailed investigation of patterns of social interaction (Gumperz, 1981), or holistic analysis of societies (Lutz, 1981). Sometimes ethnography is portrayed
as essentially descriptive, or perhaps as a form of story-telling (Walker, 1981); occasionally, by contrast, great emphasis is laid on the development and testing of theory (Glaser and Strauss, 1967, Denzin, 1978).

The tendency in most approaches to ethnography is to view it as somehow opposed to the 'positivist' approach to sociology embodied in the 'quantitative tradition'. Ethnography is seen as aligned with 'naturalism' and concerned with 'meanings' rather than 'causes'. It is viewed as aiming essentially at an understanding of the processes of interaction and the way people construe their world in interactive settings. In this sense, ethnography is usually contrasted with the attempt at causal abstraction associated with quantitative research practice.

There are probably as many definitions of participant observation as a method as there are participant observers, and any general definition is bound to be disputable. The terms 'ethnography' and 'participant observation' are often used interchangeably. Both imply certain methodic practices and a methodological attitude.

The distinction between ethnography and participant observation has become blurred as Hammersley and Atkinson (1983, p.2) illustrate when defining an ethnographer as one who

'participates, overtly or covertly, in people's daily lives for an extended period of time, watching what happens, listening to what is said, asking questions; in fact collecting whatever data are available to throw light on the issues with which he or she is concerned'

Involvement, observation and an insatiable eclecticism, all elements of participant observation, seem to be covered by this definition. The reemergence of the term ethnography draws methodic practice, such as non-participant observation, in-depth interviewing, into a common sphere with participant observation.
The core, then, of ethnography is 'getting out among the subjects of enquiry' in such a way that their perspective is engaged. Participant observation is thus the exemplary method.

However, the term participant observation has changed over time, and has not always been associated with the current wide notion of research practice associated with ethnography. Thus retrospective reconstructions sometimes attach current meanings to past research practice and in so doing are misled by them.

Lindeman (1924) first published an account of participant observation as part of his critique of current methods of investigation, notably the absurdity of surveyors in regarding their scheduled questions as free from bias. He argued for more emphasis on observation. But for Lindeman, observation was a form of asking questions and involved two things. First, the 'objective observation' of all external phenomena connected with behaviour and, second, 'participant observation from the inside'. For him, no one could do both and so joint investigations were imperative. Lindeman's approach was essentially behaviourist and concerned with the 'objective' observation of behaviour from two perspectives. The participant nature of the participant observation Lindeman recommended required no engagement with the subject's perspective.

Subsequently, in the 1940s, participant observation tended to mean the adoption of a role that would enable one to participate, to varying degrees, in the life of the subjects, in order to get first hand information (Daniel, 1940) usually as an adjunct to other research methods, notably the use of documentary material.
from other sources, such as case records. Only during the 1950s did participant observation emerge as a potentially exhaustive method in its own right (Becker and Geer, 1957), and with it the varied and extensive nature of the enterprise. Along with this came the ostensive engagement with the issue of the subject's perspective, expounded forcefully in Becker's (1967) question 'Whose Side Are We On?'.

The emergence of participant observation in its own right was related to the critique of 'positivism' and the emergence of 'naturalism'.

'Participant Observation research still produces much ethnographic description but its keynote has shifted to a more 'phenomenological' register, in which the texture of symbolic exchanges is highlighted in order to display the practical commitment of individuals to making their own sense out of their social encounters.' (Butters, 1973, p.2)

Thus participant observation involved a 'style' of sociological research

'characteristically used for seeking analytic descriptions of complex social organisations. This style emphasises direct observation, informant interviewing, document analysis, respondent interviewing, and direct participation, and is made possible in large part by repeated, largely social interaction with members of the organisation under study. The use of these techniques is organised by unusual research design in which hypothesis generation, data gathering and hypothesis testing are carried on simultaneously at every step of the research process.' (Butters, 1973, p.1)

In this sense, participant observation research was not a conspicuous feature of American sociological research until the second half of this century. The excursions into this form of research were fairly rare and virtually non-existent prior to 1940. An examination of some early examples of studies generally assumed to be based on participant observation research will
illustrate the gulf between the approach adopted and the 'naturalistic' concerns now apparently central to participant observation. First, however, the notion of case study, a far more familiar term and technique for Chicagoans, will be considered.

4.3 Case Study

Rather than participant observation, early 'ethnographic' work was oriented towards 'case studies' in which the collection of 'life histories' were regarded as important. The core of case study research was the extraction of contextualised 'attitudes' [1]. The formation of attitudes both in terms of the impact of personal experience and social milieu, were seen as crucial for an interactionist sociology. [2]

Certainly for Thomas, at least initially, the optimum approach was to concentrate on life histories. These would reveal the processes by which individual attitudes were mediated by social values, and vice versa.

'We are safe in saying that personal life-records, as complete as possible, constitute the perfect type of sociological material, and that if social science has to use other materials at all it is only because of the practical difficulty of obtaining at the moment a sufficient number of such records to cover the totality of sociological problems, and of the enormous amount of work demanded for an adequate analysis of all the personal materials necessary to characterize the life of a social group. If we are forced to use mass-phenomena as material, or any kind of happenings taken without regard to the life histories of the individuals who participate in them, it is a defect not an advantage of our present sociological method.' (Thomas & Znaniecki (1918) Vol 3, p. 1)
This statement has to be put into context. First, it is the only time that such a forceful advocacy of life history is set out in the Polish Peasant, and Thomas did not repeat it in his work throughout the twenties, indeed his position was far less dogmatic (Thomas, 1928). Second, it prefaces the one complete life history included in the first edition of the Polish Peasant and is clearly designed to legitimate the inclusion (note that the second edition saw the life history shifted from its central position and located at the end of the last volume as a kind of appendix). In practice, Thomas and the other Chicagoans rarely used complete life histories.

There was an appreciation at Chicago of the problematic nature of life history records, which was to lead to the development of attitude scales. (Thomas and Znaniecki, 1918; Stouffer, 1930; Burgess, 1944). Life history collection was cumbersome, requiring vast resources and patient and very co-operative subjects. Second, it was retrospective and therefore suspect because of unconscious distortions of reconstruction. (Although, for the same reason possibly revealing of social phenomena, as the psychoanalytic case study is revealing of 'suppressed causes' of 'disturbance'). Third, it was difficult to use for generalisation.

Consequently, there was a tendency to approximate the life history approach, and this is evident from the Polish Peasant onwards. The collection of surrogate life histories in various forms became a hallmark of sociology in the United States for more than a decade, despite a retention of the idea that life
experiences as expressed by the subject provide the essential base for eliciting subjective meaning. (Burgess, 1944). The approximations took the form of case studies of one sort or another as there was no definitive view of the course methodic practice should take in order to gather life experiences.

In their research on the problems of adjustment facing Polish immigrants to the United States, Thomas and Znaniecki had relied heavily on correspondence between Poles in the U.S. and in Poland. This data constituted a 'slice' of life history pertinent to the research area. The surrogate life histories provided by the letters revealed the personal attitudes and social values to which they responded.

Later, in his work on female delinquents, Thomas (1924) used court and social work records. These case records provided a ready source of material for the elaboration of the theory of social disorganisation and accompanying thesis of 'wishes' in the particular area of delinquency. Such case records were abbreviated life histories which bore upon the issue at hand. (They were, of course, uncritical sources but the aim of qualitative research has not been with structural critique).

4.4 The Nomothetic Orientation of Chicago 'Ethnography'

The interactionist sociology undertaken by the Chicagoans, although 'qualitative', reflected nomothetic concerns. The principles of an interactionist sociology which underpinned Chicago sociology, as Park (1939) has suggested, derived from Thomas and are set out in the Methodological Note (Thomas and
Thomas' perspective involved three central features. First, that sociology take account of subjective aspects of human interaction as well as objective ones, incorporating attitudes as well as values. Thomas thus explicitly 'codified' the prevailing sentiment among sociological researchers, in as much as the 'moralistic' concerns of reformers were objectified and locked into a humanistic empirical enquiry. Second, that social control, the principal aim of sociological enquiry, could only be approached through the discovery of social laws and that subjective perceptions must be incorporated into these laws. Third, social laws must relate to the social rather than personal milieu.

In the Polish Peasant's Methodological Note, Thomas and Znaniecki clearly stated that

'The chief problems of modern science are problems of causal explanation. The determination and systematisation of data is only the first step in a scientific investigation. If a science wishes to lay the foundation of a technique, it must attempt to understand and to control the process of becoming. Social theory cannot avoid this task, and there is only one way of fulfilling it. Social becoming, like natural becoming must be analysed into a plurality of facts, each of which represents a succession of cause and effect. The idea of social theory is the analysis of the totality of social becoming into such causal processes and systematisation permitting us to understand the connections between these processes.' (Thomas and Znaniecki, 1918, p. 36).

This central concern, however, was, as suggested above, mediated by the need to account for the subjective nature of social interaction. So, while the physical sciences provided the model for scientific enquiry into the social world, their example, Thomas argued, should not be adopted uncritically. In what amounted to an attack on those who would adopt an objectivist
'Scientific Method' which aims to find 'the one determined phenomenon which is the necessary and sufficient condition of another phenomenon', Thomas pointed to the fundamental difference between physical and social science which is that,

'while the effect of a physical phenomenon depends exclusively on the objective nature of this phenomenon and can be calculated on the ground of the latter's empirical content, the effect of a social phenomenon depends in addition on the subjective standpoint taken by the individual or the group toward this phenomenon and can be calculated only if we know, not only the objective content of the assumed cause, but also the meaning which it has for the given conscious beings ... A social cause is a compound and must include both an objective and a subjective element, a value and an attitude.' (Thomas and Znaniecki, 1918, p. 38).

Attitudes involve a process of individual consciousness which 'determines real or possible activity of the individual in the social world'. Values are data 'having an empirical content accessible to members of some social group and a meaning with regard to which it is or may be an object of activity'. Social values are different from objects in as much as the latter have no meaning for human activity. The incorporation of meaning into the causal process was fundamental for Thomas and those who followed him at Chicago. The analysis of social activity in terms of values and attitudes implied, for Thomas, an holistic approach. Prefacing a position which C. Wright Mills (1959) was to restate and expand, Thomas argued that, in studying society, 'we go from the whole social context to the problem, and in studying the problem we go from the problem to the whole social context' (Thomas and Znaniecki 1918, p.19). And, in such a procedure, Thomas claimed, one should proceed as if one knew nothing of the area, for the most usual illusion of science is that the scientist simply takes the facts as they are, without
any methodological presuppositions and 'gets his explanation entirely a posteriori from pure experience' (Thomas and Znaniecki, 1918, p. 37). On the contrary, Thomas asserted that a fact is already an abstraction and what one must attempt is to develop this abstraction methodically rather than presume that the uncritical abstractions of common-sense are adequate. This systematic process of abstraction must be done because 'the whole theoretical concreteness cannot be introduced into science'.

Central to this endeavour, then, is the need to ensure that 'our facts must be determined in such a way as to permit of their subordination to general laws' for a fact that cannot be treated as a manifestation of a law (or several laws) cannot be explained by causal processes. Following upon this proposition, Thomas, predating Popper, further asserted a 'falsificationist' principle. In noting the problem of generalising laws that are initially manifest in particular spheres, Thomas suggested that the social scientist assess the core concepts of the proposition embodied by the particular law and, should such concepts relate to other circumstances, present the law in general terms. The social scientist is therefore essentially in a position to make bold conjectures, but such conjectures must be refutable: and further, because of the ethical and moral consequences of the application of generalisable social laws by social practitioners it is necessary that

'besides using only such generalisations as can be contradicted by new experience [the scientist] must not wait until new experiences impose themselves on him by accident, but must search for them, must instigate a systematic method of observation. And, while it is only natural that a
scientist in order to form an hypothesis and to give it some amount of probability has to search first of all for such experiences as may corroborate it, his hypothesis cannot be considered fully tested until he has made subsequently a systematic search for such experiences as may contradict it, and proved these contradictions to be only seeming, explicable by the interference of definite factors.' (Thomas and Znaniecki, 1918, p65).

Early interactionism, via the work of Thomas, involved a nomothetic view of sociology based on empiricism, but one mediated by a concern that mental capacities be incorporated. Individuals through reflection can transcend social values and indeed transform attitudes. Causal relations need to take account of this.

In order to understand social phenomena, Thomas argued that one needs to be able to explore the structural determination of action and its social psychological aspects. This may best be done by concentrating on the individual case and relating the biography to its social constraints as manifest in social values. This reflects the much later view by Mills (1959), (although without developing any critique of social structure or seriously questioning the adequacy of nomological perspectives in science).

Thomas's methodological presuppositions were not, then, a refutation of nomological principles per se, but rather an attempt to develop them. Nonetheless, Thomas and the later Chicagoans are often portrayed as being overly concerned with the subjective at the expense of the objective aspects of the social world.

Other interactionists reflected Thomas' concerns, notably Park and his students. Like Thomas, they, too, adopted a nomothetic approach. Park was not a 'verstehen' or phenomenological sociologist, although his period of study under Simmel had informed his
approach. Park never developed an epistemology which detached explanation from understanding, and while sceptical of the possibility of quantifying social phenomena and their interrelationships and thus of elaborating causal relationships, he never forsook the nomological premise of social science.

Park's approach was the elaboration of observable phenomena within a Big Picture, relying heavily on an underlying social disorganisation thesis. Contextualisation, with an emphasis on history, was central to this endeavour, with life history, recorded interview or case study in one form or another being relevant to this contextualising process.

The distinction between the case study approach and participant observation is examined in more detail below with reference to particular work at Chicago.

4.5 Participant Observation at Chicago

There is a widespread assumption that 'Chicago sociology' was not only predominantly ethnographic and hostile to quantification, but that its predominant methodic practice was participant observation. For example, Cavan (1983) suggested that besides case study and statistics there was another method which was the result of Park's interest and influence and that was 'observation, participant and otherwise'. This method, Cavan claimed, was neither formalised nor named at the time but was the basis for a number of studies including Anderson (1923), Thrasher (1927), Zorbaugh (1929), Cressey (1932), Young, P., (1932), Reckless (1933). However, it was left to Becker, she suggested,
to give participant observation more formal shape (in the 1950s). Hunter (1983), too, points to Zorbaugh's work, (as had Madge, 1963) as indicative of the participant observation approach of the 'Chicago School'.

Platt (1982), however, argued that, on the basis of the examination of textbooks, methodological writings and some 'exemplary substantive works', the view of the 'Chicago School' as participant observers is misleading. The early use of participant observation was not always conscious, was more related to case study and does not fit current conceptualisations of the method.

Nonetheless, there are some grounds for arguing that participant observation in the loosest sense is evident in the work of the Chicagoans from the beginnings of their empirical endeavours. Fish (1981) showed that MacLean (1910) engaged in a series of short-term participant observer experiences as an employee in various industries in her survey of women in the labour force. Fish argued that this approach was representative of research carried out in Chicago in its very early days.

To some extent this coincides with Park's (1939, p. 3) reconstruction of the early period, in as much as he suggested that around 1910 there was a boom in graduate students going into the social sciences but that apart from 'applied social science' courses with their atheoretical orientations, there was

'no special provision for students who wanted to study a living society and no opportunity ... to study social problems in the field, or so to speak, "on the hoof". Chicago University provided that possibility, following Thomas' innovative development of the tradition he
'inherited' from Small, Henderson and Abbott.' (Park, 1939, p.3)

However this did not mean that the second decade of the century saw a blossoming of a relatively well established participant observation study tradition. The study of 'social problems on the hoof' is indicative of the growing concern to establish an empirically based social science. At its most radical this implied getting out and seeing what is going on. But such observation rarely involved any degree of active participation.

Of the forty two theses surveyed in detail, only two (5%) utilised complete participant observation (both after 1940), six (14%) used some kind of partial participant observation [3] while another seven (17%) employed casual observation, relied on past personal involvement or used the report backs of other observers. Nearly two thirds (64%) made no use of observation as a technique at all (see Appendix 6).

Even the 'Golden Era' of the 1920s was not a period in which participant observation blossomed. Cavan's (1983) suggestion, for example, that Thrasher's (1926) 'Gang' was a participant observation study is dubious. On the face of it, a thesis entitled 'The Gang' would seem perfect for such a method, but the subtitle 'A Study of 1,313 Gangs In Chicago' belies this assumption. Nobody could observe so many gangs as a participant. Indeed, Thrasher obtained most of his information through interviews. He interviewed about one hundred and thirty people consisting of sixty one gang members, a large number of social workers who made available case material, a dozen policemen, half a dozen politicians and a number of others such as lawyers and club owners. The
material gathered in this way was used qualitatively rather than quantitatively and was augmented by the written life histories of twenty one of the gang boys, newspaper reports and a mere ten of Thrasher's own observations (Madge, 1963).

Similarly, Reckless' study of vice, also cited by Cavan as a participant observation study, lacked participation to any degree. His dissertation, presented in 1925, had to rely primarily on official statistical sources. In the preparation of his book, published in 1933, he was able to augment this original study with case material made available from social workers. The study was directed to providing a picture of the location and degree of concentration of prostitution in Chicago and to assessing whether there was any correlation between vice and demographic features of Chicago as revealed by census material.

The difference between the observational approach adopted by the Chicagoans in the 1920s and that of more recent ethnographers is further illustrated by examining, in depth, three works supposedly indicative of Chicago participant observation studies.

The core features of participant observation, as discussed above, are direct observation through a participating role which enables the organisational and symbolic processes of a group under study to be scrutinised in order to assess the meanings in use that define the subjects perspective of their social milieu.
4.5.1 The Hobo as a Participant Observation Study

'The Hobo', by Nels Anderson (1923) is often cited as an early example of Chicago ethnographic work. Usually it is seen as representing the beginning of the published participant observation studies, and being the forerunner of the kind of work undertaken by Becker and others in the 1950s and 1960s.

However, there are considerable differences between the later participant observation studies and Anderson's work, to the extent that in terms of the current meaning of the term, 'The Hobo' was not a participant observation study at all.

Anderson did not live as a hobo but rather stayed in a hobo hotel in hobohemia, despite accepted wisdom,

I shared a room when I first got to Chicago with Nels Anderson. He was the hobo. He had hoboed all over the country. And he hoboed from Utah to Chicago. And he spent the previous night, before I first met him, sleeping under a concrete ledge around the smoke stack of the university power plant... He said he was used to that kind of thing because he was always sleeping outdoors or under a railway bridge with a bunch of hobos, warming up food in a tin can, and all that. Anderson did his dissertation on the hobo, he wanted to supplement his information with some surveys of hobos down in what you could call hobohemia, down at South Halstead Street. So several times on weekends he would go down there and I would go with him. We got a room in one of those cheap rooming places, almost like a flop house. We'd spend a couple of days just going around talking to people. And he established rapport very quickly with them because he was a very folksy kind of man. Maybe, Friday or Saturday night we would visit, so-called, 'Hobo College'. On one occasion he was asked to say something and he got up and gave a very nice little talk.' (Johnson, 1972)

He had 'jumped a freight train' from Utah in order to get to Chicago and had encountered hobos. He may also have adopted this form of travel on other occasions but in no way could be said to have taken on the role of a hobo for research purposes.
(Anderson, 1983). Before starting his study he had worked in a 'Home for Incurables' in Chicago which had put him in contact with hobos and it was through this contact that he developed his research. His approach was not so much participant observation as non-participant observation mixed with informal in-depth conversations. Like many of Park's later students, Anderson researched an area to which he already had access.

Neither Anderson, nor any commentators at the time referred to his work as involving participant observation. Mention of his work in the Bulletin of the Society for Social research made no mention of participant observation. Nor did his style of work reflect the concerns of participant observation practitioners of more recent decades except that, somewhat against the tenor of the times, he reflected more recent concerns in that, he was sympathetic to his subject. This made the 'insider' report he wrote, based primarily on his discussions with hobos encountered at work and in the hotel, appear to be a dispassionate, 'scientific' document. This appearance was amplified by the study being clearly at variance with what the University, at the time, saw as appropriate fields of study for its graduate students. Anderson (1983) suggested that his lack of moral stance appealed to Park and Burgess who made the decision to publish the work. They saw his study as scientific despite Anderson's lack of any substantial sociological background or use of sociological concepts. Indeed, Anderson suggested that the book was scientific without him having to work on it, simply because his background and approach was unlike that of the predominant 'clergy' at Chicago [4]. Its candid abandonment of conventional ameliorative
Wisdom seems to have been its main appeal for Park and Burgess. The book simply stated what hobos did, what types of hobos there were, how they lived in both cities and rural areas, how they were seen by other non-hobo sectors of the community, and concluded with an assessment of why hobos were disappearing which concentrated on the lack of demand for migrant labour. In short 'The Hobo' was a detailed descriptive account which analysed the usefulness of the hobo and their increasingly rapid disappearance. It lacked sociological 'pretentiousness' and was primarily a report of the state of affairs which challenged some of the taken for granted notions about hobos.

'The Hobo' study was not actually financed by the University because it was not a respectable area of research, and the funding came to Anderson from a private source. Indeed, Anderson's decision to research the hobo was not one developed through his university affiliation directly. It arose as the consequence of discussions with Dr. Reitman who was interested in the subject of the hobo and raised the money for a study from his friend Dr. William A. Evans and placed the money with the United Charities of Chicago. The director of the United Charities was Joel Hunter who became the treasurer of the hobo study committee to whom Anderson was responsible. The other two members of the committee were Reitman, 'the 'authority on the area and its inhabitants' and E. W. Burgess who, as 'scientific advisor' and chairman provided the link with the Sociology department at the University. Notably (and rarely, see also Blumethal, 1932) Anderson's study was published as a book by the Chicago University Press.
before he was awarded the M.A. for the work.

Working in the home had been an important factor in Anderson attracting the research funds, but this had nothing directly to do with Chicago University. As the study was based on his work in the home and the contacts this generated rather than as the result of any direct participation as a travelling and working hobo, there is very little grounds for according 'The Hobo' the status of an early representative of a Chicago School participant observation studies tradition.

4.5.2 The Taxi-Dance Hall as a Participant Observation Study

The Taxi-Dance Hall study (Cressey, 1929) is sometimes seen as particularly indicative of the participant observation approach of the Chicagoans as it involved a decision to engage in unobtrusive participation. The sensitive nature of the enquiry required subterfuge and the actual participation in the deviant situation is seen as indicative of 'Chicago type studies'.

As in other cases the term participant observation is nowhere used nor did the Bulletin of the Society for Social research refer to it as a participant observation study. Moreover, the extent to which the research represented even an embryonic participant observation study, in the sense explored in section 4.2.1 above, is debatable.

'Most of the data upon which this study is based was secured from the case records of social agencies, notably the Juvenile Protective Association, and from the reports of observers and investigators. Published material upon such a new phenomenon as the taxi-dance hall was found to be scanty and of little value; and formal interviews were abandoned as unsatisfactory.' (Cressey, 1932, preface)
Cressey points out that co-operation was not forthcoming and so the decision was made to carry out the study without the co-operation of proprietors and in spite of the deliberate opposition of some of them. This lack of co-operation made it logistically impossible to secure what otherwise would have been desirable statistical data. The research was forced to adopt other approaches. Nonetheless, Cressey is concerned to assert that the 'considerable amount of case material which has been amassed' over five years (of which only a small amount is included in the text) 'afford a reasonable basis for the validity of the generalizations made'.

The method finally adopted was outlined by Cressey as follows:

'Observers were sent into the taxi-dance halls. They were instructed to mingle with the others and to become as much a part of this social world as ethically possible. They were asked to observe and keep as accurate a record as possible of the behavior and conversations of those met in the establishments. Each observer was selected because of his past experience, his training and his special abilities. These investigators made it possible to gather significant case material from a much more varied group of patrons and taxi-dancers than could have been secured by any one person. The investigators functioned as anonymous strangers and casual acquaintances. They were thus able to obtain this material without encountering the inhibitions and resistance usually met in formal interviews. Further, the independent reports from different observers upon their contacts with the same individual made possible a check upon the consistency of the documents obtained. Moreover, this information concerning patrons and taxi-dancers made it feasible to secure much ancillary data from the records of social agencies.' (Cressey, 1932, preface).

This research clearly does involve the collection of ethnographic material through secret participant observation type approaches and, in the same mould as Anderson's study of 'The Hobo', the point of view of the participants appears to be taken into account. Indeed, as Bulmer (1983a) shows, Cressey discussed the
observational element of his research, in an unpublished paper, in terms of the role of the stranger in interactive situations.

However, this methodic orientation does not transform the research into a participant observation study in the current sense because, as Madge (1963, p. 119) has noted, Cressey's observers were more in accord with Lindeman's 'objective observers' than participant observers. Despite appearances, the study differs from later participant observation studies on four important counts. First, the participant observation material was support data for case records. Second, it was regarded as somewhat suspect as research material and needed a great deal of cross-verification. Third, ironically, a preconceived moral position which regarded the halls as 'unwholesome' (if temporarily unavoidable) underpinned the research. Fourth, no attempt was made to engage the perspective of the female taxi-dancers. They were talked to, but only in order to provide classificatory schemes, to assess why they adopted the profession and so on, but never in terms of adopting the dancers' points of view.

4.5.3 The Gold Coast and the Slum as a Participant Observation Study.

A similar conclusion emerges from the study of Zorbaugh (1929) 'The Gold Coast and the Slum', the third 'exemplar' of early 'Chicago School' participant observation studies, (Madge, 1963; Hunter, 1983). This study was of the extremely diverse area of Chicago known as the Near North Side, which lay just north of the central business district (the Loop). The area, one and a half
miles long by one mile wide, was on the shore of Lake Michigan and its economic prosperity and consequent social standing declined rapidly as one moved inland from the shore. Lake Shore Drive, known as the Gold Coast, was a highly desirable residential area. Backing on to this was an area around Clark Street that had become a rooming house district and beyond that was 'Little Italy', a slum area that had gone through various transitions but was, at the time of Zorbaugh's study, primarily a Sicilian enclave. Incongruously, the 'bohemian' area, known as Towertown, lay in the middle of the slum area. This was, according to Zorbaugh, a rather second rate community of artists. The Near North Side was thus an area of extremes.

The study was primarily concerned to provide a detailed description of the complexities of the Near North Side, and to that end was broken down into an examination of each of the four sections separately. A considerable amount of the empirical evidence consisted of demographic data and ecological analysis. The types of shops on North Clark Street, for example, were used as indicative of the area. The high concentration of cheap lunch rooms and restaurants was related to the rooming house district where residents had little or no opportunity to prepare meals for themselves. [5]

The latter part of the study included an historical case study of the Lower North Community Council, which ultimately failed, and which Zorbaugh used to illustrate his conclusions about the inadequacies in local community institutions throughout the area.
Although Zorbaugh made himself familiar with the area under investigation, he did not undertake the kind of participant observation of a community that was to be attempted later by Blumenthal, Warner and the Lynds. The source data for his study came principally from documents provided by residents, from life histories collected, presumably, by the author, and from case histories, particularly those in the files of the United Charities. For example, when discussing the 'Gold Coast', Zorbaugh relied heavily on fourteen anonymous written contributions. The life histories included one provided by a pawnbroker and another by a 'charity girl'. However, it is not clear whether they were written by the contributors or compiled by the researcher on the basis of extensive interviewing.

Further evidence came from a large number of essays written by school children, from a school census probably conducted on behalf of the board of education, from comments to census workers, from key informants, from personal documents such as letters, from existing records, such as the 'Illinois Lodging House Register' and the records of the Juvenile Protective Association. A survey of the rooming houses was also undertaken, but the information gathered from this appears to be little used, and there is no clear indication of the kinds of questions asked, the sampling procedure or the number of respondents involved. Very little of the evidence presented in the study seems to have been direct observation by the author.

While a descriptive ethnographic account, it was preoccupied with an ecological analysis rather than the perspective of the
subjects. Throughout, there is a taken-for-granted view that the area was 'disorganized' and that it represented the antithesis of what a community ought to be. However, Zorbaugh was at pains to point out that he was not comparing the area with an idealized community; rather, taking on Park's approach, he argued that it was necessary to accept the cultural traditions of the community as they are and attempt to understand them. To that end it was necessary to discover the nature of the community, how it operated and the impact of industrialisation upon it. Nonetheless, Zorbaugh was far more concerned with a generalised descriptive account than any insider attempt to unravel the perceptions of the subject groups. There is nothing in the methodology of the study to suggest that Zorbaugh was a part of the area. Thus, in current terms, it does not constitute a participant observation study.

4.6 Participant Observation and Community Studies

Participant observation was not a term widely used, nor a method greatly indulged in either at Chicago or within American sociology in general, prior to the 1950s, outside the field of community studies (discussed below). In confronting the labelling of early 'Chicago School' studies as participant observation, this thesis is doing more than questioning the appropriateness of an elusive label. What is fundamentally at issue is whether the Chicagoans adopted the style of 'qualitative' methodology that is conventionally attributed to them, and out of which a retrospective heritage of participant observation studies has been constructed.
The nearest the 'Chicago School' got to establishing an ethno-
graphic tradition of participant observation studies was through
its endorsement of community studies, with Blumenthal pioneering
in his 'Small Town Stuff'. The first reference to a participant
observer study in the Bulletin of the Society for Social Research
was to Blumenthal's work. In the June edition of the Bulletin for
1931 Blumenthal was listed as a new member with, as research
topic, 'A Participant-Observer Study of a Small Town'. In the
next edition, in the list of books available through the Society,
was the following entry:

'Albert Blumenthal: Small Town Stuff
The result of two and a half years of systematic
investigation, this is the first work to apply the
participant-observer method of the anthropologist to the
study of the small community in our civilization.' (Bulletin
of the Society for Social Research, Jan. 1932, p. 3)

Besides being only the second reference to participant obser-
vation in the Bulletin this announcement also pointed to the fact
that Blumenthal's research was as much influenced by anthropology
as the ethnographic work of Park's students. The sociology
department was combined with anthropology until 1929 and after
that Radcliffe-Brown taught in the Anthropology department for
six years and was followed by Lloyd Warner (in 1935) who was
Professor of Anthropology and Sociology. Both Radcliffe-Brown and
Warner taught 'a lot of sociologists to be interested in doing
field work' (Becker, 1979a) and Polsky (1980, p. 278) recalled
that rubbing shoulders with the anthropologists was an important
element in the development of field research. In the 1930s there
was a growth of community study type field work, notably the
anthropological work of Redfield in Mexico and the studies of the
South carried out by W. L. Warner and his associates; which was
paid for 'by the W.P.A. and other of those kinds of Government funds for relief' (Becker, 1979a). This research led on to the work of Dollard and the Lynds outside Chicago. This was not, however, in any sense a dominant tradition within the Department of Sociology and Anthropology.

'Middletown didn't immensely impress Chicago people, Burgess spoke favourably of it, but it was so much description and so little generalisation, there wasn't much sociology you could hang on to. It was too specifically descriptive of a little town in Indiana.' (Faris, R., 1972)

These community studies unlike the early Chicago studies (of the 1920s) involved researchers leaving Chicago and living in the community chosen for analysis. While reflecting some of the earlier concerns of the Chicago studies of particular communities, such as Wirth's 'Ghetto' and Horak's study of the Czech community, the work done by Blumenthal was different in many ways.

The approach adopted by Blumenthal had been suggested in embryonic form by Zorbaugh in his study of the 'Gold Coast and the Slum', but Blumenthal went much further in analysing the structure and organisation of Missoula, Montana, the community that he studied. In terms of method, he was a secret observer and quite concerned about the secrecy of his work, as correspondence to Burgess shows (Blumenthal, 1929).

In a letter mailed from Philipsburg, Montana and dated 20.4.1929 Blumenthal discussed methodology. He referred to spot maps of the community, which he took for granted as a methodological device, but asked if photographs may not be used, in particular, the use of an aerial photograph as the basis for such a spot map.
Blumenthal reckoned that such a photograph would give a far better idea of the community than the conventional spot map, and this is particularly the case in the kind of intimate study that he was undertaking. He noted that before receiving Burgess's letter he had intended the first chapter, of what became Small Town Stuff, to be on methodology but

'further thought has convinced me that the principal part of such a chapter would have to do with the role of the investigator in making the study and should thus be kept relatively secret.' For, the De Graff incident is suggestive of what could happen to me and my family if the true nature of my activities become public.' [6]

He even suggested writing two theses, a formal one and a secret one kept at the Department of Sociology which would be 'replete with life history materials'.

Blumenthal unabashedly asserted that his work was methodological-ly innovative in terms of the manner by which he had gathered data, but did not, at that time, refer to it as participant observation.

'It seems to me that the most promising results of my study is that of breaking the ground for the development of a technique of making intimate studies of small communities. And as I have suggested, the role of the investigator plays a very important part in his final product and should be thoroughly exposed in an adequate study which avoids the common fallacy of sociologists -- that of assuming a too nearly absolute objectivity on the part of the social researcher. To that end, the document which I have entitled 'A Diary of Topics of Conversation and my Reactions upon Them' reveals a method of bringing out the role of the investigator if he faithfully records his attitudes, how he is treated, et cetera.'

Blumenthal wanted to include six detailed life histories, a male and female from each of the following age categories; grade school child, young adult and mature adult who had lived in the community a long time. He referred to one document which
'illustrates the use of the continued interview and its function of bringing a degree of intimacy which cannot be attained in a single interview. It is the most concrete revelation I have ever encountered and only my peculiar role as confidant could have enabled me to secure it.'

Irrespective of Blumenthal's labelling of his method, one must assess to what extent this approach reflects an early attempt at developing participant observation. Besides actually living in the community and participating in its day to day activities, Blumenthal also argued for a clear contextualisation of the data collection process. Rather than the 'objective observation' of earlier studies, Blumenthal was concerned to engage the perspectives of the subjects, and considered his own involvement to be a crucial procedure for achieving this. For him, the novelty of his approach lay in his acceptance of the subjective role of the researcher. This role, he suggested, needed to be adequately documented so that the empirical data could be approached critically by others. In this respect he prefaced the perspective that became popular a quarter of a century later and directed towards all forms of sociological enquiry by Myrdal (1968, 1970).

4.7 Participant Observation and the 'Chicago School' Approach

Participant observation was not, then, a recognisably Chicago research practice until at least into the 1940s. Thus, at the 1939 conference on the Polish Peasant study, which concentrated on the human document as a research tool, a forum at which methodological issues were extensively discussed, there was but a single mention of participant observation, and this in reference to documents collected in an extensive survey in Sweden:
'The Swedish material has an all-round superiority in the fact that it includes the medical examination, the life history, the controlled interview, the letters to relatives, friends, sweethearts, lawyers, etc., and the testimony of 'participant observers'. (Social Science Research Council, 1939, p.133)

Furthermore, there is virtually no reference to a specifically Chicago approach with the exception of two references, by Thomas, both to the collection of human documents in the early part of the century

'This movement toward the collection of human document material was going on inevitably, anyway, that is, in Chicago. So this work was merely another influence on the concrete 'trend in sociology....[besides the Swedish study and a collection of material from the Jewish Daily Forward] it is not necessary to mention the important collection of human documents by sociologists of the University of Chicago where the practice has been extensive and refined' (Social Science Research Council, 1939, p. 130-2).

In his doctoral thesis submitted to the Chicago sociology department in 1940, Daniel, refers to the 'participant observation type method', as though it were far from commonplace. Daniel utilised the method, but augmented it through other devices.

Indeed, it was Whyte (1943) who probably provided the first example of a 'pure' participant observation study. Even so, this was not at the instigation of the University of Chicago, as correspondence between Burgess and Wilson (Wilson 1940) indicates. Indeed, Wilson talked of Whyte's study not as emulating the Chicago approach but rather

'You see his technique is the interview technique like Bakke's of Yale....

The only 'concession' Wilson made to a Chicago orientation is to say that Whyte is
from the sociological or socio-economic aspects of current phenomena, on what I might call the clinical side, which has been favored by W. I. Thomas and Park and you.'

Whyte, himself, noted that

'I owe a great personal debt to Conrad M. Arensberg, now at Columbia University, from whom I learned my field-work techniques.' (Whyte, 1955, p. vii)

And it was Whyte, in his methodological appendix to the second edition (1955), who was one of the first researchers to discuss the problems of participant observation directly and in some depth, which belies the view that

'In the period from 1920 to 1940 people who called themselves students of society were familiar with personal documents and participant observation.' (Bogdan & Taylor, 1975, p. 4)

While Whyte completed his research as a Chicago graduate student, he began while at Harvard and the study was of an area of Boston. The only contribution that Chicago seemed to have made to Whyte's discussion of participant observation methodology was indirectly through Arensberg having worked on Warner's 'Yankee City' research. This further supports the suggestion made above that participant observation at Chicago resided in its community studies.

It would seem then that the first classic 'pure' participant observation study was neither conceived at Chicago, nor, in terms of field work orientation, did it owe much, if anything, directly to a Chicago tradition.

While Blumer suggested the efficacy of observational studies in the 1940s, and a 'tradition' of symbolic interactionist observation research ostensibly deriving from Chicago emerged in the 1950s, there is little to suggest that the 'Chicago School',

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prior to 1940 was single-mindedly pursuing participant observation research [7]. When Becker undertook ethnographic research in the late 1940s and early 1950s he studied the 'classics' of social research

'such books as Street Corner Society, the Polish Peasant in Europe and America, and back I should say to Charles Booth and Henry Mayhew was something that a lot of us read. You know, taking that to be a kind of model of how ethnography might proceed, of the kind of detail that one would want to know about people living in a city, for instance, or about occupation.' (Becker, 1979a, p. 7)

The 'Chicago tradition' prior to 1940, then, was broadly ethnographic rather than specifically concerned with participant observation. It aimed at finding out things 'you hadn't thought of' rather than adopting the insider perspective of the subjects. That was to occur later when 'ethnographic research began to be informed by multiple theoretical viewpoints' (Becker, 1979a, p. 9).

Indeed, as late as the 1960s there was considerable advocacy of participant observation as an appropriate and novel method of research. Notable is Polsky's (1971) advocacy of participant observation for the study of deviance. He claimed that the naturalistic perspective was little used and the main target of his critique was the well known and highly regarded criminological work of Sutherland.

Similarly, as late as 1969, Coleman pointed to the lack of 'in situ' observation which is commonly regarded as the hallmark of Chicago sociology. Coleman (1969) noted that sociologists have inadequately used observation, rather they have tended to depend on individual's reports of their own behaviour, (through quest-
ionnaires, life histories, interviews). Not enough, he suggested has been done on observation in situ. He pointed to the doctoral work of both Stinchcombe and Barker on high school students and children respectively which used direct observation and to

'the work of Garfinkel and his students, in which the investigation presents verbal stimuli, not as an interviewer, but as a member of the same social system.... More useful, however, than at least the existing disappointing results of Garfinkel's work is the work in participant observation carried out by such sociologists as Howard Becker, and work described by Webb, Campbell, Schwartz and Sechrest in 'Unobtrusive Measures'.' (Coleman, 1969, p. 112).

Coleman may have underplayed the extent to which, by the late 1960s, participant research had been used, but even so his comments on the relative novelty of direct, in situ, observation belies the view that such an approach is intrinsic to the Chicago heritage. The problem in the past, he argued, was that observation was difficult because there were few aids. This, he argued, had changed thanks to electronic aids, awareness of time sampling, space sampling and sampling of roles and so on, all of which increase reliability and validity.

Without doubt some of the work done at Chicago involved methods more usually located within ethnographic orientations. However there is very little to suggest that Chicago adopted other than a nomothetic perspective on the analysis of the social world until the 1930s, when, in embryonic form, a proto-phenomenological scepticism began to emerge in the work of Blumer and Wirth. (This is examined further in chapter five). Prior to this, the interactionist perspective, reliant as it was upon Thomas' general sociological orientation, clearly adopted nomothetic principles,
and with it a degree of methodological eclecticism, as will be illustrated below, with few pieces of work that could be described as adopting a single method.

However, that does not mean that there were not clear preferences expressed by members of the department for particular methodic devices. On the one hand, Park used to adopt an attitude of extreme scepticism towards statistics, which became more acute as he grew older (Matthews, 1977); while Ogburn, on the other, was keen to develop the measurement of social phenomena. Burgess, tended to mediate between the two and make use of qualitative and quantitative techniques.

'Ogburn knew very little of the Park sociology and Park knew nothing of statistical methods. I think Park would sometimes make some grudging remark about it, disapproving of the fad for statistics. Ogburn was a Southern gentleman and he generally didn't make personal remarks about anybody, but his wife did. And the bitter personal feelings between the wives suggested that underneath both men had some feeling about the matter but on the surface they both co-operated well and I once overheard a good part of a department meeting and everything went well.' (Faris, R., 1972)

Although the 'Chicago School' myth would have it otherwise, the Chicagoans were as involved in developing quantitative techniques as they were in qualitative procedures. There were debates at Chicago, and there was, it seems 'a tension in the department about the Ogburnian's and Parkian's' (Cottrell, 1972). However, there is little evidence that any long standing alliances or factional divisions characterised the department.

'I'm not sure that they grouped into factions very much, at least I wasn't aware of it.' (Faris, R., 1972)

Importantly, there was no division into competing nomothetic and ideographic camps at Chicago.
'The two sides, the statistical and the social interactionists both wanted to build a scientific sociology.' (Dollard, 1972)

In effect there were three debates at Chicago, those relating to quantification and the associated discussions about the efficacy of case studies and statistics, those relating to instinct theory, and those, somewhat more bitter and less resolvable, revolving around Freud's theories. Of these, the instinct theory debate was the least active, for many instincts were a non-starter; and Faris laid to rest the residual instinct notion embodied in Thomas's wishes [8]. The other two debates were active.

'Park would preside and he would rumble on about stupid Freudians and statisticians, but Burgess would nod as if he was approving of what Park said and he would look across to where some of us were sneaking out and getting courses on statistics and courses on Freud and he'd twinkle because he was doing the same thing. He would, he got very interested in the Freudian contribution to sociology, to social theory.' (Cottrell, 1972)

The Chicagoans role in developing quantitative techniques is explored below. Chapter five takes up the theoretical debates which involved Freudianism.

4.8 Quantification at Chicago

The 'Chicago School' is rarely associated with the development or even use of quantitative techniques. Thomas's assertion of the 'perfect' nature of life history as a source of data, Park's apparent opposition to statistics and Blumer's attacks on variable analysis have all been taken as indicative of an antipathy towards quantification by the 'Chicago School'. This ignores the extensive development and use of statistics at Chicago.
It must be remembered, too, that up to 1930 there was relatively little use of statistics by American sociologists at all. The Committee on Social Statistics of the American Statistical Association noted, in 1929, that more sociologists ought to be interested in statistics, as well as vice versa. To that end it felt the need for

'an appraisal of the extent to which statistical methods have already been developed, utilized or foreshadowed in a variety of social and sociological studies'. (Rice, 1930)

In December of 1929, for the first time, the American Sociological Society and the American Statistical Association had joint sessions, to discuss statistical method, at their annual meetings. Similarly, Duncan and Duncan (1934), in their longitudinal survey of the interests of members of the American Sociological Society, between 1928 and 1931, concluded

'more sociologists have an interest in social psychology than any other subject, but their major interest is in social work. This being the case, those who look with disdain upon social work and social problems, and pin their hope for a "scientific" sociology on statistical sociology will find little comfort or satisfaction in these findings.' (Duncan and Duncan, 1934, p. 212)

Although it is clear from inspecting their work that the Chicagoans widely used official statistics in various ways, the assumption made by commentators is that the Chicagoans tended to make use of statistics as descriptive rather than analytic tools. This view further suggests that the development of quantification by the Chicagoans was quite different from the post war expansion centering at Columbia and initiated by the developments in public opinion polling. The use of statistics by the Chicagoans is usually not seen to fall into this mould. Chicago sociologists, it is assumed, did not specify hypotheses
for rigorous statistical testing, develop the large scale scheduled interview of a representative sample, rigorously assess the relationship between correlation and causality (and thus criteria for causality attribution), define concepts operationally nor, therefore, develop accurate measurement techniques and advanced statistical analysis.

4.9 Thomas and the Case Study versus Statistics Debate.

As has already been demonstrated, Chicago sociologists were not opposed to a 'falsificationist' nomothetic basis for sociological research. Scepticism about statistical approaches thus reflected, not an opposition to the fundamental attempt to construct causal or pseudo-causal relationships in sociology, but rather a scepticism that statistical methods could adequately grasp the subjective aspect of interaction (communication), hence the initial concern with life history. However, as indicated above, Thomas was more concerned with the attitude-value relationship rather than with a particular method. Later, as attitude testing became more sophisticated he raised no objection to it provided it could generate the information required.

'It is my experience that formal methodological studies are relatively unprofitable. They have tended to represent the standpoint developed in philosophy and the history of philosophy. It is my impression that progress in method is made from point to point by setting up objectives, employing certain techniques, then resetting the problems with the introduction of still other objectives and the modification of technique. For example, Galvani or someone else gets a reaction from a frog's leg ... this may suggest to Pfeffer or Verworn the application of electricity .... In all of this, there is no formal attention to method but the use of some imagination or mind from point to point. The operator raises the question, at appropriate points, 'What if,' and prepares a set-up to test this query. Similarly, in our own line, some of us, in connection with
some experience, raised a question, 'What would happen if we were able to secure life records of a large number of persons which would show their behavior reactions in connection with their various experiences and social situations?' After some experimentation, yourself [Park], Shaw and others have been interested in the preparation of very systematic and elaborate life-histories. In this connection it is noted that the behavior of young persons is dependent upon their social status and the regions in which they live. Studies are then made from the ecological standpoint. It is discovered that children brought into the juvenile court are predominantly from certain localities in the city. The rate of delinquency is related to gang life and gang life is related to localities. Thrasher then makes a study of the gang from this standpoint. As comparative observations multiply, Shaw undertakes to determine how the cases of boys brought into the juvenile court for stealing are connected with their gang life and determines that 90 per cent of these boys did their stealing in groups of two or more. In the search for causes of delinquency, it then appears that the delinquent and nondelinquent are often very much alike in their behavior reactions. It is then recognized that it is impossible to study the delinquent population without at the same time studying the nondelinquent, and at present we have introduced the plan of using nondelinquent groups as a control in connection with studies of the causation of delinquency. In all this, also, we move from point to point without necessarily any formidable attempt to rationalize and generalize the process. It is only in fact, so far as sociology is concerned, since we abandoned the search for standardized methods based largely on the work of dead men, that we have made the beginnings which I have indicated. (Thomas, 1928.)

Indeed, Thomas came more and more to accept the possibility of quantitative techniques providing the kind of material life histories provided and this was very much a concern of the Chicago sociologists for two decades, a concern which was shared by the wider sociological community. Throughout the twenties a general debate on the relative merits of case study and statistics centered on the reliability of case study data (Cooley, 1928; Shaw, 1931; Rice 1931). [9]

In 1930 two Ph.D theses at Chicago independently undertook empirical investigations of the extent to which attitude testing
schedules were able to provide equivalent data to the self composed life history. Stouffer, in his influential thesis, demonstrated that, for some kinds of attitudes, the administratively easier test instrument was as good as the life history record, although perhaps less subtle in the case of extreme attitudes. Brown (1930) also included an assessment of life history and attitude surveys which suggested that for delicate areas the life history was more accurate, although he deferred to Stouffer (1930), in a footnote, as being a more rigorous study in respect of 'straight forward attitudinal statements'. However, this analysis did not as Cavan (1983) suggests, ring the death knell of life history. Stouffer still saw a role for it and Burgess, in particular, was concerned to integrate case study and statistics in a synthesis. Indeed, the personal document was still a central feature of methodological discussion a decade later (Social Science Research Council, 1939).

There was no attempt at Chicago to establish a position which gave primacy to case study rather than statistical work, nor was the debate about the two methods indicative of competing camps.

'Sutherland ... was a most knowledgeable person in criminology... he was brought up to Chicago and he sort of laughed at these debates about case studies versus statistics. He just plodded right along and got case studies when he damned pleased and wanted them and used statistics and was always trying to get a little more statistical methods too.' (Cottrell, 1972)

'We had a few sessions on statistics versus case studies and most of us students regarded that as light entertainment because we found both of them useful and didn't think of it as versus. Blumer, however, stayed fairly big with this versus statistics and I think still is today. Blumer did not pick up the tight methods and has sort of gone out on a limb.' (Faris, R., 1972)

The consolidation of case study and statistics, indeed, seems to
have begun early at Chicago. Howard Jensen, a doctoral student in the latter half of the second decade of the century apparently 'felt that statistics were important although he was not willing to give them first place in his interest. He was a humanist. Humanitarian interest and he used statistics only to further that interest. But he was not narrow minded on the subject. He felt that both things were important, the other thing being case study, of course.' (Mrs. H. Jensen, 1972).

Bingham Dai obtained his doctorate in 1937 for his research into opiate addiction in Chicago (Dai, 1937). This research in progress he reported to the Summer Institute of 1935 and an article on it in the Bulletin noted

'This study consisted of two parts (i) the analysis of statistical data regarding drug addiction, and (ii) case studies based upon the long interview' (Bulletin of the Society for Social Research, June 1935). [10]

Chicagoans were, thus, more likely to adopt methods according to circumstance rather than opt for either side of the case-study versus statistics debate.

Subsequently, the case study - statistics debate then shifted emphasis from a concern with the efficacy of statistics in the collection of attitudinal data to a concern with the definitive nature of concepts. Statistical analysis of schedules required that concepts be definitive and that they be predetermined by the interviewer. The debate on 'operationalisation' was indicative of this re-orientation of the case study - statistics debate. It was not one concerned with establishing the primacy of quantitative approaches over qualitative ones per se, but rather of the possibility of a falsificationist science requiring conceptual explication and accurate measurement, (Lundberg, 1936; Waller, 1936).
Blumer responded vigorously to the movement towards operationalisation of concepts by arguing that concepts in sociology were primarily 'sensitizing' and not definitive (Blumer, 1931) [11]. This line of debate was not concerned with the efficacy of statistics in assessing attitudes, but cast doubt on the possibility of formulating concepts to a degree that measurement would be at all meaningful. Case study and other ethnographic techniques, it was argued, offered a sounder way of generating 'sensitizing' concepts.

These two phases of the debate as to the efficacy of statistics and the possibility of definitive concepts were engaged in as fully at Chicago as elsewhere and there was no 'Chicago' view which, as the myth suggests, saw the 'Chicago School' as defenders of case study and opposed to statistics.

4.10 Park's Approach to Quantification

Park is cited as a clear opponent of statistics and as providing a heritage that disdained the use of quantitative techniques.

'For Park, statisticians were worse than dirt, that they really never knew the phenomena they were studying. He made great point of the difference between knowledge about something and acquaintance with the phenomena.' (Cottrell, 1972)

He communicated this forcibly to some of his students and redirected their research methods, as Hayner recalls

'You had to start with a map so I got one of these big maps of Chicago and spotted all the hotels in the Chicago area.... I was going to cook up a questionnaire to these places. Park put his thumb on that. He wasn't into statistics, you know. You don't want to do it that way. You have to get out and visit these people and talk with them. There are all kinds of people in the hotel. Put yourself in their place. Be a good reporter.' (Hayner, 1972)
Indeed, his disdain of statistics also affected his personal relationships, according to Faris.

'Park asked me how my thesis was coming along and I told him I hadn't done anything because I was getting too interested in statistics and that destroyed my relation with Park for three years.... He was anti-statistical, I didn't realise I was hurting his feelings but he didn't approach me any more and he didn't notice me in the corridors, not until my final examination on my thesis. He liked my work, he got immediately warm again and gave me a friendly compliments and was personally warm to me ever since.' (Faris, R., 1972)

However, Park's position was not so straightforward as these accounts suggest. He noted (Park, 1939) that as early as the 1890s he had an

'understanding of the significance and the possibilities of the social survey as an instrument for social investigations.' (Park, 1939, p. 3).

Like Thomas, he was sceptical of the uncritical adoption of the practice of the physical sciences and wanted the subjective element taken into account (Wirth, 1944). The social survey, he felt, could provide some useful information but that it only showed the surface of appearances and that it masked the meanings that underlay the aggregates. Nonetheless, he approached surveys and statistics critically rather than rejecting them outright. He even taught a course entitled 'The Survey' from 1915 to 1922 (with the exception of 1921) which looked at the 'uses and practical limitations of the Social Survey' and described and compared

'technical devices for the analysis, description and presentation of sociological data with reference to the different fields in which they have been practically employed... [thus estimating] the value for science and for social reform of the results obtained.' (University of Chicago, Official Publications, 1915).

The ending of this course in 1922 was the result of the development of Park's collaboration with Burgess on the teaching of a
course entitled 'Field Studies' (begun in 1917, Burgess joined Park in teaching it in 1920) which became the only methods course in the department until 1927, and was the basis of Palmer's handbook (1928).

It would be possible to assume from this that Park saw little potential in the social survey and that with Burgess encouraged 'field work' which moved away from statistical concerns, and that the well-known studies of the 'Golden Era' were a result of this 'qualitative' orientation. While having some credibility, this interpretation accounts for only part of the story. Hughes recalled that during the period 1923 to 1927 he had a course on the social survey with Park. This, presumably, was part of the field studies course.

'It was a field operation and he introduced us to the volumes of Booth's Life and Labour of the People in London and Rowntree's study of poverty and other British studies of this kind and the Pittsburgh survey and the Springfield survey... he used these surveys emphasizing, incidentally, the demographic, statistical side of them as well as well as this dynamic human side. It's a mistake to think that Park was neglecting that side.' (Hughes, 1980b, p. 270)

Although the sociology department had no statistician of its own until 1927, despite Small's repeated requests for one dating from 1915, Chicago sociology students were directed towards the statistics courses in other departments, notably Thurstone's course in the psychology department (Blumer, 1972) and Field's courses in the economics department (Cavan, 1983, Bulmer 1981). Field developed his course to make it more suitable for social science students rather than specifically for economics graduates.
While statistical techniques were taught elsewhere an understanding of them was expected of sociology students and their active involvement in the development of small area statistics and in the Chicago Fact Books reflect the encouragement of statistical expertise and appreciation of it within the department.

Park encouraged Charles Johnson, the joint executive secretary of the Commission to investigate the Chicago Race Riots to investigate the riots using statistical techniques and himself employed a mixture of case study and statistical analyses in the 1925 West Coast Survey of Japanese immigrants (Matthews, 1977) which was published in 'Survey Magazine'. As part of this study, Park encouraged Bogardus to produce a quantitative indicator of social distance which later became the Bogardus Social Distance Scale. On his return from the Institute of Pacific relations held in Honolulu in July 1925, Park addressed the Society for Social Research and referred to the

'unusual opportunities in Honolulu at the present time to study sociological problems as controlled experiments.' (Minutes of the Society for Social Research, 29.10.1925).

Park tried to get Hughes to do a mathematical study of land values in a large city because he thought it would be the best statistical index to city growth.

"As you have no doubt heard, he didn't believe in statistics, but he wanted me to do that thesis just the same." (Hughes, 1980b, p. 256)

Other students of Park and Burgess made use of statistical techniques in the early 1920s (before Ogburn joined the staff), for example, Cavan (1928), Thrasher (1926) and Mowrer (1927). Mowrer, began his research on family disorganisation in 1920 which set out on a search for a
'fundamental and scientific analysis of marriage disorganization ... by an examination of statistics and statistical methods as these could be applied to the phenomena of divorce and desertion.' (Mowrer, 1927).

4.11 Ogburn and the Nurturing of Quantitative Techniques

Even if Park was opposed to, or at least sceptical of, statistical approaches, and Cottrell (1972), on reflection suggested that he was, Chicago sociologists, notably Ogburn, were not ignoring statistical developments in the 1920s and 1930s.

'Ogburn was doing empirical work, which is not what we would call experimental work today, he was just manipulating countings that had already been made for other purposes by the government. But he was squeezing theoretical material out of the census and other government methods of counting.' (Dollard, 1972)

'Ogburn represented a counterweight to the anti-statistical orientation Park represented and set the tone in the department for quite a while of being anti-statistician. Yet Faris insisted that it wasn't a good idea to have a sociologist falling behind because he didn't have good statistical training, so they brought Ogburn on and Ogburn made quite a splash and attracted some very good students.' (Cottrell, 1972)

The development of quantification in sociology at Chicago received a boost from the employment of Ogburn who moved from Columbia with a well established reputation. Indeed, Ogburn contributed a chapter on 'Statistical Studies on Marriage and the Family' to the text that resulted from the work of the American Statistical Association's Committee on Social Statistics (Rice, 1930) and had been editor of the Journal of the American Statistical Association. He turned down the chairmanships of sociology departments at Michigan and Minnesota (Ogburn 1929b) in order to come to Chicago where he took responsibility for the development of quantitative techniques and of quantitatively based research in the department. Nonetheless, the development of a
statistical perspective, he recalled, was not easy.

'On coming to the University of Chicago I found a much more hostile attitude toward statistics than I ever had at Columbia. Yet I fought the battle, taught all the statistics in the Sociology Department, and participated generally in the statistical work of University Committees.' (Ogburn journal, 13th June 1952)

The arrival of Ogburn, while clearly a boost for quantification in Chicago sociology, was not simply the arrival of a statistician. Ogburn had rejected the idea of restricting his intellectual development by being nothing but a statistician as early as 1912 (Ogburn journal, 13th June 1952). With the depression Ogburn spent more time on substantive sociology and became particularly interested in technology and social change. Eventually he lost track of developments in statistical theory and gave up teaching the subject altogether.

Some commentators, for example Cavan (1983, p. 414), have suggested that Ogburn's development of statistics represented a split in the department. As Ogburn's influence began to be felt towards the end of the 'Golden Era', Cavan suggested that

'statistics gained favour... and life histories began to fade as a primary means of study, to be replaced by the collection of comparable data from a number of cases that were subjected to statistical analysis.... Ogburn's arrival gave impetus to the quantitative analysis of social data and thereby contributed to the decline of case studies.' (Cavan, 1983, p. 414).

Cottrell, too, seems to suggest that the impact of Ogburn was to lead to a methodological split with him developing his own group of quantitatively oriented students. Such a view, which implicitly prevails as part of many designations of the 'Chicago School', gives credence to the view of the school as retaining 'anachronistic ethnographic techniques' for 'unscientific reformist'
purposes.

However, such views are misleading. Small had wanted a statistician in the department for more than a decade.

'Ogburn was recruited because sociology did not have a course in statistics... the realisation came up that it was an important subject and the realisation was there that Chicago ought to bring in some outsiders.... Columbia was the other strong department in the country... probably my father heard it from somewhere that Ogburn was dissatisfied at Columbia and it might be a good chance to raid... Ogburn really wanted to stay at Columbia but wasn't really decided. There was feeling against him there.... It was my father's initiative that got Ogburn here... he probably talked the others into it. (Faris, R., 1972)

Blumer (1972) thought that Burgess had pushed for Ogburn's appointment more than anyone else but that it had been a consensus of the Department that such an appointment was necessary as there was a gap to be filled.

Ogburn moved rapidly to promote a more positive image of quantification in sociology, but was not acting either to reverse a trend, or in isolation. If Park and Blumer had reservations, in the main Ogburn's arrival was seen as advantageous for sociology at Chicago. The Society for Social Research, through its Bulletin, welcomed Ogburn and advertised and promoted his courses.

Ogburn's impact was substantial and went beyond the mere importation of statistical expertise. His involvement in the department and outside was notable and he tended to adopt wide horizons. 'His concepts were primarily national whereas the others were local.' (Dollard, 1972)

Ogburn was directly involved in ongoing research in the department as well as his own heavy commitments which included
the President's Committee on Social Trends. He wrote to Ruth Newcomb on October 17th 1929

'I am working this Fall on the planning of a survey of recent social changes to be conducted under the auspices of President Hoover. I'm just finishing up the study on the comparative strength of the various forces operating in the Presidential election of 1928. I'm also hoping to finish up a study on the business cycles and politics. I have also worked during the past month of the quarter and during two and one half months of the Summer quarter on outlining the report for a plan for a nationwide cost of living study with particular reference to its scope and method. This was done for President Hoover at the request of Secretary Wilbur.' (Ogburn, 1929)

One avenue of his involvement in research at Chicago was through the Local Community Research Committee, which he joined as soon as he settled in. Ironically, in view of its general tendency, the Local Community Research Committee actually provided a focus for the development of quantitative research techniques. In the period from his appointment in 1927, to the demise of the Committee in 1929, Ogburn had begun three studies under the auspices of the Committee and two of the more quantitative students, Stephan and Tibbits, were involved in two other projects. The Committee was also responsible for establishing research professorships in the social sciences, two of which, Thurstone and Schultz, were predominantly involved in quantitative research.

Bulmer (1981) argued that the Local Community Research Committee was a major force in the development of quantitative research methods at Chicago. The committee gave considerable financial support to quantitative research in the social sciences at Chicago. Bulmer indicated four areas of funding directed to quantification besides the establishment of research professorships. These were the funding of large scale politics surveys,
the purchase of census tract data, the appointment of support staff (crucial for the time consuming process of attempting factor analytic and multi-variate techniques by hand), and the provision of physical facilities for research, notably computational machines and the space to house them.

This involvement of the Local Community Research Committee in the development of quantitative techniques towards the end of the 1920s is reflected in White's (1929a, p. 25) summary. He noted that the social sciences have not ordinarily been thought of as using or needing laboratory equipment and that

'social scientists regularly treat the community as a clinic, diagnosing on the basis of existing knowledge and insight, and prescribing with what wisdom they may possess for social ills。

In a more precise sense, however, the social sciences have now reached the point where it is open to use laboratory methods. Mr. Gosnell's experiment with a section of the Chicago electorate, applying a known stimulus under controlled conditions, reveals the social scientist at work in an out-of-door laboratory; the various analyses of personality, including the application of the technique of the psychologist and psychiatrist, involve laboratory technique and equipment provided in the new Social Science Building.

It may be more accurate to refer to this building .... as a workshop.' (White, 1929a, p. 31)

A further indicator of Ogburn's involvement in the interdisciplinary nature of the development of quantitative techniques at Chicago can be seen in his collaboration with the economics department in reconstructing their written quantitative methods paper for the doctorate. What had once been a combined accountancy and statistics paper was restructured into two self-contained papers with the statistics element greatly enhanced and including time series analysis, index number construction, multiple and partial correlation and probability distributions.
Ogburn was also quick in requesting additional full time support in the quantitative area and circulated the department with a draft memorandum (c. 1928) entitled Preliminary Draft of the Statistics Proposal

'It is highly important that a man in statistical mathematics be added to the mathematics department, this is the first wish of all of us. If, however, it is not possible to arrange for this a man should be added to the economics business staff who is very strong on the mathematics side. There should be centralised operations of all elementary and intermediate statistics work in economics, sociology, psychology, commerce and administration and social service administration. The really advanced work should be given in a new building in connection with research projects.' (Ogburn, 1928).

In the event, the Social Science Research Building was erected for such advanced social scientific work in 1929 and contained an advanced statistical laboratory. Ogburn, who had responsibility for the 'image' of the Social Science Research Building, managed to persuade his colleagues that a suitable motto would be an annotated quote from Lord Kelvin

'When you cannot measure * your knowledge is * meagre * and * unsatisfactory *'

which is engraved on the outside of the building.

Ogburn's interdisciplinary perspective did not, however, have priority over his sociological concerns. Apart from taking on R. E. L. Faris as a statistics assistant in October 1929, he developed various new courses in the department.

Ogburn's arrival, having been announced and welcomed in the Bulletin of the Society for Social Research in 1927, was followed up in January with an article which noted that

'The University has announced a new course in methods of research for the first term of the Summer Quarter, 1928, which it is hoped will be of interest to members of the
Society for Social Research, and to others who expect to attend the sessions of the Institute next August'. (Bulletin of the Society for Social Research, Jan. 1928)

The course, which was taught by Ogburn, was aimed at graduate students who had some experience in, and who were carrying on, independent research work and who wanted to meet others engaged in the same or related lines of enquiry. The course consisted of lectures, demonstrations and a twice weekly clinic, plus individual counselling from instructors. The course had been prompted by

'The steady accumulation in recent years of maps, statistics and local studies at the Social Research Laboratory of the University; the interesting investigations now in progress at the Institute for Juvenile Research... and the increasing number of social investigations carried on by other local institutions. The purpose of the new course in methods of research is to extend and complete the program of the Institute of the Society for Social Research; to make it a center where students may meet; a clearing house where methods of research may be compared and criticized.' (Bulletin of the Society for Social Research, Jan. 1928)

The April 1928 issue of the Bulletin of the Society for Social Research called attention once again to the new course in Methods of Research in the Social Sciences. This was but one of a number of new courses in quantitative methodology which were to blossom at Chicago.

In 1927 Ogburn offered an introductory course in statistics which he taught until 1929. Stouffer, one of Ogburn's graduate students, took over the teaching in 1930 and when, in 1932, Rice spent a year at Chicago, the course title was changed to 'Statistical Sociology' and he taught it for a year before Ogburn took over the new course and taught it until 1934. During his year at Chicago, Rice also ran a course entitled 'Measurement in Social Politics'. Running parallel to the introductory course was a
course entitled 'Statistical Methods' ('Methods of Quantitative Sociology', after 1932) which either Ogburn or Stouffer taught from 1927 to 1945 (with the exception of 1931, 1943).

Ogburn was supposedly to provide a course on the 'Statistics of Social Maladjustment' when he first arrived at Chicago but this was never given, although he did teach a course on 'Sociology and the Social Sciences' in which he demanded a more 'exact' approach to social scientific data and enquiry. In 1929, he began an advanced course in 'Research in Quantitative Sociology' which was available most years until 1940 and in 1936 Ogburn gave a course on 'Partial Correlation Analysis' which he repeated each year he was available until 1945.

In 1931 Ogburn first offered a course on 'Statistical Problems' which he taught between 1932 and 1935, before it became a more specific course in the 'Measurement of Relationships' in 1937 and was taught until 1942. Stouffer taught this course in 1938 and had earlier taken the 'Statistical Sociology' course in 1935. These were but a part of Stouffer's prolific involvement in teaching quantitative methodology courses in the mid to late 1930s. In 1935 he started a courses on 'Sampling in Social Research' (which was later taught by Williams in 1948 and 1949), 'Quantitative Studies In the Family', 'Quantitative Criminology' and one on the 'Applications of Probability to Sociology' which changed to 'Quantitative Aspects of Social Problems' the following year. In 1938 he inaugurated 'Quantitative Studies in Social Psychology' and in 1939 he took over and revamped Park's old course in 'Human Migrations' before, a year later, focussing
upon 'Quantitative Studies in Social Organisation' and 'Statistical Problems of Governmental Research'. Teaching developments in this field, however, were thwarted by the War and Stouffer's involvement in government research culminating in his leave of absence for Government Service in 1944 and 1945.

The courses Stouffer began in 1936 on 'Quantitative Problems in Population', 'Quantitative Studies in Population and Human Ecology' and 'Dynamics of Population' initiated a series of courses on population in which Hauser and Duncan were heavily involved. In 1948 Hauser taught 'Comparative Population Structure and Dynamics', 'Quantitative Methods for Population Research' and 'Seminar in the Analysis of Census Data' as well as the introductory course to the study of population human ecology. Hauser had already offered a course on statistical sources in 1947 and followed it up with one specifically on 'Sample Surveys as a Research Method' in 1949. In 1950 he took over a course on the 'Design of Research' which Goodman had started in 1947.

Other sociologists who took on the teaching of quantitative methodology courses [12] included Williams, Kittagawa and Hart. Williams taught 'Methods of Quantitative Sociology' in 1947, 'Introduction to Statistical Reasoning', and 'Mathematics Essential to Elementary Statistics' in 1948. Kittagawa also taught these courses during the next three years. In 1948 the more advanced statistics courses were 'rationalised' into three successive courses 'Statistical Methods of Research' parts I to III, and taught initially by Williams and Kittagawa before becoming the province of Goodman. The latter also developed a
course on statistical inference (in 1951) and on 'Recent Advanced Methods' in quantitative research a year later. In 1949, Lazarsfeld was a visiting lecturer from Columbia and offered a course on 'New Developments in Attitude Measurement' and a year later Hart (director of the National Opinion Research Center) gave a course on Research in Public Opinion.

4.12 Burgess as the Barometer of Methodological Tendencies

If Park was hostile to statistics, Burgess certainly was not. Before coming to Chicago, Burgess had been involved with social surveys and statistics. He had worked with J. J. Sippy on the Belleville Survey (1913) and with F. W. Blackmar on the Lawrence Social Survey (1916), (Burgess 1916). He encouraged students to make use of statistical data wherever appropriate. He was very much involved in developing census data for sociological use

'Burgess, it may fairly be claimed, was the father of modern census tract statistics, both by example and as a co-ordinator of pressure on the Bureau of Census in drawing up their plans for 1930.' (Bulmer, 1981, p. 315)

Burgess, in conjunction with Newcomb, produced analyses of the 1920, and 1930 censuses (Burgess and Newcomb, 1931, 1933), and encouraged the analysis of the 1934 census (Newcomb and Lang, 1934). In conjunction with this Newcomb was awarded the

'University prize for originality in research ... for his contribution of a "Single Numerical Index of Age and Sex Distribution of Population".' (Bulletin of the Society for Social Research, Dec., 1930)

Burgess' request for funding from the Social Science Research Council (Burgess, 1935a) for research that would lead to the presentation of materials on city growth, movement of population and formation of local communities by sector, prepared the ground
for the later Fact Books of Chicago (Wirth and Furez, 1938; Wirth and Bernert, 1949; Hauser and Kitagawa, 1953; Kitagawa and Taeuber, 1963) [13]. His involvement with census analysis at a multi-disciplinary level goes back at least to 1924. As chairman of the Chicago Census Committee, Burgess circulated a paper (3/6/1924) to the Social Research Committee of Political Economy, Sociology and Political Science Departments on the development of the Chicago census. This committee was the forerunner of the Local Community Research Committee and its executive committee comprised Marshall, Merriam and Burgess.

'Burgess was very instrumental in encouraging students who had been brought up to make case studies and to get qualitative data and that sort of thing to try and translate them into a more quantifiable form and to pay some attention to the statistics. He was much influenced by Ogburn, much encouraged I should say, he was already aware of the uses of quantified material, he was very much aware of the usefulness of the census data and that sort of thing and of course our delinquency area studies... Yes, that preceded Ogburn... Park was mainly grumbling about people who started on a problem with let's get some quantitative data, he wanted to start with 'let's find out about the way this person ticks' and get inside of him and inside of his community and find out what is going on. And if you have to use quantitative data [then] use it in the light of what you know, not start out with it and cover over your ignorance about it, as is what goes on with a lot of figures. So we were already using rather elementary quantitative material. And that was, I think, becoming more impressed on Faris, as Chairman, to get a statistician.' (Cottrell, 1972)

Ogburn had a considerable impact on Burgess when he arrived, and Burgess supported Ogburn. He encouraged the development of multiple factor analysis by his students, and learned the technique from Lang and Cottrell, whom he had encouraged to attend Ogburn's class (Cottrell, 1972).

'Actually we were the first sociologists to use factor analysis on sociological data in the marriage prediction studies. It was not a central thing but a technique we tried out and got some help in the interpretation of the data.
Burgess had taken the opportunity, as a full professor, of attending Ogburn's quantitative courses and utilised the extensive quantitative expertise available at Chicago in his own research. This is clear from the development of his marriage adjustment research. In an enclosure to a letter to Wirth, Burgess (1935b) referred to the initial study of prediction of marriage adjustment he had undertaken in collaboration with Cottrell. The aim was to predict, at the time of marriage, the success or failure of the relationship. He referred to the fact that approximately one thousand schedules were secured and that a high degree of reliability was found 'as indicated by coefficients of correlation ranging from .86 to .96'. The construction of a 'marriage adjustment index out of eighteen items in the schedule' allowed for assignation of numerical values to adjustment in marriage. Thus they were able to identify the factors making for success in marriage. The further development of the research was in terms of weighting the most important factors and combining them 'for prediction by the method of partial and multiple correlation'.

The association between Ogburn and Burgess persisted until their retirement in 1952. In 1950, for example, Ogburn recorded that

'one of Burgess's students, Strauss, did some research once on the reasons for choosing a mate for marriage. He investigated three reasons.... I rang Burgess yesterday and suggested that he have another research done on these same couples to see which class of choice yielded more stability and which showed more separations and divorces.' (Ogburn journal, 11th November, 1950)

Throughout, however, Burgess was concerned to ensure the mutual development of case study and statistics. In the development of
a book on the 'Family in the Urban Community', for example, Burgess (1935c) proposed that the volume 'makes use of both statistics and case study material'. Over a thirty year period Burgess consistently argued for a methodological eclecticism and against monism. In many respects Burgess's methodological comments are a barometer of prevailing direction of methodological concerns in sociology in America from 1920 to 1950.

For Burgess development of prediction studies was of the highest 'theoretical and practical importance. Increase in the efficiency, precision, and scope of prediction is a chief aim of the social sciences as it is of all science.' (Burgess, 1941 p. 55).

This involvement in prediction studies and the development of quantitative research alongside case study was also reflected in his teaching. From 1919 to 1941 Burgess had taught on the 'Field Studies' course which gradually became a student practical, in that it was 'designed to provide direction and suggestion for either special research or a community survey' (Burgess, 1952). In 1940 he began teaching 'Methods of Sociological Research' which was described as

'Methods of historical research, field observation, mapping, interviewing, evaluation of human documents, and case study as used in sociology -- especially in human ecology and social psychology -- and the relationships of these methods to statistical procedures.' (Burgess, 1952)

This continued until 1944 when Burgess taught 'Introduction to Statistical Sociology' until 1947. This course involved

'Practical methods of analysing sociological data -- the questionnaire, graphical presentation, interpretation of statistics, the nature of statistical evidence, statistical fallacies'. (Burgess, 1952)

Meanwhile he began a course on the 'Problems and Methods of Prediction' (1941) which was concerned with the theory of predic-
tion and forecasting and included a comparison of 'statistical prediction and forecasts from case studies'.

Burgess was proud of his role in pioneering prediction studies (Burgess, 1941) and either directly, or through his students, was involved in a number of such studies in various areas including school and college success, occupational adjustment, criminal recidivism, mental breakdown, adjustment in army camps, juvenile probation, adult probation, cadet camps, and selection of occupation. The first marriage prediction study ever undertaken in 1931 was published in a complete report by L. S. Cottrell and Burgess in 1939 entitled 'Predicting Success or Failure in Marriage'. A progress report on this study to the Social Science Research Committee at Chicago in the Winter Quarter of 1931 noted of the study that

'its objective is to work out a method for predicting the statistical probabilities both (a) of the continuance of the marriage status and (b) of happiness in marriage. The schedule of eight pages is placed in the hands of persons who have been married from two to five years. The revised schedule includes the shorter personality test by Professor Thurstone which it is believed will be a valuable addition to the study. In addition to securing these schedules, a small number of case studies have been secured partly for the purpose of checking the schedule.' (Burgess, 1931).

Burgess' prediction studies had a wide impact, for example the work of Lewis Terman (Psychological Factors in Marital Happiness) and E. Lowell Kelly (Study of Engaged Couples,) used the Burgess-Cottrell scale of marital adjustment, while Clifford Kilpatrick (U. of Minnesota) and Harvey Locke (Indiana U.) undertook similar marriage study work (Burgess, 1941).
These studies gradually became a test of the case study as a means of objective data gathering, as Burgess turned more and more towards a factor analytic, correlational study of prediction. He noted (1941) that the prediction work was related to other work going on at the University of Chicago which

'has been most helpful in the development of methods of research. W.F. Ogburn, who has carried on considerable work in the prediction of political behavior has given valuable advice in the initial development of both the parole and marriage studies. L. L. Thurstone and his students have been of great assistance particularly in the application of methods of factor analysis (in our first study) and of matrix algebra (in the present study) to problems of prediction. S. A. Stouffer, who has developed a significant mathematical formula for the prediction of intermigration between cities has been increasingly helpful in giving advice and suggestions upon problems of statistical and case-study prediction. His interest in the possibilities of interrelating statistical and case-study techniques goes back to his doctoral dissertation [1930]. He has had a long interest in the problems of prediction as indicated by certain of his published papers. Dr. David Slight, who has given valuable advice on psychiatric aspects of the project, has shown a strong interest in which I equally share in the possibilities of working out a joint project on psychiatric and sociological phases of behavior important for prediction.' (Burgess, 1941, p. 58-59) [14]

This comment undermines the view that even if Chicago sociologists did use statistics they did not develop the rigorous hypothesis testing and cumulative theory development principles which came to be recognised as the hallmark of the post 1945 'Columbia School'.

The Chicagoans were also not unaware of the potential or possibilities of the large scale scheduled interview or questionnaire survey. Indeed, as Bulmer (1981) has pointed out, the earliest development of this form of data gathering for social scientific rather than official purposes (although a forgotten heritage) was the work done through the Local
Community Research Committee in the politics department at Chicago (see Appendix 2). The close interdepartmental ties, which had as one of its foci the Local Community Research Committee meant that the large scale survey was an accepted and developed tool of social science at Chicago.

Burgess (1916) assessed the potential of the social survey as a sociological tool, and by the early 1920s he was considering the likely effectiveness of a large scale interview based sociological survey. This is reflected in correspondence (Burns, 1924). In her paper 'The interview and statistical investigation', which Burgess read and retained, Mary Mark discussed the interview as a tool for getting information for statistical enquiry. The purpose of the interview is

'the securing of a group of associated facts. All the facts desired have been carefully pre-determined. The interview may be said to be concerned with the filling in of the blanks in a skeleton story.... In order to be significant the total number of these stories must be large, that is to say, statistical investigation is of the extensive rather than the intensive type.

Indeed, far from failing to develop the principles of quantitative research in sociology which were later to be associated with the 'Columbia approach', Chicago based sociologists were very much involved.

Sociologists, generally, played little part in inventing statistical techniques, but were active in utilising techniques developed in other disciplines. Here, Chicago sociologists were particularly active. Ogburn's (1929c) study of the presidential election of 1928 used partial correlation techniques and this was also developed by Burgess's students, notably Shaw and
associates (1929) in their study of delinquency areas. Similarly, Burgess, following Ogburn's lead, adopted the multivariate analytic and factor analytic approaches developed by Thurstone.

In his account of prediction studies, Burgess specified the following stages. Stage one is to locate the best criteria of adjustment. The second stage is to isolate the best predictive factors on three fronts: the past experience of subject; the present personality traits; and contingency conditions. The third stage is the combination and weighting of predictive items, using 'factor analysis and matrix algebra' (Burgess, 1941 p.53). The fourth stage is the assessment of the 'feasibility of prediction from intensive case studies to discover dynamic factors difficult to arrive at by schedules and to reduce to statistical formulations either (i) by case study analysis alone or (ii) in conjunction with statistical techniques' (Burgess, 1941 p. 54)

Chicago sociologists generally aimed at explanatory accounts in the Thomsonian mould. Burgess (1944) provided a clear designation of the explanatory process in quantitatively oriented nomothetic sociology when he outlined the five steps in any factor analytic causal attributable study. These are

1. selecting criteria (of success) \([Y]\]
2. selecting determining factors \([Xs]\]
3. establishing temporal precedence
4. correlating each \(X\) with \(Y\)
5. combining (useful) \(Xs\) into a prediction score for use on others not in the study.

This outline closely parallels the Columbia model of operationalised non-spurious time prioritised correlation. (Hirschi and Selvin, 1972; Boudon, 1974; Labowitz and Hagedorn, 1971).

Burgess's views on technique tended to be very flexible, his main concern was that society be conceived of as an organism rather
than as an aggregate of atomistic units. He was prepared, then, to countenance any method that may be of use in developing sociological theory within this overall perspective. Thus he embraced all methodological developments, including advanced statistical techniques, provided that they did not swamp his underlying methodological concerns. Thus he noted that factor analysis assumes.

'...that factors operate in the individual case as in the average of all cases in the sample of the population upon which predictions are based. This assumption does violence to the clinically-minded person who perceives in each case a unique configuration of dynamic factors.' (Burgess, 1944, p. 30).

In a paper read to the Annual Meeting of the Iowa Association of Economists and Sociologists in 1927 Burgess had asserted the need for empirical work in sociology and his fundamental view of the organic nature of society. He addressed himself directly to the role of statistics and argued that they had tremendous potential and utility if used in conjunction with other techniques and were located within the theoretical perspective he advanced. To adopt an atomistic statistical monism was, for Burgess, to ignore the relationships that lay beneath the surface of ostensive appearances. In this, he reflected Park's Jamesian concern about the 'reality behind the mask' and Thomas's distinction between attitude and value. He argued that statistics alone were inadequate because it was important to

'recognise that quantitative methods deal in the main with the cruder, more external aspects of human behavior, and that some other more sympathetic and discerning method is necessary to probe beneath the surface and to depict and analyse the inner life of the person'. (Burgess, 1927, p. 112).
His suggestion had been to proffer a mixture of case study and statistics and he argued that the two were not incompatible techniques. He reminded his audience that Le Play (commonly regarded, at the time, as the 'father' of statistical analysis in social science) introduced case study as the 'hand-maiden of statistics' and that Healy's well known statistical analyses into the causes of delinquency were elaborated by the use of case study material, which acted to provide Healy with the insights his statistical analyses alone were unable to provide. Burgess was not simply advocating uncritically or naively the use of case studies. He argued, for example, that they provided a way of testing hypotheses, and suggested that the first systematic use of case studies in this respect had been the Polish Peasant study (Thomas and Znaniecki, 1918). Burgess also pointed to the suggestions of Karl Pearson that scientific method involved classifying facts, noting mutual relations and describing their sequences, and argued that case study provided just such a facility.

Burgess did not assume that case study was an established technique, however.

'The assumption sometimes made that case studies and statistics were opposed to each other, or that statistics succeeded case studies in the 1920s, does not correspond to reality. Case studies and statistics developed side by side and supplemented each other. Students might use both approaches in their dissertations, as I did in my dissertation on Suicide (Cavan, 1928)' (Cavan, 1983, p. 414).

The debates about the relative merits of statistics and case study that peaked at the end of the 1920s and the mid 1930s were not debates about an old established method (case study) being
superceded by a new growing method (statistics). As Burgess
argued, the systematic use of case study as a method was in its
infancy in 1927 and, like the rapidly expanding field of
statistics, needed to be developed and nurtured. He advocated
quite specifically that the two techniques be embraced jointly.

'It is probably sufficient to point out that the methods
of statistics and of case study are not in conflict with
each other: they are in fact mutually complementary.
Statistical comparisons and correlations may often
suggest leads for research by the case-study method, and
documentary materials as they reveal social processes will
inevitably point the way to more adequate statistical
indices.' (Burgess, 1927, p. 120).

This is a position that Burgess addressed himself to consistent-
ly for the next 20 years. Throughout he attempted to develop
research which adopted the complementary approach, his emphasis
shifting, as he became more and more involved with prediction
studies, to evaluating the efficacy of each method for predic-
tive purposes. Thus as part of his research outline in 1944 he
pointed to how his results will provide an assessment of whether

'prediction by case-study analysis of dynamic factors is
superior or inferior to prediction by statistical
techniques.' (Burgess 1944, p. 54).

Even at this stage, having made enormous use of multivariate
analysis, factor analysis and other statistical techniques
Burgess is reluctant to concede ground to quantification and
treads the delicate line that divided the two sides of the
Polish Peasant debate (Social Science Research Council, 1939).

'The main research method relied upon by students of
personality, social organization and disorganization and
collective behavior has been the personal document.
Through letters, interviews and life histories data have
been obtained by which the processes of personal and
social interaction might be analyzed. Once a process is
identified and defined then it is possible but often
difficult to develop statistical methods for more precise
and accurate measurement as in the case of ideal type
concept already discussed.' (Burgess, 1944, p. 21).

Burgess persisted in his advocacy of the synthesis of case study and statistics and pointed to how such interrelation may be achieved. He maintained that case study is a useful adjunct to statistics at an exploratory level and that it is useful for interpreting statistical findings which would otherwise be unintelligible.

Of central importance for Burgess in the further use of case study was whether or not the problems of explanatory generalisation from case study data may be resolved. Burgess admitted that the strongest supporters of case study are those interested in idiographic rather than nomothetic study. Burgess himself, reflecting the long tradition at Chicago, is interested in nomothetic study and conceded that

'so far the data available in comparison of statistical and case study predictions appear to indicate the superiority of quantitative methods. This seems in large part due to the difficulty of controlling the personal equation of the clinical investigator who tends to judge cases on the basis of his training or personal experience.' (Burgess, 1944, p. 31).

In this respect Burgess tended to underplay the shift in the debate, fostered by Blumer, which centered on the conceptualisation process. However, he referred to the 'conceptualisation' debate when addressing the penchant for scaling. He warned against the vogue of scale construction lest it proceed without 'due care for careful development of prior conceptualisation'.

It was the critique of conceptualisation, especially of the problems and aims of operationalisation, which lay at the heart of the differences between quantifiers and methodological sceptics such as Blumer and Lerner.
Burgess was, then, both alert to and interested in developments in quantification in the social sciences outside of Chicago, notably the mathematical orientation espoused by Lundberg (1929, 1936, 1942) and attempted by Dodd (1940) and the sociometry of Moreno. Burgess saw sociometry as predated by Bogardus' 'Social Distance Scale' but as being a more systematic development of it, and thus, he argued, it contributed to the bridging of the gaps between social analysis and statistics by concentrating on group interactive processes rather than the conventional approach of statistical analyses which was to deal with aggregates of atomistic items. Thus he saw great potential in sociometry as a methodological tool in his advocacy of an eclectic approach.

However, this did not deter him from his main concern and contention that statistics and case study are mutually complementary. He argued that case studies suggest problems for quantitative analysis which need case studies to interpret results fully, which leads to more problems, and so on. In other words, the interactive use of case study and statistics is compatible with a cumulative development of knowledge model. Such a model underpinned American sociological research for half a century and became embodied in middle range theorising (Merton, 1948). This is further discussed in chapter five.

4.13 Quality or Quantity? The Chicagoans' view of Quantification.

The dichotomisation of the history of sociological research in the United States into qualitative and quantitative approaches
(Mullins, 1973) embodied in a view of a methodological struggle between Chicago and Columbia is not tenable (Wilson, 1940; Coleman, 1980). There is little substantial evidence for this view. The two universities were never in a position where they opposed each other on methodic grounds in any clear cut way. The two universities have been of more or less equal repute as centres for the study of sociology since the inauguration of their respective sociology departments. Chicago was probably in the ascendancy for a short period in the 1920s and Columbia in the 1950s. The change in fortune, however, was nothing directly to do with Chicago clinging to anachronistic qualitative methods. Indeed, by the 1950s Chicago had been developing quantitative studies for a considerable time. The review of the methodological interests of some of the key figures at Chicago clearly belies the idea that Chicago was out on a limb compared with the rest of American sociology. Nonetheless, there are commentators who suggest that such was the case especially after 1930 when its dominance of the discipline is seen to draw to a close. This is investigated in chapter seven below.

The analysis above has indicated that Chicago sociology did not exhibit tendencies towards a single methodic approach. Rather, it tended to be eclectic, and certainly changed throughout the first half of the century. It maintained at root a tendency to attempt to integrate both the subjective and the objective aspects of the social world, as it saw them. This, however, was not unique to Chicago, (see Social Science Research Council, 1939), but was one side of a long term debate about the nature of social reality.
This was apparent in the debates in the Society for Social research. In its regular meetings and annual institutes, both with a large proportion of visiting speakers, current debates were vigorously enjoined.

4.14 Methodological Debates in the Society for Social Research

The regular meetings of the Society for Social Research were very much concerned with methodology. They were forums which engaged different research ideas, techniques and procedures. As the Bulletin announced on a number of occasions, there was a diversity of opinion on methodological issues which ensured healthy, and lengthy debate.

'During the Autumn Quarter (1931) from 30 to 60 people attended each meeting an increase of from three to five fold over the meetings of five years ago. Never during this quarter did the discussion cease before the time of adjournment'. (Bulletin of the Society for Social Research, Jan. 1932).

Certainly there was no dogmatism as far as method was concerned, and statistics were part of the eclectic approach of the Chicagoans. The talks given during 1929 provide an example of the engagement with the developing statistical approach. These meetings were reported in the Bulletin (1930) as excellently attended and with 'enough conflict of ideas to insure lively and critical discussions'. The talks included F. N. Freeman from the School of Education on a statistical study of Foster Children, L. L. Thurstone from the Psychology Department on a statistical technique for comparing I. Q. of younger and older children, a debate between Blumer and T. C. McCormick on the logic and scope of statistical methods, and E. Faris critically examining the
value of life history documents as data in social psychology. Nearly all the talks that year were concerned with methodological considerations. The year was capped off by a farewell dinner for Park prior to his trip to Asia and Park summed up the tendency in the Department by urging 'sociologists not to lose their human interests while amidst their abstractions and their measurements'. (Bulletin of the Society for Social Research, 1930, p. 3)

Rather than advocate impressionistic research the Chicagoans were, as the meetings of the Society for Social Research show, very concerned with methodological aspects of research. Indeed, in the person of Ogburn, the Chicagoans were represented by a severe critic of impressionistic research in the President's Social Trends research. As is obvious from the minutes of his address to the Society for Social Research on March 6th 1933, Ogburn was far from happy with the contributions of the researchers to the Social Trends study. As chairman of the committee on research he was in the position of having to sharpen up the research, which he did by posing the question 'How do you know it?'. He noted, scornfully, that only approximately 20% of researchers could forecast on the basis of their study, and that much of the reporting provided a false impression.

The extent of the methodological debate is evident in the number of meetings in which methodological discussion was the prime focus. Forty five (33%) of the addresses were mainly concerned with methodology. In the period up to 1930 nearly half (48%) of all meetings had an address in which methodology was a major concern. After 1930 the proportion dropped to 32%. While the
addresses directed to methodology were mainly from sociology staff and students (48%), other Chicago faculty accounted for 31%. While about one in three of all academic addresses (faculty and students) were directed to methodology, the ratio dropped to one in five for external, non-academic speakers. Members of the Society were more than twice as likely to address the meetings on methodological issues (65%) than non-members (35%). (See Appendix 3).

In all, thirty five (26%) of all the talks given to the society were entirely devoted to methodological issues, these included two searching addresses by Blumer. The first (23.11.31) was in relation to his research into the effects of motion pictures on attitudes, in which he collaborated with Hauser and they used a combination of life histories and questionnaires. Blumer criticised existing methods of examining mass data. The case method, he said,

'...does produce a comprehensive record of individual experience, but attempts to classify such data into types have been disappointing. The statistical method is too abstract, limited to one or two points, and provides a formula which is interpreted in the light of individual experience. Alternatively, Dr. Blumer suggested the collection of a large number of anonymous personal narratives related to the particular experience under investigation. If in these records an extreme form of experience appears in a few cases this may be indicative of a tendency toward the particular type of experience.' (Bulletin of the Society for Social Research, Jan 1932)

The second (4.3.1935) was on

"The Search for Method in Sociology". Preoccupation with method is not due to dissatisfaction with results obtained within the field of sociology. Rather it is born out of a desire to be accepted as "scientific" by other sciences. Such courting of favor has had disadvantageous consequences. Sociologists tend to become constantly dependent upon other sciences for the framework inside which their work shall go
on. They become exceedingly self-conscious regarding method and thus are led to restrict the area of their investigation to such problems as will easily lend themselves to methods and techniques accepted without question by other sciences. They have come to place an exaggerated importance, for instance, upon quantitative procedure, as witness the present extensive volume of statistical work. Its extent cannot be explained by its success inside the field of sociology, but by reference to the prestige of the physical sciences....

The discussion which followed Professor Blumer's talk revealed "...interesting differences of opinion." (Bulletin of the Society for Social Research, March 1935, p. 4)

It is notable, in this, that Blumer made the sort of comment that C. Wright Mills (1959) found it necessary to restate a quarter of a century later. In addition, Blumer's (1931) celebrated paper 'Science Without Concepts' was first presented at the Ninth Annual Institute of the Society for Social Research (1930).

The concern with methodology in the Society reflects the discussion of methods and methodology in the doctoral dissertations at Chicago. The sample of theses shows that thirty two (76%) discussed methods and twenty one (50%) included methodological or epistemological discussions. Methodic discussion increased from sixty five per cent up to 1940 to ninety per cent of theses after 1940, there was a less dramatic and statistically insignificant rise in methodological discussion from forty eight to fifty five per cent over the same period, (see Appendix 6).

4.15 Chicago Eclecticism

The Chicagoans did not represent one side of a dichotomised view of methodic practice. The 'Chicago School' was not simply anti-statistics. The rapid development of statistical analysis in social science generated some reaction from interactionists who
were sceptical about, or hostile towards, what they saw as a tendency to set aside the subjective element. Burgess is the archetypal case, despite a positive attitude towards the potential of statistics, he also displayed a certain scepticism towards quantitative techniques. In this he reflected not only Park and Thomas but the prevailing attitude within the discipline. His changing stance, however, from initial scepticism of statistics to enthusiastic usage in prediction studies is indicative of the changing nature of the view of sociology as science in the United States.

Shaw is a prime example of the eclecticism of Chicago sociologists. In his studies in the late 1920s and 1930s he utilised spot maps, statistics and case studies. He was not prepared to abandon case study for statistics maintaining that the 'case study method emphasises the total situation or combination of factors, the description of the processes or sequence of events in which the behavior occurs, the study of the individual behavior in its total setting, and the analyses and comparison of cases leading to the formulation of hypotheses.' (Shaw, 1931, p. 149).

Shaw was a student of Burgess who, as suggested above, can be seen as the barometer of methodological change at Chicago. A barometer affected not only by local pressure but also by changes in the wider sociological milieu. Burgess was far from alone in adopting this eclectic approach as has been suggested. Wirth, for example, in outlining a study of the black community in Chicago in 1939 indicated the use of three methodological approaches. These were the 'methods and concepts of the students of human ecology', 'the viewpoints and methods of those who have approached modern communities from the standpoint of the cultural
anthropologist' and the 'life history method'. This necessitated three sources of data, statistics, interviews and life histories.

'The statistical material provides the background for the entire research. A statistical study of the growth of the Negro community has been made, as well as an ecological, a demographic and an occupational study. All available statistical sources are being used to check the interview materials and non-statistical data. The interview method has been relied upon to define and document the description of the social structure. The persons interviewed have been selected with reference to the various fields of major interest into which the study is divided. Approximately eight thousand interviews have been taken in the community. Life histories ranging from one hundred to seven hundred and fifty pages in length, were obtained from twenty five persons. These have been of value in showing the impact of the culture upon the individual by portraying the social structure as it appears to the individuals living in it, and by indicating the adjustment of the individual to the culture, its sub-societies, and the total society throughout his life career.' (Wirth, 1939).

The sample survey of doctoral theses (Appendix 6) clearly reveals the eclectic nature of the methods adopted at Chicago. Only two theses were dependent on a single method and just five more relied on two methods. The range of data generation devices utilised, in some degree, by the Chicagoans surveyed were from literature review (adopted by 90%) through historical analysis (59%), document analysis (51%), informal interviews (43%), observation (36%), scheduled interviews (31%), life histories (30%), to questionnaires (14%).

Preference for methods shifted over time with literature review, historical analysis and document analysis dropping from a major technique adopted by between forty and sixty per cent of authors prior to 1940, to a mere five to twenty per cent. after 1940, and life history dropped out altogether as a major technique, being replaced by informal interviewing of a less strenuous type (a rise of from 5% to 50%)
Conversely, scheduled interviewing, and participant observation which had not been used at all as a major technique before 1940, were adopted as a major technique after 1940 in sixty five per cent and twenty five per cent of theses respectively. Questionnaires increased in major usage from nine to twenty per cent in that period. The prediction study work of Burgess et. al. was also important in pioneering quantitative orientations to traditionally 'qualitative' areas of sociological enquiry. This work relied heavily on Thurstone, Stouffer and Ogburn.

4.16 The Interdisciplinary Network of Quantifiers at Chicago.

Through these 'quantifiers' the Chicagoans contributed considerably to the development of quantitative as well as qualitative techniques. Thurstone's work on attitude scaling, factor analysis and multivariate analysis and the adaptation of the advanced correlation techniques of Pearson and associates for sociology and the development of prediction studies which took place at Chicago were important contributions to the development of quantitative analysis in sociology in the United States. The development of the 'Columbia style' had been predated by interdisciplinary work at Chicago. Central to this was not only Thurstone's work but also the large scale surveying in politics. Bulmer (1981) argued that although the large scale social survey was not used by the sociologists at Chicago, the politics department, with whom Ogburn had close ties, did pioneer the method that became so synonymous with Columbia University after 1945, (Merriam and Gosnell, 1924; Gosnell, 1927 and White, 1929).
A further important development was the critical work of Stouffer on case history (i.e. application of attitude testing to sociology) and the incorporation of the advances in British statistics following his year in London (1931-32) studying with Pearson, Yule, Fisher and Bowley.

Ogburn had a mediating role in all this, being a tutor and advisor to Stouffer, adapting and developing some of Thurstone's work and certainly advancing it, and assisting Burgess. However, much of Ogburn's own interest was in longitudinal analysis, notably time series analysis embodied in the social trends work, (one issue of American Journal of Sociology each year from 1927 to 1934 was an issue on Social changes) and the work on the President's committee which provided another aspect of the quantitative work in social science and followed on from the similar kind of work being done in economics (Mills, 1924).

There was no suggestion at least until the 1950s that Chicago sociology (in-exile) was 'non-quantitative'. Chicago had no repute for being hostile to statistics amongst contemporaries, rather the reverse was more likely the case given the reputation of Ogburn, Stouffer, Stephan and later O. D. Duncan and Fisher. The analysis of the 'coup' of 1935, in chapter six, points to an alliance between the Chicagoans and other quantitative sociologists. In addition there were strong quantitative social science links provided through the inter-departmental research committees. Indeed, when Wilson wrote to Burgess in 1940 to recommend Whyte, one of the reasons was because

'I think he feels he needs more statistics than he has had.'
The quantitative research at Chicago was not, then, undertaken by individuals working in isolation. Rather there was a strong network of quantitative practitioners within the University.

This can be seen in the close ties between quantifiers at Chicago, notably Ogburn, Thurstone, Gosnell, Douglas and Schultz and the connections they made with the departments of mathematics and biology served as a supplementary interdepartmental network which developed considerable research work. These ties were not merely transitory and there seemed to be a genuine concern that this area of work be harmoniously promoted at the highest level. As late as 1945 Stouffer, serving in the research unit of the War department at Washington, wrote to Walter Bartky head of the department of mathematics to recommend Guttman.

'Guttman's primary interest is in making basic contributions to social science. In that connection, he is, of course, very much interested in probability theory as well as in the theory of measurement. At the University of Chicago he would be a yeast which would have its influence throughout the social sciences. It seems to me that he would be a most useful addition to the committee on mathematical statistics, as well as to the social science division, and the critical Chicago atmosphere should stimulate him to continual new creative development.' (Stouffer, 1945).

Stouffer refers to Guttman's analysis and ideas as 'astonishing', as being able to 'cut like a clean knife through the subject' [of prediction of personal adjustment], that his promise has been 'fulfilled even beyond my most optimistic expectations'

'He has invented new devices of scale construction and new methods of computation which have been the basis of our work. In addition his advice to members of our staff has been uniformly brilliant and practical....Guttman has a mind like Rashevsky's...[biology at Chicago] he is the most creative and original thinker...'

Bulmer (1981) has suggested that the interdisciplinary nature of the development of statistical methods at Chicago has been a
factor in their being ignored by historians of sociology. Chicago sociology, he illustrated, was far more quantitative than the myth of the 'School' suggests.

The development of quantitative techniques at Chicago has perhaps become a 'lost heritage' because of its interdisciplinary nature. However, given the long term of this development and the significant role played by Chicago personnel in the development of quantification in social science, this is a rather too simple answer. Quantification at Chicago seems to have been deliberately ignored by historians who have been more concerned to explain the relationship of symbolic interactionism to the early 'pragmatic' base in the department.

4.17 Conclusion

It would seem that the idea that Chicago sociology was remote from the concerns of quantitative sociology has arisen through various factors. Artificial divisions have been created by historians and the 'Chicago School' located on one side of these divides. So the 'School' is seen as supporting case study rather than statistics, as undertaking participant observation research rather than quantitatively based surveys, as ignoring quantitative techniques rather than developing them. The single most powerful component of this view is the assumed 'anti-statistical' perspective, derived from Thomas and Park and proposed as an alternative to 'positivistic' sociology in the work of Blumer. This avowed qualitative perspective is seen as clearly indicative of the school and opposed to the development of quantitative
Such a perspective distorts the sociological enterprise at Chicago, not least by assuming a coherent 'School' opposed to prevailing methodological, epistemological and theoretical concerns of the discipline at large [15]. Such a position is presumed to be reflected in the advocacy of the theories of G.H. Mead by the Chicagoans. The following sections will explore the epistemological and theoretical orientations at Chicago, in relation to the perspectives in sociology in the United States in general. The role of Mead, in particular, will then be critically assessed. The discussion above has suggested that in terms of method and the development of quantification in sociology, Chicago was an integral part of American sociology.

Furthermore, the Chicagoans did not develop a phenomenological alternative to the prevailing nomological approach. Indeed, a cumulative theory model, grounded in a falsificationist approach to theorising can be seen to have underpinned the work of Chicago sociologists and was intrinsic to the general approach adopted in the United States. This will now be examined in more detail.
NOTES TO CHAPTER FOUR

1. Case study refers to individual cases, particularly information achieved through personal documents either existent or derived through interview or written by the respondent. This usage does not necessarily coincide with current usage. Chapin (1920) referred to case study as a 'technique for an intensive and many-sided study of the individual compared to the sampling of a group and the enumeration of a community. In the 1920s, too, 'field work' referred to all empirical data collecting techniques, unlike its more usual usage today which implies ethnographic study, rather than scheduled interviewing.

2. Interactionist sociology is the term used for sociological perspectives which were concerned primarily with social interaction. As Rock (1979) demonstrates, this was grounded in German social philosophy and American pragmatism. Interactionist sociology has taken various forms, but in the early period at Chicago its principles are clearly stated in Thomas and Znaniecki (1918). The work of the Chicagans can be identified as broadly interactionist. Out of this (as Fisher and Strauss (1979) suggest) emerged a specific approach labelled symbolic interactionism, which was developed as much away from Chicago as at it.

3. Complete participant observation refers to those instances where the researcher takes on the role of the group under observation and joins in on a more or less full time basis. Partial participant observation are those situations where the researcher merely engages as a participant observer on a part time or convenience basis.

4. Anderson was not an atheist or agnostic and he suggested that he had a two year struggle relating his social scientific and notably Darwinian evolutionary views to his Mormon fundamentalist background. This reflects similar views of graduates of the 1920s, as seen in chapter three.

5. Apart from Zorbaugh's interest in the Near North Side,

'W. P. Ireland and his wife have been carrying on research into the problems of the rooming house world this last year by operating a rooming house of their own on North Clark Street.' (Bulletin of the Society for Social Research, Dec. 1930, p. 4)

Like Zorbaugh's study, this research too was not referred to as participant observation.

6. DeGraff was a graduate student at Chicago who was awarded his Ph.D. in 1926 (DeGraff, 1926). What exactly the 'DeGraff Incident' amounted to is not clear as no other reference to it could be traced.

7. As late as 1947, Wirth (1947) in reviewing the development of
sociology over the previous thirty years suggested that not only had there been a move towards specialising in specific areas but also that sociologists needed specialised skills for empirical work. Wirth listed such skills which did not include participant observation as such and was dominated by quantitative techniques; social statistics, sampling, population analysis, personal documents, prediction methods, attitude testing, public opinion polling, questionnaire construction, field interviewing, and the mapping of social relations.

8. See chapter seven

9. There are various notions about the wars and battles fought between the quantitative and qualitative traditions and the exponents of particular positions reconstruct these encounters to project their side in the most favourable light. This leads to contradictions and confusions. Some wars are forgotten, other battles given exceptional prominence, and so on. Thus Blumer is seen as a major standard bearer by some historians of sociology, the early Chicago School, by others. Yet others do not seem to distinguish between the two. Howard Becker is sometimes portrayed as the principle mover in a late rearguard action mounted by qualitative sociologists which became, eventually, necessary following the coup in the American Sociological Society in 1935. Others regard this as superficial posturing, the war having already been lost in the case study vs statistics and life history vs attitude scale debates of the 1929-1931 period. Ethnomethodologists, on the contrary, see the real battle beginning only in the late 1960s when, for the first time, the focus of qualitative sociological enquiry was radically questioned. In short, battle has been joined since sociology became an empirically orientated pursuit in the United States and will probably continue to be joined while a nomological prescription informs all spheres of science.

The reconstruction of the qualitative-quantitative debate has frequently been in combative terms but this tends to exaggerate the division. While certain elements in American sociology have conflicted, there is an enormous middle ground which has tended to avoid such conflict. Eclecticism of method, and a lack of epistemological dogmatism has prevailed rather than rigid adherence to singular orientations. This has been, possibly, at least in part, a function of the distance American sociology (and social psychology) has maintained from its European counterpart. American sociology has not developed a full fledged phenomenological, structural, hermeneutic or critical-dialectical sociology and has maintained a nomological orientation.

10. In the same year Lindesmith (1937) also received a doctorate for research on opiate addiction, a study which was more dependent on in depth interviewing. Dai, had reported on his research to the Society for Social Research in April and the minutes record that it was a study based on 'a mass of statistical data' and 'repeated, or protracted interviews with
addicts', fifty of whom have been interviewed once a fortnight for a year. In addition

'statistical data is being collected for a five year period from 1928-34 on 1219 cases from the Narcotic Bureau and 326 pedlars from the same source, 834 cases from psychopathic hospital, 429 cases from the city police records, 118 cases from the Women's Reformatory at Dwight, 193 cases from the Probation Office, 70 cases from the Municipal Psychiatric Clinic and a few cases from a Behavioral Clinic for a ten year period from 1923 to 1934 and 359 cases from the Keeley Institute' (Minutes of the Society for Social Research, April 8th 1935)

The types of conclusion drawn from the data are simple correlational, e.g. addicts live primarily in the zones of transition, very few crimes of violence are committed by drug addicts, most addicts start between the ages of 20 and 25, the majority of Chicago addicts were born in other states, addiction is negatively correlated to level of formal educational qualification.

11. Blumer actually used these terms in a paper of 1954. However, he developed the thesis much earlier in Blumer (1931) and later specifically addressed social psychology (Blumer, 1940). In the paper of 1931 he concluded

'What I would declare, then, is that to use concepts in science as natural ultimates instead of tentative convenient conceptions, or to be uncritical or unreflective as to their import, is not likely to lead to genuine understanding and control'. (Blumer, 1931, p. 170).

It was to this sentiment, that Lundberg responded in 1936 with his comments on operationalisation which Blumer (1940) discounted arguing that

'The improvement in judgement, in observation, and in concept will be in the future, as I suspect it has been in the past, a slow maturing process. During the process the concept will continue to remain imprecise, but it should remain less so as observation becomes grounded in fuller experience and in new perspectives. Even though imprecise, the concept will serve, as it does at present, to help direct the line of observation and to help guide the forming of judgements involved in that observation.' (p.182).

12. Ogburn gave up teaching statistics some time before his retirement to concentrate on his substantive interests in sociology. In his journal he reflected on whether he had made the correct decision.

'Last evening, Rubyn, Harriet Welch and I had dinner at the Quadrangle Club. Nearby at a table for four sat R.A. Fisher, famed statistician with Allen Wallis and Thurstone, two U. of C. statisticians. Allen Wallis is editor of the American
Statistical Journal and Executive Secretary of the U. of C. Committee on Statistics. Thurstone has developed several important techniques and published several books on statistical methods. At lunch I saw a group of some 20 persons in the private dining room at a table where R.A. Fisher sat. I judge we gave him an honorary degree. I had no part in any of this. I was not invited or consulted. It was not because I was an emeritus. Thurstone become an emeritus this July. I was away nearly all year. But that does not explain my not being in on this statistical gathering. the pang of not being invited is a feeling I have seldom experienced.

But twenty-five years ago, more or less, I was editor of the American Journal of the Statistical Association and also president of the American Statistical Association. Then, I think the nearest to an academic ambition I ever had was to be a social statistician, which is the most exact of the scientific activities of a scientist in the social field. Yet about 1912 or thereabouts, I recall definitely rejecting the idea of being nothing but a scientist and thus of restricting my intellectual,interests .... But then came the depression of the '30's, the War and there was competition for my interests. I became more and more interested in the significance of technological change for society. I spent lots of time on Recent Social trends, a big undertaking. Then I gave up teaching statistics. But I envy those who stayed by statistics, and sometimes I think I wish I had. Clearly this envy and this regret are strong emotions. But I wonder much rationality there is to making this emotion the measure of my values, or the criterion of my action. My worship of statistics has a somewhat religious nature. If I wanted to worship, to be loyal, to be devoted, then statistics was the answer for me, my God. But a God only meets an emotional need, which has little to do with reason. I wonder would I have been content to have been only a very good statistician; and to be a good one, all one's effort and attention is needed. I doubt it. My work in technology and social change and social evolution give me much intellectual pleasure and many thrills. Yet I regret keenly that the march of statistics has passed me by. There was a vacant place at R. A. Fisher's table.' (Ogburn Journal, 14th June, 1952).

13. Burgess (1935a, p.1) began

'The purpose of this study is to assemble, present in tabular and graphic form, and interpret the materials now available through the census and other sources on the local communities and sectors of the city of Chicago. At the present time these data are available in raw and unanalysed form which makes them difficult to use for any specific practical purpose.'

These other sources included social history data on local communities collected by V. Palmer. The Social Science Research Committee at Chicago agreed to underwrite publication costs, and the research was seen as part of a programme of work involving
members of the Committee on the history, demographic and socio-economic analysis of Chicago.

14. This quote summarises Burgess' major research interest from 1930 to 1940 and shows how it relates to other research interests at Chicago and these are primarily quantitative. It reflects a wide base of interest, psychological and psychiatric as well as sociological. However, one must read this critically, in the sense that it is an application for funding, that an experienced proposal writer like Burgess will aim to include those elements he thinks will be well received and therefore may be including an 'overloading' of quantification background in order to secure funds in a climate which is more hospitable to quantified research. On the other hand, this represents a genuine interest for Burgess and reflects a concern with relating case study and statistics which has been central to Chicago research for twenty years. It reflects the interests of the Social Science Research Council as evident in the 1939 debate on the Polish Peasant and guidelines issued in the late 1930s and early 1940s, and had been a subject of debate in, for example, the American Sociological Review since 1936. Again, one might see Burgess as reflecting external concerns in his proposal, but these concerns were promoted by, among others, the Chicago sociologists (albeit from different sides). The 'canny' side of the proposal is perhaps reflected in Burgess' next paragraph which reflects the national/institutional interest in the area.

'At the present time under the auspices of the Social Science Research Council a sub-committee on Predictive Methods in Social Adjustment is making a comparative study of methods of prediction now in use in the field of school success, vocational adjustment, marriage adjustment and criminal recidivism. The members of the sub-committee are Mr. Stouffer, chairman; L. S. Cottrell, Cornell University; E. Lowell Kelly, Purdue University and E. E. Richardson, U.S. Civil Service Commission with Paul Horst, psychologist, Proctor and Gamble, making the report. The report together with its recommendations will, in my judgement, give a great impetus to the improvement of present methods and to the rise of the level of research in this field.' (p.59)

This latter point, novel method, is an area of major concern for the Social Science Research Council as shown in its circular of 1945.

15. Hammersely and Atkinson (1983) in assessing the unique contribution of ethnography, reflect the perspective adopted by the Chicagoans. Hammersley and Atkinson suggested that, unlike both sides of the 'positivist-naturalist' dispute, ethnography brings social science and its object closer together. Involvement in the field is a process which, if nothing else, leads to the challenging of the sociologists 'dangerously misleading preconceptions'. More importantly, ethnography, they argued, is valuable for its development and testing of theory. The need for this attempted reconceptualisation has, it is suggested here, arisen through the dichotomisation generated by the prevailing
history of American sociological research. The Chicagoans did not address a phenomenological sociology but rather attempted the development of theory through a 'naturalistic' approach embedded in a 'positivistic paradigm', and utilising typification processes to help achieve explanatory power.
CHAPTER FIVE

CHICAGOANS AS ATHEORETICAL EMPIRICAL RESEARCHERS
5.1. The Myth

It is ironic that, although Chicago sociology is seen to dominate the development of the discipline in the United States for several decades (Martindale, 1976), it is commonly held that the Chicagoans produced little of theoretical import, at least for contemporary sociology. Chicago's oral tradition (Rock, 1979; Fisher & Strauss, 1978; Huber 1973a) may have given the impression of a lack of theoretical development and accounts of Chicago sociology tend to propound a myth which emphasises the empirical nature of the work done at Chicago. The Chicagoans are portrayed as concerned only with describing the world, particularly Chicago, irrespective of, or even in reaction to, theoretical concerns.

'The Chicago School of Sociology, motivated by the journalist's campaigning and documentary concerns was the example par excellence of determined and detailed empirical social research.' (Taylor, Walton and Young, 1973, p. 110)

In the period prior to the second world war, the Chicagoans' theoretical contributions are usually seen as restricted to the field of urban sociology. While being characterised as pioneers in this realm, the output of the 'Chicago School' is seen as rather limited and restricted to models of the growth of cities (Easthope, 1974; Giner, 1972; Rock, 1979). It is this role as urban sociologists which is among the more enduring aspects of the myth of the Chicagoans' atheoreticism. There are still a number of commentators who see the Chicagoans as engaged principally in the pursuit of urban sociology and who refer to the 'Chicago School of Urban Sociology', (Choldin, 1980; Caldarovic 1979; Oliven, 1978; Haussermann & Kramer-Badoni, 1980;
In the period following the war, the empirical activity of the Chicagoans is again highlighted and theory is seen as a secondary consequence. This post war period is usually seen in terms of the development of deviance studies, with 'labelling theory' as the major theoretical contribution.

'The term 'Chicago School' has been used to designate a whole group of sociologists working at Chicago during this period [1920s & early 1930s]. Their major interest was in the city, and in the work of men like Robert Park and Louis Wirth they laid the foundations of what was to become the special field of urban sociology. They emphasised field work, that is, going out and collecting data rather than sitting in a study and spinning out theories. As Park kept advising his students: 'Get your hands dirty with research!!' The Chicago sociologists also had a special affinity for social phenomena that were deviant or far-out in some way. Thus they produced a string of monographs in various colourful corners of urban life, such as the world of skid row or of crime. The Chicago School was also the beginning of what was later to be called the sociology disorganization or of deviance.' (Berger P.L. & Berger B. (1976) p. 48)

In consequence the Chicagoans tend to be viewed as peripheral to the development of theoretical sociology in the United States. The Chicagoans are seen as more and more anachronistic in their concern for empirical detail at the expense of developing rigorous theoretical propositions (Madge, 1963, p. 110; Brake, 1980, p. 30). In the event, they are seen as more or less taken by surprise, and therefore excluded from, the 'grand theoretical' or 'middle range theoretical' developments embodied in structural functionalism. In short, the Chicagoans are portrayed in terms of a desire to collect 'facts' irrespective of theoretical concerns, (Rex, 1973).

This chapter examines the work of the Chicagoans to assess their
contribution to theoretical development. The next section looks at the extent to which the empirical work at Chicago was developed at the expense of theory. This is followed by an assessment of the degree to which the Chicagoans were urban sociologists. The theoretical work of the Chicagoans as a whole is considered both in terms of the conceptual development and the contribution the Chicagoans made to substantive sub-disciplines within sociology. The final section considers the theoretical work of the Chicagoans in the general context of the development of sociological theory in the United States.

5.2 The Empirical Approach of the Chicagoans

From its beginnings, the department of sociology at Chicago was in the vanguard of attempts to develop empirical research in sociology. Opposed to the general theoretical conjecture which had informed nineteenth century sociology, the turn of the century saw the beginnings of an attempt to merge social surveying with sociological theory. While not alone in this endeavour, the Chicago sociologists were very much involved in the call for empirical investigation as a basis for theoretical development.

5.2.1 The Concern with the City of Chicago

The city of Chicago was rapidly expanding during the period from 1890 to 1920 and became a focal point for a considerable number of exploratory studies. This has led to a view that the Chicagoans, notably Park with his journalistic background, were
concerned primarily with describing facets of the city of Chicago rather than developing a theoretical sociology. The implication is that the Chicagoans more closely resembled demographers than theoretical sociologists.

Thus Rock wrote,

"Park exhorted his students to chronicle the myriad phenomena that were developing in the Chicago of the 1920's and 1930's. For a time at least, Chicago sociology was virtually identical with the sociology of Chicago. It was nursed as a cartographic exercise, studying Little Sicily, the Jewish ghetto, Polonia, the Gold Coast, the slums, Hobohemia, rooming house districts and the gangs of the city." (Rock, 1979, p. 92).

The concern with the city of Chicago as a subject for empirical investigation pre-dated Park. Small argued that the Chicagoans should make the most of their surroundings for research purposes and insisted that sociology could and should be greatly developed through empirical study. Henderson and Talbot were involved in and encouraged empirical enquiry as part of their concern with social issues.

The empirical approach at Chicago began as early as the last few years of the nineteenth century (Dunn, 1895; Clark, 1897; Bushnell, 1901; Gillette, 1901; Riley, 1904; Fleming, 1905; Rhoades, 1906) but became more systematic after the 'Polish Peasant' study, researched in the earlier part of the decade, was published in 1918 (Thomas and Znaniecki, 1918). Although not an active empirical researcher himself, Small came more and more to advocate direct observational study (Dibble 1972).

Two early graduate students tenured by the department, Vincent and Thomas, were schooled in this 'empirical' environment.
Thomas did considerable 'legwork' for Henderson while Vincent provided an impetus to empirical study and also co-authored the 'laboratory manual' with Small which was probably the earliest text to outline an approach to empirical sociology (Small and Vincent, 1894). Following Vincent's departure in 1908 to take the post of President of the University of Minnesota, and with Thomas becoming more and more involved in the Polish Peasant study and the start of a shift away from applied to pure research, there was a lull in the output of theses on aspects of Chicago. Nonetheless, Small, influenced by German sociology, continued to advocate direct empirical work and encouraged Thomas and later, through Thomas, Park to set about a more detailed and systematic analysis of the city of Chicago, which was substantially influenced by the emergent German urban sociology of the early part of the century (Smith, 1979).

On his arrival at the university, Park took up the cue and for fifteen years actively encouraged students to undertake empirical research, much of it in the city of Chicago. In 1915 he wrote an article entitled 'The City: Suggestions for the Investigation of Human Behavior in the City Environment' (Park, 1915) which outlined areas for investigation and suggested procedures for action. This article, (reprinted twice in different compendiums with some revisions) is seen by many commentators as the start of the intense period of empirical activity at Chicago which sometimes goes under the label of 'The Golden Era of the Chicago School'.

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5.2.2 The Golden Era Studies

The famous studies of the 'Golden Era', such as Zorbaugh (1929), Cressey (1929), Cavan (1926), Thrasher (1926), Shaw et al (1929), Anderson (1923) and Landesco (1929) are noted for their lack of overt concern with theoretical issues. These studies, which tended to attract most attention at the time and subsequently, have been the ones which provide documentary descriptions of little known or researched social phenomena and serve as social historical texts. For that reason they have been more durable while not necessarily representing the theoretical concerns of the Chicagoans. Neither Cressey's, Zorbaugh's, Shaw's nor Anderson's work were doctoral dissertations.

Cavan (1983) has suggested that an interest in an area was all that was required in the 1920s, that no formal hypotheses, representative samples, control groups or rigid data collection methods were necessary in planning research. Tentative generalisations were made but these were purely 'concept identification' and the location of social processes in an exploratory way was all that was involved. The process was a 'gaining of insights', it was not theoretical, rather it constituted the preliminary stages of science. The period was 'not a time of theorising. Rather it concentrated on collecting facts, grouping them under concepts, and/or identifying relationships among them. These facts, concepts and relationships might be compared to building blocks; the construction of theories was to come later.... Thomas recognized the need for developing theories ... Wirth seemed opposed to theory construction. The time had come to theorize, but Chicago sociologists seemed reluctant to take this step.' (Cavan, 1983, p. 416). [1]

This reflects a general view of the Golden Era as lacking
theoretical orientation. Bierstedt (1981, p. xi), for example, regarded Park as having had a great influence over his students but that he 'exhibited little interest in sociological theory'. An often cited quote from Park is used to support this view.

'You have been told to go grubbing in the library, thereby accumulating a mass of notes and a liberal coating of grime. You have been told to choose problems wherever you can find musty stacks of routine records based on trivial schedules prepared by tired bureaucrats and filled out by reluctant applicants for aid or fussy do-gooders or indifferent clerks. This is called "getting your hands dirty with real research". Those who counsel you are wise and honorable; the reasons they offer are of great value. But one more thing is needful: first hand observation. Go and sit in the lounges of the luxury hotels and on the doorsteps of the flophouses; sit on the Gold Coast settees and on the slum shakedowns; sit in the Orchestra Hall and in the Star and Garter Burlesk. In short, gentlemen, go get the seat of your pants dirty in real research.' (Park, 1920).

This quote is usually annotated to Park telling his students to 'get their hands dirty with research', (e.g. Berger and Berger, 1976). The implication is that Park extolled the virtues of empirical data collection at the expense of theoretical endeavours. This actually misrepresents what Park said. Park was not simply calling for empirical data instead of theoretical conjecture but was demanding a combination of direct empirical and theoretical work, and suggesting that documentary sources are of themselves insufficient without some first-hand experience of the social world.

The retrospective views and the research work of other Chicagoans confirm the importance of theorising during the 'Golden Era'. Ogburn, for example, endorsed Bierstedt's view that Park had a great influence over his students but noted that

'I saw little of [Park] and almost never was in a conversation or discussion with him; and yet I admired his
contributions to sociology, which were ... a contribution of concepts, well thought out, and well selected as to importance. I cannot recall any research he ever did, yet his concepts were a real contribution and have been adopted widely by sociologists.' (Ogburn journal, 4th April 1955)

Cottrell (1972) remarked that there was a lot of German sociological theory, for example, infused into Chicago, which reflects the content of the Park and Burgess text (1921). Dollard went further and inverted Cavan's recollections

'My notion about sociology was that it was wildly theoretical and verbal and philosophical but through Ogburn I saw that something could be gathered which was very tangible.' (Dollard, 1972)

Hayner, clearly pointed to the mix of theory and empirical data, which Park offered.

'At the time we thought we were getting too much philosophy from Park, but in retrospect that is what we needed. We needed his ability to have concrete experiences and then generalize significantly from that experience.' (Hayner, 1972)

These recollections indicate a concern at Chicago for both empirical enquiry and theoretical development. It may be that the staff, notably Park, were too helpful or overbearing in providing theoretical frameworks and concepts as some of the interviewees suggest. This may have prompted Cavan and others to feel dissatisfied with the theoretical endeavours at Chicago.

'The 'hotel life' thesis came really from Park, he was pushing studies down in the Loop district and that appealed to me... Park had wanted me to study the slum but I didn't want to study the slums. There was another girl who was more interested in slums. So I said, 'you can have it'. (Hayner, 1972)

'I'd rather do my own thesis and not have one handed to me by Park.' (Faris, R., 1972)

The development of 'inductive theorising' through the attempts to generalise empirical observation certainly involved elements of
what Cavan called 'concept identification', but also amounted to more than a construction of building blocks.

The recollections of the Chicagoans are borne out by an inspection of their work. While the empirical study may have been prompted by an interest in an area, the students were expected, and helped, to locate their data in a general theoretical framework, and indeed, even the famous studies of the period were far less bereft of theory than some commentators suggested (Madge, 1963). Indeed, the survey of Ph.D. theses (Appendix 6) shows that eighty six per cent were directly concerned with specific theoretical issues. Some of these, particularly the work of Young (1924), Simpson (1926), Blumer (1928), Neumeyer (1929), Brown (1930) and Stonequist (1930), were directed entirely to an analysis of theoretical constructs.

Why the view that such research was atheoretical should have grown up is not easy to pin down, other than to suggest that a selective reading of research work may have been responsible. The myth, then, becomes self-perpetuating. Theoretical concerns are not seen as central to Chicago sociology and thus the theoretical contribution is ignored. The style in which most of the work published in the University of Chicago Press Sociological Series is written may also have contributed to the view that theory was of little importance to the Chicagoans. The tendency was for them to present their sociological enquiry in 'ordinary language', which possibly led to an underestimation of their theoretical content. The utilisation of a simple documentary style and the extensive incorporation of subject's verbal and
written comments, possibly serves to deflect the reader from the social theoretical content.

Further, it may be that, in a period of rapid development of sociological conceptualisation, some of the pioneering empirical studies of the Golden Era which were researched in the 1920s were not as 'polished' theoretically as they might have been when finally published in the Sociological Series of the University of Chicago Press in the 1930s.

Whilst attempting to infuse empirical observation into sociological research, the Chicagoans were not, however, unconcerned with sociological theory. The nature of the theoretical contribution of the Chicagoans is examined in the next sections.

5.3 Urban Sociology at Chicago

The 'Chicago School' is seen as providing the major traditional approach to urban sociology (Evers, 1975; McGrath and Geruson, 1977; Dotter, 1980) and its evident concern with empirical study of the city of Chicago has meant that, in assessing the theoretical impact of the 'Chicago School', many commentators refer only to those theoretical contributions which relate to urban sociology. Of these, the zonal model (Burgess, 1925) has achieved notoreity. This 'ideal typical' model is usually regarded as an interesting but essentially limited or naive model of city growth (Rex, 1973). The implication is that Chicago sociologists did lots of empirical work on the city but were unable to combine it into any systematic theory (Madge, 1963).
Two issues arise here, the first concerns the extent to which the Chicagoans were urban sociologists, the second, the extent to which their research in the area developed theory.

It is usually assumed that the 'Chicago School' was heavily involved in urban sociology and essentially founded the sub-discipline in the United States. Lofland (1983), however, has investigated the supposition that Chicago sociologists concentrated on urban sociology, and concludes to the contrary that

'The heritage of Chicago, then, is the virtual absence of a specifically urban sociology.' (Lofland, 1983, p. 505).

Her analysis intended to show that Chicago sociology was concerned with the private rather than the public realm. She illustrated this contention by grouping the two hundred and twenty one Ph.D. and M.A. theses awarded degrees at Chicago between 1915 and 1935 in terms of their focus of attention. Only five, she asserted, could be said to do with the public realm 'by any stretch of the imagination'. These she listed as Hayner (1923), Russell (1931), Cressey (1929), Anderson (1925) and Weinberg (1935). The latter three, she suggested, could just as easily be regarded as social problem theses.

While Lofland's classification is contentious, (she relies totally on titles not content and provides a classification system which is not mutually exclusive, and her assertion that only 'public' studies (as she defines them) should be taken to be indicative of urban sociology), her analysis does point a questioning finger at those who would circumscribe Chicago activities in terms of urban sociology.
The sample survey of theses reveals that ten (24%) were specifically investigations of some aspect of the city of Chicago or its immediate environs. (See Appendix 6). Similarly, only one fifth (18%) of the regular presentations to the Society for Social Research were about Chicago. Up to 1930 a quarter of talks (27%) were on Chicago, after 1930 this dropped to ten per cent. Furthermore, discussions of Chicago were usually from visiting speakers. Only six per cent of the addresses given by the sociology faculty focused on Chicago while forty one per cent of addresses from non-academic speakers were directed to Chicago. (See Appendix 3).

The two elements of Chicago research work which are usually referred to as representing their theoretical contribution to urban sociology are Park's ecological perspective with its contingent concept of natural areas bounded by transportation and other barriers within which distinct actions developed (Turner, 1967; Tiryakian, 1979a; Dotter, 1980; Komorowski, 1978) and Burgess' concentric zone thesis.

The clearest manifestation of the theories of human ecology is still often taken to be the concentric zone theory. Thus Easthope (1974) suggested that the main work done at Chicago was in the field of human ecology and that it was codified in the concentric zone thesis.

'Three research workers developed this concept [of concentric zone]: Thrasher, Zorbaugh and Shaw. Each of these may be said to have brought out in greater detail and given empirical evidence for, theoretical concepts developed by Park and Burgess.' (Easthope, 1974, p. 66).

Burgess 'systematised ecological communities into concentric
zones, each with its unique culture' (Cavan, 1983, p. 412) and these zones provided the means to social mobility, (Cottrell, et al., 1973). Mowrer (1927) identified family types for the zones, Frazier (1931) studied the zonal differences for black family life in Chicago and Shaw and McKay (1931) showed decline in juvenile crime through the zonal belt.

While acknowledged as early contributions to urban sociology both the ecological approach and the zone thesis have subsequently tended to be viewed as simplistic models of the internal structure and growth of the city. Neither are seen as having any substantial impact on the development of urban sociological theory (Easthope, 1974; Madge, 1963; Rex, 1973). Some commentators go further and imply that the Chicagoans did a disservice to the development of urban sociology. For example, Haussermann & Kramer-Badoni (1980) argued that the central 'urban ecological model' of the Chicagoans dissects reality into a multitude of variables, yet despite them, is analytically weak, as is evidenced by its concepts of urbanism and urban density. In short, they saw the urban sociology of the 'Chicago School' as 'positivistic' and indifferent to history and meaning.

Others (Oliven, 1978; Dotter, 1980) see Chicago urban sociology as involving a pioneering approach which, for the first time saw the city as an independent variable. Indeed, Oliven (1978), as well as Vergati (1976), while not directly disputing the 'positivistic' core of Chicago urban sociology, however, suggested that there was another element to the theoretical contributions of the Chicagoans in the field of urban sociology and this came through
the work of Wirth. They argued that he was the first to formulate a sociological and socio-psychological theory of urbanism in which the city was an explanatory variable, and thus overcame the biological perspective present in the ecological approach. This work paralleled Redfield's anthropological thesis of the folk-urban continuum.

5.4. Conceptual Development

Besides developing concepts intrinsic to the development of the field of urban sociology, the Chicagoans also developed a number of other significant and enduring concepts in fields as diverse as the sociology of race, deviance, the family and technology and culture. Central to most of these developments was the concept of disorganization.

5.4.1 Social Disorganization

It has been suggested (Carey, 1975) that the work of the Chicagoans was underpinned by a 'social disorganisation paradigm', particularly evident in the period from around 1910 until 1930. Whether this really represents a 'Kuhnian paradigm' is debatable (Harvey, 1982; Martins, 1972; Eckburg and Hill, 1979) but it was a substantive underlying organising principle resembling a paradigm; although it was itself subsumed within a more general functionalist-interactionist orientation.

Social disorganisation was central to the sociological endeavour at Chicago and had been ever since its development in the 'Polish
Peasant'. The Polish Peasant emerged as the first attempt to elaborate the Chicagoans general theoretical perspective. Thomas was the focus through which the diverse elements of the perspective came together, and, in collaboration with Znaniecki, the empirically based analysis of the adjustment of Polish rural emigres to American urban life was produced.

The thesis of social disorganisation was important as an orientation for early interactionist work. Social disorganisation explains stability in terms of consistent attitudes and values inculcated by individuals which will both satisfy personal desires and provide outlets for action. However, there was nothing immutable about this stability. Indeed, on one level, as societies constantly changed, they were always disorganised to a certain extent. On another level, individuals, although constrained by social norms which shape the personality, were able to transcend the prevalent norms as and when they obstructed progress to a more comprehensive state of organisation. Temperament therefore played a part in the accommodation of the individual to the social milieu. This 'temperament' was embodied in Thomas's 'wishes'. These wishes (initially response, recognition, security and new experience) were identified by Thomas as the motive force behind human action and moulded attitudes of individuals [2].

This approach thus made social psychology an integral part of sociology. The legacy of Thomas's social psychological component is far-reaching. Thomas had dispensed with the organic view of individuals as products of a given environment who merely reacted
to stimuli. He had provided a place in social action for conscious reflection. He had provided a breakthrough that transcended the assumptions of nineteenth century American sociology. Thomas had severely challenged the idea of basic or immutable forces as determinants of social action. He had not entirely dispensed with the idea, his 'wishes' hark back, but their very name implies something indeterminable. Thomas stood at the crossroads of the challenge to immutable forces, the incorporation of conscious reflection shook the very foundation of the old preconception of original forces. He had, in effect, reassured the 'ability of man to affect his own destiny'.

Although Thomas was forced to resign from the Chicago faculty in 1918, he remained a member of the Society for Social Research and his activities were reported in the Bulletin. His theoretical influence persisted and perhaps grew stronger during the 1920s. Carey's interviewees reflect the importance of Thomas,

'His [Thomas's] spirit was quite pervasive around the place. The way to get at life and the problems and the knowledge you need to analyse what people were doing and how they behaved and so on, was to do what Thomas did.' (Cottrell, 1972)

'The four wishes were being taught but that was felt to be too instinctual by most sociologists at the time although Thomas's legacy was still around, and of course, his Polish Peasant, we all had to read it.... Going over to empiricism and that was partly due to Thomas too.' (Dollard, 1972)

'Thomas' social psychology was alive in my father's social psychology courses [1928], he took that course over from Thomas and carried on that tradition with much the same method and frequent reference to Thomas. In the seminar Blumer had, we had to read a number of theoretical statements from the Polish Peasant. Yes we were quite aware of Thomas and we picked up his favourite expressions. One of them is 'a thing is real if it is real in its consequences'... The definition of the situation, we couldn't have gotten along without that. Thomas's contribution was there in the spirit of the investigation,
concepts that he contributed and his whole outlook in social psychology.' (Faris, R., 1972)

Park certainly made no attempt to undermine the Thomasian perspective. The sample survey of theses shows that Thomas was cited in the bibliography of 50 per cent of theses. This tended to be concentrated in the period up to 1939, Thomas being cited in fourteen (64%) of the twenty two theses up to that date and only seven (35%) of the twenty theses in the sample submitted from 1940.

'Social disorganisation' was an integral and explicit part of the theoretical development of the vast majority of those which could be described as having produced a developed theoretical perspective. For example, Anderson (1923) utilised the general theoretical perspective of social disorganisation in his study of hobo-hemia, Thrasher (1926) adopted it as the basis for explaining the zonal variations in gangs, and as Cressey (1929) noted, Zorbaugh (1929) investigated social disorganisation as it related to the 'interstitial areas of our cities', and himself used the thesis in his own study of taxi-dance halls.

Indeed, the famous studies of the 1920s can all be seen as empirical analyses of the theoretical orientation grounded in the thesis of social disorganisation. Cavan's 'Suicide' (1926), Mowrer's 'Family Disorganisation' (1924), Wirth's 'Ghetto' (1926), Hiller's 'Strike' (1924), and Reckless's 'Vice in Chicago' (1925) all explicitly refer to the concept of social disorganisation, taking the essential nature of the concept for granted. In fact, rather than being an empirical validation of the zonal model, the mapping method so widely used at Chicago in
the 1920s was central to the assessment of indicators of social disorganisation. The concentric zone thesis itself depended upon the concept of social disorganisation.

The theoretical ideas developed by Thomas were widely known and used throughout the discipline. The social disorganisation thesis and its associated concepts of 'definition of the situation' and 'social becoming' were among the few established and long lived concepts to emerge from the early part of the century. There are references to the 'definition of the situation' in the papers of the Society for Social Research in the 1930s which indicate a widespread familiarity with the concept and the Social Science Research Council sponsored conference on the Polish Peasant, 1939, took the concept for granted, it required no explanation.

Indicative of the centrality of the concept of disorganization is its inclusion in a list of major sociological concepts suggested by Nisbett (1962, p. 67).

'My interest in sociology as an art form was stimulated recently by some reflections on ideas that are by common assent the most distinctive that sociology has contributed to modern thought. Let me mention these: mass society, alienation, anomie, rationalization, community, disorganization ... all of them have had lasting effect upon both the theoretical and empirical character of sociology.'

Within the framework of social disorganization, the Chicagoans were instrumental in several major conceptual and theoretical developments.

5.4.2 Race Relations Cycle

Probably the most significant and enduring impact of theorising
at Chicago was in the field of the sociology of race. Concepts such as marginality and acculturation became more fully developed into one of the major theories to emerge from the sociological work of the department, namely the assimilation theory which became popularised as the race relations cycle.

Park, rather more than Thomas, was content to aim at something less than an holistic theory of society and the context for research was resolved into general theories of interaction at Chicago in the succeeding decade. Park advanced the idea of a four stage process of interaction, drawn largely from his research into, and experience of, immigrants and of black-white relationships. The stages identified by Park were competition, conflict, accommodation and assimilation. This four stage process was originally labelled the 'race relations cycle' because it grew out of the work Park had done in that field at Tuskegee. This cycle was outlined in his Introduction to Steiner (1917) and was to become increasingly refined through the work of students, notably, Young (1924), Wirth (1926) and Brown (1930).

Park's students increasingly concentrated on race and collective behaviour. Apart from anything else, the development of race studies at Chicago under Park's guidance clearly belies the impression that the Chicagoans were mere urban ecologists. Matthews (1977, p. 157) suggested that Park had far more impact on race studies than on any other area of sociology.

The Chicagoans developed the area of race continuously from 1915 to 1950 under the guidance of Park and then Wirth, examining the sociology of race in relation to various ethnic minorities and
through various orientations from sociological through social psychological to psychoanalytic (Reuter, 1919; Detweiler, 1922; Young, 1924; Brown, 1930; Stonequist, 1930; Frazier, 1931; Doyle, 1934; Cox, 1938; Daniel, 1940; Strong, 1940; Alexander, 1942; Parrish, 1944; Walker, 1945; Hill, 1946; Faw, 1948; Janowitz, 1948; Turner, 1948; Cothram, 1949; Hale, 1949; Marcson, 1950; Starr, 1950; Reitzes, 1950; Quinn, 1950; Lewis, 1951; Haimowitz, 1951; Edwards, 1952). Wirth (1948) noted that of his three areas of substantive interest

'my main love is the field of race relations and minority problems. I have published a number of things in this field including a number of articles in the Journal, a little monograph for the Social Science Research Council on "Problems of Minorities in War Time," a chapter in Linton's book on "The Science of Man in the World Crisis" entitled "The Problem of Minority Groups", which some of my friends think is one of the best things in the field, probably because it attempts to establish a typology of minorities. In this connection I co-operated with the Myrdal projects and published with Herbert Goldhamer a monograph in that series on miscegenation. I am, as you may know, the President of the American Council on Race Relations and the Chairman of our University Committee on Education, Training and Research in Race Relations.' [3]

By 1930 the race relations cycle had become firmly entrenched in Chicago sociology and beyond, and was a taken-for-granted theory in the analysis of the interaction of diverse cultures. By the 1950s it had become extended into a general theory of interaction of groups. Wirth noted that

'Correlative to social organization is the study of social interaction in all of its phases which deals with such processes as contact and isolation; competition; conflict; accommodation, and assimilation.' (Wirth, 1948).

In their textbook on sociology, Ogburn and Nimkoff (1960, p. 111) made this generalised race relations cycle compatible with the structural functionalist approach. The theory persisted in U.S. sociology generally until the 1960s. Martindale (1960, p.
256) noted
'To this day there are persons who do not feel they have covered the basic subject matter of sociology until they have discussed competition, conflict, accommodation and assimilation.'

Agocs (1979) suggested that the assimilation perspective of the 'Chicago School' has dominated North American studies of urban ethnic settlement. The notions of marginal man and social distance (Bogardus), became taken-for-granted in U.S. sociology and relatively recent use can be found in Martin and Franklin (1973, p. 48), Ferrarotti (1977), and Kasdan (1970, p. 4) while Gordon (1964) utilised the race cycle.

5.4.3 Social Change and Cultural Lag

Ogburn was interested in social change and the influence of technology on social change. Central to his approach was the notion of culture. For Ogburn (1937) 'culture cut the chains that tied sociology to biology'. Culture was a holistic notion, being the whole product of social interaction, manifested in the society's controlling mechanism. Sociological enquiry, for Ogburn, was directed to the effect culture had on individuals. He advocated the study of culture as a whole, that is an analysis of 'western civilizations' as cultural wholes as ethnologists had done for 'primitive' cultures. Of the study of particular facets of modern civilization, Ogburn saw the study of the city as most closely approximating a cultural approach. Ogburn and other culturologists (D. Thomas, White) extended this in an attempt to interrelate different aspects of modern society into an analysis of the cultural whole. A cultural emphasis led to the analysis of
the impact of different factors on social change. Such factors were the impact of inventions, diffusion of cultural traits, the nature of culture contacts, social attitudes and resistance towards change, and the stock of knowledge.

Ogburn considerably developed the sociological analysis of technology. He saw the immutable forces of technology subsuming the individual in the sense that social evolution would take place irrespective of any individual historical figure, although accepting that the nuances of evolution are mediated by human activity. Ogburn thus tended to look for explanations of social disorganisation at a less individual and a more cultural level. He thus resolved the Thomasian context into the four stage process of invention, accommodation, diffusion and adjustment. Ogburn's general theory of change had its particular referent, the cultural lag hypothesis, (Ogburn, 1922). The interrelatedness of culture, the primary effects of inventions in producing change and the adaptive character of non-material culture 'led directly to Ogburn's famous concept of cultural lag' (Gough, 1942). Ogburn argued that social change lead to strain because there was a delay or lag in the assimilation of mechanical invention and scientific discovery by social organisations, philosophies and popular habits. Culture is forced to adjust to technological change, but there is a period of 'disorganization'.

Ogburn's work on culture and social change, although begun before moving to Chicago, became widely known in the interwar years to the extent that 'Ogburn's ideas were familiar to sociologists who had never read any of his books' (Gough, 1942, p. 1). The
American Journal of Sociology devoted one issue to the analysis of social change for a number of years and Ogburn wrote or edited books on social change throughout the twenties and thirties (Ogburn, 1922, 1927, 1934). His emphasis shifted to a closer study of the impact of technology (Ogburn, 1933, 1934b, 1937, Ogburn & Nimkoff, 1955) and in conjunction with Dorothy Thomas (Ogburn and Thomas, 1922) raised issues about the nature of scientific change and discovery which were developed by Merton (1973) and White (1969) and are still an issue in current discussions of sociology of science (Brannigan, 1981).

5.4.4 Deviance and Labelling Theory

Taylor, Walton and Young (1973, p. 111) saw the Chicago ecological tradition at Chicago as a major source of theoretical schema for the development of deviance studies.

Park (1929, p. 36) noted that

'It is assumed that people living in natural areas of the same general type and subject to the same social conditions will display, on the whole, the same characteristics.'

This ecological approach to deviance was developed at Chicago during the 1920s and Sutherland, later, developed deviancy theory to include elements of social processes as well as structure thus including differential association along with differential social organisation (Dotter, 1978). Sutherland's association theory which suggested that criminal activity is produced primarily through exposure to others having criminal attitudes and engaged in criminal activities. That is, deviant acts are learned and individuals are liable to engage in deviant activity if they are
exposed to an overabundance of criminal activities as compared to non-criminal associations. The soundness of this thesis was debated into the 1970s (Vold, 1958; Cressey, 1962; Sutherland and Cressey, 1966; Box, 1971)

The predominant emphasis, then, amongst the early Chicago informed social disorganisation theorists of deviancy was on the 'normlessness' of delinquent areas. Later subcultural theorists (Cloward and Ohlin 1960, Cohen, 1955) influenced by Merton, used the concept of disorganization in a different way. They posited an anomie thesis which suggested that while cultural goals were widely diffused and internalised there was no corresponding achievement frame.

The more recent and well known work in the field of deviancy by researchers associated with the 'Chicago School', notably Becker, Lemert, Matza, and Polsky has its roots in the social disorganisation theories of the early Chicagoans. They attempted a fairly radical development of a social, rather than biological, theory of deviance by proposing the relative nature of deviant activity. This was expressed through the articulation of the deviant's viewpoint within a functionalist perspective (Celinski, 1974). Labelling theory grew out of Sutherland's work, in part as a critique of its limitations in respect of taking the view of the other. The role of the 'Chicago School' in the field of deviancy studies is widely accepted, and represents the 'classical environmental analysis of deviant behavior' (Caldarovic, 1979).
5.4.5 Other areas of theoretical and empirical study

As has also already been indicated, Burgess's personality research was of more note to him than his concentric zone thesis, but his real and enduring interest was with the sociology of the family. Burgess' family studies changed the face of American sociology of the family to the view that the family was 'a closely interacting group of people playing different roles' (Cavan, 1983, p. 412-413). Cottrell (1933) further developed the study of the family, working closely with Burgess.

Following his own work on the Chicago Real Estate Board, Hughes (1928), engaged in and encouraged a further generation of sociologists to investigate the sociology of work (Hughes, 1958), of organisations (Hall, 1944; Smith, 1949) and of professions (Hughes, 1970). Very little research had been attempted in this field, especially in the United States, until Hughes sparked off interest at Chicago on his return to the University in 1938. Ritzer (1978) has suggested that of three identifiable paradigms in the sociological study of professions the 'process paradigm' flows out of Hughes' work in the 'Chicago School'. Only very recently has the process paradigm began to wane in importance as the study of professions has moved more towards the analysis of power relationships.

Burns (1980), argued that the ecological perspective on organisational change represents the most significant development in contemporary organisational research and that this can be traced back to the writings of the 'Chicago School'. The Chicago model of ecological, economic and cultural organisation provides a
novel examination of the relationship between organisations and environment through its concentration on natural history within a social disorganisation framework. While these innovations in organisational study were inadequately formalised, Lawton argued, they have continued in the work of contemporary theorists.

Park also prompted the development of a substantial research tradition in the field of collective behaviour and mass society, which was developed in the work of Edwards (1927) and more recently by Shibutani (Witzgall, 1978). Park's own doctoral thesis was 'The Crowd and the Public', (Park 1972) an interest he retained throughout his life. Park provided one of the classic definitions of collective behaviour (Blake, 1978) and, by conceptualising the crowd as an object rather than a set of collective processes generated a perspective which has persisted through the work of succeeding writers.

The sociology of knowledge was another area developed at Chicago, largely through the work of Wirth and Shils, and the doctoral candidates they supervised (Whitridge, 1946; Duncan, 1948). This is explored in more detail in section 5.6.2 below.

5.4.6 Summary of the theoretical contribution of the Chicagoans

Thomas provided the basis for a general theoretical orientation which underpinned much of Chicago sociological work. The Chicagoans developed the social disorganisation thesis in various ways, gradually discarding the psychologistic elements such as the 'wishes' and developing a more rigorous, although not entirely homogeneous, interactionist theory in particular areas. Park and
many of his students developed the area of race relations, Burgess's students concentrated on the family and Ogburn's on social change and psychoanalysis. Wirth, Hughes and Stouffer encouraged a further generation to develop these areas and a more focussed empirical testing of specific theories evolved. These later generations developed the sociology of work, of organisations, of deviance, of mass society and the sociology of knowledge.

On a more general level, the Chicagoans avidly engaged in the debate that revolved around Freudianism. Indeed, there was probably a greater, and to some extent more clear cut division in the department over the efficacy of Freud's theories for sociology than there was about the value of quantitative techniques.

'I know my father thought some of Burgess' interests were shallow and ridiculous, he thought some of the others were quite good though.... My father did not care at all for psychoanalysis or Freud and Burgess did. My father would make quite critical comments about Freudian concepts in his class but would not mention Burgess or attribute them to him.... Ogburn had been analysed and was a fairly convinced Freudian....' (Faris, R., 1972)

'Ogburn's students became less and less the journalistic Parkian oriented students and more and more the quantitative orientation. But Burgess sort of straddled that.... He studied statistics and attended meetings of the psychoanalytic institute uptown to get hold of the Freud business.... I got quite interested in psychiatric theories and started quite an intensive bit of reading in the writings of Freud. In fact I read all of them, all his published works... The only thing we had on Freud was an attack, a target of antagonism. Louis Wirth who was a little more my senior, but still one of the younger members of the faculty, actually I got to be quite a Freudian, I had quite a Freudian phase, he really viciously attacked me as a person who was not properly orientated to the sociological problems.' (Cottrell, 1972)

'Blumer, Park and Faris were opposed to it [Freudianism]. Burgess was a very diplomatic sort of chap. He didn't confront any of his colleagues. But if he found a student with an interest he would encourage it....' (Dollard, 1972)
However, this disagreement did not lead to the fragmentation of the department, as the sociology developed at Chicago had an interactionist base that, in practice, cut across psychoanalytic concerns.

The tendency to view Chicago sociology as essentially empirical and therefore as atheoretical reflects a confusion of first hand empiricism with 'abstracted empiricism'. On the contrary, Chicago sociology was involved in the development of a plethora of sociological theories in numerous areas and reflected the cumulative theoretical style which came to be known as 'Middle Range Theorising', (Merton, 1948).

5.5 Chicago Theorising and Sociology in the United States

The view that Chicago sociology was essentially concerned with empirical data collection, or the view that its theoretical work was confined to the pioneering stage of urban sociology, are both indicative of an idea that somehow Chicago stood outside the general development of American sociological theory.

The emphasis on the empirical concerns of the Chicagoans in presentations of their work tends to act to separate them from the sociological mainstream. While the Chicagoans were in the vanguard of the shift from 'armchair theorising' to 'inductive theorising' they were not alone in this endeavour nor, indeed, were they the sole pioneers in empirical data collection.
5.5.1 The Development of an Empirical Base for American Sociology

The social survey movement, based on ameliorist concerns of British social surveyors in the nineteenth century, was well established in the United States by the time the Chicagoans adopted an empirical base for sociological theorising during the 'Golden Era'. (Furner, 1975; Schwendinger and Schwendinger, 1974).

Following in the footsteps of the Booth survey of London, a survey movement sprang up in the United States. The Pittsburg Survey (1909 - 1914) was followed by the establishment of a survey department within the Sage Foundation and by 1928 its director reported 154 general surveys and 2621 specialist surveys (Faris, 1967, p.8). These were not sociological surveys, and as Faris points out, much of the survey tendency

'took its own course, diverging early from academic sociology and finding a close partnership with organised welfare activities'

Nonetheless, as Burgess wrote in 1916 while at Ohio State University, the example of the Belleville and Lawrence surveys directed by F. W. Blackmar at the University of Kansas was indicative of the increasing involvement of sociologists in social surveys. Burgess wrote that the social survey of a community is

'the scientific study of its conditions and needs for the purpose of presenting a constructive program for social advance' (Burgess, 1916)

He referred to the work of the social surveyors such as Kellogg, Rowntree, Addams and Booth, and said that sociologists should be gratified at these developments of their academic, experimental
procedures, which beginners in all sociology departments in the country have been initiated into and learned of the utility of the survey as a scientific instrument.

Indeed, Park's course at Chicago on the social survey (from 1915 to 1921) provided the critical framework in which empirical work was developed. His critique of the social survey was not only that it tended to overemphasise statistical data and reformist concerns but that, in so doing, it also distanced itself from sociological theorising. In this, he reaffirmed the stance of Small and Thomas and reflected the growing tendency towards 'inductive theorising' in American sociology.

Outside Chicago, other sociologists were engaging empirical data in their attempt to develop sociological theorising. Notable was the work at Columbia under the directorship of Giddings which included Ogburn's early research; developments at the University of Southern California; as well as the research done at Michigan (Cooley, 1930; Rice, 1931). Indeed, Lapiere (1964) recalled that 'one man' departments all over the country were springing up with the aim of developing a credible empirical base for sociological theory.

Thus, as Chicago sociologists became more and more concerned with empirically based study rather than 'armchair theorising', they reflected the emerging tendency in American sociology as a whole.
5.5.2 The Epistemological Basis of 'Standard' American Sociological Theory.

While the Chicagoans were among the pioneers in the development of a sociological theorising grounded in empirical data, they were far from unique in this endeavour; and their empirical data collection orientation should not be viewed, in itself, as indicative of a separation of 'Chicago sociology' from the direction of mainstream sociological theorising. Also, however, it is necessary to address the extent to which Chicago sociologists developed a distinctive 'interpretive' or 'phenomenological' approach [4]. The following sections attempt to set out the approach to sociology in the United States which emerged out of the 1920s and prevailed into the 1960s (following Mullins (1973), this will be called Standard American Sociology) and to assess the extent to which the epistemological perspectives at Chicago differed from it.

5.5.3 The Cumulative-Falsificationist Model

Over the course of the twentieth century, American sociology has emerged as an empirically grounded endeavour, adopting, in the main, what may be described as a falsificationist model of the production and validation of sociological knowledge [5]. In conjunction with such a model, there has been a tendency to accept the idea of the cumulative development of theory (Social Science Research Council, 1939; Merton, 1945; Mills, 1959; Willer, 1967).

Arguably, American sociology has also been dominated (if not
exhausted) by two general theoretical perspectives, 'interactionism' and 'functionalism'. While not entirely compatible these two perspectives overlap and do not constitute the theoretical base of two distinct traditions. Rather they concur on many elements of the sociological process. Both are essentially nomothetic, both are anti-behaviourist, both focus on group processes, both are empirical and non-critical. The one approach assess the function served within a social process by a particular phenomenon, the other assesses the interactive process in order to see the way action is mediated by social processes. Clearly they are interrelated, to some extent the opposite sides of the same coin.

The prevalent model of sociological knowledge in American sociology emphasised cumulative theory, falsificationism, and nomological concerns incorporating meaning adequacy (Harvey, P., 1982). The tension that existed concerned the extent to which the problems of establishing adequacy at the level of meaning should inhibit nomological concerns. The Conference on the Polish Peasant, 1938, clearly reflected these concerns and acted as an indication of the pervasiveness of the prevalent model. No alternative, 'anti-positivistic' model emerged from it; despite the existence of an embryonic critique in the work of Blumer and of Wirth.

The Conference on the Polish Peasant was an important, well-documented and widely known debate; even if the participants were not 'representative' of the entire gamut of American sociology. A 'centralist' approach to sociology clearly emerged from it
despite some scepticism over the nomothetic possibilities voiced by some of the Chicagoans.

The discussion on the Polish Peasant which took place in 1938, clearly indicates how the main institutionalised perspective in sociology saw the nature of the discipline. While the analysis of the conference that follows shows the points of debate, it also reveals a strong commitment to a more or less agreed view of the nature and aims of sociology. Chicago does not stand outside the general approach.

5.5.3.1 The Conference on The 'Polish Peasant'.

The conference on the Polish Peasant (Thomas & Znaniecki, 1918) was called by the Committee on Appraisal of Social Research of the Social Science Research Council, in New York, on December 10th 1938. The main contributors of those present were three Chicago professors, Herbert Blumer, Louis Wirth, Samuel Stouffer in addition to G. W. Allport, Read Bain, Max Lerner, W. I. Thomas and W. W. Waller.

The debate followed Blumer's written critique of the Polish Peasant. Initially the debate was directed to the efficacy of personal documents as a device for testing theoretical assertion. In developing his written submission Blumer pointed to the premises underlying the Polish Peasant. These were, first, the need of a plan of research suited to a complicated changing society that may be applied to any society undergoing transition, and, second, the declaration that the understanding of human life necessitates the grasping of the subjective factor.
Thus any sociological study should involve both the external factors (social values) and the internal factors (attitudes).

Given the above, Blumer argued that two things were necessary. First, to develop a guiding theoretical scheme which will set hypotheses, that is, provide a framework for interpretation and analysis. Blumer reckoned that in the 'Polish Peasant' all the theories are developed in 'intrinsic relation to these basic concepts of attitude and value'. Second, the scheme needs source data that will reveal the 'subjective factor in human experience and which, at the same time, will meet the usual requirements for scientific data, viz., that one can always go back to these data and that other workers may have access to them' (Social Science Research Council, 1939, p. 108).

The resulting debate thus tended to turn attention from the particular problems of personal documents as evidence to address the problem of proof, causal attribution and objectivity in sociology given the 'need of recognizing and considering the subjective factor in human experience'.

The conference spent considerable effort looking at the implications of Blumer's critique for the human document as data, and in so doing broached the nature of scientific enquiry, of society and of alternative approaches to sociology. Throughout, however, certain elements were taken for granted. Most notable was the primacy of inductive theorising. Interestingly, however, there was also a constant concern to ensure that sociology adopted approaches which do not regress to atheoretical fact collection any more than it should to accept non-empirical abstract theorising. In short, the idea that sociology should
progress in terms of a cumulative development of theory grounded in empirical data, in a manner which Popper was to codify in the terms of a natural science debate as 'falsificationism', underwrote the discussion.

The concern was with how different types of approach could provide 'objective', that is 'reliable', data. The accepted approach closely resembled what Merton (1948) came to call middle-range theorising [6]. Indeed, prefacing this, Lerner noted,

'Here we have the empirical data, here we have the abstraction into which we are attempting to fit them. Modify the abstraction to fit the data and go and collect more data to fit the new abstraction; there is a constant interaction between them.' (Social Science Research Council, 1939, p. 177).

The debate was concerned with the realist position which is central to the Polish Peasant's methodological note, that the subjective factor be incorporated into social analysis. The concern of the conference when assessing human documents was that while they provide some 'attitudinal' insights, they may not be 'objective' in as much as they may prove to be inadequate for consistent inductive inferential or testing purposes.

There were differing views on the degree to which the human document could be treated as reliable, 'objective', or self evident facts in the sense of data in the physical and biological sciences. This assumed that the model of science rested on a factual base. (Social Science Research Council, 1939, p. 114).

Human documents, Bain contended, were non-specific instances,
reflecting a whole culture and could not be seen as data in the sense of data in the natural sciences. Blumer and Wirth defended the principle that documents, although specific instances, may provide abstracted data. They argued that the document can be used as a specific instance through an abstractive process which indicates what aspect of the document is being considered through an application of a prior conceptual scheme. Wirth drew on Thomas for illustration.

'Thomas does this by saying that this is a case of restlessness or new experience. He says, 'Now I am focusing on those elements in the human document which to me incorporate this particular motive or this particular attitude'. (Social Science Research Council, 1939, p. 119)

Wirth and Blumer argued that no data is as simply abstract as the ideal model of the physical sciences pretends, and that in the social sciences, one must incorporate meaning. However, these points are subverted by the underlying falsificationist objectivist position.

Nowhere is there any suggestion that data has meaning only in terms of its theoretical base, (thus the approach resembled what Lakatos (1970) refers to as naive falsificationism), nor that the culturological frame raises problems of a hermeneutic nature. On the other hand, the realist position was rarely threatened and only Stouffer and Bain had any reservations about the necessity to include the subjective factor; and that only in terms of the possibility of some valid sociology having put aside the subjective aspect. Stouffer, in asking whether the taken-for-granted subjective factor really is so vital, pointed to the prediction studies of Glueck, Burgess and Vold which are 'very
important for social action' and which provide 'fairly valid conclusions and generalizations' but are independent of the 'meaning of any of this activity to the individual'.

Nonetheless, there was agreement on the view that social science does involve, for whatever reasons, problems different to the natural sciences, and that these problems must somehow be overcome if sociology is to enhance its objectivist, falsificationist, scientific credibility. There was a general concern that sociology should avoid a 'nihilistic' attitude, given that social scientific research can only be seen as plausible not definitively validated, and that no laws (or approximations to laws) as directive of action can be drawn up at anything but a trivial level.

Bain summed up the cumulative falsificationist orientation to science and its application to social science as follows:

'Those grand generalizations always get tested by being broken up into a great number of simple problems. What we call 'progress' in all the natural sciences, among which I would include the social sciences, has come about through the development of the art of stating simple or unequivocal propositions, or hypotheses, which are capable of empirical test. When enough such propositions have been tested and retested and all of them are logically consistent with the grand generalization, it may be said to be verified. The empirical verification of no one single hypothesis relevant to the general theory of organic evolution can be said to be adequate proof of it, but when thousands of such simple single hypotheses have been verified, and they all hang together - none of them are clearly negative cases - we eventually come to accept the general theory of organic evolution as an actual valid scientific fact.' (Social Science Research Council, 1939, p. 161).

One aspect of the debate is an overall desire for a synthesis of grand abstraction and scientifically precise empiricism, a view that methodological monism is not particularly desirable, and
that eclecticism should be further developed rather than discour-aged. This is reflected in Thomas' own reappraisal of the Polish Peasant in which he commented that he would put methodological considerations out of view if doing a similar study and restated his view that a mixture of approaches is most suitable, i.e. life histories and statistics, including factor analysis.

In an appended summary to the transcript, Bain (the transcript editor) indicated what he saw were the main divisions among the conferencees, and which reflected differences in American sociology. There was disagreement over the nature of social phenomena, methods by which they can be studied, the possibility of laws and of the testing of generalizations. Bain suggested two opposing views of validation which he saw revolving around the issue of the possibility of social laws. On the one hand the view that social laws were impossible given that they could not grasp 'values'. The alternative view, accepting the possibility of social laws

'emphasizes the idea that laws are possible because there is considerable uniformity and permanence in the occurrence of observed and observable social phenomena, whether they be called 'objective' or 'subjective'."

Bain then identified four theoretical positions expressed at the conference, first, illuminative insight, second, organising concepts, third, logico-systematic analysis and fourth, delimited empirical research. The last is opposite to illuminative insight, it is reductionist and demands framing propositions for testing, therefore requires relatively simple problems, few controlled variables, accessible and permanent data which are uniform and repeatable.
'It is the most highly abstract way of dealing with concrete, i.e., experienceable, reality. It stresses verification by repetition, prediction, application, external and internal logical consistency. It is based on the probability calculus, it is actuarial or statistical. It advocates the development of precision instruments for use in observation, recording or manipulation, as the indispensable prerequisites for sound scientific work in any field. It holds that the history of science is the history of scientific technology.... The general methods and point of view is the same for all science, though necessarily the particular methods, techniques and technological devices used will vary greatly with the data being studied... All scientific data are abstractions... It is out of the cumulative findings of such simple, particular, highly abstracted empirical researches that the material for valid general scientific theories must come. It is by such research only that 'causal validity' can be ascertained and upon it, at long last, that all 'meaningful validity' must depend. (Social Science Research Council, 1939, p. 201).

Bain concluded that none of the Conference participants saw any of these positions as adequate alone, all agreed that some kind of synthetic position was required.

'However the trend is towards the type of research called 'delimited empirical'; logico-systematic analysis is increasingly dependent upon such research; organizing concepts tend to grow out of such research and to be tested by it in the general manner described. This is a continuous process. The specific researches make imperative the revision of organizing concepts and general theories, and such revision by logic-systematic analysis sets new problems for further empirical research which requires the development of new or improved precision procedures which depend upon the invention of new or improvement of old technological devices of observation, recording and manipulation along with new or improved methodological skills and procedures.' (Social Science Research Council, 1939, p. 202).

Thus Bain clearly laid out the cumulative falsificationist model which had emerged as prevalent in American sociology. Ironically, this 'delimited empirical' approach with its reductionist emphasis actually provided the potential for a division in American sociology. Its failure to take into account the theory laden nature of observation, meaning adequacy, the nature of historical evidence or the cultural frame which Bain referred to
provided the basis for a fundamental critique of the falsificationist approach to sociological research. This critique existed in embryonic form at Chicago in the position advocated by Wirth, Blumer and even in the 'culturological' approach of Chicago's major quantitative practitioner, Ogburn. Nonetheless, as will be examined below (section 5.6), the Chicagoans tended to remain within the prevailing tradition rather than engage it in a radical way.

While the Polish Peasant Conference was a major event in American Sociology and illustrative of the development of a consensus orientation towards sociology, it is, of course, not a definitive statement. However, similar issues were raised and discussed within very similar constraints when, for example, the American Sociological Review was inaugurated and the issue of operationalisation engaged (Lundberg, 1936; Waller, 1936). Similarly the earlier discussions about the relative efficacy of case study and statistics were contained within a framework of debate which took for granted the essentially nomothetic concerns of the cumulative theoretical approach (with the possible exception of Cooley, 1930 [7]).

5.5.4 The Chicagoans' General theoretical and epistemological orientation.

The codification of the nature of sociological theory encapsulated in Merton's call for 'Middle Range Theorising' had been an ongoing practice in American sociology since the 1920s. Merton's work merely served to formalise and clarify elements of confusion
in the prevailing cumulative theoretical tendency. Chicago sociology was not at variance with prevailing tendencies, as was illustrated in the analysis of the Polish Peasant conference. While Stouffer was more inclined towards a view sympathetic to attributing causes in the social world, Blumer and Wirth were more sceptical although, like the other speakers at the conference endorsed a cumulative falsificationist approach.

Chicago sociologists had, since Thomas, adopted a variant of the cumulative falsificationist approach. In terms of Merton's categorisation of theory, the Chicagoans, in practice, reflected his concerns. Thus, methodological debate was not set apart from theoretical development. The Chicagoans were eclectic in their methods (see chapter four), and Wirth reflected the Department's position when proposing the core elements of a Master's programme which made no provision for a separate methodology course. For the Chicagoans methodology was an integral part of theory.

Similarly, while concerned to clarify concepts, notably disorganisation, prejudice, marginality, and interaction, the Chicagoans did not consider these clarifications an end in themselves, but merely an adjunct to the development of theory. Such theory, as has already been discussed, was developed through empirical observation, rather than 'armchair speculation'. However, it was not 'post-factum' theorising (Merton, 1948), as studies were both underpinned by a general theoretical orientation (social disorganisation with its associated demand for a consideration of attitudes and values) and located within particular theoretical discussions. Burgess (1944) reaffirmed
that the mere collection of facts does not constitute sociology. The adoption of a cumulative-falsificationist model for sociology effectively undermined the long-term division between nonimalists and realists (Lewis and Smith, 1981). In the wake of the Polish Peasant debate, Burgess readdressed this division and enquired as to the suitability of physical and biological scientific models for the study of the social world.

The realist position, for Burgess, implied that society is a reality to be studied through social processes such as communication, collective representation or social control; in short society is seen as organic and existing in the 'interaction and intercommunication of its members' as in Comte's social consensus, Durkheim's collective representation, Simmel's social forms of interaction, Weber's ideal types, Sumner's folkways and mores, Small's group, Cooley's sympathetic introspection and Park's collective behaviour. Nominalists, on the other hand, concentrated on individual physiological and mental processes, such as Tarde's imitation, Giddings' consciousness of kind and Allport's denial of 'group' and 'institution' as analytic concepts.

In reviewing the nominalist-realist debate, Burgess noted that while realism emerged victorious, the nominalist position retained some credibility. Burgess' position was that while, in the past, the nominalist-realist issue had been contentious the debate had moved onto a new plane in which a synthesis (dominated by the realist position) engaged in a more subtle debate. The synthesis suggested that while study of society required that
'the distinctly social aspects of human behavior cannot be studied adequately by the analysis of mental processes within individuals but requires examination of the social processes involved in their interaction.... there actually are aspects of human behavior which may be studied under a conception of society as an aggregate of independent individuals, and other aspects which can only be adequately defined and examined by the opposing conception of society as a reality of which its members are products.' (Burgess, 1944, p. 2).

He argued that, in view of this, there was a convergence between the nominalist and realist positions in practice which belied their epistemological distance. In effect, he argued that the nature of the attempted scientific study was characterised by the two essential guiding criteria of falsificationist 'Scientific Method', namely, the formulation of working hypotheses, and the 'objective use of an objective method of verification or disproof of the hypotheses that can be repeated by other[s]' (Burgess, 1944, p.8).

Burgess's review of the realist-nominalist debate dispensed with the stale dichotomisation and effectively illustrated the synthesis achieved by the concern to establish a sound objective science grounded in a cumulative-falsificationist pragmatic model which tended to be adopted at Chicago as elsewhere. This is evident in the research work of the Chicagoans and emerged from the Polish Peasant conference where (publicly at least) there was, in principal, a nomological consensus amongst the participants at the conference.

5.6 Chicago Alternatives to the Prevailing Model.

The Chicago sociologists, notably Blumer and Wirth, had, as part of their theoretical repertoire, the basis for an alternative to
the prevailing model. Indeed, Blumer is often seen as fundamentally opposed to the tendency towards a 'scientific sociology', while Wirth is seen as providing a radical alternative based on a German sociology of knowledge tradition (Burgess, 1944).

The debate on the Polish Peasant (Social Science Research Council, 1939) had clearly indicated the root of this opposition. However, as has been suggested above, while Wirth and Blumer provided the grounding for a non-falsificationist position during the debate, they subsumed these concerns under the standard approach.

5.6.1 Blumer and Symbolic Interactionism

Blumer was openly sceptical of the possibility of causal laws. The value of the Polish Peasant for him, then, was its focus on understanding. The fact that it appears in his critique not to be capable (methodologically) of achieving the control over society that it wanted does not bother Blumer. He suggested that understanding may provide a better resource for control than a nomothetic sociology. Regrettfully, Blumer accepted that his position located social research between 'scientific laws' and 'literary insight' and saw no short term resolution of that situation.

Blumer was not alone in advancing the scepticism about the possible 'objectivity' of the social sciences as they stand. He was supported by Lerner who questioned

'the whole comparison that I have found so often being made between the natural sciences and the social sciences. I
think one of the things we have suffered from has been that sense of inferiority that comes from our not being able to turn ourselves into natural scientists. I think we ought to recognize that and also recognize that there are attainable social generalizations that are worth making, and then to talk about research in terms of getting at those relatively attainable generalizations'. (Social Science Research Council, 1939, p. 144).

Although there is little evidence of it in the Polish Peasant Conference, Blumer is often regarded as a major figure in the development of a 'qualitative' or 'interpretive' sociology (Filstead, 1970; Denzin, 1970; Butters, 1973). The development of symbolic interactionism formalised his position and its adoption by some of the Chicagoans is seen as indicative of the 'Chicago School's' continued exclusion from mainstream development of sociological theory. Indeed, while symbolic interactionism provided a basis for an alternative to the prevailing falsificationist orientation that pervaded American sociology, no such fully articulated alternative emerged from the 'Chicago School'. As Thomas (1983b) noted, Chicago was not the home of a critical ethnography, indeed, it was more the centre of a synthetic sociology, utilising eclectic approaches and a non-critical methodology, and reflecting mainstream American sociology.

So, although, the emergence of a strong symbolic interactionist approach at Chicago orchestrated by Blumer is sometimes seen as indicative of a radical shift in the Chicago tradition away from the nomothetic-falsificationist concerns of the earlier interactionists and of the enduring approach advocated by Burgess, towards an ethnographically based phenomenological perspective, this overstates the division in American sociology prior to 1960.
An analysis of the symbolic interactionists at Chicago from 1930s onwards shows that they did not form a separate approach from the general developments at Chicago, nor were they phenomenologists. Further, there was no clear attempt at methodic prescriptions until at least the late 1950s, and then as much for pragmatic reasons (the nature of the researched groups) as out of methodological preference. The methodological debate (even as late as the 1960s) did not detach participant observation from the general methodological concerns of middle range theorising. In fact, Blumer never laid the groundwork for a 'truly' phenomenological critique of positivism, always resisting any charges of radical subjectivism, and it was Goffman, if anyone, who provided the route to an alternative conceptualisation of the sociological enterprise which was developed at Berkeley as 'late symbolic interactionism' and emerged (eventually) in ethnomethodology, (Scott, 1968).

The approach and orientation of those sociologists at Chicago who adopted the label of symbolic interactionist, or to whom such a label has retrospectively been applied, did not divorce themselves from the mainstream of interactionism at Chicago. There is, at no point, a shift indicative of a large movement towards a peculiarly symbolic interactionist perspective. Symbolic interactionism grew out of the general perspective and orientation at Chicago in line with similar debates in American sociology. The Polish Peasant Conference is illustrative of the essential tension in the discipline but also reveals that no fully articulated and programmatic alternative was in evidence. Sociology as science and the nature of both science and sociology was under,
and has remained under scrutiny. Blumer's reservations were but a single articulation of what may be seen to be a post war 'qualitative style', however, he did not represent a 'Chicago School' position, nor was he leader of a set of sociologists who developed anything like a phenomenological critique or research practice.

In many respects, Blumer was out on a limb at Chicago. Janowitz (1980) argued that Blumer's theoretical position was distinct from the general perspective at Chicago and this is reflected in the social distance he tended to maintain from other faculty and graduates.

'I was fairly close with Herb Blumer although he was quite remote and distant, not too friendly with anybody.' (Cottrell, 1972)

However, Blumer tended never to develop a position which would serve to detach himself entirely from the prevailing sociological approach. This is evident in his own research at Chicago. An example of the approach is provided in a letter of application for funding to continue his study (with H.W. Dunham) on cataonic dementia praecox, written to Wirth as chairman of the Social Science Research Committee at Chicago, June 13, 1936. He outlined work to date, which very much reflects the general concerns of the Chicagoans. These concerns related to the environmental factors affecting action, social interaction, personality traits, attitudes and values.

Blumer noted that the catatonic dementia praecox boys in the study were excluded from childhood associations with delinquent boys at that point when delinquent acts are planned and that
there was 'definite proof' that the catatonic boys were not delinquent despite being open to the same environmental stimuli as the delinquent boys. The result was a negative value placed on delinquency by the catatonic boys. Such boys tended to be conformist, timid, self conscious, attached to home and anxious. Such anxiety occasionally erupted into psychotic behaviour.

Blumer intended to develop the research along two lines, the first a deeper investigation of personal experiences of the catatonic boys, and compare these with similar boys in other areas of the city. Second, to

'construct a testing device in the form of a questionnaire built around the differences in traits of behavior existing between the catatonic boys and the delinquent boys. I hope to make this device into such a form that it will be possible to isolate out the two types from one another with ease...' (Blumer, 1936)

Blumer wanted to check his feeling that it is the discrepancy between the 'psychological tempo of the community' and the 'personality disposition of the catatonic' which leads to psychotic outbreaks.

In much the same way, later symbolic interactionists, for example Becker, Geer, and Stauss, did not adopt a position oppositional to the prevailing nomothetic-falsificationist model. Indeed, Becker was a student of Everett Hughes, who more closely reflected the sociological orientation of the majority of Chicagoans, (Faught, 1980).

Even into the 1960s, the work of these noted symbolic interactionists who are generally assumed to have taken their cue from Blumer failed to adopt an approach at variance with the main-
stream. Indeed, the contribution to the methodological debate in the 1960s by Becker and others was less inclined towards an anti-nomothetic position than Blumer. Becker (1958), Geer (1964), Becker and Geer (1957, 1957a, 1960) and Glaser and Strauss (1967) may have preferred and attempted to legitimize a 'qualitative' approach, yet they offered no fully developed 'phenomenologically'-based alternative.

Becker (1958) and Geer (1964) provide a clear illustration of the methodological and general theoretical orientation of the 'later Chicagoans', and indicates the essentially nomological nature of their endeavour. The 'qualitative style' was the 'loyal opposition' (Mullins, 1973) in the 1950s and 1960s to variable analysis techniques in as much as it offered no critique of the cumulative-falsificationist epistemology of structural functionalism. Indeed, Becker and Geer (1957), were concerned to legitimate participant observation as a 'valid' data collection process and to show that participant observation could and should be a systematic technique. Becker and Geer argued that participant observation is not simply an exploratory tool of social research, and that it can generate and test theory and thereby conform to the taken for granted standards of middle range theorising.

For Becker, participant observation was typically concerned both to discover hypotheses and to test them. The problem for Becker was that given the vast amount of 'rich' but varied data, how does one analyse it systematically and present conclusions convincingly?
Becker reasserted the cumulative model view of the development of sociological knowledge by suggesting that participant observation research was sequential and analytically inductive. He pointed to three distinct stages of the fieldwork and a final analytic stage once the fieldwork was completed. The stages were, first, the selection and definition of problems, concepts and indices, second, the checking of the frequency and distribution of phenomena, third, the construction of social system models and fourth, the post fieldwork stage of final analysis and presentation of results.

This four stage process rested upon a falsificationist model of knowledge production. Becker suggested that, after constructing a model specifying the relationships among various elements the model is successively refined by searching for negative cases, thus accommodating the 'Popperian principles' of conjecture and refutation at the level of individual testable statements. Becker suggested that

'While a processural model may be shown to be defective by a negative instance which crops up unexpectedly in the course of the fieldwork, the observer may infer what kinds of evidence would be likely to support or to refute his model and may make an intensive search for such evidence'. (Becker 1958, p. 408).

The final post-fieldwork stage is systematic and involves checking and rebuilding models with as many safeguards as the data will allow, notably by cross classifying all items so that checks can be made as complete as possible.

This approach can, as Becker suggested,

'profit from the observation of Lazarsfeld and Barton that the "analysis of 'quasi-statistical data' can probably be made more systematic than it has been in the past, if the
logical structure of quantitative research at least is kept in mind to give general warnings and directions to the qualitative observer." (Becker, 1958, p. 409)

There is little in Becker (1958) to challenge the prevailing model. The identification of necessary and sufficient conditions reflected the nomothetic approach. The reductionism inherent in isolating basic phenomena and the attempts to build up and elaborate existing sociological theory with its taken for granted social system framework also reflected the concerns of middle range theorising. Becker made no attempt to critique the epistemological underpinnings of prevailing scientific sociology nor took into account Blumer's (1956) reservation.

In an account of ongoing fieldwork practice (published in Hammond (1964), but written around 1960) Geer reaffirmed Becker (1958). She reframed Becker's sequential model in terms of the generation and testing of working hypotheses and their combination into compound propositions. Reflecting Becker, she saw the first operation as consisting of the testing of 'crude yes-or-no propositions', the second stage as 'seeking negative cases' or setting out 'deliberately to accumulate positive ones'. One disconfirming instance, she argued, forces modification. A simplistic falsificationist model is reaffirmed, such that confirmation of 'what is' is accomplished by eliminating 'what is not'. The third stage is elaborated, by Geer, into a proto-path analytic model, following the suggestions of George Polya (1954).

Despite concluding that the first days in the field may transform a study out of recognition, Geer (1964) merely reflected Becker's earlier comments, and did not propose an interpretive ethnography
with distinct epistemological possibilities. She persisted with the advocacy of a value free or neutral observational research. The researcher should not contaminate the research environment by appearing to take sides.

Yet Geer considered problems of interpretive methodology, when, for example, she referred to the problem of empathy. However, the critical potential was not developed and the discussion framed in terms of confronting prejudices.

Mullin's reference to symbolic interactionism in the post-war period as 'loyal opposition' is most apposite, given the gradual ascendency of the functionalist element of the interactionist-functionalist heritage in the 1950s through structural functionalism. That Mullin's 'loyal opposition' did not develop the radical anti-nomothetic critique it might have is probably due to the retention of a pragmatic base rooted in 'Kantian idealism' (Rock, 1978). Even later interactionist developments, (Goffmann, 1959; Scott, 1968) and the emergence of ethnomethodology, were unconcerned to either develop a phenomenological orientation or engage the idealist base which informed American sociology (Rock, 1978). Ethnomethodology, in fact, attempted to synthesise Schutz and Parsons in its radical reformulation of participant informed symbolic interactionism (Filmer, 1972).

5.6.2 Wirth and the German Sociological Tradition

Wirth, like Blumer, advanced a potentially divisive view of sociological research. He drew on two elements of a German socio-
logical tradition and used them to raise fundamental questions about the prevailing nomothetic cumulative-falsificationist model. Essentially, he called into question the possibility of sociological explanation and the possibility of non-ideological social science.

He adopted Weber's concept of 'verstehen' as a framework for reconsidering the complex interrelationship between 'value' and 'attitude'. During the Polish Peasant debate Wirth introduced Weber's distinction between meaning and causal adequacy, by actually reading a lengthy section from Weber (1947). However, rather than confront the epistemological problems of a hermeneutic nature which underpin the understanding-explanation controversy the idea of meaning adequacy was corrupted, by Wirth, and he relabelled 'adequacy' as 'validity'.

For Wirth the importance of the 'verstehen' approach was that it helped to differentiate between 'insight which reveals meaning' and 'causal explanation'. In terms of the Polish Peasant debate, Wirth argued that Thomas's scheme could not be derived from the material itself because that would imply that facts speak for themselves, whereas 'We know facts do not speak for themselves.' (Social Science Research Council, 1939, p. 122). However, if attitudes and values were clearly defined we could verify whether a particular attitude or value was to be found in a document. But no simple definition exists, and this Wirth suggested was because there is a mistaken assumption that values are objective. The mistake, he claimed, is not that values are subjective but that one can ascribe 'objectivity' to sociological data at all.
Wirth, in developing a critique based on Weber, appeared to be on the verge of questioning the fundamental taken for granted view of falsificationist science. However, this was not the case.

'I think we all agree that science is invariant relationship, but this cannot be determined from the observation of a single case. The continuous corroboration by inspection of single cases after single cases adds to the security of the generalizations by proving that the relationship as first observed was not the result of factors other than those specified. That is what I think we mean by proof. Single cases can be used (a) to illustrate the plausibility of a hypothesis before it has been tested by a series of observations in a number of cases. They also can be used (b) to illustrate the operation of a relationship already incorporated in a proved generalization. Regarding the security of judgement of documents, I see no insuperable difficulties if definitions are always unambiguous and exhaustive.' (Social Science Research Council, 1939, p 123).

In effect, then, the adoption of a verstehen informed approach was essentially a means towards explanation. Burgess (1944) presented an idiographic-nomothetic distinction but addressed it in terms of explanatory potential. Thus methods were assessed as to whether they were explanatory or merely exploratory.

'Applicable to personal documents are two methods of interpretation (1) nomothetic or the comparative study of documents in order to arrive at generalizations and (2) idiographic, or the appreciation of the individual case in all its individuality and completeness. Allport asserts that prediction can be made upon the basis of the ideiographic [sic] study of a single case, a claim that has been challenged by others.' (Burgess, 1944, pp. 14-15).

This is an effective denial of the concept of idiographic understanding in the sense of rejecting causal/explanatory laws. In Burgess's paper, idiographic becomes restricted to the Weberian sense of meaning and causal adequacy. For him, the problem became one of methodic problems of control of the researcher (i.e. reliability) when using life histories and attempting 'sympathy, empathy, recipathy, insight and intuition', (Burgess, 1944, p. 15). This perspective lay at the heart of the
adoption of a Weberian approach by American sociologists.

Nonetheless, in his work in the sociology of knowledge, Wirth, along with Shils, were, potentially, developing another route towards a critique of nomothetic sociology. The work in the sociology of knowledge, heavily influenced by German theorising, was seen as radically different. Burgess divided conceptualisation into two camps, a traditional approach from Comte to the present which, he suggested, devises conceptual schemes for analysis of social change, social structure or social function and a sociology of knowledge approach, based primarily in Germany and centering on the theories of Max Scheler and Karl Mannheim, which

'strresses the importance of studying society through an understanding of past, current and emerging ideologies ... the relativity of conceptual systems.' (Burgess, 1944, p.10)

Wirth was a notable scholar of German sociology (Ogburn 1936), enhanced by his year in that country on a Guggenheim Fellowship in 1930. Along with Shils, who was well acquainted with the work of Mannheim, (Minutes of the Society for Social Research, 5.3.1934), he translated 'Ideology and Utopia' and 'The Sociology of Knowledge' into English, providing an introduction to locate the texts in an Anglo-Saxon context.

For Burgess,

'the outstanding value of the sociology of knowledge for social research inheres in its function of manifesting the intimate interaction and interdependence of social life and social science. The understanding of this relation is essential to the investigator in grasping the nature and limitations of research and in appreciating the conditions which permit and handicap activity' (Burgess, 1944 p. 11-12).
Wirth's development of a sociology of knowledge approach revolved around the notion of ideal type and reflected the concerns developed by Parsons (1937, 1951). Functionalism adapted ideal typification in a non-hermeneutic way and reflected the long term concerns of US sociology with German social philosophy which had underpinned much social theorising at Chicago as well as elsewhere (Dibble, 1972; Rock, 1978). Wirth and Parsons more explicitly developed this continental influence. Wirth was more radical in the sense of pursuing the 'relativistic nature of knowledge' and of 'ideology' (which Burgess acknowledged), while Parsons was more concerned with a synthesis (including Durkheim) and dissipated some of the central concerns of, e.g. Mannheim, utilising the more familiar concepts of value and norm (and hence central value system) instead of ideology. Parsons, thus, in adapting to an American setting watered down the critique implicit in the concerns of the German sociology of knowledge approach.

Wirth's development of the approach, as evident in his contributions to the Polish Peasant Conference, made no attempt to engage the dissipation of the critical element of the German sociology of knowledge approach apparent in Parson's development of functionalism. The result was limited and uncritical, essentially merely pointing to the interrelationship of research and milieu and questioning the possibility of an absolute objectivity.

Any potential for an alternative critical approach encapsulated in the sociology of knowledge orientation was defused by events. The rise of McCarthyism created a context in which any radical
sociology was difficult to sustain. More specifically, the
premature death of Wirth in 1951 effectively saw the end of any
chance of a radical alternative, based on a 'German philosophy of
knowledge' approach, being developed at Chicago [10].

The critique of the prevailing cumulative-falsificationist model
that can be identified in the work of Blumer and Wirth did not
materialize into a fully articulated alternative practised by the
Chicagoans. Rather, elements of symbolic interactionism and the
sociology of knowledge approach tended to be absorbed in the
prevailing model. There was no alternative 'Chicago' approach,
Chicago remained mainstream, epistemologically and methodologic-
ally, very much a part of the emerging cumulative-falsification-
ist theorising which came to dominate what Mullins (1973)
described as standard American sociology.

Arguably, only a few small scale alternatives to the prevailing
nomothetic approach existed in the United States. These included
the development of phenomenology at Buffalo and at the New School
of Social Research (Spiegelberg, 1976), besides the work of
individuals critical of the prevailing tendency such as Mills
and those early social critics who fell under the umbrella of the
'New Sociology' (Horowitz, 1964a, 1964b). Added to this, could be
those sociologists who adopted a policy of retrenchment to social
problems, encapsulated in the publication of the journal of the
same name.
5.7 Conclusion

It is argued here, then, that Chicago sociology was neither atheoretical empiricism nor was it restricted to urban sociology. Certainly the Chicagoans promoted empirical work but always alongside theoretical development. Wirth (1938), in proposing specialised sociology training in the department suggests four quarterly courses, social psychology, social organisation, population and ecology and methods of investigation. He added

'The above curriculum does not contain any reference to what is known as sociological theory; this is deliberate because it should be infused into every course we give and should not be separated out into a special course'.

Park's scepticism of statistics (explored in detail in chapter four above) was not evidence of an epistemological disagreement with the nomothetic base of interactionist sociology. Rather, it was a concern that aggregates were unable to adequately incorporate the subjective factor into nomothetic analysis. Wirth, (1944, p.4) noted of Park, that

'Objectivity in the realm of the social ... was to be achieved not primarily by collecting facts and ignoring values but by overtly examining values and especially by becoming conscious of those values that we take for granted.'

Park saw social science as a natural science up to a point, that is, its methods were a good starting point but that 'one would soon enough encounter the values, morals, and preferences of men before which the methods of natural science would prove inadequate'. He thought that society was not a closed system and that it should not be seen as either an 'artefact' or 'as a system of mechanical forces', rather, society was a set of reciprocal claims and expectations and mutual understandings.
This methodological orientation was intrinsic to the theoretical development of sociology at Chicago and in the United States in general.

As has been shown, the Chicagoans worked at various levels of theoretical concern. Over time, there was a tendency to move from general holistic views of the social world to specific testing of theories, thus reflecting the direction being taken by the sociological profession in its attempt to legitimate sociology as science. The Polish Peasant study, and the synthesis embodied in it by Thomas, constituted the initial break with the 'armchair theorising' of the past (as in the work of Sumner, Ross, Tarde, etc.). Thomas's theoretical orientation encapsulated in the 'social disorganisation paradigm' became resolved into general theories of change and interaction and much of the work done revolved around those theories and particular developments of them.

Later generations of Chicagoans became more concerned with particular issues and methodological confrontation. However, abstracted empiricism (Mills, 1959) was certainly no part of Chicago sociology up to 1950. That element of the Chicago mythology which suggests that the 'School' was atheoretical is not borne out though the Chicagoans did have strong empirical concerns. They developed theory at various levels through an explicit inductive approach. Chicago sociology of the 1920s and 1930s was self consciously an attempt to 'objectively develop theory'. It was, as Park would have it, 'Big Picture' sociology, based upon general theoretical perspectives evolving out of a
context for study synthesised and bequeathed to the Chicagoans by Thomas.

Chicago was not operating in isolation in the generation of theoretical concepts, nor was its major theoretical orientation, social disorganisation, a concept restricted to Chicago. Chicagoans provided conceptual frameworks and freely embraced concepts developed elsewhere. Chicago was not characterised by doctrinal debates (Rock, 1978).

What was important, as Burgess (1944) argued, (pre-dating C. Wright Mills and Robert Merton), was to combine data collection and abstract theorising. He suggested that the work of Thomas, Park and their students were examples of such sociology and that the operationalism of Lundberg was in danger of assuming that the operational definition be equivalent to the conceptual definition thereby jeopardising theoretical development.

Empiricism was important to the Chicagoans but was not an end in itself. From the first, empirical observation was ordered and categorised and inductive theorising was attempted, notably through attempts at typification which gradually became more sophisticated as American sociology adapted Weberian ideal types to its own needs. Indeed if any one aspect of the work done by the Chicagoans can be said to be indicative of their approach it is the penchant for typification that probably has the largest claim. The reports of research presented to the Society for Social Research by both internal and external speakers contain numerous references to various attempts at typifying interactive processes, subject groups and functional objects. Many of the
dissertations written by students throughout the period 1915 to 1950 contain a classificatory scheme as an important element of theoretical development.

Burgess, reflecting developing perspectives throughout the discipline, was instrumental in the encouragement of ideal typification at Chicago. By the 1940s Burgess argued that it was the only appropriate methodic development for dealing with personal documents. His view of ideal typification which he says is derived from Simmel, Tonnies and Weber is a process of

'abstracting from concrete cases a characteristic ... accentualizing it and defining it clearly unambiguously and uncomplicatedly by other characteristics'. (Burgess, 1944, pp. 15-16).

Burgess admired the role of ideal typification in Weber, Simmel and Sorokin but was concerned that ideal typical constructions may not be actually represented by concrete phenomena. Here Burgess touched on the very core of abstraction, for he wanted abstractions to mirror 'reality' whereas he remarked that the above concern does not worry ideal type analysts who maintain that only approximations may be located in society. For Burgess, the lack of 'mirroring abstraction' inhibits measurement, as one is left with the problem of degree of approximation. Thus, Burgess detached ideal typification from verstehen.

Burgess, following the developing practice in the United States, suggested that ideal typifications provided the endpoints of scales (which more closely represented the variations of concrete, normally distributed, phenomena). This provided him with a link to statistical analysis. He also noted the ineffec-
tiveness of critiques of ideal typification from 'statistically minded students' who argued that gradation of scales with peaking in the centre undermines ideal typical dichotomisation. Burgess maintained that such a view was fallacious because the endpoints are still clearly conceptually sound. Burgess, like the contributors to the conference on the 'Polish Peasant', redefined ideal typification to correspond with nomothetic, measurement concerns and 'utilised' it to incorporate personal documents into quantitative work.

The Chicagoans were far from atheoretical empiricists divorced from the development of sociological theory in the United States. It was not until the late fifties that any distinction between the orientation of the Chicagoans of the twenties and prevailing structural functionalist perspectives could be identified. In 1939, for example, Parsons wrote to Wirth thanking him for his review of 'The Structure of Social Action' and noted that the synthesis contained in the book, although directed to Durkheim, Weber and Pareto, also incorporated the theoretical position of Dewey, Mead, the 'cultural anthropologists' and 'I think, your own colleagues' (Parsons, 1939).

Blumer, similarly, suggested that a concern with structure only emerged as a dominant orientation in recent times.

'I think the fundamental premise in the case of Park and Thomas and the associates there at Chicago is just that of recognizing that a human group consists of people who are living. Oddly enough this is not the picture which underlies the dominant imagery in the field of sociology today. They think of a society or group as something that is there in the form of a regularized structure in which people are placed. And they act on the basis of the influence of the structure on them. This is a complete inversion of what is involved and I would say the antithesis of the premise that
underlay the work of Park and Thomas.' (Blumer, 1980b, p. 261)

Nonetheless, this development towards a structural approach, towards looking at people as if they were products of social factors, did not suddenly occur around the fifties, as is often assumed. Rather, as has been illustrated above, it emerged throughout the preceding quarter of a century and Chicago university played its part in this change [11]. Retrospective accounts which focus only on a narrow output of sociological work from the 1920s at Chicago and compare it with sociological practice in the late 1950s and early 1960s are misleading in detaching the Chicagoans from the evolution of sociological theory in the United States.
NOTES TO CHAPTER FIVE

1. Cavan's choice of Thomas and Wirth as indicative of the staff at Chicago is strange. Thomas had left in 1918 and Wirth was a graduate student himself until 1926, the year Cavan received her doctorate, and merely an instructor from 1926 to 1929 (before moving to Tulane University).

Further, Cavan's suggestion that Wirth was opposed to theory construction is surprising. Wirth was particularly interested in theoretical developments in sociology and was himself concerned with the sociology of knowledge. As part of this endeavour he circulated the department with a memorandum, part of which pointed to the traditional theoretical concerns of sociology.

'As a general discipline sociology seeks to understand what is true of human behaviour by virtue of the fact that man everywhere leads a group life.... The analysis of personality and collective behavior falls into a branch of sociology known as social psychology. The analysis of social institutions and of social structures and the processes of social interaction through which these structures come into being and change constitutes the field of social organisation. The environmental factors, resources and the technology conditioning populations, communities and social life generally, and the extent to which their relationships between man and man are, among other factors, influenced by the habitat, constitute human ecology. (Wirth, 1938)

This hardly seems to indicate a lack of concern with theoretical enterprises, even if it was written, in all probability, some time after Cavan was acquainted with Wirth. Nonetheless, Cavan would presumably have been acquainted with Wirth's own thesis, which, although providing an extensive historical analysis of the development of the Ghetto, further developed and refined Park's race relations cycle.

Wirth (1948) reflecting on his involvement in sociology at Chicago summed up the approach to theory and practice adopted at Chicago.

'Insofar as we wish to be a science we must seek to establish valid generalizations. Hence, we are concerned with a description of unique instances only insofar as they can be used for the establishment of generalized descriptions and more abstract general propositions. We should try to carry our findings to as precise a point of measurement as the data and our techniques allow. I do not, however, agree with those who believe that measurement is the only criterion of science. The propositions at which we arrive should have predictive value, but here again quantification is not a necessary element in prediction.... In my work in theory, especially through my years of teaching it to graduate students, I have tried to emphasize that theory is an aspect of everything that they do and not a body of knowledge separate from research and practice.'
2. Commentators have suggested that this drew upon, or was consistent with, Durkheim's anomie thesis. There is a line of argument (Farbermann, 1979; Tiryakian, 1979a) that suggest a kind of continuum from the 'Durkheimian School' to the Park-Burgess 'Chicago School'. Farbermann (1979) contended that 'What Park wanted to discover were the physical, social and psychological mechanisms through which society tamed its members. In attempting to delineate the social mechanisms of control, [Park] leaned heavily on Durkheim's conception of collective representation and Cooley's notion of the primary group; for the physical mechanism, he drew on the perspective of ecology: for the psychological mechanism, on Thomas and Znaniecki's view of personal evolution as well as Sigmund Freud and Alfred Adler's notion of sublimation and compensation' (Farbermann, 1979, p. 12)

Park and Burgess adopted the idea of the 'corporate existence of the social group', as something more than 'the sum of the parts' as 'the fundamental fact of social control' from Durkheim. The group is 'fundamental in forming the social nature and ideals of the individual'. Farbermann implied this to mean that Park saw the individual as 'largely determined by forces and processes over which he had but faint awareness and little control'. Thus Park saw manipulation for control purposes as possible because the individual has to fit into a pre-existing world. Drawing on Thomas and Znaniecki, Park cited personal disorganisation as pointing to an inevitable and constant struggle for personal self-expression. This struggle arises out of the basic motivational forces of the psyche, as summarised in Thomas' four wishes.

3. In 1935 a Divisional Seminar in Race and Culture Contacts had been established at Chicago, meeting weekly under the direction of Blumer, Park, Redfield and Wirth and 'had the co-operation of about thirty graduate students from various parts of the University' (Wirth 1935). The following reported to the Seminar in 1935: Wirth, Redfield, Blumer, Lohman, Pierson, V. E. Daniel, M. Sprengling, A. Baker and Warner of Chicago, plus J. H. Johnson (Virginia), Park (Fisk), Tomasic (Rockefeller Foundation), Mitchell (Washington D.C), Hansen (Miami), Malinowski (LSE), Reuter (Iowa), J. Merlant of the United States Military Academy and P. Nash of the Klamath Reservation, Oregon.

4. No attempt is made here to define the terms 'interpretive', 'phenomenological' or 'positivistic' as they are not used as a basis for comparison but merely indicative of the type of general contrast implied by some commentators when comparing, for example, 'Chicago sociology' with the structural functionalism of the Columbia sociologists, (Bogdan and Taylor, 1975)

5. The use of the term falsificationism here is indicative of the kind of approach that predominated in U.S. sociology throughout the period of this study. This is not intended to represent an assertion about the nature of sociological enquiry in terms of a discussion as to whether American sociology is
characterised by inductivism or hypothetico-deductivism, both of these are subsumed within the term falsificationism, especially as characterised by Lakatos (1970). Lakatos defined, in effect, three levels of falsificationism, naive, sophisticated and the refined version of sophisticated falsificationism to be found in his own methodology of scientific research programmes. The use of the term here is not meant to refer to any one of these in particular, but to embody the central tenets of falsificationism, viz. conjecture and refutation, cumulative progress through empirical validation of theoretical, falsifiable statements, and the acceptance of the impossibility of deductive or inductive proof. The niceties of the debate as to how science progresses, which underpins Lakatos' distinctions of types of falsificationism is not germane to the use of the label here. As has been shown elsewhere (Chalmers, 1978) all falsificationist models ignore, in the last resort, the value laden nature of observation, see science as ultimately self-legitimating through its own protocols and divorce scientific knowledge production from the wider scientific milieu. It is this critique combined with the general characteristics of falsificationism that makes the term appropriate as a descriptor of American sociological endeavours in the period under consideration.

6. Merton codified the cumulative theory approach in various articles in the 1940s, which became the basis for the middle range theorising perspective so important to structural functionalism. The preponderant view, to which Chicago sociologists contributed was elaborated in Merton's exposition of middle range theorising. The reference to middle range theory in the thesis is directly to Merton's formulation, although it is argued that such a formulation reflected sociological practice to which the Chicagoans subscribed.

Merton (1948) noted the continuity of theory and cited various instances including some integral to Chicago, notably the 'conflicting self' or 'marginal man'. He pointed to developments in this sphere of theorising but suggested that the central problem of conflicting roles

'has yet to be materially clarified and advanced beyond the point reached decades ago. Thomas and Znaniecki (1918) long since indicated that conflicts between social roles can be reduced by conventionalization and by role-segmentation.'

(Merton, 1948, p. 515)

Merton (1945, 1948, 1949) in laying out the basis of middle range theorising attempted to forge a clear link between empirical research and social theory. Rather than 'the social theorist high in the empyrean of pure ideas' being replaced by the researcher 'equipped with questionnaire and pencil and hot on the chase of the isolated and meaningless statistic', he saw the interaction of theory and empirical research with empirical data informing theory and vice versa. In practice, however, he maintained that there were still those sociologists who did not link theory with research. Merton(1949) reflected Bain's contribution to the Polish Peasant debate of a decade earlier
(Social Science Research Council, 1939) when he identified six approaches to theorising: methodology, general sociological orientation, analysis of concepts, 'post factum' interpretation, empirical generalisation and sociological laws. Methodology, he argued has nothing to do with substantive theorising. Conceptual analysis, Merton argued, is indispensable if confined to clarification of key concepts. However, to think of conceptual manipulation and definition in itself as theorising is spurious. Applying conceptual schemes in a 'post factum' and (heinously) ad hoc manner to data, similarly does sociological theorising a disservice. Indeed, Merton scathingly attacked approaches which collected data and then subjected them to interpretive comment. He regarded such approaches as having the logical structure of clinical enquiry because they do not test pre-designed hypotheses. This applied to both statistical and case-study data. The result is a merely plausible explanation. General orientations merely indicate the approach, such that Durkheim's orientation was that social facts should be sought in the facts that preceeded it, and Znaniecki and Sorokin (amongst others) invoked a 'humanistic coefficient' as orienting principle. They are non-empirical generic orientations and must be specified in terms of empirical generalisations. In isolation such generalizations are nothing more than summaries of observed uniformities in observational data. It is the combination of concept clarification, orientation, empirical generalisation within a theoretical frame that provides sociological theory. At the extreme this manifests itself as sociological laws. While this status is rarely achieved, Merton argued that it is possible to work towards it through the cumulative development of theory. Middle range theorising provides that possibility. Merton saw 'middle range theory' as the pragmatic answer to the continuing development of sociology which, he admitted must 'ultimately meet the canons of scientific method'. In this respect, Chicago sociologists would not have disagreed.

7. 'It was more and more borne in upon me that I could never really see the social life of man unless I understood the processes of mind with which it was indissolubly bound up. I saw that there was a gap between the ideas of structure and function I had so far been working on and the actual motives and behavior of men, which left the former somewhat hanging in the air...' (Cooley, 1930, p. 30). Referring to his work in the first decade of the century. Letter 3/6/1924

8. Methodically, this 'standard' view required the invention of an instrument for the study of particular phenomena and the question arose as to whether such an instrument existed. Given the role of the reflective consciousness

'an instrument is needed which provides the investigator access to the inner life of the person and to the web of intercommunications between persons.' (Burgess, 1944, p.9).

Burgess argued that the life history, like the microscope in biology, was an important element in study of the particular as
it gets 'beneath the surface of the externally observable' (Burgess, 1944, p. 9).

Nonetheless, for sociology to develop objective theory an objective record of behaviour is needed, according to Burgess, therefore

'the perfection of the interview and its recording are of signal importance for sociological research.' (Burgess, 1944, p. 10).

9. Chicago based researchers also tended to adopt a functional approach in the main and functionalism was taken for granted. For example, Dollard (1931) was described as elucidating a

'functional relationship between the activities of the gang and the economic organization of the society on which it preyed.' (Bulletin of the Society for Social Research, June, 1931, p. 1)

10. In addition, Shils was not an official member of the sociology department staff from 1948 to 1957. As Bulmer points out (personal correspondence) he taught part of the year during this period but was attached to the Committee on Social Thought. However, the extent to which Wirth may have developed a critical (neo-Marxist) alternative despite the prevailing tendency is difficult to judge. Parsons, with whom Shils was to work, killed off the critical potential by anaesthetising ideology in terms of 'values', 'central value system' etc., (Centre for Contemporary Cultural Studies, 1978).

11. Blumer noted that the structural approach

'was already beginning to emerge, interestingly enough, in Chicago right at the university there, back in the late '20s. It was well-represented by a very, very able, almost colossal figure in his own right, namely Thurstone - L. L. Thurstone - the psychologist, with his work on attitude studies, that [work] having an enormous influence on the work of Stouffer inside our department.' (Blumer, 1980b, p. 265)
CHAPTER SIX

THE ROLE OF MEAD IN THE 'CHICAGO SCHOOL'
6.1 The Myth

The fourth of the myths about the Chicagoans analysed here is that which relates to the influence of G.H. Mead on the 'School'. At its extreme, this myth is stated in terms of the centrality of Mead to all the work done in the 'Chicago School'. Thus Ciacci (1972) argued that German idealism, pragmatism and evolutionism were combined in Mead's work and became part of the 'Chicago School'. Mead's ideas showed up in the work of Thomas, Park, Burgess, Wirth and R.E.L. Faris and later in the work of Blumer and Hughes. Indeed it went further and can be seen in the preoccupations of Peter Berger and Alfred Schutz.

A less all embracing and more widespread view is that despite the diverse origins of interactionism, Mead was the 'founding father' of symbolic interactionism. This can be found in statements of two central figures within the tradition, Herbert Blumer and Manford Kuhn.

'A view of human society as symbolic interaction has been followed more than it has been formulated. Partial, usually fragmentary, statements of it are to be found in the writings of a number of eminent scholars... Charles Horton Cooley, W.I. Thomas, Robert E. Park, E.W. Burgess, Florian Znaniecki, Ellsworth Faris, James Mickel Williams... William James, John Dewey and George Herbert Mead. None of these scholars, in my judgement, has presented a systematic statement of the nature of human group life from the standpoint of symbolic interaction. Mead stands out among all of them in laying bare the fundamental premises of the approach, yet he did little to develop its methodological implications for sociological study.' (Blumer, 1962, p. 179)

'The year 1937 lies virtually in the middle of a four-year period which saw the publication of 'Mind Self and Society', 'Movements of Thought in the the Nineteenth Century', and 'The Philosophy of the Act'. It would represent the greatest naivete to suggest that thus the year 1937 represented the introduction of symbolic interactionism. We are all aware of the long development: from James, Baldwin and Cooley to
Thomas, Faris, Dewey, Blumer and Young.... Nor is it the fact that Mead represents the fullest development of the orientation that makes so significant the posthumous publication of his works. Mead's ideas had been known for a very long time. He had taught University of Chicago students from 1893 to 1931. His notions were bruited about in classes and seminars wherever there were professors conducting them who had studied at the University of Chicago.... No the significance of the publication of Mead's books is that it ended what must be termed the long era of the "oral tradition", the era in which most of the germinating ideas had been passed about by word of mouth.' (Kuhn, 1964, p. 61)

Mead has thus been acknowledged by most commentators on interactionism and symbolic interactionism as the main 'founding father' of that intellectual orientation (Fisher and Strauss, 1978, p. 483; Deutscher, 1973, p.325; Mullins, 1973 ). He is assumed to be provider of the general theoretic orientation which later became encapsulated in the symbolic interactionist approach propounded by Blumer. (Manis and Meltzer, 1978, p.1; Lindesmith, Strauss and Denzin, 1977; Lauer and Handel, 1977, p. 9; Littlejohn, 1977; Kando, 1977, p. 104; Meltzer, Petras and Reynolds, 1975; Ritzer, 1975, p. 97; Warshay, 1971, p. 28; Meltzer and Petras, 1970; Petras, 1966; Young and Freeman, 1966, p. 564; Woodward, 1945, p. 235; Faris, 1945, p. 554).

The identification of Mead with the roots of symbolic interactionism (Huber 1973a, 1974; Schmitt, 1974; Stone et al, 1974) has led to him being given considerable prominence within the 'Chicago School'. His relationship with the Chicagoans and the assumption about his central role in the genesis of a symbolic interactionist perspective are analysed below.

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6.2. Mead's Direct Involvement with the Department of Sociology at Chicago

The extent to which Mead was an important figure in the 'Chicago School' has, however, come under closer scrutiny recently, despite Fisher and Strauss's (1978) assertion of the fruitlessness of such analysis [1]. Mead's direct impact on the Chicagoans is not as clear cut as it once was assumed to be, either in terms of his pedagogic input nor the assimilation of his ideas.

Mead is usually assumed to have had a very important role in the development of Chicago sociology not least because of the direct teaching link he had with the Department of Sociology. This link, however, is not as strong as is often popularly supposed (Goddijn, 1972a; Mullins, 1973).

Mead taught a course in Advanced Social Psychology in the Philosophy Department (until 1932) which was an available option for sociology students. Mullins (1973) suggested that this was a dynamic course but as Faris (1967) and Carey's interviewees confirm not everyone found Mead's course enrapturing. Mead, it seems, was not a dynamic lecturer, tending to 'think out loud' and rarely providing opportunities for questions. Carter (1972), for example, recalled that Mead was brilliant but difficult. Mead, it seems, was largely unavailable for informal discussion with students.

Furthermore Lewis and Smith (1981) asserted that relatively few students took his course. On the basis of the examination of class enrolments, citations and so on, Lewis and Smith suggested that Mead was only peripherally involved in sociology at Chicago.
Kuklick (1984) disputed the figures used by Lewis and Smith and their interpretation. She calculated that '72.2% of the recipients of Ph. D.'s from 1910 to 1924 had studied with Mead'. Further, when Faris was appointed to the Department in 1919 he taught a social psychology course which was heavily reliant on Mead's views and acted as a surrogate for Mead. This appears to be unsupported by Carter's (1972) recollections, though.

'Cooley was their [the sociology faculty's] God - Faris quoted Cooley all the time.'

Kuklick further asserted that to attempt to assess Mead's impact by counting formal citations in theses, texts and articles is spurious because his ideas were so widespread that they were taken for granted. This is attested to by Anderson's statement, quoted by Lewis and Smith that

'he did not seek personal exposure to Mead because he was "getting Mead second hand enough for my needs".' (Kuklick, 1984, p. 1436)

This idea that Mead's views were so 'taken for granted' is not necessarily supported by an assessment of his engagement with the Society for Social Research. When he addressed the Society in 1929 he drew a very large attendance of over fifty graduates and faculty. This, however, seems to be the only occasion that he did talk to the Society throughout the 1920s. Mead, unlike some faculty in other departments, was, surprisingly for someone supposedly so central, not a member of the Society.

Furthermore, after Mead's death (up to 1935) only one session was given over to discussing Mead's philosophy. This session was the occasion of an address by Morris from the philosophy department on the nature of the 'significant symbol'. It is also notable
that Morris edited the post-humous publication of Mead's work, rather than any of the sociologists, on whom Mead was supposed to have had such an enormous impact. It is rather too glib to suggest that Mead was so taken for granted that first hand exposure to his ideas were not necessary for the Chicagoans. Park and the other sociologists, Thurstone, Gosnell and Lasswell all regularly addressed the Society.

6.3 Mead's Theoretical Impact on the Early Chicagoans

Various commentators (e.g. Faris, 1967) have suggested that Mead had a direct impact on the theoretical developments in the Department of Sociology at Chicago during his lifetime. Strauss, on the other hand, in the introduction to the 1964 edition of Mead's collected papers (Mead 1964), noted that despite Mead's early influence on the philosophy department, the sociologists did not begin to notice him until the 1920s and even then Thomas and Park drew little directly from Mead. Mead was not even included in the readings in the Park-Burgess text of 1921. The vast majority of students and staff in the sociology department at Chicago appeared not to utilise Mead's social psychological perspective directly during his lifetime. The sample survey of doctoral theses at Chicago (Appendix 6) shows that there were hardly any citations of Mead, twelve (29%) of the forty two theses examined cited Mead but only four actually used Meadian concepts, and three of the four were submitted after 1940. Both Thomas and Cooley are cited far more frequently than Mead. Cooley is referred to in twenty of the sample (48%), thirteen (65%) of these were submitted before 1940. Thomas is cited in half the
sample, two thirds of these prior to 1940.

Lewis and Smith (1981) argued that Mead had little direct influence, except on a small group of graduate students. And this influence only emerged after 1920, at a time when enrolments in Mead's classes were declining. This, they claimed, can only be attributed to the role played by Ellsworth Faris and later by Herbert Blumer, both of whom were somewhat at variance with the theoretical perspectives of the rest of the department.

Cavan pointed out that Ellsworth Faris was in many ways distinct from the other faculty members in being a social psychologist interested in personality and was a sharp critic of both the local projects and also of the instinct hypothesis of psychologists which (in one form or another) seemed to be retained by the Chicagoans around the 1920s (Small's interests had become Thomas's wishes). Faris was a little remote, a loner (Cavan, 1972) and it was he who provided the basis for Blumer's development of social psychology which Janowitz (1980) regarded as out of the mainstream of the work in the department.

6.4 Mead as 'Founding Father' of Symbolic Interactionism

While Mead may be seen as somewhat peripheral to the activities of the 'Chicago School' of his day, he is nonetheless usually seen as the central founding father for the symbolic interactionism that emerged in the later developments at Chicago and spread, or were developed elsewhere, such as at Iowa (Petras & Meltzer, 1973; Carabana, 1978). If this is the case, this suggests that there is a dual tradition at Chicago, an early Thomas-Park
tradition and a later Mead-Blumer tradition.

6.4.1 The Dual Tradition Thesis at Chicago

Fisher and Strauss (1979) have attempted to put the position of Mead into perspective [2]. They asserted, essentially, a dichotomous tradition at Chicago, the interactionism of Thomas and Park and the symbolic interactionism developed by Blumer and based on Mead.

'There would, then, seem to be at least two interactionist traditions, each grounded in a different intellectual history .... While some interactionists owe little or nothing to a Meadian perspective, the work of others is rooted in both Mead and what is nowadays called the Chicago-style perspective, which derives in fact, mainly from Thomas and Park. A younger generation, coming more lately to interactionism and in a period after the Chicago Department of Sociology had radically changed in character, seem to divide - some moving toward Meadian interactionism, others doing work in accordance with the spirit of Chicago-style sociology. Still others draw on both sources of interactionism' (Fisher and Strauss, 1978, p 458).

Whether there was two traditions, as Fisher and Strauss contend, with some overlap so that the role of Mead has been taken to be intrinsic to both, while really only germane to the later development, needs to be examined. Crucial to any divergence in the sociological tradition at Chicago would be the role of Elsworth Faris.

Strauss (1964), Kuklick (1984) and Faris, R. E. L. (1967), emphasised the importance of Ellsworth Faris on the emergence of Mead and suggest that during the 1940s Mead entered the mainstream of sociological thought at Chicago and elsewhere and became the social psychologist for sociologists.
There is some evidence that Faris offered an alternative to the pragmatic view of social psychology central to the department. For example, the Society for Social Research devoted three sessions (25.2.1927, 10.3.1927, 27.4.1927) to 'Problems in Social Psychology' at which Park and Faris presented their views.

Park saw social psychology as a subject concerning the individual and the community. He asked what is the role in communication of 'the sympathetic participation of one person in the feeling of another?' An individual admits that another has claims on him or her when one places him or herself in the position of the other and finds it appeals to his or her feelings. Social psychologists are also interested in the natural history of the conventionalisation of appetites. Material relating to this may most profitably be gleaned from ethnology with a view to answering the main question of the relation of community to human nature.

Faris, on the other hand, argued that community needs to be considered from four standpoints: spatial grouping, associations, social movements and the Zeitgeist. On one side there are elements of the community, on the other the impulsive individual. Personality develops out of this interaction and communication is a process of this interaction. Personalities are classifiable into two broad classes, the modal and the extremes.

In his 'Of Psychological Elements' (1936), Faris demolished the lingering remnants of the instinct theory of social psychology, which L. L. Bernard (1924) had substantially weakened. Faris attacked notions such as 'interests' (Small) and 'wishes' (Thomas) as being vestiges of old and unsound motivational
doctrines. The long running disagreement over these notions, he maintained, arose because it was impossible to agree on something that did not exist. He turned round the idea that society is the construct of individuals and argued that society produces personalities and these

'will be found, not in the individual self at all, but in the collective life of the people.' (Faris, 1936, p. 167)

However, this did not constitute a break with the Thomas inspired tradition at Chicago. All it served to do was to clear away the archaic clutter of residual motivations from a theoretical orientation already well grounded in the development of the social self. The important role played by Cooley, at least in as much as Park promoted his views, lay in his elaboration of the social self, through his concept of the 'looking glass self'. Faris, then, pursued a line of thought that may be traceable back to Mead's influence, evident in his review (Faris, 1936a) of Mead's posthumous work ('Mind, Self and Society') in which he maintained that the title belied the author's intention and argument and suggested that 'Society, Self and Mind' would have been more fitting. This, however, does not mean that Faris was out of step with the general perspective that underpinned the sociological work in the department.

6.4.2 The Single Interactionist Tradition at Chicago.

It would seem unlikely, then, that two separate traditions with distinct roots and adopting different theoretical perspectives and methodologies developed at Chicago in the 1930s. There was, though, no uniformity of approach in the sense of a
single practice devoted to a narrow theoretical base. The discussion in the previous chapter, however, indicates that Chicago developed a sociology in accord with the predominant view of the discipline in America. This sociology was at root nomothetic, falsificationist and directed to a cumulative growth of knowledge model. Interactionism was not at variance with this perspective but embedded in it. The role of Mead in this development is paradoxical. He, and those sociologists like Faris and Blumer who regarded Mead as their theoretical mentor, were to some extent peripheral to the central sociological enterprise; yet Mead served as a focus for a sharpening up of the rather loose general theoretical perspective which pervaded (and indeed continued to pervade) Chicago sociology.

Taking on board Mead more systematically, if not adopting his perspective entirely, engendered a more cutting analysis of certain aspects which had been inadequately analysed in the development of a sociological perspective. Examples of this are: Faris' critique of residual instincts; Wirth's and Blumer's analysis of the construction of meaning and the nature of the self; their combined critique of the 'naive' notion of scientific method expounded in the Conference on the 'Polish Peasant'.

So, although Blumer's development of Mead was somewhat peripheral to the mainstream of Chicago sociology and never constituted an alternative tradition distinct from the Thomas-Park heritage, it did not engage, as has been shown in chapter five, with the prevailing approach in American sociology to which Chicago sociology subscribed.
There are, however, critics who argue that the links made between Chicago sociology and Meadian social psychology are extremely tenuous and constitute a complete misreading of Mead. Blumer, himself, is singled out as the responsible party in creating a myth which legitimates his approach to symbolic interactionism in terms of Meadian constructs. In short, if the epistemological, theoretical and methodological divergences between Blumer and Mead can be sustained as critics maintain, then the assumed role of Mead is clearly misleading.

6.5 Differences Between Mead and Blumer: The Recent Debate

Over the last twenty years a debate as to the Meadian underpinnings of (Blumerian) symbolic interactionism has simmered. The attribution of the genesis, of what Blumer came to label symbolic interactionism, to Mead conceals, it is argued, fundamental differences in Blumer's and Mead's approaches. Since Bales (1966) reply to Blumer (1966) concerning the nature of operationalisation which incidentally questioned Blumer's appropriation of Mead's perspective, several acrimonious exchanges have taken place between Blumer and sceptics. The problem with this kind of argument is that of a 'correct' exegetical analysis of the compared theorists (Cook, 1977). Thus, no attempt is made here to determine 'what Mead really said'. The debate will be outlined below and its significance and relevance to the examination of the work of the Chicagoans as a whole assessed.

Blumer is accused of differing from, or distorting Mead in a variety of ways. These may be grouped under the headings of
epistemological incompatibility, theoretical divergence and methodological incompatibility. These, however, should not be seen as mutually exclusive.

6.5.1 Epistemological Incompatibility

Lewis and Smith (1981) argued that pragmatism as a philosophy was not a unified approach and that Mead like Peirce was a realist while James and the other pragmatists were essentially nominalists. Blumer, and the Chicagoans, they argued, derived their pragmatism from James and Dewey and thus espoused a nominalist perspective. Thus, they argued, not only did Mead have far less contact with the Chicago staff and students than is commonly supposed, his epistemological orientation set him apart from his contemporaries and was even alien to the theoretical perspective he is supposed to have generated. Lincourt and Hare (1972) associate Mead with a 'continuous tradition on selfhood' made up from Peirce, Royce and Wright which anticipate contemporary symbolic interactionism.

Bales (1966) ignored the nominalist-realist debate and simply accused Blumer of being a philosophical idealist, unlike Mead, who was a 'pragmatist and social behaviorist'. Carabana (1978) extended this accusation to all symbolic interactionists. McPhail and Rexroat (1979) similarly argued that Mead was a consistent Pragmatist who rejected realism and idealism. For him, reality is presumed but science orders observed events through 'convergent responses which establish objective facts'. Like Lewis and Smith, they suggested Blumer derived his pragmatism from James and Dewey
rather than Peirce and Mead, and tended to vacillate between idealism and realism. Sometimes he presented reality as depending upon how it is perceived, at other times he insists that reality 'talks back' and does not 'bend to our conceptions of it'. For Mead, objectivity is consensual, for Blumer, it is contingent upon a perceptual event. Thus Blumer's claimed pragmatist stance 'bears no resemblance to Mead's position'.

Blumer repeatedly asserted that Mead's ontological position is the same as his own, namely that there is a real world but that it does not have a basic intrinsic makeup but changes as humans reconstruct their perceptions of it. This is neither idealist nor realist.

6.5.2 Theoretical Divergence

Bales (1966), Stewart (1975), Lewis (1976), and Stryker (1977) have all pointed to theoretical differences between Blumer's symbolic interactionism and Mead's social behaviourism. Such differences are to do with, first, the centrality of 'self', second, the relationship of the 'I' and the 'Me' and, third, the universality of significant symbols.

On the first point of difference Blumer emphasised the active moment of the self (Carabana, 1978) and this is regarded as at variance with Mead. Bales (1966), suggested that Mead placed social interaction at the centre of analysis rather than mind, or self, or society and that starting with any one of these elements as fundamental was a dead end because the other two could not be derived from it. Blumer ignored this warning and, according to
Bales, seemed to start with the self, and in this sense he was not a social behaviourist like Mead. For Mead, the self arises out of human interaction and, thus, is an 'interposed process between stimulus and response'. Blumer saw the self as fundamental and thus argued for ascertaining the meaning that objects have for the social actor. He was opposed to an external 'objective' view of actors and actions. Mead, Bales argued, adopted both perspectives. Indeed, one could hardly conceive that social interaction, out of which emerge mind, self and society, is merely what the participant defines it to be.

Blumer, however, maintained that Mead did see the self as central. For Mead, human action is action that is built up through interaction with one's self and that objects come into being only in relation to the self. That the self is formed through interaction, Blumer argued, is irrelevant to this central proposition.

On the second point of difference, the distinction between the 'I' and the 'Me' is regarded as central to Mead. However, Bales (1966) argued that whereas

"Mead distinguishes between the "I" (the process) and the "Me" (the object or structure) - both aspects of the self. Blumer prefers to emphasize the "I"; he says that the self is a process and that those who say it is a structure are mistaken." (Bales, 1966, p. 545).

Stone and Farberman agreed with Bales' interpretation of the "I" and the "Me".

'The "I" is transforming; the "Me" transformed. As Mead put it, "the me is a me which was an I at an earlier time," and not the other way around". As Louis Wirth used to emphasize: "In the beginning was the act!" Clearly, only as a result of action can we transform unformulated experience into
formulated knowledge. We must socialize, formulate or universalize experience to maintain the human dialogue that is human life. In this way, the unique, relative and percipient "I" emerges as the universal, structured and communally organized "me". (Stone and Farberman, 1967, p. 410)

Blumer, in response to both Bales and to Stone and Farberman, reasserted that Mead definitely saw the self as a process and not as a structure, that the 'I' and the 'Me' were not process and structure respectively.

'The "I" and the "Me" Bales has introduced into the discussion were regarded by Mead as aspects of an ongoing process - the "Me" setting the stage for the response of the "I", with the expression of the "I" calling in turn for control and direction by the "Me". To say that one (the "I") is process and the other (the "Me") is structure is nonsense; both were treated by Mead as aspects of action. Mead saw the "self" not as a combination of the "I" and the "Me" but as an interaction between them.' (Blumer, 1966b, p. 547).

On the third point of theoretical difference, Stone and Farberman (1967) argued that Blumer had not penetrated to the core of Mead's thought as he failed to explicate the nature of significant symbols.

'What is lacking in Blumer's presentation is a firm grasp and explicated statement of the significant symbol as a universal - its meaning fundamentally established, transformed and re-established in an on-going conversation.' (Stone and Farberman, 1967, p. 409).

They pointed to the dichotomy which fundamentally divided Blumer and Bales. For Bales, people are beings who selectively apprehend and sustain a unique perspective of the universe. This was rejected by Blumer, in favour of a view which saw people as acting interactively to test apprehensions and attitudes.

'Now we presume that Mead's great contribution is the demonstration that this dilemma is false: the production of a significant symbol everywhere and always is a particular production which mobilizes shared perspectives by its very universality.' (Stone and Farberman, 1967, p. 410).
For some problems, they contended, the focus of attention is the particular act; for others, it is the universal. The stance of the observer is the universal stance, for this requires a grasp of the world in terms of generalizations. Blumer, for the most part, explicitly accepted this position, but often drifted towards a 'subjective nominalism' similar to 'Cooley's sympathetic introspection'.

Mead's critique of Cooley, they argued, led him to assert that permanence and structure is anchored in universal symbolism, that explanation is not effected by concentrating on process rather than structure but that the 'explanation of one cannot be accomplished without the explanation of the other'.

In replying to Stone and Farberman, Blumer denied the dichotomy which he is supposed to have set up to distinguish between himself and Bales. Blumer referred to the original article in which he outlined that group life consists of fitting together participants' actions through a process of adaption of developing acts so as to grasp each others perspectives. In so doing, participants use universal significant symbols. These universals do not however imply common action but are the basis for articulated action. Irrespective of any explication of universal symbols, Blumerian symbolic interactionism assumes the universality of social symbols.
6.5.3 Methodological Incompatibility

McPhail and Rexroat (1979) suggested that Mead is not the forefather of Blumerian symbolic interactionism, nor is Blumer's theory and methodological perspective a contemporary extension and manifestation of the 'Meadian tradition'. They argued that there is a divergence in the methodological perspective of Mead and Blumer which rests upon divergent ontological assumptions. Mead's emphasis on systematic observation and experimental investigation is quite different from Blumer's naturalistic methodology, and Mead's theoretical ideas are not facilitated by Blumer's naturalistic enquiry, nor does this latter complement Mead's methodological perspective.

Blumer's emphasis on sensitizing concepts, McPhail and Rexroat claimed, was contrary to Mead's more definitive approach. Even if Mead regarded scientific laws as provisional, they acted as benchmarks against which exceptions can be noted and acknowledged as contradictions to be explained. All theories and beliefs are sources of hypotheses to confront contradictions.

Similarly, McPhail and Rexroat regarded Blumer's concentration on observation techniques as indicative of an attempt to derive the essential nature of objects to the exclusion of the 'reconstruction of observed fact'. This concern of Blumer's, with phenomenological essences at the expense of empirical evidence, they argued was in contrast to Mead who essentially saw scientific enquiry as problem solving.

They further argued that Mead treated hypotheses as tentative
solutions grounded theoretically and subject to empirical test and that he thought experiments constituted the method of modern science, as essential procedures for generating knowledge. He emphasised exact definition of the problem and careful techniques of data gathering and execution of the experiment along with the obligation of experimenter to specify replication procedures. Whereas Blumer, they suggested, derived hypotheses inductively and atheoretically from empirical instances and rejected hypothesis testing because it seldom 'genuinely epitomizes the model or theory from which it is deduced', neglects the search for negative cases and is limited to the particular empirical circumstances of the test.

McPhail and Rexroat argued that Blumer is wrong to assume an implicit and separate methodology in Mead vis a vis the social sciences. For Mead, the psychological laboratory, as with the physical laboratory, serves to 'render specific, exact and hence formally universal the instruments and behavior of untechnical conduct'. Blumer's demand for investigation which is naturalistic, i.e. directed 'to the given empirical world in its natural ongoing character' as opposed to 'a simulation of such a world' is seen by McPhail and Rexroat as opposed to Mead who 'chastizes critics of experimental research'.

Blumer's reply (1980) was to state that his views of social reality and of naturalistic research had been distorted and that through their efforts to reduce Mead's thought to a narrow scheme of how human social study should be examined, McPhail and Rexroat had misrepresented Mead's view of scientific method and of social
behaviour. This misrepresentation, he suggested, is to 'justify and promote a special mode of scientific enquiry that relies on controlled experiments or on observation closely akin to those made in controlled experiments. [Cottrell, L.S., (1971), O'Toole and Dubin (1968), and Smith, L. (1971)] But they also regard themselves as followers of George Herbert Mead. They are, thus, forced to interpret Mead in such a way as to support their methodological orientation. They seek to do this in two ways. First, they try to interpret Mead's thought on "scientific method" in such a way as to uphold their methodological preference. Second, they endeavor to depict Mead's "social behaviorism" in such a manner as to fit their experimental or near-experimental commitment.' (Blumer, 1980, p. 415).

Blumer argued that his emphasis on sensitizing concepts was not at variance with Mead, who saw no definitive concepts in social science so no possibility of the rigorous testing carried on in physical science. The sensitizing concepts provided a way to grasp the empirical reality and a basis for discovering more analytic concepts.

6.6 An Examination of the Recent Debate and How it Relates to the Work of the Chicagoans.

The debate outlined above clearly raises severe doubts about the way Blumer interpreted Mead. The overall view of Blumer's critics is to suggest that Mead is not the intellectual progenitor of symbolic interactionism. Blumer, of course, vehemently denies the divergence between his and Mead's perspectives. Part of the problem may lie in the type of comparison made, any critique of the 'purity' of a line of thought is bound to show discrepancies as the tenets are developed or utilised in diverse fields. This also has a bearing in the reverse direction in so far as the disciple adopts an attitude of unassailable insight. The line of thought is reconstructed retrospectively in terms of its current
Rather than reconsider this exegetical debate in terms of the adequacy of Blumer's interpretation, the core features will be examined in terms of their relation to the work of the Chicagoans as a whole. Whatever the accuracy of Blumer's interpretation, it was the basis for a development of the general approach adopted and developed at Chicago. Even if some of Blumer's ideas were peripheral to the general thrust of Chicago sociology (Janowitz, 1980), he was still an integral part of the 'Chicago School' and developed his ideas within that general framework of research. It is all very well to retrospectively accuse Blumer of misrepresenting Mead, what is at issue, however, is, given a non-dichotomous view of the development of Chicago sociology, just how important was Mead's theoretical influence?

The debate has generated three areas in which Mead is seen as being at variance with the Chicago approach. Mead is regarded as having a fundamentally different epistemological basis. The Chicagoans are seen as ignoring or subverting Mead's central theories. The research practice of the Chicagoans is seen as substantially different from that which Mead advocated.

6.6.1 The Epistemological Difference between Mead and the Chicagoans

General philosophical labels which pertain to epistemological and/or ontological perspectives, such as realist, nominalist, behaviourist, and pragmatist are, if applied loosely, liable to obscure rather than reveal differences and may serve to provide
an artificial way of distinguishing perspectives. Bales and McPhail and Rexroat claim that Blumer is not a true pragmatist as was Mead, certainly not in any consistent manner. Bales referred to Blumer as an idealist and McPhail and Rexroat accused Blumer of vacillating between realism and idealism. Similarly, Mead is regarded as a social behaviourist while Blumer is not. These ill-defined categories, however, do not aid an analysis of substantive differences.

Lewis and Smith clearly define and utilise what they regard as the central dichotomy of the historical period under study, namely nominalism versus realism, to illustrate the divergence between Blumer and Mead. Heritage (1981) has suggested that to evaluate Mead's contribution in terms of an ancient dichotomy which has been philosophically discredited is to ignore the relationships that cut across this arbitrary and ultimately illusory divide. Similarly, Denzin (1984) and Kuklick (1984) accuse Lewis and Smith of 'presentism'. Coser (1975), more pragmatically, maintained that there is no need to draw too sharp a distinction between the inputs of Mead and nominalist pragmatists, such as Cooley, to symbolic interactionism. Such differences as there were were of style not content.

Farberman, in his analysis of the 'complex paradigm' of the Chicago School of urban ecology, redrew the battle lines, suggesting that the Chicagoans were

'partially at odds with the newly emergent brand of American social psychology propounded by Mead and Cooley.... [who] insisted that the initial building blocks of self-identities were warm, intimate face-to-face relationships and with this contention, laid down an axiomatic challenge to the urban sociologists. (Farberman, 1979, p. 16).
The nominalist-realist dichotomy suggested by Lewis and Smith also founders on the presumption that the Chicagoans could be distinguished in terms of their allegiance to one perspective or the other, and the generalisation that they were inclined towards nominalism. Burgess (1944) quite clearly espoused a realist approach, which on general epistemological grounds would not have divorced him from Mead. Yet the picture is complicated because Burgess did not directly reflect Meadian concerns, his work rarely referred to Mead or to Mead's widely known (at least post 1935) theoretical terms (such as significant other etc.). Similarly, to simply suggest that Park was a nominalist and therefore epistemologically at variance with Mead is inadequate as a basis for denying Mead's impact. While the Chicagoans were aware of the nominalist-realist distinction they did not consider themselves bound to one or other perspective. For the Chicagoans, pragmatism in general provided the categories for an analysis of the social world and they did not tend to distinguish clearly the genesis of such categories, as Park noted in referring to the style of work adopted at Chicago in the 1920s.

'This approach became a logical scheme for a disinterested investigation of the origin and function of social institutions as they everywhere existed, and was in substance an application to society and social life of the pragmatic point of view which Dewey and Mead had already popularized in the department of philosophy. Implicit in this point of view ... is the conception of the relativity of the moral order and the functional character of social institutions generally.' (Park, 1939, p.1)

Park in discussing the nature of social psychology referred to the notion of adopting the role of the other which he saw as a pragmatic notion derived from Cooley and Mead. (Bulletin for Society for Social Research, December, 1927). To regard Park and
the Chicagoans as nominalists and essentially non-Meadian necessitates dismissing Park's own assessment of the development of his and his contemporaries' work.

In his extensive analysis of interactionism, Rock (1979) did not distinguish various strands of pragmatic influence on symbolic interactionism. Rather he argued that symbolic interactionism has its epistemological roots in the German philosophical tradition, from Kant through to Simmel. American Pragmatism assimilated much of this tradition and symbolic interactionism grew out of a fusion of the early interactionists (Park and Thomas) and the psychology of Dewey, Cooley and Mead.

The tendency in the debate on the role of Mead as progenitor of symbolic interaction to retrospectively reconstruct an epistemological divide between the Chicagoans and Mead can, it seems, be quite reasonably disputed as 'presentism' given the eclectic way that the Chicagoans absorbed and developed pragmatic categories. It is inadequate to deny Mead's importance on the basis of these retrospective divisions.

6.6.2 The Theoretical Divergence Between Mead and the Chicagoans

It would seem, then, that Mead was a general influence on the Chicagoans, but merely one of a number of different and relatively undifferentiated influences. As a pragmatist he was part of the general fund of ideas the Chicagoans drew on. His particular theories were, however, selectively appropriated by the Chicagoans. In section 6.3, above, it was suggested that there is little evidence that Mead's theories were adopted or developed
extensively by Chicago sociologists during his lifetime. Subsequent development of his social psychological theories by Blumer and other symbolic interactionists of the 'Chicago School' have been criticised for their misrepresentation. What is important in assessing Mead's theoretical impact is to assess the way the Chicagoans used ideas which were central to Mead's theories.

In terms of the important concept of 'self', for example, the development of the idea by the Chicagoans does not rely entirely on Mead's view. As Rock (1979, p. 102), has pointed out the self was an important concept for pragmatists (and formalists) in general.

'Pragmatism and formalism have both raised the self of the observer to a position of special prominence. Not only was the self a source and synthesis of all viable knowledge, it constituted the elemental unit of sociological analysis. It was thus simultaneously an intellectual subject and an intellectual object. The self is taken to be a social construct, emerging from language, which lends order to all interaction. It is man made conscious of himself as a social process, and its basis is a reflexive turning-back of mind on itself. Reflexivity is made possible by the social forms and it advances the evolution of those forms. It is in the self that a fundamental grammar or logic of the forms is allowed to unfold. All social phenomena stem from that logic so that a socially formed mind and the processes of society display a unity.' (Rock, 1979, p. 102)

Rock (1979, p 166), however, suggested that the central concept of 'self' as developed by Mead expressly excluded much of the complex, unobservable phenomena and processes that later symbolic interactionists included.

'In its original formulation, the interactionist model of the self offered a limited but useful description of the relations between mind, body and society. It was useful because it referred to observable and communal processes which shaped mind. It permitted a synthesis of the different phases of social and individual processes into one master scheme. The model was limited because it did not pretend to
embrace private, subjective experience. It was not comprehensive or phenomenological. Rather it adhered to the behaviourist principles which Mead had advanced... In its phenomenologically revised form, the self has also lost much of the practical utility which it once enjoyed. It has become a somewhat mysterious process whose problematic qualities are little appreciated by the revisionist interactionists. ' (Rock, 1979, p. 147)

This does not mean to say, however, that later symbolic interactionists have disassociated themselves from the pragmatic, and notably Meadian, heritage in any definitive way. The core of the symbolic interactionist perspective, Rock argued, is as it was developed by Cooley, refined by Mead, expounded in sociological terms by Faris, and developed by Blumer. Essentially, symbolic interactionism

'conceives the self to be the lens through which the world is refracted. It is the medium which realises the logic of social forms. Fundamentally, however, the self emerges from the forms. It is made possible only by the activities and responses of others acting in an organised manner. A self without others is inconceivable. Its doings and shapes must be understood as a special mirroring and incorporation of the social process in which it is embedded. Because language and society are taken to be historically and analytically prior to mind interactionism does not proceed by deducing social phenomena from consciousness. Neither does it assume that individuals are 'given' and therefore unproblematic. It is the self which arises in sociation, not sociation from the self. As Luckmann argued, Mead's description is characterised by 'a complete reversal of the traditional understanding of the relation between society and the individual'. Anchoring analysis in the geometry and grammar of the social forms, interactionism is also able to furnish a conception of social structure which is relatively free from scientific reification. Structure is animated by the everyday behaviour of people, not by an immanent and sui generis logic of its own.' (Rock, 1979, p. 146)

From this point of view, then, other areas of theoretical difference between Mead and Blumer may be recast. There is, for example, no problem of theoretical disjunction in relation to the nature of objects, of the relation between structure and process or of the construction of universals. The dispute about the
structural nature of the 'me' and its relation to the 'I', Rock obfuscates through his analysis of Mead's constructs which he sees as complex and deliberately problematic. However, the mystification accorded the concept by later symbolic interactionists has not negated the essential Meadian interactive process between the 'I' the 'Me'. (Rock, 1979, pp. 119-126)

What this suggests is, again, that the exegetical analysis misses the point. Rather than deny Mead's importance on such grounds, which entirely ignores the Chicagoans own view of the extent to which they (individually) appropriated Mead's theories, a more salient critique of Mead's theoretical importance can be offered in terms of the centrality of his theories generally. Ideas subsequently attributed to Mead such as the 'self' were part of a general fund of pragmatic ideas upon which the Chicagoans drew. Mead offered a particular development of the theory of the self which was part of the adaptive process undertaken by the Chicago sociologists in recasting philosophical constructs for purposes of sociological research. This adaptation permits a relatively easy dismissal of the concurrence of Mead and, say, Blumer on purely exegetical grounds. What such a critique ignores, however, is the spirit of the adoption of Meadian constructs. They were, then, part of general development of pragmatic ideas and the real confusion has come about as the result of Blumer, and others, asserting their primacy, and critics pointing out the discrepancies in the interpretation of Mead. Essentially, there is little to suggest that Mead's theories were generic to the development of Chicago sociology. Rather, Mead, like Cooley, Dewey and James provided ideas the Chicagoans selectively drew
upon and merged with other non-pragmatic theoretical perspectives.

6.6.3 The Methodic Difference Between Meadian Prescriptions and Chicago Practice.

The third area of concern is that the sociological and psychological work produced by the Chicagoans, particularly that of the later generation symbolic interactionists, does not match the sort of work that Mead or a Meadian would prefer. Considerable sociological work has been done by symbolic interactionists assuming that their epistemology is rooted in Mead. Whether or not it is what Mead would have preferred seems to be a matter of conjecture, and the discussion is of importance only in terms of providing a legitimating founding father for a particular methodic orientation. Rock suggested that the fusion of formalism and pragmatism that underpins symbolic interactionism and which sees the self as central, is essentially served by participant observation because it inserts the self of the sociologist into the research setting.

The Chicagoans, as illustrated in chapter four, were methodologically eclectic. Up to the 1950s there was no commitment to any particular method. The development of a tendency towards participant observation by some later 'Chicago' symbolic interactionists may be regarded as the result of Mead's influence irrespective of the apparent marginality of Mead (Platt 1982a). On the other hand, the development of participant observation may be seen as either a pragmatic development given that much of the research was in deviant areas, or as the result of a concern to study the social world of the subject group from the inside,
irrespective of Mead's thesis about the nature of the self.

Whether Mead demanded 'experimental' type research or not is clearly a contentious point. In practice, the Chicagoans did not abandon the essential tenets of nomothetic research and it was only the later development of symbolic interactionism into a more radical analysis of the scientific method and the nature of social interaction ostensibly based on Mead, which has created confusion about the relationship between Mead's concerns and the work of Chicago sociologists.

Much of the development of this phenomenologically informed perspective was effected away from Chicago, notably by Blumer himself, Goffman and the emergent ethnomethodologists at Berkeley. Indeed, the embryonic anti-positivist relativism evident in Blumer's perspective which became mixed with Schutzian phenomenology in the emergence of ethnomethodology was quite at variance with the Chicago orientation.

6.7 Conclusion: Why was Mead Seen As Important?

One must ask why Mead is seen as so important, and thus accredited the role of principal theoretician of, at least the later, 'Chicago School', when indeed, most of the Chicagoans exhibited little of his overall theoretical position?

Those who invoked Mead either used his social psychology as a convenient framework without incorporating the wider presuppositions of his position or simply slotted some of his ideas into a Park-Thomas framework (Fisher & Strauss, 1979).
essential elements of that framework owe little directly to Mead, rather they are the product of the German tradition fused with a general Pragmatic critique of early American sociology. Thus Dewey, Cooley and James had as much impact on the development of the Chicago sociological approach as did Mead, as, for most of the Chicago interactionists, no single strand of pragmatism caught their attention or led to a factional division within the Department. Indeed, the analysis of doctoral dissertations shows that Mead's theories are referred to and used only rarely (certainly up to 1940) whereas Cooley is often cited as the provider of social psychological theories and categories. This is not at all surprising if, as Lincourt and Hare (1973) suggest, Mead's theory of the self was, in many respects, anticipated by other pragmatists such as Wright, Peirce and Royce. Mead's emergence as a major figure (and Cooley's relative 'decline') only occurs after 1935 (following the departure of Park) in the wake of the publication of 'Mind, Self and Society' (Mead 1934).

Lewis and Smith suggest that the reason for the widespread view that Mead provided the philosophical underpinnings of the 'Chicago School' are twofold. First, the uncritical acceptance of the symbolic interactionist reconstruction of their intellectual history which has come to dominate histories of the 'Chicago School'. Second, the tendency for intellectual history to concentrate on the 'great man' approach and therefore need to identify 'founding fathers' [4]. A recent introductory text to phenomenological sociology illustrates Lewis's and Smith's contention and confirms the reconstructed importance of Mead's
Symbolic interactionism stems from the works of John Dewey, Charles Horton Cooley, Robert Park, W.I. Thomas and George Herbert Mead, among others. Although interactionists continue to differ among themselves as to the meaning and importance of various concepts related to symbolic interactionism, Mead's formulation in 'Mind, Self and Society' represents the most comprehensive and least controversial presentation of the perspective to date. (Bogdan and Taylor, 1975, p. 14).

The legitimacy of the Mead-Blumer line of symbolic interactionism is attested to by numerous writers. Yet, the establishment of this tradition appears to owe much to the role of Blumer himself in the development of Chicago sociology and the determined advocacy of his own brand of interactionism. In order to legitimate his perspective he argued forcefully that he provided the most faithful development of Meadian constructs and, by degrees, has been taken by historians of symbolic interactionism to imply that Mead encapsulated the core of Chicago Sociology. Thus, for example, Meltzer et al, (1975, p. 55) state

'Blumer has elaborated the best known variety of interactionism - an approach we call the Chicago School. This approach continues the classical, Meadian tradition.'

Through the assertion of a 'pure' heritage derived from Mead, Blumer and subsequent historians (especially those sympathetic to Blumerian symbolic interactionism) have generated a taken-for-granted view of the centrality of Mead. Once established, this myth generates its own momentum and, in the case of the development of symbolic interactionism, a tradition of work evolves which takes this mythical element as 'true'.

In short, the attempt to legitimate symbolic interactionism has
given Mead a role in the 'Chicago School' he did not have. This role is not merely the product of Blumer's own accentuation of Mead it is also a result of the other elements of the 'Chicago myths' spelled out above. Blumer cannot, however, be entirely vindicated from responsibility. He has suggested that his is the purest form of interactionism and implied that its progenitor was Mead and that Mead gave a new dimension to pragmatism. This position is reflected in Rucker (1969) who argued that Mead took up and developed 'Chicago Pragmatism' and that it is through Mead that sociology incorporated pragmatic epistemological presuppositions. Coser (1975) seems not to share this view that Mead was so important.

Despite close links between Mead and Dewey, both academically and personally, Coser (1975, p. 355) suggests that Blumer has attempted to set Mead apart. He wrote that

'Blumer relates that Mead would sometimes point with a bit of sarcasm to the profuseness of Dewey's output and to his attendant tendency to write sloppily and with lack of precision.'

Coser comments that during their association at Chicago, 'Mead was content to play second fiddle to Dewey's resounding first violin'. Indeed, there appears to be little support for any view that Mead was cynical of Dewey's philosophy. For example, in an address to the Society for Social research at Chicago, Mead referred to Dewey in the following glowing terms.

'His statement of ends in terms of their means reached American life as no earlier philosophy had. In the profoundest sense John Dewey is the philosopher of America'.
(Minutes of the Society for Social Research, 7.11.1929)
There are other reasons for the prominent position attributed to Mead in the practice of sociology at Chicago. Besides the fact that he taught a course offered to sociology graduates at Chicago, Mead is seen as the only major theoretician at the time in the social sphere and it became taken for granted that symbolic interactionism was rooted in Meadian social psychology. This interpretation gained credibility as structural functionalism became important because, as Coser (1975, p. 340) suggested,

'It is hardly a subject of dispute that modern role theory from Linton and Parsons to Newcomb and Merton has been enriched by freely borrowing from Mead.'

This is a point echoed by Strauss (Mead, 1964, p. xii) and Fisher and Strauss (1978, p. 488) who suggest that functionalists have frequently taken 'bits and pieces from the interactionists' armamentarium' especially constructs like 'the significant other' or 'role taking' which eventually transformed Mead's dynamic development of the self into a static notion fitting the structural functionalist ideas of 'status', 'role' and 'reference groups'.

Finally, as the later generations were more affected by a narrowing of methodological focus and developed the sociology of deviance, so too there was a tendency towards adoption of Meadian constructs particularly in terms of adopting the role of the other. As suggested above, there was a shift away from the overt moralising of criminological studies towards an attitude of enquiry that demanded the deviant perspective be engaged sympathetically, (Becker 1967)
NOTES TO CHAPTER SIX

1. 'Most treatments of interactionism as a school of sociological thought or a general intellectual position designate George Herbert Mead as one of its founding fathers. The ambiguous character of such terms as 'Chicago School', 'interactionism' or 'symbolic interactionism', makes it difficult - and perhaps fruitless - to argue with such claims. Mead's importance as an intellectual figure and his association with the theory of 'interaction' is well established.' (Fisher & Strauss (1979, p. 483).

2. This follows an earlier comment by Strauss who noted that there were several streams of faculty influence in the Department, some of whom gave more prominence to Mead. By way of illustration Strauss offered an autobiographical note.

'Before I went to Chicago as a graduate student in 1939, I had been directed to the writings of Dewey, Thomas and Park by Floyd House, who had been a student of Park in the early twenties. House never mentioned Mead, that I can recollect. But within a week of my arrival at Chicago, I was studying Mead's 'Mind, Self and Society', directed to it by Herbert Blumer.' (Mead 1964 p xi)

3. Arguably Cooley's influence did not decline, but shifted. Goddijn (1972a) suggests that Cooley had a considerable effect on the quantitative approach developed by Lazarsfeld and Stouffer in the post World War Two period particularly in relation to Cooley's ideas on small groups and reference group behaviour.

4. Lewis and Smith (1981) only develop their analysis up to 1935 and make no attempt to account for Mead's posthumous influence and why he has been accepted as founding father of symbolic interactionism given his marginality. They make no attempt to explain how Blumer adapted Mead and thereby influenced an important mid-century tradition within American sociology (Harvey, 1983). This tradition owes a lot to Blumer's work, ideas and institutional role. He developed the core concepts of the symbolic interactionist position from Mead, whether accurately or otherwise. Mead was seen as the progenitor of a tradition.
CHAPTER SEVEN

CHICAGO DOMINANCE
7.1 The Myth

A widespread notion about the Department of Sociology at Chicago is that it managed to dominate sociology in the United States from 1890 to 1930.

'Established in 1892, the University of Chicago department of sociology dominated general sociology and sociological theory until the 1930s... Other departments, such as that of Columbia, initially chaired by Franklin Giddens [sic], were not able seriously to challenge Chicago's preeminence during this period.' (Coser, 1976, p. 146)

After the mid-thirties, in the wake of a concerted attack on its leadership, it is presumed to have declined rapidly (Mullins, 1973; Tiryakian, 1979a; Martindale, 1976, Coser, 1978, Goddijn, 1972b).

This myth of the 'Chicago School' throws up three aspects for investigation. First, to what extent was the 'Chicago School' dominant and how was that dominance manifest? Second, was there a point in the 1930s when the supposed Chicago dominance became undermined through a concerted attack. Third, was Chicago, therefore, 'in decline' after the 1930s, and if so to what extent can this be said to be a rejection of 'Chicago School' sociology rather than the result of the expansion of sociology in America?

7.2 The Nature of the Dominant Role of Chicago in the Development of American sociology.

The University of Chicago certainly figured prominently in the discipline, especially from 1890 to 1925. The fact that it was the first sociology department in an American university and was only seriously challenged by Gidding's department at Columbia, gave it a 'head start' on departments which might rival it for
recognition. Chicago was also prominent on the Social Science Research Council and in the national sociological organisation. It took on large numbers of graduate students during the first four decades, and granted more Ph.Ds than most of the other leading departments together. Thus the Department at Chicago became well publicised through its former students.

However, there is a tendency to overstate the degree to which Chicago dominated American sociology. Contrary to the views of some commentators (Coser 1976; Gouldner, 1970; Hinkle and Hinkle, 1954; Martindale, 1960), Chicago was not alone in the field.

7.2.1 Research in Social Science and The Social Science Research Council

In the period up to 1930, Chicago University was one of half a dozen universities well endowed with research monies and having a research environment which allowed them to play a major part in the development of sociological research.

Even within this small group Chicago University tended to play a large part, although they were by no means domineering. Various factors led to Chicago University's early pre-eminence.

The Local Community Research Committee at Chicago (which later became the Social Science Research Committee) was probably the first university based organisation to adopt the concerns of the Social Science Research Council at a local level (Bulmer, 1980), and this Chicago approach came to be emulated by other institutions.
Research in the social sciences at Chicago was becoming well established and encouraged by the end of the twenties. Ogg's (1928) report to the American Council of Learned Societies singled out the University of Chicago as one of the most research oriented universities in the United States in respect of the social sciences and humanities [1]. The flexible teaching loads, easily obtainable sabbaticals, the recognition of research professorships with small teaching loads and the use of research as the prime basis for judging promotion and salary increases contributed towards the research environment at Chicago. [2]

By the standards of the time, the University of Chicago had, in 1928, exceptionally large, although limited term, research funding. The social sciences had an enormous budget of $143,000, of which about $100,000 was administered by the Local Community Research Committee. A considerable amount of this money came from the Laura Spellman Rockefeller Memorial Foundation. An additional fund of $100,000 spread over five years, was also available for publication of material in all fields. However, Chicago had not always been so well endowed and this represented a considerable advance over the previous five years as, in 1922, Chicago University did not indicate that they were in receipt of regular funds through which research in any departments could be financed. (Bulletin of American Association of University Professors, 1922, (Vol 8), p. 32). As an indication of its financial standing, and rapid improvement, between 1924 and 1927, the University of Chicago raised around twenty million dollars in endowments.
Nonetheless, despite the relatively healthy environment, and the ability of some sociologists to tap the research funds, sociologists, as noted in chapter four, were not the sole, or even the major, beneficiaries of the money administered by the Local Community Research Committee.

Nor was Chicago alone in its support of research. Rather, in the late 1920s, it was but one of a small group of six universities that positively encouraged research in the social sciences. The other members of this group were Columbia, Harvard, North Carolina, Yale and California.

President Butler of Columbia University noted, 'at Columbia the spirit of research is everywhere active and persistent' (Butler, 1925, p. 38) and considerable research was taking place in the social sciences while the Columbia University Press offered a ready outlet for research work. The appointment of five new professors in 1926 principally for research work (including one in statistics and one in economics) further stimulated research. In 1925 the Columbia University Council had created a Council for Research in the Social Sciences with the duty of furthering co-operative research. In its first year it administered in excess of one hundred thousand dollars derived partly from the University and partly from the Laura Spellman Rockefeller Memorial Foundation. In addition, the University received numerous large gifts, and there was an emergency fund for research purposes ($40,000) appropriated annually by the president.

Research at Harvard, too, was deemed to be of the highest importance from 1910 onwards, and facilities were greatly
improved, with financial support being increased from an 'insignificant figure to several hundred thousand dollars' per year. The development of the Widener Memorial Library into one of the finest in the world, and the reduction of teaching burdens from 1927, helped research endeavours. In addition the Milton Fund amounting to one million dollars provided an annual sum of fifty thousand dollars for research of which (in 1927) the social sciences and humanities received about a third, although very little went directly to the promotion of sociological research. However, there were additional funds for research in the social sciences and humanities, most important was the fifty thousand dollar grant for five years from the Laura Spellman Rockefeller Memorial Foundation for the work of the Bureau of International Research.

The University of North Carolina similarly gave a great deal of support, from relatively meagre funds, to research in the social sciences. Apart from an annual grant of twenty five thousand dollars from the Graduate School and the Smith Fund available to all departments, the University's Institute for Research in Social Science administered a research fund of sixty five thousand dollars annually, granted by the Laura Spellman Rockefeller Memorial Foundation for five years.

The University of North Carolina was the motor force behind the series of southern university social science conferences that began in 1925. Highly motivated towards research it created a research atmosphere in the social sciences and attempted to stimulate research in various ways including the publication of
an extensive annual review of research in progress, the launching of local surveys in rural social economics, making liberal provision for publication through the University of North Carolina Press, and the editorship of 'Social Forces' besides the establishment of the Institute.

Yale University was also gradually developing a research ethos in the social sciences, this was helped by the establishment of both the Sterling Memorial Library in the late 1920s, and the Institute of Psychology in 1924 which was grant aided by the Laura Spellman Rockefeller Memorial Fund. About forty thousand dollars per annum were available for research in the humanities and social sciences, mainly income from the Sterling bequest.

The University of California, later to be a major contributor to sociological research was, in 1927, beginning to establish itself as a research centre. About half of The Searles Fund ($10,000 per annum) was available for research in the social sciences and humanities. In addition the Laura Spellman Rockefeller Memorial Foundation provided a grant in 1927 to support a research institute of child welfare for a period of six years.

On the fringes of this group were several others including Cornell University, the University of Illinois, Stanford University, and the University of Cincinnati. All of these had small amounts of money established for social science research or were otherwise promoting such research [3].

In terms of research funding and encouragement, then, the University of Chicago was not alone but part of small group of
institutions. However, as Bulmer (1984, 1985) suggested, apart from Chicago, there was little development of sociology in these centres. During Giddings's later years sociology at Columbia was in a 'parlous state'; Yale had a very little post-graduate work in sociology; and there was no sociology department at Harvard until 1930. In most places sociologist operated in 'one-man departments' (Lapiere, 1964).

From the late 1920s onwards the development of sociology in the United States came to be dominated by the fifteen universities who constituted the core of the Social Science Research Council. The fifteen universities were California, Chicago, Columbia, Harvard, McGill, Michigan, Minnesota, North Carolina, Northwestern, Pennsylvania, Stanford, Texas, Virginia, Wisconsin and Yale.

The Social Science Research Council was founded in 1923 because there was an imperative need for an agency with a concern for the common research interests of all social scientists. The disciplinary societies were weak and narrow minded, colleges and universities were more concerned with teaching than research in social science [4]. The lack of any other body with a similar interest in the over-all research problems of the social sciences demanded its creation.

'The one goal of the Council since its beginning, of course, has been the advancement of research in the social sciences by any effective available means.' (Burgess, 1944b)

This co-operative auspices made it easier for the universities involved to attract research funds from foundations such as the Rockefeller Foundation and the Laura Spellman Rockefeller Memorial Foundation. Further, government initiatives in the social
sciences were also directed to these institutions. This was facilitated by the establishment of the Committee on University Social Science Research Organisations of the Social Science Research Council.

This group of fifteen universities received their principal social science research funds from the Rockefeller Foundation after 1930. The aim of this committee was to exchange information on the problems of social science research and the administration of research in the universities. It held an annual conference to which representatives of research foundations, government and non-academic research organisations were invited. The Rockefeller Foundation had hoped that such a committee of the Social Science Research Council would facilitate co-operation among the various universities it financed, and establish lines of communication for sharing experiences and for co-operative ventures. Such a body could also record and evaluate research. Besides the annual conference the committee visited research centres to collect first hand information on work in progress.

Through this committee, the Social Science Research Council formulated its approach. In 1929 it adopted seven objectives; improving research organizations, developing personnel, improving and expanding materials, improving research methods [5], facilitating dissemination of materials, methods and results, facilitating research projects, and enhancing public appreciation of social sciences. Following the 1936 recommendations of the Committee on Review of Council Policy these became the four categories for the appointment of committees on research planning.
and appraisal, on research agencies and institutions, on research personnel and on research materials.

The situation was reviewed again in 1944 and the following criteria for supporting research were made: advance of scientific methods, inventing or improving research instruments, repetitive study, interdisciplinary experimental studies, appraisal of research methods, studies integrating methods from different disciplines, and pilot studies in new fields. This firmly put the onus on methodological analysis and large scale research enterprises. Columbia University, through its considerable involvement in research on the Second World War and the consequent establishment of the Bureau of Applied Social Research, stole a march on many other institutions, especially Chicago, who were not undertaking specific large scale research on the war (Wirth, 1942)[6].

Following recommendations made in 1945 the organisation of the Social Science Research Council was changed, with the Committee on University Social Science Research Organisations giving way to the Committee on Organisation for Research in the Social Sciences, in 1946. This new committee was formed because the annual conferences were rather limited, the original fifteen universities constituted a dated grouping because other universities with no formal social science organisations had started doing social research, and new research organisations outside the universities had come into being. The new committee, which was therefore not restricted to universities,

'undertook the formulation of a program of study of the problem of research organisation in relation to the improvement of research in the social sciences. It discontinued the annual conferences and [through preliminary
widespread enquiries discovered that] the organisation of social science research was about a decade out of date.'(Wirth, 1949)

In a review of this organisation, Wirth (1949) noted that the conferences were concerned with getting further financing rather than with the substantive problems of the social sciences or the technical problems of developing research organisations. However, the Committee

'was the one real bridge that allowed for co-operation that was established in the first two decades of the Social Science Research Council's operations. Through the informal discussions among the members present at these conferences much was learned about the successful and unsuccessful experience.'

The reorganised committee still placed social science research in the universities at the forefront, argued that such research had become team research with technical backup and that this made research organisation important although it was no substitute for ideas. And while the Committee recommended that each organisation establish a committee, in effect equivalent to the Social Science Research Committee at Chicago [7], the maturation of social science research now made it impossible for any University to dominate.

7.2.2 The Specification of Sociology

In 1911, the American Sociological Society appointed a committee of ten university professors to suggest appropriate subject matter for a fundamental course in sociology. The members of this committee were Jerome Dowd of Oklahoma who was chairman, Charles Cooley (Michigan), James Dealey (Brown), Charles Ellwood (Missouri), H. P. Fairchild (Yale), Franklin Giddings,
(Columbia), Edward Hayes (Illinois), Edward Ross (Wisconsin), Albion Small (Chicago) and Ulysses Weatherly (Indiana).

The committee set out to determine what is taught in beginning courses in sociology and to recommend appropriate subject matter. Three hundred and ninety six institutions were approached. However, the main thrust of the report was the approaches adopted by the ten institutions represented on the committee. These were influential in setting the pattern of introductory courses in sociology. There was general agreement amongst the committee as to content. Broadly it consisted of three areas, first 'The Socius', (the nature of 'man', hereditary and environmental factors in the constitution of the social self); second, 'Social Organization' (group formation, class and caste, institutional development and democracy); third, 'Social Process' (evolution, competition, the development of the family and the state).

Although different emphases were placed on these three broad areas, none of the committee deviated greatly from the essential nature of the content. However, Giddings and Small argued strongly for a practical rather than simply theoretical orientation to sociology.

'Perhaps I do not attach quite so much importance to the selection and arrangement of topics for a fundamental course in sociology as some of my fellow-teachers do. I have come to think that the essential thing is to develop painstaking habits of sociological study. Many topics are available, but whatever ones be chosen the pupil must be required to attempt certain simple exercises and complete them in a workmanlike manner.' (Giddings, 1911, pp. 628-629)

Small concurred with this, and wrote that he subscribed to the

'first three sentences in Professor Giddings' statement as representing my fundamental position.' (Small, 1911, p. 635)
Chicago university was, then, not surprisingly, very involved in the early specification of the compass of sociology as a theoretical discipline, but along with Columbia pioneered in the pursuit of empirical enquiry at the 'grass roots' level.

7.2.3 Empirical Research and Publication

Chicago University, through its sociology department, was at the forefront of the development of empirical research in sociological research. Three factors were important in establishing empirical research at Chicago. Chicago sociology was, as noted above, well supported in its research endeavours by the University and stole a lead along with a handful of others. As time went on, though, this was inevitably pegged back by other places. The growth of the city of Chicago, which the Chicagoans used as a 'natural laboratory', gave the 'Chicago School' an impetus which led to its considerable impact (Goddijn, 1972b). In addition, the Chicagoans produced some notable empirical studies in the first quarter century, including the monumental 'Polish Peasant' (Thomas and Znaniecki, 1918), and the widely read compendium of articles on research in the urban environment entitled 'The City', (Park & Burgess, 1925). As Wirth (1947) suggested, the era of empirical research was just starting in 1915 and was centered on Chicago. Park (1915) and Thomas and Znaniecki (1918) marked

'the beginning of a new epoch in sociology and which justify the designation of the year 1915 as symbolic of the debut of sociology as an empirical science.' (Wirth, 1947, p. 274)

The development of the University of Chicago Press (supported by generous grants to aid publication) allowed much of the work done in the department to be published and thus to become widely
available (Tiryakian, 1979a). Again, the explosion in sociological publications that took place later in the century dissipated the effect of the privileged position that the Chicagoans, along with a couple of other universities, had once had.

The empirical studies were also augmented by three text books produced by Chicagoans which became seminal works of reference. Small & Vincent (1894) set out an approach to the study of sociology, Thomas (1909) provided an early exposition of the theoretical and empirical base of sociology and Park and Burgess (1921) became, for many years, the ultimate reference text, combining, as it did, a large number of readings from a variety of perspectives with a critical commentary, selected bibliographies, topics for written work and discussion questions.

7.2.4 The American Sociological Society and the American Journal of Sociology

The founding of the American Sociological Society was another initiative from Chicago, and the University's sociology department was prominently represented among its officers (until 1936). Chicago, along with Michigan, also tended to dominate the midwest regional association.

The creation of the American Journal of Sociology at Chicago, published through the University of Chicago Press, provided another means by which Chicago influenced the discipline both before and after 1936, at which date it ceased to be the official journal of the Sociological Society. However, the journal, despite having a Chicago-based editorial board, published a wide
variety of contributions, and was not simply the mouthpiece of the Chicagoans.

7.2.5 Spreading the Word

Chicago concentrated on graduate work and produced many more qualified graduate students in the period up to 1935 than any other university [8]. These graduates became widely appointed to posts in sociology departments, and an informal network was created with the influential Society for Social Research at Chicago as its focus.

Chicago's initial 'dominance' had come from being the first and most potent sociology department and providing many of the faculty in new departments from the ranks of its own graduate students. Barnhardt (1972) recalled that Small encouraged him to go into teaching and had recommended him to three different institutions who had contacted Small for advice on appointments. Some of the graduate appointments from Chicago actively promoted the university.

'I went to Vanderbilt and took courses in sociology under Walter Reckless and Ernest Kreuger. They were ardent Chicago, in fact they were called by the faculty at Vanderbilt, "missionaries from Chicago".' (Cottrell, 1972)

Some commentators have suggested that this was not unusual and that Chicago produced disciples intent on spreading the word.

'Through the 1920s the department of Chicago was the one real center of sociology in the U.S.. It is my impression, one that I cannot document, that most of the men who came out of the Chicago department during this time were fairly passive disciples of the 'Chicago School' - mostly trained in the ideas of Park, if not by him, and that they went out to spread the good word with a strong sense of mission. (Lapiere, 1964)
This, however, ignores the lack of 'allegiance' many of the Chicago graduates felt towards Chicago. Cavan (1983) for example, says that she felt no obligation to Chicago, that she was neither aware of, nor engaged in, any 'missionary zeal' to promote Chicago sociology wherever she went. Nor did she feel obliged to return to Chicago to renew her 'Chicago spirit'. Indeed, she stated that after the 1920s her contacts with her old department were sporadic.

Anderson (1983) reflected that he knew of the 'Chicago School' in advance of going to it because Swenson, his tutor at Bingham Young University, knew of Chicago's interest in sociology and recommended it to Anderson, because the Chicagoans (Anderson recalls, fifty years later) 'work with new ideas'. Further, Anderson was taught by a Chicago graduate at Utah who talked a lot about Park, some about Small and mentioned Faris and Burgess, and also referred to the 'Introduction to the Science of Sociology' (Park and Burgess, 1921) which he apparently treated like a bible and which Anderson says the tutor was lucky enough to own, unlike himself who could not afford to buy it.

7.2.6 Summary of the Nature of the Chicago Dominance.

Nonetheless, despite this widespread involvement in the discipline, the University of Chicago did not control American sociology. It was just an influential member of the core group of universities. For some time it had perhaps more organisational pre-eminence than many others but this was somewhat curtailed by the so-called 'coup' in the American Sociological Society of 1935.
and the subsequent establishment of the American Sociological Review.

'The hegemony of the University of Chicago over the field stimulated resentment among sociology departments in other centers. This resentment tended to center on the fact that the official journal of the American Sociological Society, the American Journal of Sociology, had always been owned by the University of Chicago and edited by a member of its faculty.' (Matthews, 1977, pp. 181-182)

Matthews described the coup as a 'palace revolt' which signalled that other centres 'were emerging strongly enough to challenge Chicago's long held role of leadership', notably Columbia and Harvard.

7.3. The 'Coup' as Heralding the Decline of Chicago

In 1935 the American Sociological Association voted to establish its own independent journal, the American Sociological Review. Prior to 1936, the American Journal of Sociology, printed by The University of Chicago Press and with an editorial board consisting of Chicago sociologists, had been the official organ of the society. This decision to establish the American Sociological Review is often seen as a move by sociologists in America to free themselves from the dominance of Chicago. This discarding of Chicago is viewed in a variety of ways.

Martindale (1960), reflected the most popular view of the significance of the coup when he described it in terms of a methodological confrontation in which 'positivist' quantifiers confronted the conservative humanism of Chicago. Thus the coup is taken as symbolic of the final victory of 'hard' quantitative sociology over the 'soft' sociology epitomised by the
'Chicago School'. Faris (1967) saw the coup in terms of an activist group challenging the Chicago value neutral approach. Kuklick (1973) saw it as merely the tip of the iceberg that was forming around structural functionalism, from which Chicago supposedly absented itself or was excluded. Developing this, others see the coup as a representation of the recognition of the anachronistic nature of Chicago sociology. Martindale (1976, p. 141), for example, taking his cue from Faris (1967) wrote,

"Forces were in motion that would transform the character of American sociology. As its unquestioned center of dominance, the Chicago department had to either assume leadership in the transformation or be thrust aside. It did not possess the charismatic leader who could assume the role.... A new epoch was dawning [following the coup] that would see the point of gravity in sociology shift decisively toward quantitative methodology and toward theoretical collectivism. The capitals of sociological culture in America were relocating from the Midwest to the coasts."

Some commentators go further and argue that the period of Chicago dominance hindered the development of a scientific sociology and the coup symbolised the maturing of the discipline in the United States with the emergence of a more scientific approach, embodied in structural functionalism.

"The first dominant school of thought, the Chicago School, crystallized around World War I and continued until the early thirties. The second dominating school, the functionalists, succeeded the Chicago School in the forties and fifties after a period of interregnum." (Wiley, 1979)

Reflecting on the period up to 1935 Coser noted

"The end of the Chicago dominance may conveniently be dated as 1935, when the American Sociological Society, previously largely, but not wholly dominated by the Chicago department or Chicago-trained scholars, decided in a minor coup d'etat to establish its own journal, The American Sociological Review, thus severing the long time formal and informal links of the discipline to the University of Chicago department. Two years later the appearance of Talcott Parsons' The Structure of Social Action heralded the emergence of a theoretical orientation considerably at variance with that developed at the University of Chicago."
This new orientation was largely to dominate American sociology for the next quarter of a century. Having gradually become institutionalized and largely professionalized in the years [up to 1935], having passed through a period of incubation during the years of Chicago dominance, sociology could embark on its mature career.' (Coser, 1978, p.318).

Thus, the suggestion is made that Chicago dominated and in so doing somehow hindered the development of American sociology. This is further expanded by Tiryakian (1979a) who, on a more general level, saw Chicago's empirical concerns, combined with their predominance of the discipline up to 1930, as having inhibited the development of a unifying 'paradigm' for sociology. Such a 'paradigm', it is argued, only became established in the 1940s following the pioneering work of Parsons, and became concretised in the emerging structural functionalist perspective. Tiryakian gave the impression that Parsons was alone in wanting to develop a holistic theory in the 1930s.

'While formulating his basic paradigm in the 1930s, he was a "voice in the wilderness" at a time when American sociology was predominantly empirical, atheoretical and positivistic; Parsons' central notion of "action", synthesising elements from four major European figures, was, in a sense, not that much of a radical departure from the native American tradition of pragmatism and voluntarism found in Mead, Park, Thomas and Cooley. However, whereas these men had had illuminating insights, Parsons was to insist on a general theory of action.' (Tiryakian, 1979a, p. 228)

Such a perspective entirely misconstrues prior sociological endeavours, ignores the inter-disciplinary approaches adopted at many universities and their encouragement through the Social Science Research Council. It gives the impression that sociology only came theoretically of age in the post-Parsonian era, and that structural functionalism alone and for the first time conjoined theory and research.

'The collaboration of Parsons the theorist with Sam
Stouffer the empirical researcher, and the similar pairing of Merton with Lazarsfeld, seemed ample proof that the new paradigm could integrate sociological analysis and research. (Tiryakian, 1979a, p. 229).

A particularly insidious connotation of the above is that only with the overthrow of the Chicago dominance of sociology as epitomised in the 'coup' within the American Sociological Society, could a theoretical sociology emerge in America.

The following sections will examine the validity of these assertions, and the extent to which the coup can be seen to herald a decline in the fortunes of the 'Chicago School'.

7.3.1 The Nature of the Coup

Lengermann (1979), in a re-examination of the coup, argued that the coup was not about method but that opposition to Chicago was 'bound together, not by a theoretical viewpoint, but by an organizational ideology of anti-elitism'. (Lengermann, 1979, p. 194).

Indeed, the main contention of the anti-Chicago group at the time was the threat to career chances from the 'patronage' of Chicago in a time of increasing pressure on jobs, due to the depression [9].

That the coup was a political exercise is confirmed by Cavan who noted that the other sociology departments

'began to chafe under the dominance of the Chicago group... and definite steps were taken to increase leadership in the American Sociological Society from non-Chicago sociologists.' (Cavan, 1983, p. 418).

At the meeting which established the American Sociological Review, organisational changes in the American Sociological Society were also voted in and all but one Chicago supporter
(Dorothy Thomas) failed to be elected to executive or committee posts. However, Lengermann argued that the meeting of itself did not constitute the 'rebellion' against Chicago, rather it was the cumulation of a five year sustained opposition to the Chicago base which began with Ogburn's election to the presidency of the society in 1928. This election, arguably, brought to a head the concern of the anti-Chicago group that Chicago had too dominant a role. The Bulletin of the Society for Social Research (Feb. 1933) noted that seventy four members of the Society were registered at the American Sociological Society's Annual Conference in Cincinnati (Dec. 1932).


The Chicagoans were aware of the undercurrents of dissatisfaction with the national association, but did not ascribe it to their own pre-eminence. For example, in the discussion of the American Sociological Society Conference of 1932 which took place in the meeting of the Society for Social Research on 9 Jan 1933, 'most of the reporters paid more attention to the undercurrents felt in the national society than to the
papers read at the formal meetings. Many seemed to feel that there were more or less serious tensions in the organisation which were giving rise to preliminary millings about which may follow through some sort of social movement which in turn may eventuate in new institutional structures within or without the mother structure'. (Minutes of the Society for Social Research, 9. 1. 1933)

The tension that followed Ogburn's election was not a function of Chicago providing the President of the Society. Chicago had, of course, provided the president of the society before, but the rebels had not been so organised before nor had the Chicago base been so firm. Ogburn was not only a Chicagoan, but was also a quantifier and was supported by other quantifiers.

The result was a gradual build up of combatants that had Chicago and the quantifiers on one side and, on the other, a diffuse group with no obvious identifiable theoretical or methodological or institutional links whom Lengermann described as 'association men', supported by a wider (and in the last resort crucial) group whose links were geographic (from the East and Southwest). This group were 'agitated and divided by theoretical issues' and acted spontaneously in their discontent with Chicago's influence. The two sides were acting politically and over five years each side came to the ascendancy in turn. It was only when the opponents managed to manoeuvre into a position when they could motivate the large band of general sympathisers that they effected the 'defeat' of the Chicago group.

An examination of the two camps reinforces the political rather than theoretic or methodological nature of the division. On the Chicago side were the Chicago faculty, W.I. Thomas, a group of Chicago graduate students from the 1930s and some earlier
graduates and members of the Midwestern and Southern regional associations. In addition Stuart Rice, S. A. Chapin, Dorothy Thomas, Kimball Young and George Lundberg among other quantifiers strongly supported Chicago.

The other side was effectively lead by L. L. Bernard, a graduate of Chicago in 1910 who had been a professorial lecturer for two quarters at Chicago in 1927 but failed to get a full time post at the University. He was supported by J. Davis of Yale, W. P. Meroney of Baylor (M. A. Chicago, 1922), Newell Sims of Oberlin and Harold Phelps of Pittsburgh. These were the collaborating group that organized the opposition in an overt and direct way.

They were in turn supported more or less strongly by C. North, (Chicago doctorate, 1908), M. C. Elmer (a Chicago doctorate, 1914), Earle Eubank (Chicago doctorate, 1915), W. C. Smith (Chicago doctorate, 1920), Floyd House (Chicago doctorate, 1924 and assistant professor at Chicago in 1925 and 1926), Howard P. Becker (a Chicago doctorate, 1930), Willard Waller (MA, Chicago, 1925, and author of a reply to Lundberg supporting Blumer's position in the first issue of the American Sociological Review), O. D. Duncan (a quantifier who later obtained a doctorate from Chicago, 1949, and was appointed an assistant professor in 1950), Read Bain, M. Parmelee, F. H. Hankins, J. Bossard, M. Davie, C. Dittmer, S. Kingsbury, J. Lord, H. Miller, J. J. Rhyne, E. A. Ross, M. M. Willey, and J. M. Williams. In addition the membership of the Eastern, Southwestern and Ohio regional societies supported the anti-Chicago group.
In the event each side legitimated their position, in one way or another, by suggesting that they, rather than the opposition, adopted a scientific attitude. Those who supported the Chicagoans adopted two different approaches. The quantifiers argued that operationalisation and/or measurement were central to a scientific sociology, while the Chicagoans themselves, in the main, espoused a value free scientism. Bernard, on the other hand, argued that his opponents were a

'group of men who are dominated by a viewpoint that is almost wholly unscientific' (Quoted in Lengermann, 1979, p. 190, footnote 9)

7.3.2 The Significance of the 'Coup'

If the coup was essentially political, did it have any other significance than an overdue rearrangement of administrative responsibilities within the discipline?

An alternative assessment of the significance of the coup, to that propounded by critics, was that it opened up sociology. Reflecting on the coup, Park (1936a) wrote to Blumer that sociology ought to broaden its horizons. For him, Chicago, rather than inhibiting the development of the discipline, was the focus through which such a broadening could be effected. He recognised that the University of Chicago had

'been put on the spot by the recent attacks upon it and is more or less forced to make itself the protagonist of academic freedom'

This, Park argued, it could do in a variety of ways, some already initiated. Principally, it should

'not use the academic rostrum for the purpose of making political speeches but to use the freedom and detachment
which University life offers to investigate the problems that agitate the public."

In which respect, he was heartened by the lead given by Burgess in his

'presidential address in which he raised and sought to answer the question "What contribution can sociology make to social planning?"'"

Also, Park approved of the broadening of the compass of the Institutes of the Society for Social Research and urged that the American Journal of Sociology (which he saw as needing to attract a new readership) adopt a broader approach in view of an increasingly sophisticated public. The time, he argued, was ripe for a review of fundamental points of view and a reorganisation of research on a broader front.

'The questions that are agitating the public now are fundamentally political. I am convinced that the issues raised can be studied objectively and that we may lift the whole level of sociological thinking by attempting to define and investigate these problems rather than merely discuss them.'

In the event, the developments within the Social Science Research Council along with the outbreak of war saw sociology take on a new orientation with the Chicagoans playing, as has been suggested, a significant role in it.

7.4 The Nature of Chicago's Decline

The loss of official recognition for the American Journal of Sociology is, then, seen by some commentators as the beginning of the end of the 'Chicago School'. It is regarded as the point at which Chicago sociology went into decline. (Bernard, 1973; Coser, 1976 & 1978; Kuklick, 1973; Martindale, 1976; Madge, 1963; Odum 1951; Faris, 1967; de Bernart, 1982; Goddijn, 1972b).
The Chicagoans are portrayed as introspective and parochial, and, with the loss of Park, unable to maintain momentum.

7.4.1 The Introspection of the 'Chicago School'

The decline of the 'Chicago School' is sometimes framed in terms of Chicago's parochialism. Chicago is seen as losing touch, through its introspective attitude which was dominated by complacency and conservatism.

This caricature is misleading on several grounds. The Chicagoans did not 'retreat' and avoid discussion of theoretical, methodological or substantive issues. As is clear from the discussion in chapters four and five, Chicago sociology was continuously involved in methodological and theoretical developments.

The Chicagoans were certainly not insular, they were very much a part of sociology on a national level through their involvement in formal organisations notably the American Sociological Society and the Social Science Research Council. In addition, the American Journal of Sociology remained a major sociological journal and continued to be edited and published at Chicago. The Summer Institutes of the Society for Social Research continued to develop a broader perspective. The Society had constantly appealed for such broadening.

'The Summer institute has become one of the most interesting and valuable events of the year for sociologists and students of sociology at the University of Chicago and neighboring schools. Its purpose is to serve as a clearing house for current research projects. Here students and faculty members bring their hypotheses, data, and conclusions and submit them to the shafts of friendly criticism from some 75 or 100 fellow research workers.... Moreover, if any members, especially at schools other than
the University of Chicago, know of fellow instructors or graduate students whose research could profitably be brought before the Institute, a prompt note about it would be appreciated. The plan this year is to have a still larger number of research reports from other graduate departments than in the past. Increased contact with points of view in other universities and with points of view in departments closely allied with Sociology at the University of Chicago should be stimulating.' (BSSR, June 1929, p. 1)

This expansionist attitude was repeated in 1930

'Reports are not limited to members, however. If members know of others who are doing research work falling within the scope of the institute, a word about it will be appreciated'

and went further in 1931 with the Institute being the most ambitious to date with the three Mid-West universities cooperating with a central theme of regionalism. In November 1932 the Bulletin carried an article on page 1 which noted that

'Your officers and executive committee are in agreement that these tendencies [of growth] should be, or are, in the direction of greater inclusiveness of membership and of participation. The Society desires both persons who are engaged in research among all of the social sciences (and not in sociology merely); and who are members of institutions throughout the north central region (and not only at the University of Chicago).... Information concerning interesting research undertakings in other social fields and in other institutions which might profitably be brought to the attention of members is therefore solicited.'

This broadening of interest and appeal is reflected in the doubling of the number of non-sociology staff at the university who addressed the society over the period 1924 to 1935 and the increase from six to thirty one per cent in talks on philosophy and other social sciences over the same period, (see Appendix 3).

The Society for Social Research had applied for membership of the American Sociological Society as a regional chapter in 1934 and, even after the coup, continued with this affiliation. In 1937, Phelps, Secretary of the American Sociological Society wrote to
Bernhard Hormann, Secretary of the Society for Social Research at Chicago to ask for advice and consent to the formation of a Conference of Secretaries of regional societies, (Phelps, 1937).

Besides the formal involvements of the Chicagoans the informal network of relationships with graduate students working in the discipline, and communications with other academics on issues ranging from tenure recommendations to academic discussion all continued uninterrupted.

The Chicagoans had fairly close academic links with other major sociology departments, through both visiting lectureships and personal contact and correspondence. During the thirties, for example, Burgess taught at Columbia and Park at Harvard. Visiting lecturers at Chicago included Talcott Parsons (see Appendix 1) and Wirth communicated extensively with Parsons, particularly in relation to Parsons' 'The Structure of Social Action' which Wirth reviewed. Parsons also sent Wirth a preview of his address to the American Sociological Society of 1937 titled 'The Role of Ideas in Social Action' (Parsons 1937a). Wirth also had a long standing friendship with Robert Lynd, who would have liked Wirth to join him at Columbia (Lynd, 1941b), and with Howard Odum of North Carolina. Wirth was also on first name terms with Robert Merton and Paul Lazarsfeld and was instrumental in the invitation extended to the latter to teach at Chicago in 1949.

The Chicagoans were also very much involved in Government sponsored research, notably Works Program Administration (WPA) and Federal Employment Relief Agency (FERA) projects, with, for example, Blumer working on narcoticism and Sutherland working on
probation and parole and, in conjunction with H. J. Locke, studying men living in Chicago shelters. Hauser was granted two years leave of absence (1934-5) to work on FERA projects and Ogburn was heavily involved in government sponsored research throughout the thirties. Such activities prompted the following comment in the Bulletin of the Society for Social Research (Dec. 1934, p. 3)

'DEMAND FOR SOCIOLOGISTS
It appears that it is a good rule, if a sociologist is unaccounted for in these days of the New Deal, to look for him either in Washington or somewhere on the staff of the F.E.R.A. Conservatively estimated, about 10% of the Society members are occupied in this manner. The following are in Washington: P. M. Hauser, C. S. Newcomb, E. J. Webster, S. A. Stouffer, F. F. Stephan, J. O. Babcock, E. D. Tetrau, H. G. Woolbert.'

Nor were the Chicagoans parochial in attitude. The penchant for studying Chicago was, rather, pragmatic. And, of course, not all the Chicagoans spent all their time on the issues of the city of Chicago. The financing of much research on the city came from external funds, notably the Laura Spellman Memorial Fund, and later the Rockefeller Foundation. The attraction of these monies was not indicative of parochialism. Many of the staff had commitments beyond the narrow confines of the University, notably through government sponsored research. A stubborn parochialism was not the cause of its 'decline' in the 1940s.

7.4.2 The Loss of Park

Matthews (1977) attributed what he saw as Chicago's detachment from the changes overtaking the discipline in the 1930s to Park's influence. Chicago suffered, he suggested, by Park's retirement.

'Without Thomas or Park to provide a dominant personal force
and inspiration, the deficiencies of Chicago’s theories and methods became more apparent.’ (Matthews, 1977, p. 179) [10]

Park's going is seen as the final passing of an holistic perspective at Chicago. However, it is quite likely that Park would have 'retracted' into a specialist field at Chicago as he did at Fisk. Race studies had always been his major concern (Blumer, 1972; Barnhardt, 1972; Matthews, 1977), and this he developed in two ways, according to a letter written to Wirth in 1938 (Park, 1938). Park was concerned with the investigation of the moral and personal social world which, in terms of his own research, involved the investigation of

'two types of 'world' the Bohemia of the Lower North Side in Chicago where Park House is located, and Cedar Street, in Nashville Tennessee. The first is an area like Greenwich Village; the other is an underworld, such as every metropolitan city supports. But it is an underworld of Negroes. I am interested in exploring these underworlds, which are characteristically cultural and racial melting pots, in every part of the world.'

His method of investigation, reflecting his increasing intolerance of statistical study as he grew older was mainly based upon 'life histories of a generally psychoanalytic bias' which were designed to

'throw light on the nature of the intimate and relatively closed moral and personal order to which the individual person is most responsive, and they throw light also upon the processes of acculturation which take place within the limits of such a minor cultural unit.' (Park, 1938)

After 1935, despite Park's attempts from his outpost in Fisk to encourage his former students in the 'type of sociology that Chicago had made its own', Matthews claimed, the Chicago department's ascendancy was rapidly waning. This he attributed to a complex of forces. These were

'an increasing concern with the scientific status of the field, reflected in a preoccupation with methodology; the
rise of other sociology departments as centers of research and graduate instruction; the absorption of major European sociological theories; and changing concepts of the proper role of the sociologist in relation to the society he studied' (Matthews, 1977, p. 179)

That Chicago was unable to cope with these changes, Matthews attributes primarily to Park's influence and legacy.

Methodology was becoming more self conscious at Chicago in the 1930s. This was in conjunction with a changing appreciation of the notion of objective science. Explanation of an external reality via classification procedures derived from direct but unverifiable and unsystematic observation was no longer deemed adequate. Instead, the twin gods of validity and reliability were being invoked. Yet, Matthews claimed that the 'analysis of complex situations in terms of the subjective perceptions of actors pushed Park and the 'Chicago school' away from statistics. More intensely as he grew older and the demand for statistics grew, Park came to despise it as 'parlour magic'.... This aversion to statistics, however, meant that as sociological research became more quantitative in the late twenties, the development isolated Park himself as an exotic.' (Matthews, 1977, p. 179-180).

Similarly, as the thirties progressed, the new generation of sociologists began to adopt alternative perspectives to the prevailing interactionist-functionalist axis. Weberian and Marxist perspectives were taken up directly, although the life of these as autonomous roots to analysing the social world were curtailed as structural functionalism muted the phenomenological potential of the Weberian approach and McCarthyism inhibited the growth of Marxist perspectives. Nonetheless, in the late 1930s and 1940s, Parkian concepts appeared to be rather limited. Blumer (1980b), Hughes (1980b) and Matthews (1977) all point to the distortion of his social ecological approach by later generations.
who concentrated on spatio-statistical studies in 'psychological behaviorism', led to the ecological approach being fiercely attacked, Alihan (1938). Park accepted the criticism of his approach on the whole with the exception of 'some malicious interpretations' (Matthews, 1977). Ironically, by the mid fifties, the ecological approach was going through a revival (Schnore, 1958).

The rise of other major centres, notably Harvard and Columbia, [11] Matthews argued, was the result of the adoption of either a methodic or theoretic orientation which was alien to the Park inspired 'Chicago School'. Columbia developed a 'highly rationalized, efficient and large scale organization of research' which lacked 'personal inspiration' and was 'easily reproducible and multiplied'. This contrasted sharply with the individualistic research at Chicago which relied heavily on the 'inspiration of a great teacher and the personal flair of the researcher'. Harvard, whose prominence owed much to Parsons' system theories, drew on the European theory at the expense of the traditional American pragmatists. While Park had been aware of these 'great continental masters', their ideas never penetrated 'beyond the horizon of Park's intellectual spectrum'.

'Finally, the academic climate of the thirties was unfriendly to Park's determinedly detached, apolitical approach to research. The number of sociologists working in public agencies increased, and many came to consider their role as that of manipulative elite, consultants to a powerful state rather than an active, rational public. As they became involved in the practical problems of the depression era and the challenge of Fascism from abroad, an open commitment to social engineering and political involvement replaced the Parkian image of the concerned scholar as detached observer and midwife to attitude change.'(Matthews, 1977, p. 183)
Park shied away from political doctrine (Blumer, 1972) and his continuing detachment from this new perspective on the relationship between the sociologist and society is clearly reflected by his letter to Wirth (Park, 1941). In it Park referred to a petition he had been asked to sign. He wrote that he was usually 'allergic to pressure groups' but in this case supported 'the President and the policy of the Government in this crisis'. He was 'in favor of militarism' but was 'not interested in the defeat of Germany, nor the destruction of the Nazi regime, in order to preserve the English Empire, much less to preserve the existing regime in Russia. I am interested, however, insofar as such a defeat will discourage international crime and aid in the creation or restoration of international order'.

However, the Chicagoans were not of an accord with Park's view. As discussed in chapter four, the Chicagoans were fully involved in methodological innovation up to the 1950s. Besides the work of the faculty a cursory glance at the Ph.D. theses produced between 1930 and 1950 shows a heavy concern with correlation and prediction studies and with attempts to isolate causal factors, (Appendix 5). A considerable amount of effort was directed towards testing prediction instruments and measuring devices, as epitomised in Reiss (1950) 'The Accuracy, Efficiency, and Validity of a Prediction Instrument' and Star (1950) 'Interracial Tension in Two Areas of Chicago: An Exploratory Approach to the Measurement of Interracial Tension'. In this respect the Chicagoans were responding fully to the initiatives coming from the Committee on University Social Science Research Organizations in 1929, 1936 and 1944. [12]
While they tended not to have the acknowledged personnel in the rapidly expanding quantitative field in the post war period, they were not slow in importing the required expertise and adopting the new techniques. Unusually, they lagged behind the innovations, to some extent, rather than leading them.

Theoretically, the Chicagoans, notably through the developments in general theoretical orientations being made by Blumer and Wirth, were integrally involved in the assimilation of European perspectives. Blumer had spent a sabbatical year in France on a Social Science Research Council Fellowship in 1932 with the aim of discovering the theoretical perspectives dominant in that country and Wirth was very much influenced by Weber and the German sociology of knowledge approach. Both Blumer and Wirth were very well versed in European sociology (Ogburn, 1930). That the department lost their services around 1950 left a gap which was not easily or quickly filled.

Although Park might not have been inclined towards co-operative research, this was not reflected by the Chicagoans at large. Among its recommendations, the Ogg Report (1928) proposed that research be more effectively organised, that it should follow the pattern of broad social science research as evident at Chicago, Columbia and North Carolina. Several departments at Chicago were involved in interdepartmental organizations, including the social sciences through the Local Community Research Committee. Park, as was suggested in chapter three, tended to be somewhat remote from the Committee. Burgess, and later Ogburn and Wirth, however, were very much involved with the Local Community Research Committee,
the Social Science Research Committee that succeeded it, and the Social Science Research Council, of which Burgess was Chairman from 1945 to 1946.

While Park may have been disinclined to breach his apoliticism, this was not the case with the other members of the department. Wirth was concerned with the political implications of sociological enquiry and specifically the defeat of Nazism (Wirth, 1941); and Ogburn too, considered the sociologist's role to be more than the detached observer (Ogburn, 1942). In a scathing attack on popular folk lore, customs and norms, presented as a retrospective on the peculiarities of mid-twentieth century Americans, Ogburn abandoned the 'objective reporting of attitude' for a thinly disguised ridicule of popular ignorance. For example, he noted that

'The adults sometimes had a childish faith in experts. The opinions of a Negro boxing champion named Joe Louis were eagerly sought on political matters, especially as to whom to vote for in a presidential election. The connection between the strength that could deliver a knockout blow to an opponent's chin and wisdom in political matters seems not to have been questioned.' (Ogburn, undated, pp. 2-3)

Similarly, writing around 1952 on McCarthyism, Ogburn again directed attention against the folly of popular misconceptions and their dangers, in a manner that reflected the new role of the sociologist.

'Granting the need of stamping out communism, it hardly seems necessary to turn our political institutions upside down in order to do it.... However, as a slogan [communism] has very broad appeal to a people whose warlike patterns are activated and who want to fight an enemy, at home if not abroad. The leader of such an emotional drive may well become a hero to many who respond with extreme devotion. This extreme devotion helps to explain why followers are not alienated by McCarthy's gross behavior. (Ogburn, 1952, p. 1-8)
Throughout his career, Ogburn had been concerned with major social and political issues, from his early research for the 'President's Mediation Commission' on the strike in the lumberjack industry (Ogburn, 1917), through his involvement as Director of Research on the Presidents' Research Committee on Social Trends (1933), and his subsequent social trends research which was directed towards the problems of the Depression era and war years. Indeed, his 'detached' enquiries and observations on the second world war and the cold war with Russia that followed resulted in him being labelled alternately 'pro-Nazi' and 'pro-Soviet', the latter during the McCarthy era. Burgess, too, had been involved with national policy initiatives through his work on the Wickersham Committee and he was chairman of the Family and Parent Education sub-committee of the White House Conference, (Burgess, 1934).

However, during the 1920s, such involvements were individual endeavours and

'the thrust of concern was to get out and study the world, not get involved in these controversial issues of public policy'. (Blumer, 1972, section 2, p. 7)

It was during the 'New Deal' that things began to change, though, and when Ogburn edited 'American Society in Wartime' (1943) the majority of the Chicago faculty contributed, including Faris and Park who were, at that time, emeritus professors.

Matthews' account of the decline of the 'Chicago School', then, is integrally related to the surpassing of the ideas of Park. However, the Chicagoans were, as has been examined above, far from constrained by Park's ideas. For example, in his journal
Ogburn recalled

'I was glad to speak [at the dedication of the Robert E. Park Hall at Fisk University] for five minutes in tribute to my former colleague at the University of Chicago, Robert Park... He had great influence, distinguished students who did excellent research, and a considerable following.... All the other members of the Department of Sociology were students and followers of Park except Faris. Two or three times in these early months [of 1927] we began conversations, but they never went well. I don't know why. I think I got the idea rightly or wrongly that [Park] was trying to tell me what was what, and I did not recognize anything new in what he said. I thought also perhaps wrongly that he would like to have had me one of his followers.... I am sure I am too sensitive about being anyone's follower, especially if that person in any way tries to dominate me. I usually don't mind an egotist or how much he displays his egotism, so long as it is not accompanied by a love of power, especially a power to be exercised over me.' (Ogburn journal, 4th & 5th April 1955)

Park certainly had an influence on the work produced by most of the other Chicagoans in the 1920s but to restrict the notion of the 'Chicago School' to this limited sphere of operations is to exclude the majority of the work undertaken by the Department of Sociology at Chicago. The Chicagoans did not stand still but engaged, as has been shown, widely in the debates and the activities which Matthews indicated transcended their traditional approach.

That the Chicagoans had moved ahead of Park's rather out of date conception is illustrated by the special meeting of the Chicago University Social Science Research Committee on Friday and Saturday, the 1st and 2nd of December 1939, to commemorate the first decade of the Social Science Research Building. A large number of invited social researchers from the United States and Canada attended this prestigious occasion. The Friday morning session was addressed by C. E. Merriam on 'Urbanism' and H. Bruere on 'The Social Sciences in the Service of Society'. The Saturday morning
session, chaired by L. J. Henderson of Harvard was, on a survey of research and was addressed by Ogburn on 'Social Trends' and by Thurstone on 'Factor Analysis as a Scientific Method'. The latter was discussed by W. Line of Toronto University and E. L. Thorndike of Columbia. The luncheon speaker was Beardsley Ruml (of the Social Science Research Council) who commented on the prospects of social science research. The afternoons of both days were given over to round table sessions on 'generalization in the social sciences', 'integration of the social sciences', 'quantification' and 'social science and social action'.

Those very features Matthews had indicated were overtaking Park, were, then, assimilated by the Chicagoans. If the 'Chicago School' suffered a decline, it has to be explained in terms other than the loss of Park.

7.4.3 Structural Factors Leading to Chicago's Decline

Structural changes in sociology inevitably brought about a lessening of the dominance of any one department or group of departments. The rapid expansion of sociology, the increase in specialisation within sociology and the consequent narrowing in focus of research areas and realms of competence made it impossible for one department to dominate the whole discipline, even if it had been a possibility earlier when sociology was seen in more holistic terms.

While the Chicagoans did not dominate the discipline, they were, nonetheless, more influential in some areas than ever before. However, as the 'Chicago School' has retrospectively been
associated with method and this was not an area they dominated after 1930s then they are seen to be in decline.

Changes in research organisation were being made too. For some time the organisation and goals of the Social Science Research Council had been under review and by 1945 the structure was seen to be a decade out of date. Previously, funding from the Rockefeller foundation had been concentrated on a group of fifteen universities. This changed after 1945 and Chicago University's influence, like most of the universities in the group, diminished. It is debatable the extent to which Chicago influence diminished as a result of the coup. Arguably, Chicago experienced a natural 'decline' given the structural changes in the discipline. Sociology was changing rapidly after 1945. Many more sociologists were vying for recognition, large numbers of sociology departments were challenging for research monies and universities were establishing social research units. Organisational structures such as the Social Science Research Council were also changing in view of these developments. Increasing specialism and the search for new fields meant that sociologists with broad interests, especially those associated with bygone eras were eclipsed. Reflecting on this, Ogburn wrote, at the time of his retirement from Chicago

'I put much time and effort on social action in the community, state and nation. I saw I could not do this and maintain my scientific research work. So when I went to Columbia in 1919, my urge to help make the world a better place to live in, was transferred to helping make the social sciences more scientific; and so for 20 years there was much scientific organizational work. But looking at the records of all this, it all seems dead and gone. I helped found the Social Science Research Council, served for years as chairman of its most important committee, the Problems and Policy Committee, and then was for three years chairman of
the Council. Now when I happen to go up to the S.S.R.C. headquarters in New York Central Building, I am practically unknown and my work forgotten. They never send me any communications, and I don't know what is going on.' (Ogburn journal, 13th September 1952).

7.5 The Extent of Chicago's Decline

The decline of Chicago was essentially a natural result of the development of the discipline rather than a rejection of a 'Chicago Approach'. The coup acted as a focus for the rejection of Chicago's administrative 'domination' of sociology. The structural changes in the discipline made this inevitable. The Chicagoans were not isolated or rejected, even if the substantive work of some of them was forgotten in the post war period. However, their inclusion in the elitist Sociological Research Association [13] along with many of the members of the American Sociological Society who had voted against the Chicago nominees in 1935, points to the continued involvement and prominence of the Chicagoans in the discipline nationally. Furthermore, a year after the coup, the Chicagoans were back in executive positions in the American Sociological Society, with Ellsworth Faris as President and E. W. Burgess on the Executive Committee. Charles Johnson of Fisk, and a close friend of Park and member of the Society for Social Research was Vice President. Three of the remaining four ex-presidents who were members of the executive committee and half the six elected members were also members of the Society for Social Research.

After 1935, and even in its 'doldrum period' in the 1950s, Chicago never dropped out of the circle of half a dozen most
influential sociology departments which included Columbia, Michigan, California, Carolina and Harvard. Chicago remained part of the privileged group that tended to benefit most from the Social Science Research Council and, despite the 'setbacks' of the 'coup', it remained centrally involved in national and regional societies. The speakers at The Annual Institutes of the Society for Social Research continued to be drawn from a wide spectrum within the discipline.

Indeed, there is little to suggest that such prestige did anything other than wane following the spectacular rise of Columbia in the immediate post World War Two era. This rise itself followed what was probably Columbia's own 'lowpoint'. The 1940s was not a period in which Columbia rushed to fill the void left by Chicago. According to Lynd (1941a, 1941b), Columbia was providing a 'shallow training' for sociologists, and he produced a memorandum suggesting a radical revision within Columbia University [14]. It was not until Paul Lazarsfeld was appointed and the Bureau of Applied Social Research became established after the second world war that Columbia University got the impetus that made it the leading sociological research institution, (Coleman, 1980).

The waning of Chicago, irrespective of the advances made elsewhere, was inevitable given the expansion and diversification of sociology in the United States. What is surprising is that Chicago University exerted such a strong organisational influence for so long, especially as, up to 1935, it had never been a large department in terms of tenured staff. Chicago's impact was bound
to decrease as more and more institutions developed sociology departments and sociological research programmes.

7.6 Conclusion

During the fifties Chicago was less innovative than some of the rival departments, but this was more a function of the simultaneous loss of key personnel in 1951-52 than of the cumulation of a downward spiral. Ogburn and Burgess retired in 1952, (as did Thurstone). The War had inhibited the recruitment of new faculty or the elevation or development of existing faculty to take over. The death of Wirth in 1951 could not be planned for. Stouffer moved to Columbia University via the War Department and Shils' involvement with the department between 1948 and 1958 was limited due to his leave of absence on government service and his involvement on the Committee for Social Thought.

Chicago was bereft of key personnel who might have developed the quantitative techniques that came to dominate in the 1950s. That is not to say that Chicago had nobody to develop this area, and indeed, much fine work was done by Duncan, (before his surprising departure to Arizona, where he faded out of the limelight, (Coleman, 1980)). Goodman and Hauser also made important contributions, but without the same recognition afforded the work done at Columbia and Michigan. The restructuring of the Department at Chicago through the recruitment of quantifiers like Blau came rather late and, according to some sources (Janowitz, 1980), failed to establish a credible environment through which pioneering quantification might have prospered.
It is, furthermore, misleading to equate any decline in the role of Chicago University with a decline in ethnographic research.

'I don't think there ever was a decline in the amount of ethnographic research... I started doing research in 1946, 47, 48, doing ethnographic research, and I've done it ever since. And lots of other people were doing it, at the same time I was. And it's true that, that as survey research became more important, people, especially in the Eastern part of the United States that the notion that it was dominating sociology, that was sort of a parochial kind of view that people at Harvard and Columbia essentially had. And I never saw any evidence of it myself, because, you know, we were just going right on doing what we were doing, and there were lots of us. In fact, I noticed some years ago that there was a very interesting phenomena... I took the list of [McIver] prize winning books and .... out of, I think, perhaps I'd say ten or fifteen, I think one or two were done with the use of those quantitative techniques. So in fact, quantitative techniques were not as dominant as people thought.' (Becker, 1979a, pp. 15-17)

The post World War Two era was also supposed to have been the point at which Chicago's ethnographic orientation became most developed. It was in 1952, however, that Blumer moved to Berkeley and Geer, Vidich and Glaser were not tenured at Chicago in the 1950s. Howard S. Becker was an instructor in 1951 and 1952 following his award of a doctorate in 1951. His next official link with the Department was in the late 1950s through his work on the project sponsored by Community Studies Incorporated of Kansas City, Missouri [15]. Strauss was an assistant professor, too, for five years 1954-1958 inclusive, before moving away from a department that was becoming increasingly dominated by quantitative interests. Retrospective accounts suggest that the tradition that Becker et al developed at Chicago at this time was the end of a longer tradition of ethnographic work. On the contrary, however, such work was in many senses the beginning in that a new validation of participant observation, as such, was attempted; later to lead to a questioning of value neutrality.
In the event the 'coup' which is supposed to have undermined the influence of the Chicago School on the theoretical and methodological development of sociology, actually serves to show that the 'Chicago School' was not a homogeneous and united grouping of practitioners standing at a distance from prevailing theoretic and methodic tendencies. On the contrary, what seems to be an improbable alliance between quantifiers and Chicagoans is only improbable if the two groups are seen as exhibiting irreconcilable theoretical or epistemological differences. The argument developed throughout this thesis undermines such an assumption. The quantifiers may be seen as a committed theory group (Lengermann, 1979) advocating radical positivism, with an interdisciplinary sub-group at Chicago. The Chicagoans as a whole, however, were not so cohesive. Arguably, they did not constitute a 'School' at all.
NOTES TO CHAPTER SEVEN

1. The report concluded that, in the humanities in general, a lot of research was going on but that it was of poor quality, being badly planned, poorly executed and barren of significant results. Methods of investigation, it maintained, were imperfectly developed, with, in general, too much concern with applied research. The report noted a tendency for specialisation to destroy co-operation, but noted the exceptions of both Chicago and Columbia where co-operative efforts in the social sciences were evident.

The report made the following suggestions. First, that research should be more directed to 'pure learning'. Second, that research and learning should be more closely related. Third, that graduate work should be better organised. Fourth, that increased attention be paid to research methodology. Fifth, that research be more effectively organised, that it should follow the pattern of broad social science research as evident at Chicago, Columbia and North Carolina. Sixth, that systematic periodic surveys of research projects be undertaken, either through the publication of 'research in progress' as in the case of North Carolina and Minnesota, or through the annual publication of faculty research, as in the case of Columbia, Cornell, Chicago, Michigan and Virginia. Seventh, the university system should develop specialisation and division of labour.

2. Only a lack of special provision for attendance at conferences and inadequate clerical assistance were pointed to as factors that Chicago might improve upon.

3. The following Universities were listed as having very little or no specific provision for research in the social sciences: Indiana University, University of Nebraska, Princeton University, Washington University, State University of Iowa, Johns Hopkins University, University of Kansas ($250 in 1925-6) Michigan, Minnesota ($1000 in 1927) Missouri, Northwestern University, Ohio State University, University of Pennsylvania ($3151 in 1926) University of Wisconsin ($7500 in 1927), Clark University.

4. This feeling that universities and colleges devoted too much time to teaching was not a universally held view. In a newspaper report with the headline 'Book Writing Professors are Scored by Speaker at Meeting of Sociologists', (Anon, 1913) a student at the Sociology society conference was reported to have been concerned that lecturers spent too much time writing for their own benefit and too little engaged in teaching.

5. The Committee on Scientific Methods of the Social Science Research Council undertook a thorough study of research methods in the latter part of the 1920s. Two Chicago University faculty were members of this committee of eight, L.L. Thurstone from the Psychology Department and Edward Sapir, an anthropologist in the Department of Sociology. The rest of the committee were Horace Secrist (Northwestern University) as chairman, A.N Holcombe, W.I. King, Mary Van Kleeck, R.M. MacIver and F.J. Teggart.
6. Poffenberger, Chairman of the P&P Committee of the Social Science Research Council, circulated members in April 1944, on research methods, notably concerning the opportunity, afforded by the war, of monitoring attitude scaling techniques.

'Techniques for the measurement of attitudes and opinions have been used for years as laboratory devices by psychologists and sociologists. More recently their use has been widely extended and their popularity greatly increased with their adoption for opinion polls and for surveys of attitudes by government bureaus and by several branches of the armed forces. Specialised procedures for administration and statistical treatment are employed by the various groups and champions have arisen to defend one or another of the favoured techniques.... P & P have authorised an enquiry into the feasibility of a thorough appraisal of attitude measurement in the armed forces and government bureaus... The methods of great potential and value are certain to be widely employed for a variety of purposes in the next few years. Therein lies the real danger that their utility will be heavily oversold and that inadequate techniques will flourish. Indeed, there is a reason to expect an attitude measuring boom after this war similar to the mental test boom that followed the last war. Something may be done now by the Council in order to make the outcome in this instance less unfortunate. P & P is now considering such a critical survey which would include suggestions for further research into methods of construction, administration and validation of all such measuring instruments. In undertaking such a survey the Council would be making a contribution in the field which is common to all the social sciences for one can easily foresee applications in every one of our disciplines.'

It seems Chicago University did not take up this cue.

7. The Committee proposed that each university have an organisation which took responsibility for acceptance and expenditure of all research funds and to represent the university in relations with external funding bodies. To appraise the research needs of the social sciences. To furnish counsel and guidance in the planning and design, prosecution and appraisal of research projects. To discover and foster research talents and interests of university staff and to provide facilities for carrying on research. To report such interests and needs to the general university administration. To facilitate communication between research workers between and within institutions. To provide a continuous record of research in progress, completed and planned and to facilitate publication. (Social Science Research Council, Committee on Organisation for Research in the Social Sciences Report, 1946.)

8. For example, Chicago produced 113 Ph.Ds up to 1935 while Columbia, the second largest produced only 50 in sociology. Between 1954 and 1968 the situation had changed with Chicago
producing 163, Columbia 172, and Harvard 120 and Berkeley 84.

9. The question of patronage, however, was perhaps not quite as overriding a concern as it was projected. Certainly Chicago faculty were regularly consulted with a view to recommending staff, but there was no attempt to infiltrate their own graduates into departments. They wrote openly and in a non-partisan way when requests were made for suggestions and opinions. A reply from Ogburn (1930) to Hankins (who in 1935 was to vote against Chicago in the 'coup') is worth quoting at length

'Dear Hankins:
In answer to your letter of the 15th about a man for Sociology at Smith, how would Bernard do? He is much interested in the history of sociology. I think he is a little hard to get along with. You know him, of course, quite well. House at Virginia is much interested in social theory and the history of social doctrines. He was thought very highly of here by Small and many others. Another very good man, who is an instructor here now, is Herbert Blumer. He has a fine critical head, is very much interested in social theory, knows French sociology particularly well and German sociology also pretty well. He reads both languages quite fluently. He is a very good teacher. His interests are a good deal like those of Cooley perhaps. Our plan, I think, is to keep him on here at Chicago, though he might be willing to go away. Another possibility is Louie Wirth at Tulane. He has a very keen mind. He is a Jew, however. At the present time he is in Germany and is especially well versed in European sociology. I think very highly of Wirth also. Another man worth considering is Dawson at Montreal who is one of the best men turned out here for some time. He is the head of the department and might be a little hard to pry loose. I think Malcolm Willey is one of the finest young men we have in sociology. He is probably the best teacher of any of these and has really extraordinary abilities along these lines. I believe he was a student of yours at one time. I think Willey is an unusually promising man. Let me know if I can write more fully about any of these men, or if none of them are suitable I can try some others. Abel at Columbia might be worth considering also. I don't envy your problem of finding a good man. Good men are now wanted by Michigan, Minnesota, Illinois, and Oberlin. With cordial good wishes, I remain,
Sincerely yours,
William F. Ogburn.

Bernard, House and Willey all voted against Chicago in 1935

10. In a sense (not explicitly developed by Matthews) Park's going did leave a gap. Referring to many of the students in the sixties Blumer and Hughes argued that Park developed them, although they were not particularly bright.

'He took these people and he brought out of them whatever he could find there.' (Hughes, 1980b, p. 267)
'Park had an extraordinary amount of patience in working with someone who had an interest and, consequently, Park would take individuals who - most of them men... - who were really very mediocre... but in working with them, he would just continuously draw out, lay out new lines along which to proceed, just leading these individuals more and more into a depthlike kind of knowledge of the area in which they were working. And some of the better monographs which came out, frankly, were of this sort.' (Blumer, 1980b, p. 268)

11. See note 8.

12. See, for example the following Ph.Ds: Cottrell (1933), Lang (1936), Cox (1938), Reeden (1939), Dunham (1941), Devinney (1941), Campisi (1947), Bowerman (1948), Turner (1949), Shanas (1949), Lunday, (1949), Nelson (1949).

13. The establishment of the Sociological Research Association following the 'coup' was not the product of a Chicago separatist movement, but was, and remained for many years, an elitist conclave of sociologists including (by 1940) Burgess (president in 1942), Ogburn, Thomas, Merton, Parsons, Lazarsfeld, Stouffer, and Bain.

The first president in 1936 was F. Stuart Chapin (University of Minnesota), the secretary-treasurer was E. B. Reuter (University of Iowa) and the remainder of the initial Executive Committee were Donald Young, Robert MacIver and Stuart Rice. The first annual meeting to take place in conjunction with the December annual meeting of the American Sociological Society was to be addressed by Dorothy Thomas and Thorsten Sellin with Warren Thomson, F. H. Hankins, G. B. Vold and W. I. Thomas as discussants. A further session analysing Lundberg's paper 'The Thoughtways of Contemporary Sociology' was to be lead by Herbert Blumer along with Read Bain and Samuel Stouffer. The membership clearly cut across 'factional' lines. As Park (1939) noted, the Association was not concerned with factional divisions but rather with research practice. In correspondence between Chapin (1936a, 1936b) and Wirth (1936a) the fear was expressed that the meeting of the Association may be gate-crashed by Society members opposed to the small selective nature of the group, then numbering about fifty.

14. Lynd was also concerned that this was a national problem, and that sociology was not highly thought of in government circles. He wrote to Wirth

'Having done "Knowledge for What ?" I'm inclined to say: "Enough, let's get to work and stop talking". But this creeping extension of "the sociology of this and that" is dangerous from the point of view of what students are trained to do. The predicament of our department is not unique, and the shallowness of the training of young sociologists is recognized in Washington.' (Lynd, 1941a).
15. Howard S. Becker received his doctorate at Chicago in 1951 and was an instructor in the Department in 1951 and 1952. He does not appear again on the Official Publications until 1959, when he is listed as assistant professor (at Kansas). This listing is repeated in 1960 and 1961. Prior to 1959, Becker undertook field work on the study of a state medical school, sponsored by Community Studies, Incorporated, of Kansas City, Missouri. A project directed by Everett C. Hughes. Blanche Geer was Becker's field work partner on the project and Anselm Strauss was a member of the research team.
CHAPTER EIGHT

THE CHICAGO SCHOOL AND UNIT APPROACHES IN METASCIENCE
8.1 Introduction

In chapters three to seven a number of myths about the 'Chicago School' of sociology were examined. These myths occur both in introductory textbooks and in more advanced discussions of sociological theory. Commentators resort to taken-for-granted ideas about the 'Chicago School'. It is frequently seen as concerned with reform, as adopting ethnographic techniques with little concern for theoretical issues, and as having been dependent on the ideas of George Herbert Mead. Generally, the Chicagoans are portrayed as having dominated sociology in the United States up to the mid thirties, before their isolationism and intransigence in the face of the growth of a more scientific approach to sociology led to a rapid decline in their influence.

Most of the discussions quoted to illustrate the myths about the 'Chicago School' are relatively brief and superficial accounts, rather than detailed historical case studies. However, the 'Chicago School' has also been taken as a critical case by some sociologists of sociology concerned with developing systematic theories about the growth of sociological knowledge. The studies by Mullins (1973) and Tiryakian (1979a, 1979b) are examples. Examining in some detail their accounts of the 'Chicago School', and the theoretical models of sociological knowledge production they present, will provide the basis for a review of the cogency of unit approaches in metascience.
8.2 Examination of Mullins' Analysis of the Chicago School as a Network

Mullins' model proposes four stages of development of a scientific theory group: the normal stage; the network stage; the cluster stage; and the speciality. The first is characterised by a gestalt switch and the use of a new perspective by the group, the eventual outcome of which is the genesis of an environment or context in which puzzle solving can take place. This environment becomes gradually established as pairs and triads of communicating practitioners gradually form into a network which thickens into a cluster before becoming institutionalised as a speciality.

Mullins' construction of the 'Chicago School' up to 1935 was rather more concerned to provide a basis for his elaboration of the emergence of symbolic interactionism as a specialism than it was with providing a comprehensive account of the work of the Chicagoans. Mullins adopted the view that a Chicago-based symbolic interactionism emerged in the 1940s and 1950s to challenge the growing dominance of structural functionalism. He did not investigate the earlier work at Chicago in detail but claimed that the 'Chicago School' prior to 1935 passed through the four stages of his network model:

'Clearly the data on the 1892-1935 period of Chicago sociology look very much like a group progressing through the four stage model.... The group should be studied further to see whether it fits the four stage model'. (Mullins, 1973, pp 69-70, footnote 7).

Mullins did not attempt to elaborate how Chicago-based symbolic interactionism emerged out of 'Chicago School' sociology of the pre-war era. There is an assumption that as the 'Chicago School'
declined, 'Chicago sociology' gave way to structural functionalism, located in such centres as Columbia and Harvard. This became the 'Standard American Sociology' of the post war era and 'symbolic interactionism' with its genesis in the old 'Chicago School', emerged as the 'loyal opposition'.

'Following the fate of all good intellectual groups, the Chicago cluster's students were hired elsewhere, its best teachers and researchers aged and retired or died and Chicago sociology became routinized as a speciality (in this case as the majority of the discipline)... However, this period has only an indirect effect on contemporary sociology filtering through standard American sociology and symbolic interactionism.' (Mullins, 1973, pp. 69-70, note 7).

Mullins suggested that the 'Chicago School' began to split up in the 1940s and that the 'demographers like Shils' began to 'integrate themselves with standard American sociology', while a group influenced by Mead began to form around Herbert Blumer.

The stages of this development, Mullins characterised as follows. The normal stage, up to 1931, was dominated by Mead as intellectual leader. He was assisted by Thomas, Burgess and Faris and between them they trained Blumer, Cavan, R.E.L. Faris, Frazier, Hughes, Reckless and Kreuger in the 1930s. Blumer, Cavan, and Hughes along with Burgess remained at Chicago and became the core of the emergent symbolic interactionist network which formed between 1931 and 1945. Mead's influence led to a focus of attention on the concept of self and the nature of interaction. It was Mead, according to Mullins, who emphasised observation, participation and introspection. Thomas and Znaniecki's 'Polish Peasant' (1918) converted Mead's theoretical ideas into 'a major part of sociology'.

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Following Mead's death a small but growing network of symbolic interactionists, with a core at Chicago (including Strauss, Lindesmith, Merrill, Dunham, Cottrell, Rose, Wallin and Weinberg) were increasingly influenced by Blumer. Blumer then became the 'organizational chief for symbolic interactionism' and he was instrumental in establishing the basis for it to emerge as a speciality. Blumer (1938) provided a programmatic statement for symbolic interactionism and in the period from 1945 to 1952 was important in the rapid growth in its influence.

Thus, the network at Chicago 'thickened into a cluster'. Most members were Blumer's students (Becker, Hughes, Shibutani, Shanas, Strauss), and the university was the major research and training centre for symbolic interactionism. The preferred research methods were life history and participant observation; though the former was infrequently used. After 1952, the Chicago base for symbolic interactionism broke up with Blumer and Goffman moving to California and with little new development in the field taking place at Chicago. Symbolic interactionism had become a diversified speciality. The move from Chicago signalled the 'beginning of the end' of symbolic interactionism as, according to Mullins, it was not as 'socially' successful as standard American sociology, and intellectually it was caught between a phenomenologically based ethnomethodology and standard American sociology. [1]

In applying his model to symbolic interactionism and inferring its applicability to the Chicagoans up to 1935, Mullins has presented a highly selective and misleading description of the
'Chicago School'. First, Mullins considered only a limited number of theoretical developments at Chicago. Second, he assumed a sharp division between the Chicagoans and structural functionalism. Third, he placed Blumer at the hub of Chicago sociology and thereby overemphasised the importance of Mead in the development of the school.

The major theoretical ideas at Chicago identified by Mullins were Park's interpretation of 'Simmel's formalism', 'ecologism', and 'Meadian social interaction'. Thomas was demoted, no mention was made of social disorganisation, and Dewey's and Cooley's contributions were overlooked. In view of the exploration of the breadth of theoretical interests at Chicago in chapter five, Mullins' account appears rather limited. Mullins ignored any reference to race studies at Chicago, despite their prominence in the theoretical and empirical work undertaken in the Department. Similarly, Ogburn's study of culture and Wirth's work in the sociology of knowledge were ignored by Mullins, although they were no less marginal than those elements he identified. Certainly there was no obvious attempt at Chicago to recruit staff specifically for urban studies. Thomas' recruitment of Znaniecki and Park, for example, was prompted by an interest in race and ethnic studies rather than urbanism. Similarly, as Lofland (1983) has argued, and as a review of the titles of theses up to 1920 indicates, doctoral students were not directed to any narrow specialism. Furthermore, Mullins completely ignored the development of quantitative techniques at Chicago and the considerable amount of empirical work which took place using them prior to the 1950s. To imply, as his model does, that there was a
single intellectual focus and a specific mode of operation is to ignore the diversity of theoretical and methodological interests of the Chicagoans.

Mullins' characterisation of American sociology after 1940 as having a dominant trend and a loyal opposition was best served by emphasising the differences between structural functionalism and symbolic interactionism and thus implying that the heritage of the latter is somehow distinct from the former. Mullins' account of the 'Chicago School' leads one to a view of Chicago sociology as temporally and intellectually distinct from 'standard American sociology'. The normal stage of standard American sociology identified by Mullins is of functionalism rooted in the 'disconnected' work of sociologists outside Chicago who came together as a cluster after 1935. Symbolic interactionism, rooted in the surpassed Chicago tradition, emerged to re-engage 'standard American sociology'.

This clearly represents the 'dichotomous' tradition thesis of the growth of American sociology throughout the first half of the twentieth century, and it ignores the integral involvement of the 'Chicago School' in the discipline methodologically (chapter four), theoretically (chapter five) and organisationally (chapter seven). It also overstates the disjunction between Chicago affiliated symbolic interactionists and 'standard American sociology'. In effect, Mullins dismissed all of the Chicago influences on standard American sociology which, he maintained, was originated by Parsons. This thesis has shown, however, that the theoretical and methodological ideas to be found at Chicago
in the first half of the century were not strikingly different from those prevalent in other centres of sociology. An institutional network of Chicagoans (initially Small, Vincent and Thomas, and later Thomas, Park and Burgess) was augmented by communicative structures outside the institution, within the wider frame of American sociology as a whole. Other centres housed Chicago graduates; Chicago sociologists were involved in many discipline-wide organisations; and the Chicagoans did not gang up on one side in academic or organisational debates, witness the discussions in the Society for Social Research and during the 'coup' of 1935. There is little evidence to support the insularity thesis at the cluster stage in Mullins' model when applied to Chicago. Having developed a stable and more formal graduate training programme at Chicago, 'outside' contacts did not become narrower and limited to people with similar research interests. The Chicagoans did not become insular, nor was the identification of the 'School' by name indicative of a distinct approach (chapter seven). Rather, the 'Chicago School' was identified as a leader in the field, as a well-resourced and 'progressive' centre rather than as at variance with the wider sociological mileu.

Mullins' reconstruction of a post-war interactive community of researchers linked to Chicago clearly presented them as reliant on the theoretical constructs and methodological prescriptions of Herbert Blumer. Blumer, in turn, was assumed simply to have developed Meadian social psychology. No account is taken of the Blumer versus Mead debate of the 1960s (see chapter six). Mullins' reconstruction built towards a reification of the role
of Blumer. In order for his thesis to hang together, Blumer must be seen as central to Chicago sociology and not as a peripheral character. Blumer is afforded the status of perpetrator of the Meadian tradition by Mullins, (Elsworth Faris is given little prominence), and placed at the centre as the organisational and intellectual leader of later Chicago sociology. To this end, Mullins located the tenets of symbolic interactionism in Blumer (1938). However, these tenets are primarily interactionist notions, as opposed to symbolic interactionist per se, and were available in the 'Polish Peasant' (Thomas and Znaniecki, 1918) and in the 'Introduction to the Science of Sociology', (Park and Burgess, 1921), both of which were widely read at Chicago and elsewhere.

Mead, himself, Mullins assumed, had provided an underlying perspective at the heart of the pre War 'Chicago School'. However, the examination of the role of Mead (in chapter six) raises severe doubts about his central role in the early 'Chicago School'. His ideas were evidently not widespread amongst the Chicagoans. It is stretching a point, for example, to suggest, as Mullins does, that Thomas' 'Polish Peasant' study (Thomas and Znaniecki, 1918) was an empirical test of Meadian theories. Such a claim ignores the differences in the views of Mead, Cooley, James and other pragmatists and the fact that, while the Chicagoans tended to draw on a variety of pragmatist ideas, Thomas was closer to Cooley than to Mead.

Similarly, Mead's direct pedagogic link with the Chicagoans was not as strong as is supposed by Mullins. Mead's death, rather
than cause the break-up of Chicago sociology as Mullins claimed, actually served to provide a boost to one particular area of interest of the Chicagrans. A much more important factor affecting the number and quality of graduate recruits in the 1930s was probably the retirement of Park. However, the most significant determinant of recruitment was the economic recession of the 1930s.

In addition, Mullins view that, in Chicago from 1945, there was a cohesive symbolic interactionist group centering on Blumer, Hughes and Becker, is an overstatement of the way the work and ideas of the Chicagrans developed. At the very least, it ignores the differences of emphasis within this grouping and in particular ignores the evidence which clearly indicates that Hughes was not a Blumerian symbolic interactionist and that Becker was a student of Hughes. Mullins' whole account is based on a questionable linking of figures together into convenient groupings, creating artificial labels, (e.g. calling Franklin Frazier a symbolic interactionist) and assuming that Hughes, Becker and so on carried the same 'anti-standard American sociology' banner as Blumer.

Mullins seems to have relied heavily upon secondary sources in constructing his account of Chicago, notably Faris (1967). He also appears to have done little or no primary research of his own, and he clearly leaned heavily on taken-for-granted views of the history of the pre-war period in American sociology. This contrasts with his work on the phage group which involved a more systematic attempt to reconstruct the historical processes
involved. In studying Chicago, Mullins started from the assumption that there was a symbolic interactionist community at Chicago in the 1940s and 1950s and constructed the history of Chicago before this time as a prelude to the emergence of this group. However, his accounts of both pre and post 1935 'Chicago sociology' are defective. Moreover, while it seems that Chicago's influence waned from the late 1930s onwards, largely because of the growth in number and size of sociology departments elsewhere, there is little evidence for the claim that 1935 marks a change in the character of Chicago sociology, nor a separation of the work of the Chicagoans from that of the discipline in general. In short, Mullins account of Chicago is seriously inaccurate and as such cannot provide the basis for a sound assessment of his theoretical model.

8.3 Examination of Tiryakian's Analysis of Chicago as a School

Edward Tiryakian also used the 'Chicago School' as a case study to illustrate his argument that the production of scientific knowledge is located in schools. For him, a school consists of a small group of practitioners in close contact who establish an alternative approach to a subject discipline and gradually get themselves established as a distinct and viable sub-group. Eventually, for a successful school, the distinctiveness of the school gets lost as more and more practitioners take up elements of the school's central ideas and the school gradually becomes institutionalised as part of the discipline.
A school, according to Tiryakian, has a charismatic leader, committed to teaching students, who presents a clear expression of the way in which reality is to be approached and this forms the basis of a revolutionary paradigm. The leader draws on the theoretical ideas of a significant precursor and, usually, develops a programme of validation and modification of the precursor's constructs. The followers are committed to the paradigm and validate it through empirical study. A central feature of a school is that it provides its members with a sense of mission, rather like a religious sect. The school members perceive themselves as radical outsiders taking on the conservative establishment, and as such are excluded and develop their own organs for the dissemination of ideas.

According to Tiryakian, following World War One, the Department of Sociology at Chicago gelled into a 'community marked by organic solidarity' and fulfilled the criteria for a school. Park was the leader-candidate because of his direct involvement with students and his encouragement of direct empirical research. Indeed, Park's innovation was methodological, namely his suggestion that the city be treated as a natural laboratory. The 'Introduction to the Science of Society' co-authored with Burgess (1921) became the manifesto of the new school.

For Tiryakian, then, Chicago sociology was rooted in the ecological model. However, there was also another important input from a different direction. This was the 'intersubjective dimension', inspired by German idealism and Simmel's formalism, but also 'greatly reinforced' by Mead's work and ideas. This
element was encouraged by Park, who communicated it to his 'lieutenants', notably, Blumer, Wirth and Hughes, who themselves trained a later generation of students. Thus, Tiryakian argued, the school's paradigm incorporated both an internal subjective and an external objective approach. Following Park's departure in 1933, the school lost its inner cohesion and by 1940 the paradigm had lost its vitality.

Tiryakian's reconstruction of the 'Chicago School', like that of Mullins, is selective and misleading. First, he delimited the 'Chicago paradigm' in terms of two basic theoretical orientations: ecologism and Meadian social psychology. Second, he portrayed the Chicagoans as essentially concerned with ethnomethodological approaches. Third, in constructing a 'school', he presents the Chicagoans as a sectarian group.

In Tiryakian's account of the 'Chicago School', Park's ecological model is of central importance and the work of the 'followers' is seen in terms of their elaboration of the ecological model of the city. Despite Park's impact on the work of the Chicagoans during the second and third decades of the century, Park can hardly be said to have developed a revolutionary paradigm, (see chapters four and five). Such an accolade must go to Thomas, if it is to be given to anyone; yet Tiryakian ignored Thomas's influential concept of social disorganization. Conversely, Tiryakian attached little importance to Park's development of race studies and the assimilation thesis.

Like Mullins, Tiryakian also emphasised both the active role of
Mead in the early years of the school and his influence on the early Chicagoans. Tiryakian was thus able to identify a precursor for the 'School', as demanded by his model. Mead is shown as influencing Park who, in turn, influenced Blumer and others. The role of Faris in developing Mead's ideas is ignored altogether. The discussions in chapters five and six have suggested that the Chicagoans had wide theoretical interests and that Mead's influence was quite limited.

Tiryakian's model places methodological development at the centre, as the dynamic for change, and Park is seen as the focus for methodological development. While Park certainly encouraged the analysis of different facets of the city there is a strong case for arguing that the innovation lay with Small and Thomas and that Park's contribution lay in implementing it (chapters four and five). Tiryakian, like Mullins, also assumed that the methodological approach adopted at Chicago was predominantly ethnographic, particularly emphasising participant observation. He overlooked the Chicagoans involvement in more 'quantitative' approaches (chapter four).

Tiryakian's account of the 'Chicago School' presented it as developing along sectarian lines. While some commentators saw Chicagoans as missionaries spreading the word of Park (Lapiere, 1964) and others saw Chicago as 'different' (Cavan, 1983), the extent to which it is reasonable to see the Chicagoans as a whole as having a sense of mission is debatable. As suggested earlier, the idea that Chicago represented a distinctive approach to sociology does not seem to have been strong in the period up to
1950 (chapters four, five, seven). Although some Chicagoans did feel that Chicago was the best department [2], the idea of a 'Chicago School' embodying characteristic theoretical and methodological ideas seems to have been a retrospective construction by Chicagoans and others. Thus, it is doubtful if Small or Ogburn or Faris saw themselves as radical outsiders taking on the conservatism of the establishment. Rather they would have seen themselves as an integral part of the sociological fraternity, contributing to the development of sociology by extending the theoretical base through empirical validation. Certainly they challenged taken for granted presuppositions. Small criticized the religion-based perception of sociology as social amelioration, (chapter three). Faris challenged the instinct thesis and Ogburn the lack of concern with culture, (chapter five). But they did not see themselves as part of a new and distinct school confronting a conservative establishment. This is evident in their involvement in the discipline generally, most notably in administrative and research fields (chapter seven).

The sectarian nature of the 'school' is associated in Tiryakian's model with the emergence of exclusive organs of dissemination. The Chicagoans were certainly responsible for the development of national focii for the exchange of ideas through the American Sociological Society and the American Journal of Sociology. However, while they retained editorial control of the latter, it was not an organ of Chicago sociology; witness the variety of contributions and the lack of significant difference between its contents and that of the American Sociological Review, (see chapter seven).
Tiryakian attempted to establish a set of core presuppositions representing the 'Chicago School'. However, it is difficult, as has been shown, to establish a set of theoretical or methodological presuppositions germane to the Chicago endeavour that were somehow distinct from American sociology in general. It is purely arbitrary to select, as Tiryakian does, aspects of the work done at Chicago as somehow preceding more general adoption in the discipline as a whole and then to suggest that they constituted the presuppositions of a school.

Like Mullins, Tiryakian's primary source of information was Faris (1967), augmented by Carey (1975) and Matthews (1977). But even given this reliance on secondary sources he has been very selective in his interpretation. Tiryakian's account seems to have been designed to fit a reconstructed view of research programmes (in which Kuhnian and Lakatosian notions overlap), and he provided the outline of what he argued was a succession of progressive programmes which went from Durkheim to Park to Parsons. Such an idea, as the illustrative analysis of the contribution of Chicago outlined above suggests, leads to gross oversimplification of the nature of the production of sociology in the United States and the role of the Chicagoans in it. To suggest that first Park and later Parsons provided the hard core of successive research programmes which constituted the principal orientation of American sociology is to decontextualise their contributions and to ignore the impact of the discipline upon their work. It appears to represent the 'great man' thesis in a new guise.
8.4 The Foundations of Mullins' and Tiryakian's Models.

Mullins and Tiryakian both claim to base their models on Kuhn's analysis of the growth of scientific knowledge. However, both adapt this thesis to their own ends. Three major discrepancies between their models and that of Kuhn are evident and these raise questions about the status of Mullins' and Tiryakian's work.

First, Kuhn's idea that paradigms monopolised their relevant fields and that subsequent paradigms in a field were incommensurable is breached. Tiryakian dispensed with it altogether, for him there may be more than one dominant paradigm at any one time, and, while a scientist may feel a 'mission' in spreading the 'school's' paradigm, this does not necessarily imply an immersion in a paradigm that predetermines the way the world is viewed (in respect of a particular discipline area).

Mullins' model has it that normal science resides in a speciality, but that the speciality is only part of a paradigm. A successful speciality emerges as the result of the institutionalising of a social network of scientists. Further, the speciality, which is the epitome of the normal scientific process for Mullins, is initiated following a common 'Gestalt switch'. This 'Gestalt switch' is indicative of a shared paradigm. But, Mullins argued that not all members of the discipline, or even of the speciality will necessarily share that paradigm in all its details. In addition, the progress from the 'Gestalt switch' which informs the paradigm group to the speciality (which itself does not totally encapsulate the ideas of the scientist) is one which may run parallel to other areas of
involvement for the scientist. These alternative areas of research may go beyond the boundaries of the 'new' paradigm. Given that scientists may operate within more than one speciality, according to Mullins' model, one is left with the unanswered question as to whether these specialities may be from within different paradigms. If so, of course, Mullins has abandoned Kuhn's incommensurability postulate.

Second, Kuhn (1970, pp. 177-178) following empirical research in the area, had suggested that the likely size of a scientific community is around one hundred people and that this is the context within which his paradigmatic model of the growth of scientific knowledge operates. He indicated that in the social sciences, which he regarded as pre-paradigmatic, the model works less clearly, if at all. Unlike natural science, which exhibits little competition between schools with incompatible perspectives, social science is riven with disputes, and therefore social scientists are unable to take the foundations of their field for granted and engage in more esoteric and efficient development of science through puzzle solving (working unknowingly towards the next revolution).

Mullins and Tiryakian ignored this by, in effect, identifying paradigms with much smaller communicating groups. Of importance to both is the 'master-apprentice' type model, where the role of an intellectual leader is crucial in developing ideas. The net result of the emergence of a speciality or the institutionalisation of a school is not the transcendence of a paradigm but the carving out of a niche within a composite paradigm which contains
a number of specialisms.

Third, there is therefore, little concern in Mullins' or Tiryakian's models with the paradigmatic nature of the transformation of scientific knowledge. The concept of paradigm implied by both Mullins and Tiryakian is that of Kuhn's notion of exemplar rather than the wider concept of metatheoretical orientation. For Kuhn (1970), scientific progress was located within a paradigm which encompassed 'the entire constellation of beliefs, values, and techniques' shared by a scientific community. Sub-disciplinary paradigms, to which Mullins and Tiryakian allude, do not fulfil this criterion. (Kuhn, 1970; Martins, 1972). Kuhn's secondary concept of paradigm as exemplar has no meaning outside this wider paradigmatic framework. In concentrating on the exemplary element of paradigms, Mullins and Tiryakian have disengaged Kuhn's notion of paradigm from his thesis of the production of scientific knowledge. Kuhn's model of puzzle solving, anomalies, crises and revolutions is laid aside. Instead, knowledge is seen to be a function of social groupings and the access to means of legitimation. The focus of attention is not the subject matter but the social interaction networks (Hull, 1978).

In many respects, both Mullins' and Tiryakian's work reflect the influence of Lakatos. Mullins saw researchers working in different research programmes and as fertilising one from the other. He also adopted a process of rational reconstruction of the progress of a programme, admitting 'external' elements such as luck, institutional affiliation, etc., as factors. Mullins
treated them as rational factors, in the same way that Lakatos absorbed components of an ostensibly external genesis into his internal rational reconstruction. However, Mullins differed from Lakatos in concentrating on community structure; while Lakatos' research programme model concerns itself with problemshifts and the way in which the content of science is rationally transformed. For Mullins, changes in science, especially social science, resided in fashion and persuasion, aided and abetted by determination, institutional facilities and so on. Although Mullins intended to go beyond a 'schools' approach by examining the development of the theories themselves, in the event he simply offered an account of theoretical developments within a four stage community structure.

Similarly, while clinging to Kuhnian concepts, Tiryakian explicitly adopted Lakatosian concepts and attempted to integrate them into his epistemological base. Tiryakian attempted to relate his notion of 'school' to Lakatos' idea of scientific research programme. Thus the 'presuppositions' of a 'school' are defined by Tiryakian as

"those often implicit ontological groundings of a general theory: presuppositions are not empirical constructs like hypotheses, empirical propositions, and articulated theories. They are the existential as well as metaphysical foundations, the basic definitions of the situation, the basic approaches to reality which are not falsifiable by any rational or empirical means.... (Tiryakian, 1979a, p 218).

This closely reflects the 'hard core' of a Lakatosian scientific research programme. The 'school' approach concentrates more on the programmatic nature of the research enterprise than on its paradigmatic qualities (Tiryakian 1979a; Faught 1980; Farberman 1979). The scientific research programme is formulated by the
leader and is the product of an on-going school which disseminates its product to the whole profession. The leader provides the basic 'hard core' of the scientific research programme which is passed on to 'immediate followers and associates.' (Tiryakian 1979b, p. 2).

Tiryakian, however, failed to provide a convincing connection between 'schools' and 'research programmes' because he nowhere disentangled the incompatible Kuhnian and Lakatosian concepts and he concentrated on the 'hard core' of research programmes (the search for presuppositions tends to reflect this) rather than on the 'positive heuristic' which is the driving force behind Lakatos' model of the growth of science.

8.5 Summary of Mullins' and Tiryakian's Models

In many respects, then, Mullins' and Tiryakian's models tend to drift away from their Kuhnian moorings. Even so, does it matter whether they are not perhaps as closely aligned to Kuhnian precepts as they claim, and do they, in themselves, provide a useful way of assessing the production of scientific knowledge?

In one key respect the answer to these questions are the same. In drifting away from Kuhn's thesis, they have sacrificed the explanatory mechanism that Kuhn projected and have not replaced it with a satisfactory alternative. In deviating from Kuhn's model, Tiryakian and Mullins have tended precisely towards the very approach which Kuhn set out to challenge. Kuhn's paradigm model aimed at an explanation which attempted to account for the progress of science, one which was not committed to a view of
scientific progress as linear and rational and also attempted to provide a basis for explaining the apparent quantum leaps in scientific progress. Mullins and Tiryakian are much less concerned with the progress of science and re-present conventionalism in its most primitive form: that is they are preoccupied with the identification of units and, in so doing, the investigation of how knowledge develops is of secondary importance to them. Instead of providing a development of Kuhn's thesis, Mullins offered, in embryonic form, a model which argued for the primacy of social organisation within the constraints of a general theoretical orientation (which he labelled a paradigm). Mullins' model concentrated on the 'working environment' and relationships among scientists, assessing the potential institutionalisation of research programmes. While Mullins' suggested factors which are likely to lead to the success of research areas, he made no attempt to investigate the way paradigms change and the relationships of the social structure to crises and revolutions. Mullins demonstrated his model inductively through selected cases, but did not broach questions about the genesis and form of networks, what relation they have with the production of scientific knowledge, the process of innovation and change nor, even, the relationships between institutionalisation and legitimation. There was nothing intrinsic to his model which allowed for elaboration of these areas of concern. His is, as Truzzi (1974) has commented, a thesis about associations of scientific workers rather than an analysis of theory groups. Mullins provided some insights into research as a career, but his model is inadequate as a theory of
the production of knowledge as it fails to explore the relationship between research careers and knowledge production processes, implying that knowledge resides in organisational structure.

Much the same might be said of Tiryakian. He ignored the process of discovery. While the isolationism of the 'school' may be a suitable arena for nurturing a new research programme, it does not provide the cross-fertilization from other areas of work so important in innovation. For Lakatos, the 'school' is the cloister of dogmatism. In this respect, Tiryakian's 'schools' thesis fails to take account of the critique of the 'school' as metascientific unit offered by Crane (1972). For Crane the school is stagnant and does not add to scientific knowledge except in an esoteric way. The school, for her, must run out of steam because of its isolation. In effect she sees it as a stubborn, narrow research programme with nowhere to go. Tiryakian sees schools as part of normal science. In effect Tiryakian has taken schools to replace paradigms which represents a return to the approach which Crane originally questioned.

8.6 The Potential of a Unit Model

Although Mullins' and Tiryakian's models appear to be limited as metascientific frameworks, this does not necessarily mean that the unit approach has no utility. What does emerge from the analysis of their work is that one must be careful in using such concepts as 'school', 'research unit' or 'network'. Clear definitions of the concepts are necessary, especially if the unit is to be somehow internally dynamic and regulatory. Taken-for-
granted views of the constituents of a school, its activities, orientations and theoretical endeavours, must be suspended. A critical engagement with the historical evidence is essential. To adopt prevailing myths about historical entities uncritically is to run the risk of elaborating a thesis about knowledge production which is unproductive.

The key to the efficacy of a unit approach is that the criteria which serve to identify units should be consistent with theoretical ideas about how knowledge grows. Membership, citations and so on are merely indicators of interactive units rather than frames for assessing the process of science production. A unit approach would have more potential if, instead of concentrating on ideas, personnel or institutional groupings, the unit was viewed in terms of its knowledge transformative processes. One way to do this would be to focus on the processes of critique within a unit and how the critique is carried out, institutionalised and legitimated. There would be no need, then, to attempt to construct barriers around an intellectual enclave, either in terms of personnel or subject matter. The dynamic and changing nature of the enterprise would be the focus of investigation, rather than the underlying presuppositions, genesis and history of an idea, or gelling together of a group of practitioners.

The tendency towards an internalist perspective evident in unit approaches would also be avoided by adopting this approach. Tiryakian and Mullins, for example, construct the school or emerging unit as internally consistent, and as providing a set of
internal justificatory and legitimating criteria. Apart from the problems of cross-fertilisation of ideas, particularly acute in Tiryakian's model, this internalistic orientation disengages the unit from both the wider discipline in which it is located and also the social milieu. The investigation of a school or unit is thus usually in terms of how it develops a new sub-area rather than how it engages with the discipline and acts to transform the stock of scientific knowledge.

In the case of the 'Chicago School', for example, it constituted a metascientific unit in as much as it incorporated an open, and accessible, critical process which was integral to the work of practitioners both directly involved in work, of various sorts, at Chicago and of others in communication with those based at Chicago. The Chicagoans extensive involvement in American sociology (chapter seven) made the 'Chicago School' one of the foci through which developments in sociological knowledge in the United States were directed. This critical process at Chicago was institutionalised, as Park (1939) suggested, in the Society for Social Research. As the discussions throughout the thesis have indicated (especially chapters two to four), the society acted as a supportive association of sociologists affiliated, in one way or another, to Chicago. The aim of the society, as set out in its constitution, was to disseminate knowledge and act as a clearing house of ideas. The accessibility of the society, its summer institutes, communication network, frequent discussion meetings, and regular bulletin all served to advance this aim.

A unit thesis which investigates the processes by which knowledge
is developed and evaluated would provide a basis for rational selection and interpretation of unit indicators. It would also discourage reliance on secondary sources and thereby, perhaps, avoid the retelling and perpetuation of myths.

8.7 Conclusion: Units, Myths and Metascience

In general, when referring to units, notably to 'schools', most commentators adopt the term quite loosely and it is usually directed to a number of interrelated ends. First, school is used as a categorising device in an attempt to provide a root through the diversity of the history of sociology, be it at a theoretical, substantive or methodological level. Second, this mapping of history has been used as a means for marking out territory by representatives of different theoretical approaches. Third, the school is used as an exemplar of a particular approach to sociology, particularly directed at the colonisation of a sub-discipline. None of these are directly concerned with a metascientific enquiry which would critically engage these categorical and demarcation processes.

The problem that arises in these non-metascientific usages is that a large degree of over-generalisation takes place with secondary accounts piling on each other and leading to mythologisation. This is in part a result of the reliance on memory and oral tradition by sociologists engaged in writing about the history of sociology, and a lack of detailed research to check what they presume they know. This thesis, in the first instance, has illustrated how such a process has occurred in the case of
the 'Chicago School of Sociology' by focusing on five myths about the work of those sociologists in, or associated with, the Department of Sociology at Chicago.

The designation of the 'School' motivated by each of the three concerns above has drawn on and reinforced the myths, while such myths also provide suitable handles for historians and sociologists of sociology to grasp. The analysis of the myths above has suggested some of these interrelations, for example the legitimating role of Mead in the history of symbolic interactionism, the establishment of an historical tradition of participant observation research, the exemplary nature of early Chicago urban sociology, and the dichotomisation of American sociology into 'qualitative' and 'quantitative' traditions as a framework for locating the development of the discipline.

Myth generation is both a function of the process of constructing history and of prevailing conceptualisations of the areas of knowledge to which the history relates. There is an interrelationship between historical accounts and taken-for-granted contemporary conceptualisations such that, for example, in constructing the history of the 'Chicago School' current ideas about the nature of sociology inform the historical reconstruction of its component parts and, conversely, historical accounts, written for whatever purposes, provide case data that inform a general conceptualisation of the history of sociology.

The examination of the myths of the 'Chicago School' suggested why specific myths might have arisen. In more general terms, however, the implication has been that myth construction is an
almost inevitable consequence of the development of academic disciplines and their historical reconstruction. Such reconstruction has tended to be a presentist or 'Whig interpretation' (Butterfield, 1931) of the history of the contribution of significant figures ('great man' history) or of the progress of influential ideas ('great ideas' history). Sociologists and historians of science have, however, come increasingly to question the historical reconstruction of a discipline in these terms.

The debate in the philosophy of science, stemming from the Popper-Kuhn engagement and taken up by Lakatos, among others, has, in two ways, generated a critique of historiography of science. First, it has raised questions about the relationship between history and the rationality of science. Second, the concern to specify the community framework of scientific knowledge production has undermined both the sweeping construction of ideational traditions and the naive idealistic assumption of history as the work of individuals.

Tiryakian's and Mullins' view is that the 'unit' approach to the history of sociology overcomes many of the problems of the 'great man' and 'great ideas' perspectives. This view, as has been suggested, is grounded in the paradigmatic view of the development of science deriving from Kuhn (1962b) which has brought a reassessment of traditional notions of the 'progress of science'. As shown, however, the unit approaches developed by Mullins and Tiryakian have severe problems. They have internal inconsistencies, they certainly conflict with each other, despite their
ostensive Kuhnian underpinnings, and they incorporate some elements that appear more germane to a Lakatosian approach. That Kuhn and Lakatos are incompatible clearly raises problems in this respect. Furthermore, of course, the underpinnings embodied in Kuhn's and Lakatos' models are themselves open to severe criticism, not least because they adopt an essentially internalistic perspective which fails to provide any mediation of science and society and of past and present (Chalmers, 1978; Feyerabend, 1975 & 1975b). Kuhn and Lakatos, and the models based on them, are all 'historicist' in that their models appropriate history selectively in order to establish the credibility of their frame [3]. This approach thus inhibits detailed empirical investigation.

While providing a basis for a critique of the 'great man' and 'great ideas' approaches to the history of academic disciplines, most 'unit' approaches do not provide a satisfactory alternative because they, too, tend to lead to distortions of the knowledge production process. A unit approach is not, of itself, immune to the construction of myth.

In the case of the historical reportage of the 'Chicago School of Sociology', the unit approach has accentuated the development of myths and has fed through to affect the very nature of the sociological enterprise and thus of what constitutes sociological knowledge. This, then, suggests that the usage of terms such as 'Chicago School' are of limited value and should be approached critically when undertaking metascientific work or examining the history of science. This is particularly clear when a 'school'
appears to have a number of overlapping designations, as was explored in chapter two in the case of the 'Chicago School'.

The critical examination of the myths of the 'Chicago School' suggested an alternative characterisation of the work and impact of the Chicagoans to that popularly held. The 'School' was an integral part of American sociology, developing as the discipline developed. It was early concerned with social reform but not in isolation from theoretical understanding, and rapidly moved away from reformist concerns as the discipline attempted to establish a more overt scientific basis. This shift coincided with the institutionalisation of the knowledge transformative processes in the Society for Social Research. The Chicagoans were concerned with empirical data collection and tended towards methodological eclecticism. However, they did not neglect theory and developed theoretical concerns in line with the general development of the discipline and drew on a number of different traditions, particularly pragmatism, of which Mead was but one source. Chicago sociology had been prominent in America throughout the first half of the twentieth century and was particularly dominant administratively in the discipline for a number of years.

The detailed analysis of the history of the 'Chicago School' highlights some severe problems for the unit approach. However, this, of itself, does not mean that a unit approach is an unsuitable way to proceed. It may well be preferable to the simplistic cumulative theses usually embodied in the 'great man' and 'great ideas' approaches. The issue is not so much the focus of attention of the history, and thus of the metascientific
enquiry, as the process of engagement with the historical material. Identifying the unit is merely the beginning rather than the climax of such metascientific enquiry.

In constructing a unit, decisions have to be made about the way it is circumscribed. The identification of the unit is of major importance because it clearly colours the way in which the historical evidence is approached. Linking people together into research units requires some thesis about the criteria for knowledge production. The problem, for unit approaches is, then, the determination of the criteria.

One suggestion is that rather than build a stage by stage model to accommodate a revised Kuhnian thesis, as Mullins does, or to tightly define roles within an ideal type community in order to accommodate a research programme thesis, as Tiryakian does, a metascientific unit analysis should concern itself with the processes by which cross-fertilisation of ideas through critique is managed. The focus should be on the way in which the body of sociological knowledge, to which members apply themselves, is transformed through critique. There is no requirement to concentrate on reconstructing groupings of ideas or people and thus, rather than adopt 'conventional' or taken-for-granted categories, a critical engagement with the historical evidence is encouraged. The unit is seen as dynamically interacting with established knowledge rather than as the harbinger of a segregated orthodoxy or the cultish development of a heresy. [4]

In this sense the unit is a community circumscribed in terms of an institutional affiliation and a communicative network for the
transmission and critique of ideas. Such a network may be restricted to direct interpersonal relations, or based on more formal structures such as conferences and institutes, and may or may not be supported by one or more journals. In the long run, a periodical would appear to be important in sustaining the coherence and momentum of a critical unit. It is not necessary, however, that the journal be 'wholly controlled' by the unit, but rather that it re-present the processes of critique. The work of the unit may or may not be directed to a specific subject matter or involve elaborations of a core theory. The dynamic of the school may then be perceived in Kuhnian terms as dependent on puzzle-solving within a wider discipline or sub-discipline (Martins, 1972), with occasional revolutions transforming the paradigm; or in Lakatosian terms, as the cross-fertilisation of researchers working in different research programmes located within a school (or possibly even across school boundaries).

Essentially, then, the focus of attention is on the supportive association of researchers (which must be able to attract or generate research monies and have the facilities to undertake research) which acts constructively to criticize the research endeavours of its members. The emphasis is on study of the development of knowledge through critique, rather than the pursuit of presuppositions, core ideas, subject boundaries or groupings of practitioners in and for themselves. The case study of the 'Chicago School' presented in this thesis provides an initial exploration of this approach. Assessment of its fruitfulness requires further research. However, such research must be based on primary sources rather than trading on myth.
1. Mullins' account of the emergence of symbolic interactionism only provides a partial history of the 'Chicago School'. One is left to infer how his model may apply to the earlier period. Perhaps he had something along the following lines in mind. A 'normal' period lasting up to World War One with Small, Henderson, Talbot, Vincent and Thomas developing the ground for empirical enquiry at Chicago in this period with some degree of co-authorship (Small and Vincent, 1894). This paradigm group was not contained solely at Chicago, rather Small and Henderson were part of the group of inter-university sociologists who attempted to specify the sociological enterprise, as illustrated in chapter seven.

A network stage, in which relatively unstable pairs and triads of scientists engage in regular communication over a period of time could be seen to apply to the Chicagoans from about 1900. The institutional core of this network included Small, Vincent and Thomas, and later Park and Burgess. Presumably, the intellectual product that Mullins has in mind is the ecological study of 'the city'.

The nineteen twenties saw a degree of concentration of students at Chicago, and a large number of them were supervised by Park and Burgess as such Park had the institutional base to train students, although far from all of them 'co-operated' in any development of a particular perspective (Faris, 1972). Through the University of Chicago Press, an outpouring of publications took place. Thus the 'School' seemed to be indicative of a cluster operating, to some degree, independently of the wider sociological fraternity. In Mullins' model clusters are identified by name by outsiders and indeed Chicago was identified in various ways (although as indicated in chapter two there was no clearly explicit notion of a 'Chicago School' until the 1950s and 1960s). Chicago was seen as the 'place to be', as innovative, as administratively dominant in the discipline (hence the 'coup' of 1935). This administrative dominance by Chicago up to 1935 is indicative of strong and centralised speciality.

2. Chicago University was recognised as 'the place' to do sociology. A reputation had been established by 1915 which identified Chicago as one of the most innovative and prestigious places for sociological research (Blumer, 1972; Bartlett, 1972). Cavan, (1983, p. 408) says that although there was no notion of a 'Chicago School' in the 1920s

'I am sure other graduate students felt as I did - that the department of the University of Chicago was the place to study sociology. We had no doubt about the superiority of the department.... This feeling was shared by the faculty.'

This was backed up by the expanding output of publications from the department, headed by the monumental and enduring 'Polish Peasant', (Thomas and Znaniecki, 1918). Chicago sociologists produced some influential text books in those early years,
notably Small and Vincent (1894), Thomas (1909), and Park and Burgess (1921). The reputation was further sustained by graduates who left and encouraged students from their own institutions to take up a place at Chicago. (Anderson, 1983, Carey's interviewees, 1972). In addition there was a feeling within the department at Chicago that they were doing something different. Thomas (1983a, p. 389-390) quoted correspondence from Cavan which says that despite never hearing the term 'Chicago School' the 'students of the 1920s knew that their approach was "something special" and that they were perceived to be leaders in social analysis.' Similarly, Park (1939) suggested that there was something novel in the sociological approach at Chicago in the period around 1910-15 (before he arrived), that it was sociological and opposed to the moralistic, social problem orientation of the past.

3. This reflects the situation in the philosophy of science where historical material is used as a basis for supporting a given thesis about the generation of scientific knowledge. See, for example, Holton (1973), Howson, ed, (1976). This is also evident in the development of the sociology of science, for example, Barnes (1972), Mulkay (1972). It would seem, however, that when it comes to their own subject area, sociologists tend to be less scrupulous in examining the historical evidence and presume to know the subject.

4. While the final draft manuscript was being typed and reproduced, my attention was drawn to a new book by Martin Bulmer 'The Chicago School of Sociology: Institutionalization, Diversity and the Rise of Sociological Research' published by the University of Chicago Press in December 1984. Because of the timing, no account was taken of the contents of this book, apart from that which has already been published in the form of articles. However, an examination of the contents page would suggest that it seems to approximate the critical approach to units of knowledge production suggested here.
APPENDIX I
PERSONNELIN TIE DEPARTMENT
Or SOCIOLOGYAND ANTHROPOLOGY
AT THE UNIVERSITY OF CHICAGO1892-1963
Table la:

Sociology

Faculty

1892-1920 [11

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Small A. W
Henderson C. R
Talbot M
Thomas W.1
Vincent G
Zeublin C (6)
Bentley A. F
Raymond J. H
Taylor G.
Woodbeed H
Bedrord
S. W
Burgess E. W
Sutherland E. H
Handman M. S
Abbott E. [71
Rainwater C
Park R. E (81
Znaniecki F
Faris E

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Table 1bi Sociology

Faculty

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Bedford S. W
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Park R. E
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Faris E4444411111111111111
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Simpson E. N
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Shaw C
Wirth L777743333333322222222222
Blumer H9977741333333333333332222
222222222222211111112222
Ogburn W.E
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Webster E. J
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Cressey P. F
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S. A [101
Stouffer
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Sutherland E. H
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Rice S. A
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L. S
Cottrell
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Davis M.M
Hauser P777772222
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Johnson E. S
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Do Vinney L. C
07774446666
Lohman J. D
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Warner W.L [111
077777433
Shils E. A 1121
4444455555554
Hughes E. C
Bonner H06666
07777
Winch R. F (131
0094433
Whyte W.F
S. C
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Gilfillan
Williams J. j
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Swenson G.E
Goldhamer M3333
Horton D999
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Duncan 0.1)
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Reiss A. J
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Roy D. F
Shibutani T.
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Junker B.
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Smith H. L
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### Table 1: Sociology Faculty 1951-1963

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**Key to Tables 1, 4, 6 & 8**

1. Head of Department
2. Professor (inc. Distinguished Service Professor)
3. Associate Professor
4. Assistant Professor
5. Professorial Lecturer
6. Lecturer
7. Instructor
8. Docent
9. Assistant, Assistant Graduate Scholar, Associate or Research Associate
0. Fellow (only those included who became full members of staff of the Department of Sociology. Table 3 provides a full list of fellows)
E. Emeritus Professor
R. Retired

* Source: The Official Publications of the University of Chicago. These are issued annually and the information in these tables are collated from these publication.*
Notes to Table I

1. Research assistants who did some teaching but whose careers in the Department of Sociology at Chicago did not develop are not included in Table 1. These are listed in Table 2 up to 1932. Fellows of the Department are listed in Table 3.

2. Henderson was (associate) Professor of Divinity until 1904 before his appointment as head of the Department of Ecclesiastical Sociology. This became the Department of Practical Sociology in 1915.

3. Talbot was associate professor of Sanitary Science before moving to the newly created Department of Household Administration in 1904.

4. All mention of Thomas were erased from the Official Publications for 1917 prior to publication in the wake of the court case which lead to Thomas' enforced resignation, despite acquittal.

5. Diner (1980, p. 526) noted that Vincent became Dean of the junior college in 1900 and Dean of the Faculty of Arts, Literature and Science in 1907.

6. Zueblin did a lot of his teaching in the Extension Division (Diner, 1980, p. 526). The courses he taught, according to the Official Publications of the University of Chicago were: Elements and Structure of Society, Municipal Sociology and Structure of English Society.

7. Abbott taught part time. In 1908 she was the assistant director of the newly created School of Civics and Philanthropy. This became the School of Social Science Administration in 1920 at which time Abbott gave up her connection with the Sociology Department.

8. There is some difference among sources over the date of Park's inclusion in the Department of Sociology. Diner (1980) says that Park taught a course on 'The Negro' in Autumn of 1913. Matthews (1977) concurs. Raushenbush (1979) says that this was an address and that the first course given by Park was in June 1914. (He was originally located in the Divinity Department before being transferred to Sociology). Park actually wrote to Washington, June 1914, that he had twice as many students as last year. The Official Publications do not list Park on the Sociology Faculty in 1914, nor is there mention of a course by him in that year. In 1913 he is recorded as a professorial lecturer for the autumn. In 1915 he only taught in spring and summer quarters and in 1916 only during the summer quarter.

9. Shaw was an associate member of the Department from 1935 to 1942.

10. Stouffer was granted leave of absence for Government Service 1944 and 1945.

11. Warner is listed as 'Sociology and Anthropology Professor' in each year of his tenure except 1936, 1937, 1952, 1953.

12. Shils was granted leave of absence for Government Service 1943 and 1944.

13. Winch was granted leave of absence for Government Service in 1943 and for military service in 1944 and 1945.

14. These staff were also associate staff, see Table 6.

Table 2: Assistants in the Department of Sociology (and Anthropology) 1922-1932 [1]

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Notes to Table 2

1. These assistants are included in Table 1 only if their careers developed beyond 'assistant' in the sociology department at Chicago. Those included in Table 1 are marked by an asterisk. The Official Publications only listed assistants up to 1932.
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| 1907 | G. A. Stephens | |
| 1907 | A. H. Barron | |
| 1907-09 | L. L. Bernad | |
| 1908 | E. F. Fenton | *
| 1908 | A. R. Mead | |
| 1909 | R. B. McCord | |
| 1909 | E. F. Colburn | |
| 1909, 1910 | E. S. Bogardus | |
| 1910 | W. L. Chenery | |
| 1910, 1911 | A. M. Durand | |
| 1910 | D. I. Pope | |
| 1911, 1912 | E. H. Sutherland | *
| 1911 | A. N. Woodworth | *
| 1911, 1912 | R. F. Clark | *
| 1912 | S. A. Queen | *
| 1912 | W. T. Cross | *
| 1913, 1913 | R. W. Foley | *
| 1913 | V. W. Brooder | *
| 1913 | P. C. Coleman | *
| 1913, 1914 | E. E. Eubank | *
| 1913 | R. M. Leavell | *
| 1914 | H. M. Bacon | *
| 1914 | A. B. Lemstrom | *
| 1914, 1915 | E. B. Reuter | *
| 1915, 1916 | R. W. Stone | *
| 1916, 1917 | H. E. Jone | *
| 1916, 1917 | E. D. Sandersen | *
| 1917 | F. Thrasher | *
| 1918, 1919 | W. B. Bodenhafer | *
| 1918, 1919 | F. G. Daniel | *
| 1918, 1919 | J. Horak | *
| 1918, 1919 | A. C. Zurnbrunn | |
| 1920 | R. W. Nelson | |
| 1920 | K. San | |
| 1920 | L. M. Sapha | |
| 1920 | G. S. Rossow | |
| 1921 | V. M. Ames | |
| 1921 | K. E. Barnhart | |
| 1921 | E. R. Mowrer | |
| 1922 | A. C. Earl | |
| 1922 | H. Neuller | |
| 1922 | E. H. Shideler | |
| 1923 | M. H. Neumer | |
| 1924, 1925 | D. E. Poctar | |
| 1924 | M. W. Roper | |
| 1924 | T. C. Wang | |
| 1925 | G. E. Kessling | |
| 1925 | R. Fox | |
| 1925 | G. E. Breeze | |
| 1926, 1927 | C. D. Gower | |
| 1926, 1927 | E. T. Thompson | |
| 1926, 1927 | E. L. Remmel | |
| 1926, 1927 | W. T. Watson | |
| 1926, 1927 | C. M. Wu | |
| 1928, 1929 | D. C. Clark | |
| 1928-29 | E. H. Schettler | |
| 1928 | P. H. Neibit | |
| 1928, 1929 | E. V. Stonequait | |
| 1930, 1931 | R. E. Farris | |
| 1930, 1931 | C. C. Van Vechten | |
| 1930-32 | H. P. Hayes | |
| 1932 | C. H. Young | |
| 1932 | E. S. Johnson | |
| 1933 | F. E. Merrill | |
| 1933-35, 37 | L. C. De Vinney | *
| 1933 | R. H. Looman | *
| 1933, 1934 | R. D. Pierson | *
| 1934 | T. W. Gardlund | |
| 1935, 1936 | R. O. Lang | *
| 1935, 1936 | F. E. LaViolette | *
| 1936 | C. W. Hart | *
| 1936 | C. N. Elliot | *
| 1937 | E. A. Shia | *
| 1937 | B. L. Horman | *
| 1937, 1938 | L. M. Spencer | *
| 1938 | W. A. Hillman | *
| 1938, 1939 | H. D. Duncan | *
| 1938 | O. Hall | *
| 1939 | A. J. Jaffe | *
| 1939, 1940 | F. E. Moore | *
| 1939 | S. Provoas | *
| 1940 | P. Mann | *
| 1940 | H. Bonner | *
| 1940 | I. V. Toabe | *
| 1941, 1942 | E. Shanas | +*
| 1941, 1942 | W. F. Whyte | *
| 1941 | R. F. Winch | *
| 1941 | A. M. Roswell | *
| 1941 | R. Freedman | *
| 1941 | S. F. Miyamoto | *
| 1942 | E. Clark | *
| 1942 | F. Marcello | *
| 1942 | N. DeCampos | *

Note to Table 3

1. These fellows are included in Table 1 only if their careers developed beyond 'assistant' in the sociology department at Chicago. Those included in Table 1 are marked by an asterisk. Those fellows who became Associate staff are marked with a '+' and are included in Table 6.
### Table 4: Anthropology Staff in the Department of Sociology and Anthropology up to 1929

<table>
<thead>
<tr>
<th>Name</th>
<th>Position</th>
<th>Years</th>
</tr>
</thead>
<tbody>
<tr>
<td>Starr F.</td>
<td>Assistant Professor</td>
<td>1892-94; Associate</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Professor 1895-1922; Associate Prof. Retired 1923-28</td>
</tr>
<tr>
<td>West G.M.</td>
<td>Docent</td>
<td>1892-94</td>
</tr>
<tr>
<td>Miller M.L.</td>
<td>Assistant</td>
<td>1899-1900</td>
</tr>
<tr>
<td>Dorsey G.A.</td>
<td>Assistant Professor</td>
<td>1905-1914</td>
</tr>
<tr>
<td>Cole F.C.</td>
<td>Associate Professor</td>
<td>1924-1928</td>
</tr>
<tr>
<td>Supir E.</td>
<td>Associate Professor</td>
<td>1926-1927; Professor 1928</td>
</tr>
<tr>
<td>Wallis W.D.</td>
<td>Professor</td>
<td>1927</td>
</tr>
<tr>
<td>Redfield R.</td>
<td>Instructor</td>
<td>1927-1928</td>
</tr>
<tr>
<td>Spier L.</td>
<td>Professor</td>
<td>1928</td>
</tr>
</tbody>
</table>

### Table 5: Extension Staff 1892 - 1934

<table>
<thead>
<tr>
<th>Name</th>
<th>Position</th>
<th>Years</th>
</tr>
</thead>
<tbody>
<tr>
<td>Bemis E.W.</td>
<td>Associate Lecturer</td>
<td>1892 to 1894</td>
</tr>
<tr>
<td>Fulcomer D.</td>
<td>Lecturer in 1893</td>
<td>1893</td>
</tr>
<tr>
<td>Howerth I [1]</td>
<td>Associate 1894, Instructor 1898 to 1900, Assistant Professor 1901 to 1910</td>
<td></td>
</tr>
<tr>
<td>MacLean</td>
<td>Assistant Professor 1921 to 1934</td>
<td></td>
</tr>
<tr>
<td>McDowell H</td>
<td>Resident Head of the University Settlement 1892 to 1934</td>
<td></td>
</tr>
</tbody>
</table>

Note to Table 5

1. Diner (1980) maintained that all Howerth's teaching was in the Extension Division and in its special section for school teachers. The Official Publications do not make this clear after 1900.

### Table 6: Associate Members (from 1948)

#### Population Research & Training Centre
- Kittagawa E [1] 9 9 4 4 3 3 3 3
- Cuzzort R 7
- Duncan O.D. 3 3
- Taeuber E 4 4

#### National Opinion Research Centre
- Kurt C.W. [2] 3 2 2 2 2 2 3 2 2 2 2 R2 R2 R2
- Starr S.A. 9 9 9 3 3 3 3 3 3 3 3
- Marks E.S. 3 3
- Elison J 3 3
- Shanaa E 3 3 3 3 3 3
- Kriemberg L 3 3
- Rossi P [2] 2 2 2
- Feldman J 3 3
- Johnstone J.W. 3 3
- Miller N 3

#### Farm Study Center
- Litwak E 9 9 9

#### Community Studies Inc., Kansas City, Miss.
- Becker H.S. 4 4 4

#### Industrial Relations Center
- Nelson C 9 9 4 4 4 4
- Breen L 4 4

#### Frankfurt School
- Horkheimer P 3 2 2 2 2

Other Associates or Research Associates who did not develop career at Chicago up to 1963
- Beller B.N 9 9
- Rosenthal 9 9
- Farber B 4 4
- Wohl R 9 9 3 3
- Zeiser H 2 2 2 2 2 2 2 2 2 2
- Chin L.E. 9 9 9
- Mayeroshon R 4 4 4
- James R.M. 4 4
- Moore J.W. 4 4
- Ennis P.H. 4 4 4 4

<table>
<thead>
<tr>
<th>1948 49 50 51 52 53 54 55 56 57 58 59 60 61 62 63</th>
</tr>
</thead>
<tbody>
<tr>
<td>1. Kittagawa was linked to the Chicago Community Inventory in 1955 &amp; 1956.</td>
</tr>
<tr>
<td>2. Director of The National Opinion Research Centre.</td>
</tr>
</tbody>
</table>

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### Table 7: Staff in Other Departments Listed in the Official Publications of the Department of Sociology

<table>
<thead>
<tr>
<th>Name</th>
<th>Position</th>
<th>Years</th>
</tr>
</thead>
<tbody>
<tr>
<td>Puttkamer E.</td>
<td>Professor of Law</td>
<td>1933-53</td>
</tr>
<tr>
<td>Rheinstein N.</td>
<td>Professor of C.</td>
<td>1943-53</td>
</tr>
<tr>
<td>Kincheloe S.</td>
<td>Professor of R.</td>
<td>1947-55</td>
</tr>
<tr>
<td>Sherman H.</td>
<td>Professor of E.P.</td>
<td>1948-1951</td>
</tr>
<tr>
<td>Campbell D.</td>
<td>Associate Professor in Psychology</td>
<td>1951-53</td>
</tr>
<tr>
<td>Barton A. H.</td>
<td>Associate Professor in Law and Sociology</td>
<td>1953-54</td>
</tr>
<tr>
<td>Strodbeck F.</td>
<td>Associate Professor in Law and Sociology, 1954-63</td>
<td></td>
</tr>
</tbody>
</table>

### Table 8: Visiting Lecturers [1]

<table>
<thead>
<tr>
<th>Year</th>
<th>Visiting Lecturer</th>
<th>Institution</th>
<th>Position</th>
</tr>
</thead>
<tbody>
<tr>
<td>1895</td>
<td>E.A Rose</td>
<td>Leyland Stanford Univ.</td>
<td>2</td>
</tr>
<tr>
<td>1896</td>
<td>L. Ward</td>
<td>Smithsonian Institute</td>
<td>4</td>
</tr>
<tr>
<td>1915</td>
<td>J.H Raymond</td>
<td>New York City College</td>
<td>4</td>
</tr>
<tr>
<td>1916</td>
<td>E.L Holton</td>
<td>Kansas State Agricultural College</td>
<td>2</td>
</tr>
<tr>
<td>1917</td>
<td>G.C Howard</td>
<td>Nebraska Univ.</td>
<td>1</td>
</tr>
<tr>
<td>1918</td>
<td>E.C Hayes</td>
<td>Univ. of Illinois</td>
<td>1</td>
</tr>
<tr>
<td>1919</td>
<td>E.C Hayen</td>
<td>Univ. of Illinois</td>
<td>1</td>
</tr>
<tr>
<td>1920</td>
<td>C.A Ellwood</td>
<td>Univ. of Missouri</td>
<td>1</td>
</tr>
<tr>
<td>1920</td>
<td>W.B Bodonhofer</td>
<td>Univ. of Washington</td>
<td>2</td>
</tr>
<tr>
<td>1920</td>
<td>M.S Thompson</td>
<td>Cornell Univ.</td>
<td>3</td>
</tr>
<tr>
<td>1922</td>
<td>L.L Benard</td>
<td>Univ. of Minnesota</td>
<td>2</td>
</tr>
<tr>
<td>1923</td>
<td>L.L Benard</td>
<td>Univ. of Minnesota</td>
<td>2</td>
</tr>
<tr>
<td>1924</td>
<td>L.L Benard</td>
<td>Univ. of Minnesota</td>
<td>2</td>
</tr>
<tr>
<td>1925</td>
<td>R. McKenzie</td>
<td>Univ. of Washington</td>
<td>3</td>
</tr>
<tr>
<td>1927</td>
<td>E.E Eubank</td>
<td>Univ. of Cincinnati</td>
<td>2</td>
</tr>
<tr>
<td>1927</td>
<td>L.L Benard</td>
<td>Tulane Univ.</td>
<td>5</td>
</tr>
<tr>
<td>1928</td>
<td>F.N House</td>
<td>Univ. of Virginia</td>
<td>2</td>
</tr>
<tr>
<td>1929</td>
<td>L. Wirth</td>
<td>Tulane Univ.</td>
<td>3</td>
</tr>
<tr>
<td>1929</td>
<td>K. Young</td>
<td>Univ. of Wisconsin</td>
<td>3</td>
</tr>
<tr>
<td>1930</td>
<td>R. McKenzie</td>
<td>Univ. of Washington</td>
<td>2</td>
</tr>
<tr>
<td>1930</td>
<td>R. McKenzie</td>
<td>Univ. of Washington</td>
<td>2</td>
</tr>
<tr>
<td>1930</td>
<td>E.F Young</td>
<td>Univ. of California</td>
<td>2</td>
</tr>
<tr>
<td>1930</td>
<td>T.C McCormick</td>
<td>Univ. of Oklahoma</td>
<td>2</td>
</tr>
<tr>
<td>1931</td>
<td>E.N Simpson</td>
<td>Univ. of Virginia</td>
<td>2</td>
</tr>
<tr>
<td>1931</td>
<td>E.C Hughes</td>
<td>McGill University</td>
<td>2</td>
</tr>
<tr>
<td>1937</td>
<td>T. Paraona</td>
<td>Harvard Univ.</td>
<td>4</td>
</tr>
<tr>
<td>1939</td>
<td>H.J Locke</td>
<td>Indiana Univ.</td>
<td>4</td>
</tr>
<tr>
<td>1941</td>
<td>O. Hall</td>
<td>Brown Univ.</td>
<td>7</td>
</tr>
<tr>
<td>1947</td>
<td>S. Kimball</td>
<td>Univ. of Michigan</td>
<td>3</td>
</tr>
<tr>
<td>1947</td>
<td>S. Reiser</td>
<td>Univ. of Wisconsin</td>
<td>3</td>
</tr>
<tr>
<td>1949</td>
<td>P. Lazarsfeld</td>
<td>Columbia Univ.</td>
<td>2</td>
</tr>
<tr>
<td>1959</td>
<td>K.M Kapetia</td>
<td>Univ. of Bombay</td>
<td>4</td>
</tr>
<tr>
<td>1960</td>
<td>J.D Short</td>
<td>Washington State College</td>
<td>3</td>
</tr>
<tr>
<td>1960</td>
<td>S. Eisoneladt</td>
<td>Hebrew University</td>
<td>2</td>
</tr>
<tr>
<td>1961</td>
<td>J.D Short</td>
<td>Washington State College</td>
<td>3</td>
</tr>
<tr>
<td>1961</td>
<td>A.L Stinchcombe</td>
<td>Johns Hopkins University</td>
<td>4</td>
</tr>
</tbody>
</table>

**Note to Table 8**

1. Only those visiting lecturers recorded in the Official Publications are listed. Visiting lecturers taught in the Summer quarter except: * spring and summer quarters, ** autumn and winter quarters.
APPENDIX TWO [1]

THE LOCAL COMMUNITY RESEARCH PUBLICATIONS 1923-1929

A. Books and monographs published under the auspices of the Local Community Research Committee fall into various headings:

1. University of Chicago Social Services Series
   - Abbott, E., 1926
   - Breckenridge, S.P., 1924
   - Breckenridge, S.P., 1927

2. Social Service Monographs (numbered)
   - Beeley, A., 1927 (No.1)
   - Breckenridge, 1928 (No.3)
   - Hathway, N., 1928 (No.4)
   - Hughes, E.A. & Stuenkel, 1929 (No.8)
   - Ladewick, E., 1929, (No.7)
   - Nims, E., 1928 (No.2)

3. Materials for the Study of Business
   - Baker, N.F., 1927
   - Montgomery, R.E., 1927
   - Schultz, H., 1928
   - Warne, C.E., 1926
   - Wolf, H.D., 1927

4. Social Science Studies (numbered)
   - Studies in geography:
     - Duddy, E.A., 1929 (No.15)
     - Fryxell, F.M., 1927 (No.5)
     - Goode, J.P., 1926 (No.3)
   - Studies in politics:
     - Byrle, H.C., 1928 (No. 10)
     - Gosnell, H.F., 1927 (No.4)
     - Johnson, C.O., 1928 (No.11)
     - Herries, C.E., & Gosnell, H.F., 1924 (No.1)
     - White, L.D., 1927 (No.9)
     - White, L.D., 1929 (No.14)
     - Woody, C.H., 1926, (No.2)
   - Studies on the census:
     - Jeter, H.R., 1927, (No.7)
   - Studies in history:
     - Beckner, E.R., 1929 (No. 13)
   - Studies in sociology:
     - Palmer, V., 1928, (No.12)
   - Studies on welfare:
     - Chicago Civic Agencies, 1927 (No.6)
   - Studies in economics:
     - Houghteling, L., 1927 (No.8)
     - Staley, E.A., in press (No.16)

5. University of Chicago Sociological Series
   - Mowrer, E.R., 1927
   - Wirth, L., 1923
   - Zorbaugh, H.W., 1929

   - Park R.E and Burgess, E.W., 1925
   - Thrasher, F.M., 1927

7. Studies in the Packing Industry [3].
   - Rhoades, E.L., 1929a
   - Rhoades, E.L., 1929b
   - Rhoades, E.L., 1929c
In addition the following publications were not included under a series banner:

Abbott, E., 1924
Region of Chicago Base Map, 1926

All the above were published by University of Chicago Press

The following were published elsewhere:

Williams, D., & Skinner, M.E., (undated)
Millis, S., (undated)
Park, R.E., contribution to Gee, ed, (in Press)
Bruce A.A. et al, 1928
White, L.D., 1925

B. Journal Articles

In addition there are a large number of journal articles listed.

Sociologists with articles in the list are
E.W. Burgess (8 articles)
F.E. Frazier (2 articles)

Other major publishers
S.E. Leland (7 articles all in National Real Estate Journal)
L.L. Thurstone (13 articles)

Location of articles

Africa
American Economic Review
American Elevator and Grain Trade
American Journal of Psychiatry
American Journal of Psychology
American Journal of Sociology 3 [4]
Annals of American Academy of Politics and Social Science 2
Annals of American Sociological Society
Distribution and Warehousing
Educational Record 2
International Journal of Ethics
Journal of Abnormal and Social Psychology
Journal of Educational Psychology
Journal of Experimental Psychology 2
Journal of General Psychology
Journal of the American Statistical Association 3
National Real Estate Journal 7
Proceedings of the National Conference of Social Work 2
Psychological Review 3
Religious Education
Social Forces 3
Social Service Review 2

Of the list of research studies completed but not published as of 1929, seventeen could be described as sociological which is less than those on welfare or political science. From the titles and personnel the following breakdown: (5)

23 economics
18 welfare
17 sociology
9 politics
6 union/labour relations
2 anthropology
1 history
1 psychology
1 census/population trends

Sociology research in progress: (1929)

Conway, MA, 1926
Cressey, MA, 1929
Glick, MA, 1928
Hayner, Ph.D., 1923
Lieffer MA, 1928

plus work referred to as follows

Ogburn, W.F., Fertility According to Occupations and Social Classes.
Ogburn, W.F., Ranking of Different Influences in the Last Presidential Election
Ogburn, W.F., Variability in Birth Rates in Different Civilizations
Shaw, Clifford, Juvenile Delinquency
Stephen, F.F., Public Recreations in Chicago
Tibbits, R.C., Immigrant Groups in Chicago
Tibbits, R.C., Social Forces and Trends in Settlement Neighborhoods.
LOCAL COMMUNITY RESEARCH COMMITTEE MATCHED FUND AGENCIES: 1923-1929

Official sources:
- City of Chicago
- Smithsonian Institution
- U.S. Children's Bureau

Foundations and institutes:
- Wiebolt Foundation
- Rosenwald Foundation
- Commonwealth Fund
- Helen Critenden Memorial
- Institute of Economics
- Chicago Historical Society
- American Institute of Criminal Law and Criminology

Clubs:
- City Club of Chicago
- Commonwealth Club
- Union League Club
- Chicago Woman's Club
- Rotary Club
- National League of Women Voters

Special Interest Groups:
- Chicago Real Estate Board
- Chicago Heart Association
- Chicago Urban League
- Chicago Council of Social Agencies
- Chicago Foundlings Home
- Evangelical Orphanage and Old People's Home
- Federation of Settlements
- Henry Booth Settlement House
- Child Guidance Centers
- Northwestern University Settlement
- Chicago Commons Association
- Chicago Immigrant Protective League
- American Home Economics Association
- Illinois Association for Criminal Justice
- Illinois State Federation of Labor
- Joint Service Bureau
- Scholarship Association For Jewish Children
- Association of Community Chests and Councils
- International Advertising Association
- Institute of Meat Packing
- Lower North Child Guidance Center

In addition various donations by individuals were made towards matched fund research.

FOOTNOTES TO APPENDIX TWO

1. Source: Smith & White, 1929.

2. There is no record of Numbers 5 and 6.

3. These were short monographs less than 35 pages.

4. The contributors from the Local Community Research Committee to the American Journal of Sociology were Thurstone, Gosnell, and Douglas. None of these were sociologists.

5. One study unclassified, viz. Brown, E., 'Chicago Typothetae'.
APPENDIX 3

THE SOCIETY FOR SOCIAL RESEARCH AT THE UNIVERSITY OF CHICAGO


The following is a copy of a document from the University of Chicago, Regenstein Library, Special Collections Department, Wirth Paper. It is undated but is probably the original constitution of the Society with two amendments appended.

THE SOCIETY FOR SOCIAL RESEARCH

The University of Chicago

Constitution

1. The name of this organization shall be the "The Society for Social Research of the University of Chicago".

2. The purpose of the Society is to bring about the cooperation of persons engaged in social research and social investigation.

3. In order to stimulate and promote efficiency in research and investigation among its members, the Society will develop the following activities:

   a. The permanent registration of its members.

   b. A clearing house of investigation and research, to assemble bibliographies, to collect pamphlet literature, and to formulate methods.

   c. The organization of an advisory committee to provide service, in the supervision of research, to the members of the Society, and to promote the publications of standard works in research and investigation.

4. Membership shall be open to graduate students in the Department of Sociology, and to other persons who have attained professional standards of research approved by the executive committee.

5. The initiation fee is one dollar. The payment of the initiation fee and the annual dues of one dollar shall entitle a member to the services of the Society.

6. The secretary-treasurer of the Society shall, under the suggestion and direction of the executive committee, organize and conduct the activities of the Society. He shall be appointed by the executive committee.

7. The official year of the Society is October 1 to September 30, inclusive.

8. The Executive Committee of the Society is composed of the President, Vice-President and Secretary-Treasurer of the Society, and two other members appointed by the President.

9. Members shall be elected by the Executive Committee upon written application by the candidate upon the official blanks to be secured from the Secretary-Treasurer.

10. The regular meetings of the Executive Committee shall be held each Tuesday before the regular meeting of the Society.

11. The President of the Society shall be elected by the members, at the last meeting of the Summer Quarter.

12. The Trustees of the Society shall be composed of the members of the Executive Committee, with two additional members elected annually by the Society.

13. Amendments to the Constitution may be made by a two thirds vote of the members present and voting at a regular meeting, providing that the amendment shall have been submitted in writing at the previous regular meeting.

14. A quorum shall consist of one third of the resident members.

Amendment I.

The trustees of the Society shall be composed of the members of the Executive Committee, with the Dean of the Graduate School of Arts and Literature, who shall serve ex officio.

Minutes of the meeting of the Exec. Comm. on Dec. 9, 1925, of the Society, Dec. 9, 1925, and of the society, Jan. 14, 1926, record the presentation and adoption of this amendment:

'Any individual eligible for membership in the Society may be elected to honorary membership by the Executive Committee'.

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A later Constitution, again undated incorporated the amendments, gave more power to act to the Executive Committee and made membership more open. The following is a copy of a document also located in the Wirth Papers.

THE SOCIETY FOR SOCIAL RESEARCH
The University of Chicago

CONSTITUTION

1. The name of this organization shall be the "The Society for Social Research of the University of Chicago".

2. The purpose of the Society is to bring about the co-operation of persons engaged in social research and social investigation. The Society will endeavor to develop such activities as will stimulate and promote efficiency in research and investigation among its members.

3. Membership shall be open to faculty members in the Social Sciences, graduate students in the Social Sciences who are engaged in a program of research approved by a member of the faculty, and to other persons who have attained professional standards of research approved by the Executive Committee.

4. Members shall be elected by the Executive Committee upon written application by the candidate upon the official blanks to be secured from the Secretary-Treasurer.

5. The Executive Committee shall be empowered to elect honorary members.

6. The initiation fee is one dollar. The payment of the initiation fee and the annual dues of one dollar shall entitle a member to the services of the Society.

7. The following officers of the Society shall be elected by the members for a period of one year at the annual business meeting at the Summer Institute: the president, vice president, executive secretary, secretary treasurer, and two editors of the Bulletin.

8. The executive secretary of the Society shall, under the suggestion and direction of the executive committee, organize and conduct the activities of the Society.

9. The Executive Committee of the Society shall be composed of the six elective officers and two other members to be appointed by the president from the Social Science faculty for terms of two years which shall terminate in alternating years. The powers of the Executive Committee shall be those usually exercised by executive committees and in addition those specifically provided in this constitution.

10. Meetings of the Executive Committee shall be at the call of the president or executive secretary.

11. The official year of the Society is October 1 to September 30, inclusive.

12. There shall be an annual audit of the books of the Society under the direction of the Chairman of the Department of Sociology. This audit shall be reported at the annual business meeting of the Society at the Summer Institute.

13. Amendments to the Constitution may be made by a two thirds vote of the members present and voting at a regular meeting, providing that the amendment shall have been submitted in writing at the previous regular meeting and providing that announcement of the intention to propose amendments and to act upon amendments shall be made on the respective regular meeting notices sent to all resident members.

14. A quorum shall consist of fifteen members.
The following list of members is compiled from various sources and may not be comprehensive. The sources used were a typed list headed 'Dues Received 1925-1926', those listed are indicated in column one of the table; The Bulletin of the Society for Social Research of Dec 1926, those listed are noted in column 2; The Bulletin of the Society for Social Research, Jan 1928 (col 3). A list of members from the files of the Society for Social Research, undated, but which, on the basis of the location of members was almost certainly drawn up in 1932 (col 4); a similar list, also undated, but again almost certainly from 1935 (col 5). Columns 6 and 7 provide an indication of the 'active' members of the Society, as these refer to respondents to questionnaires sent out [1] and, in the case of the last column, indicate those members who were referred to in the December issue of the Bulletin, the latter being indicated by the letter M. The date in brackets is the date of joining the Society. Where no date is given, the individual was a member before 1925. Where the year only is given, this is the best estimate available for year of joining. Where the month and year is given this is accurate to within a month either way.

<table>
<thead>
<tr>
<th>Name</th>
<th>Dues</th>
<th>BSSR</th>
<th>Memb</th>
<th>BSSR</th>
<th>Memb</th>
<th>BSSR</th>
<th>BSSR</th>
</tr>
</thead>
<tbody>
<tr>
<td>A:</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>D. Achtenberg (Oct 1935)</td>
<td>I</td>
<td>I</td>
<td>I</td>
<td>I</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>E. A. Ahrens (7.11.27)</td>
<td>0</td>
<td>0</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>C. O. Anderson (Mar 1933)</td>
<td>I</td>
<td>I</td>
<td></td>
<td></td>
<td></td>
<td></td>
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395
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<td>C.R. Simpson</td>
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<td>C.W. Smith</td>
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<td>R.C. Smith</td>
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<td>A.W. Smull</td>
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<td>L.S. Smythe</td>
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<td>H.M. Snyder</td>
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<td>S.K. Weinberg</td>
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<td>F.L. Weller</td>
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<td>L.V. Whitney</td>
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<td>L. Wirth</td>
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<td>H.G. Woolbert</td>
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Key:
I = In Residence or local
O = Out of town
F = Recorded as faculty member or as having address at faculty exchange
* Membership date not located but likely between 1930-35 (and prob 1934)
** Membership date not located but likely to have been during 1935
*** No record of the date of joining could be located, these members just appeared on the 1932 list. It is likely they joined in that year.

Notes to membership list

1. In Dec 1934 the Bulletin reported the result of a questionnaire sent to members. 234 letters sent out (92) 44% returned with Society for Social Research membership dues, 12 discontinued membership, 7 returned as having been sent to the wrong address and 123 non-responses. Column 6 shows those respondents who indicated their research and whom the Bulletin included in its summary. On Nov 1st 1935 a further questionnaire sent to 239 members, 97 (45.6%) replied and paid, 5 to pay in Dec., 7 discontinued, 3 incorrect address, 127 non responses. 56 of replies indicated research. A number of letters sent abroad: 5 China, 4 Hawaii, 3 Canada, 2 Japan, 1 Brazil, Czechoslovakia, France, Mexico, Russia, Switzerland. Column 7 shows those members of the Society who responded to the questionnaire and whose work was included in the summary of the responses in the Bulletin. Other members mentioned in the December 1935 issue of the Bulletin are indicated by an M.

The reports of 152 meetings of the SSR between 1924 and 1935 were analysed. Some of these meetings were business meetings and some were report backs on the American Sociological Society Conference. A total of 140 addresses were presented to the meetings of the Society on which information was available in the period 1924-1935.

### Table 1: Year of address by speaker's auspices

<table>
<thead>
<tr>
<th>Auspices</th>
<th>Years</th>
<th>1924-26</th>
<th>1927-29</th>
<th>1930-32</th>
<th>1933-35</th>
<th>All</th>
</tr>
</thead>
<tbody>
<tr>
<td>Sociology Faculty</td>
<td>6 (17)</td>
<td>5 (19)</td>
<td>9 (26)</td>
<td>13 (36)</td>
<td>33 (24)</td>
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<td>Sociology Students</td>
<td>13 (37)</td>
<td>3 (11)</td>
<td>5 (15)</td>
<td>2 ( 6)</td>
<td>23 (17)</td>
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<tr>
<td>Other U.C. Faculty</td>
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<td>12 (44)</td>
<td>13 (34)</td>
<td>12 (33)</td>
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<td>3 (11)</td>
<td>5 (15)</td>
<td>6 (17)</td>
<td>20 (15)</td>
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<tr>
<td>Outside non-academics</td>
<td>4 (11)</td>
<td>4 (15)</td>
<td>6 (16)</td>
<td>3 ( 8)</td>
<td>17 (13)</td>
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<tr>
<td><strong>Totals</strong></td>
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<td><strong>27</strong></td>
<td><strong>38</strong></td>
<td><strong>36</strong></td>
<td><strong>136</strong></td>
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Figures in brackets are column percentages rounded to nearest whole number. Four missing values.

### Table 2: Year of address by members of the Society

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<th>Membership</th>
<th>Years</th>
<th>1924-26</th>
<th>1927-29</th>
<th>1930-32</th>
<th>1933-35</th>
<th>All</th>
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<tr>
<td>Members</td>
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<td>12 (43)</td>
<td>17 (45)</td>
<td>20 (56)</td>
<td>74 (53)</td>
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<td>16 (57)</td>
<td>21 (55)</td>
<td>16 (44)</td>
<td>66 (47)</td>
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<tr>
<td><strong>Totals</strong></td>
<td><strong>38</strong></td>
<td><strong>28</strong></td>
<td><strong>38</strong></td>
<td><strong>36</strong></td>
<td><strong>140</strong></td>
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</table>

Figures in brackets are column percentages rounded to nearest whole number.

### Table 3: Year of address by major concern(s) of address

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<th>Major Concern(s)</th>
<th>Years</th>
<th>1924-26</th>
<th>1927-29</th>
<th>1930-32</th>
<th>1933-35</th>
<th>All</th>
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<td>14 (52)</td>
<td>12 (32)</td>
<td>16 (44)</td>
<td>40 (29)</td>
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<td>13 (48)</td>
<td>12 (32)</td>
<td>15 (42)</td>
<td>45 (33)</td>
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<tr>
<td>Substantive Issues</td>
<td>15 (42)</td>
<td>4 (15)</td>
<td>17 (46)</td>
<td>15 (42)</td>
<td>51 (38)</td>
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<td>5 (14)</td>
<td>1 ( 3)</td>
<td>19 (14)</td>
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<tr>
<td><strong>Number of addresses</strong></td>
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<td><strong>27</strong></td>
<td><strong>37</strong></td>
<td><strong>36</strong></td>
<td><strong>136</strong></td>
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</table>

Figures in brackets are percentages of all meetings in each three year period that were addressed by speakers and on which information is available rounded to nearest whole number. Note that as some addresses covered more than one area the column percentages do not add up to 100%.

### Table 4: Year of address by discipline area of address

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<th>Discipline Area</th>
<th>Years</th>
<th>1924-26</th>
<th>1927-29</th>
<th>1930-32</th>
<th>1933-35</th>
<th>All</th>
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<td>13 (48)</td>
<td>23 (62)</td>
<td>24 (67)</td>
<td>81 (60)</td>
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<td>Welfare &amp; Reform</td>
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<td>3 ( 8)</td>
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<td>9 (33)</td>
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<td>1 ( 3)</td>
<td>16 (12)</td>
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<td>6 (22)</td>
<td>13 (35)</td>
<td>11 (31)</td>
<td>32 (24)</td>
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<tr>
<td><strong>Number of addresses</strong></td>
<td><strong>36</strong></td>
<td><strong>27</strong></td>
<td><strong>37</strong></td>
<td><strong>36</strong></td>
<td><strong>136</strong></td>
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</tbody>
</table>

Figures in brackets are percentages of all meetings in each three year period that were addressed by speakers and on which information is available rounded to nearest whole number. Note that as some addresses covered more than one area the column percentages do not add up to 100%.

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Table 5: Year of address by focus on a study of Chicago in address

<table>
<thead>
<tr>
<th>Chicago</th>
<th>Years</th>
<th>1924-26</th>
<th>1927-29</th>
<th>1930-32</th>
<th>1933-35</th>
<th>All</th>
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</thead>
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Table 6: Auspices by major area(s) of concern

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<th>Theory</th>
<th>Method</th>
<th>Subst.</th>
<th>Practical</th>
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<td>15 (28)</td>
<td>17 (33)</td>
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<td>11 (20)</td>
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<tr>
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Table 7: Auspices by discipline area

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<td>1 (7)</td>
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<td>3 (21)</td>
<td>9 (64)</td>
<td>22 (69)</td>
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<td>3 (21)</td>
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<tr>
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Table 8: Auspices by focus on a study of Chicago in address

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<th>Pre 1930</th>
<th>1930-1935</th>
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<td>5 (63)</td>
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399
Table 9: Membership of the Society by major area(s) of concern

<table>
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<tr>
<th>Membership</th>
<th>Area of Concern</th>
<th>Theory</th>
<th>Method</th>
<th>Subst.</th>
<th>Practical</th>
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<tr>
<td></td>
<td>Theory</td>
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<td>36 (65)</td>
<td>34 (67)</td>
<td>6 (30)</td>
</tr>
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<td></td>
<td>Pre 1930</td>
<td></td>
<td></td>
<td></td>
<td></td>
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<td></td>
<td>Members</td>
<td>14 (64)</td>
<td>20 (71)</td>
<td>15 (79)</td>
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<td>8 (29)</td>
<td>4 (21)</td>
<td>9 (64)</td>
</tr>
<tr>
<td>Total</td>
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<td>55</td>
<td>51</td>
<td>20</td>
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Figures in brackets are column percentages rounded to nearest whole number.

Table 10: Membership of Society by discipline area

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<th>Discipline area</th>
<th>Soc. (1)</th>
<th>Welfare</th>
<th>Psych.</th>
<th>Soc. Sci (2)</th>
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<td>5 (31)</td>
<td>9 (28)</td>
</tr>
<tr>
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<td>11 (69)</td>
<td>11 (69)</td>
<td>23 (72)</td>
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<td>81</td>
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Figures in brackets are column percentages rounded to nearest whole number.

Table 11: Membership by focus on a study of Chicago in address

<table>
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<th>Membership</th>
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<th>No</th>
<th>Total</th>
<th>Yes</th>
<th>No</th>
<th>Total</th>
<th>Yes</th>
<th>No</th>
<th>Total</th>
</tr>
</thead>
<tbody>
<tr>
<td>Members</td>
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<td>14</td>
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<td>68</td>
<td>8</td>
<td>31</td>
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<td>14</td>
<td>36</td>
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<tr>
<td>Non-members</td>
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<td>76</td>
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</table>

Figures in brackets are row percentages rounded to nearest whole number.

Notes to tables

1. Sociology and population studies
2. General social science, philosophy, anthropology, economics, politics and public administration.
3. These totals do not correspond exactly to those in Table 3 due to missing values in the auspices.
4. These totals do not correspond exactly to those in Table 4 due to missing values in the auspices.
# APPENDIX FOUR

## COURSES OFFERED IN SOCIOLOGY BY THE DEPARTMENT OF SOCIOLOGY AND ANTHROPOLOGY

Table 1: Courses offered 1913-1925

<table>
<thead>
<tr>
<th>Course Number[2]</th>
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<th>Lecturer</th>
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<td>3 (230)</td>
<td>1913 1924</td>
<td>SocialOrigins</td>
<td>Burgess 1917-19</td>
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<td>4 (240)</td>
<td>1923 1924</td>
<td>Social Evolution and Social Organization</td>
<td>Faris 1920-24</td>
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<td>6 (260)</td>
<td>1914 1924</td>
<td>Modern Cities [9]</td>
<td>Burgess 1917-24</td>
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<td>7 (270)</td>
<td>1916 1924</td>
<td>Social Pathology</td>
<td>Burgess 1918-20</td>
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<td>11 (351)</td>
<td>1921 1924</td>
<td>The Family</td>
<td>Burgess 1919-24</td>
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<td>14 (310)</td>
<td>1922 1924</td>
<td>Study of Society</td>
<td>Burgess 1920 &amp; 1924</td>
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<tr>
<td>15</td>
<td>1913 1924</td>
<td>Elements of General Sociology</td>
<td>Park 1920-24</td>
</tr>
<tr>
<td>16</td>
<td>1914 1916</td>
<td>History of Sociology from the Beginning of 19th Cent.</td>
<td>Bodenhofer 1921</td>
</tr>
<tr>
<td>16A (316)</td>
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<td>History of Sociology from the Beginning of 19th Cent.</td>
<td>Bernard 1922-23</td>
</tr>
<tr>
<td>16B (317)</td>
<td>1915 1924</td>
<td>History of Sociology in the United States from 1865</td>
<td>Small 1914-24</td>
</tr>
<tr>
<td>17 (312)</td>
<td>1917 1924</td>
<td>Conflict of Classes in Modern Society</td>
<td>Small 1914-24</td>
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<td>18</td>
<td>1913 1924</td>
<td>Ethics of Sociology</td>
<td>Small 1914-24</td>
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<td>1913 1915</td>
<td>Development of Sociology in Germany Since 1870</td>
<td>Small 1914-24</td>
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<td>19A</td>
<td>1921 1923</td>
<td>General Sociology</td>
<td>Small 1914-24</td>
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<td>1915 1915</td>
<td>The Growth of Militarism in Germany</td>
<td>Bodenhofer 1921</td>
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<td>Bernard 1922 &amp; 1924</td>
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<td>1922 1924</td>
<td>History of Sociology</td>
<td>Small 1914-24</td>
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<td>Social Forces in Modern Democracy: United States</td>
<td>Small 1914-24</td>
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<td>1914 1924</td>
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<td>Social Forces in Modern Democracy: France and Germany</td>
<td>Small 1914-24</td>
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<td>1916 1918</td>
<td>Seminar: Problems in General Sociology</td>
<td>Small 1914-24</td>
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<td>Conflict and Fusion of Cultures</td>
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<td>The Oriental</td>
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<td>1918</td>
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<td>Research Problems in Social Psychology</td>
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<td>Causes and Prevention of Poverty</td>
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<td>1918</td>
<td>The Playground Movement</td>
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<td>1924</td>
<td>Teaching of Sociology in Colleges</td>
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The following courses were listed in 1914 with a course number as indicated. They were courses in other departments which, from 1915 to 1924, were only appended to the Official Publications of the Sociology and Anthropology Department as 'brought to the attention' of students. In some cases, these same courses had been 'brought to the attention' of students prior to 1914. These courses are indicated

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Notes to Table 1

1. Source: Official Publications of the University of Chicago. Courses in the Department of Sociology and Anthropology which were specifically Anthropology are not included.

2. All course were numbered. Courses with a given number were replaced from time to time, hence a several titles for the same course number. The whole system was revised in 1925 and those courses retained given a number in the new system, these numbers are recorded in square brackets.

3. All dates refer to academic year commencing August

4. Courses dated 1913 usually began before that date

5. The course structure was revised in 1925 and a new numbering scheme devised. Courses noted as ending in 1924 continue after that date if a new course number is indicated. See Table 2 for the period 1925-52

6. This course is listed as 'elementary' in 1925, and probably was so before that date.

7. Course 2 to 8 inclusive are listed as 'intermediate' in 1925

8. See Course 11 in 1921

9. See Course 50 in 1918. In 1915 this course subtitled 'Municipal Sociology'

10. NG indicates that although the course was listed it was not apparently given during these years. In some cases a lecturer is specified although the course was not taught. These are usually shown in the listing of each course.

11. NAT indicates that although the course is listed between 1914 and 1925 it was never actually taught as far as can be ascertained from the records

12. This course was sometimes referred to as 'The Social Survey'

13. This course was subtitled 'Introduction to Collective Behavior' from 1918-24

14. See Courses 76-78 after 1919

15. Matthews was in the Divinity School

16. Social Science Administration course no. 20

17. Social Science Administration course no. 21

18. ND indicates that the course was not offered in these years (which lie between the first and last dates the course was listed)

19. Social Science Administration course no. 6

20. Course titles in the Official Publications of the sponsoring department vary slightly in some cases.
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Key: AN Department of Anthropology
DV School of Divinity
ED Department of Education
PY Department of Psychology
ZO Department of Zoology

Notes To Table 2

1. Where the course is not given and no lecturer is specified, then no entry is usually made in the list of lecturers in this table. Where the course is apparently given but no lecturer is recorded in the Official Publications, then an entry of 'Unspecified' will be made.

2. NO indicates that the course was not offered in these years (which lie between the first and last dates the course was listed).

3. From 1926-1930 this was course number 101

4. Courses with final dates of 1952 usually continued after that time, the survey only covered the period up to 1952.

5. This course was entitled 'Introduction to the Study of Society' up to 1931 and 'Introduction to the Study of Sociology' from 1932 to 1937.

6. NG indicates that although the course was listed it was not apparently given during these years. In some cases a lecturer is specified although the course was not taught. These are usually shown in the listing of each course.

7. Course number 310 from 1925-1943

8. This course was not offered in 1946. From 1947 onwards it was number 369

9. This course was numbered 326 before 1937. From 1934 it was entitled 'Collective Behavior'.

10. This course was entitled 'Social Origins and Social Institutions' from 1937

11. This course was number 351 until 1943

12. This course numbered 364 after 1934

13. This course was number 365 from 1940-42 and 1949-50

14. In 1943 this course was renumbered 321.

15. This course was entitled 'Sampling' in 1935

16. This course was identical to Statistics 303, Business 323 and Economics 316

17. Mass was in the Education department, this course was identical to Education 311B & Home Economics 336
18. Course number 457 from 1948-49

18b. This course entitled 'French Sociology' from 1939.

19. This course referred to as 'Seminar in Quantitative Studies in Social Psychology' from 1940

20. This course changed number to 323 in 1943

21. This course changed number to 324 in 1945 and to number 424 in 1948.

22. This course was entitled 'The Modern Community' before 1938.

23. This course had a variety of names. From 1935-37 it was entitled 'Problems of the Modern Community'; from 1942 to 1943 it was 'Comparative Study of the Social Organisation of Contemporary Communities' and from 1944 'Contemporary Communities'.

24. This course was titled 'Technology and Social Change' from 1933. Course number 437 in 1948. Title changed to 'Technology, Social Change & Urbanisation' in 1951.

25. This course changed number to 442 from 1948-50

26. This course numbered 344 in 1948

27. Changed number to 346 from 1943-44

28. In 1936 this course was number 345

29. From 1939 this course was titled 'Professions'

30. This course is identical to Anthropology course 351.

31. Matthews was in the Divinity Department

32. This course was entitled 'The Social Orientation of Children' before 1939 and 'The Social Development of the Child' from 1943 onwards.

33. It was numbered 362 in 1929 and 364 from 1943. In 1951 its title changed to 'Human Ecology and the Urban Community'

34. Puttkamer was in the Law Department.

35. The title of this course from 1933 was 'Organised Crime and Criminal Culture'

36. This course was titled 'Quantitative problems In Social Disorganization' from 1939

37. This course was titled 'Theory of Personal Disorganization' from 1934

38. Courses 381 and 382 for the same period also had the same title

39. This course titled 'Communication and Social Solidarity' in 1948

40. This course was number 328 in 1944

41. This course was number 402 from 1941-1943

42. This course was number 402 from 1931 to 1940

43. Courses 404 and 405 identical for these dates

44. Courses 404, 405 and 406 identical for these dates

45. Courses 405 and 406 identical for these dates, course 407 identical from 1946 to 1951

46. Courses 407 & 408 identical for this date

47. Courses 408 & 409 identical for this date

48. Courses 411 & 412 identical for this date

49. Allie was in the Zoology department

50. Courses 424 & 425 identical for these dates

51. Courses 427, 428 & 429 identical for these dates

52. Course number changed to 448 in 1949. Course 450 identical in 1950

53. Courses 434 & 435 identical for these dates

54. Course 441 identical from 1933 to 1938, Courses 442 and 443 identical from 1935 to 1937


57. Courses 474 & 475 identical for these dates.

58. Number 478 in 1933. Courses 477 and 479 identical from 1933 to 1938 and from 1942 to 1944.

59. Also identical to Home Economics 312

60. Also identical to Home Economics 312A & Psychology 312A

61. Also identical to Home Economics 312B & Psychology 312B

62. Also identical to Home Economics 312C & Psychology 312C

63. Also identical with Education 317
APPENDIX FIVE

PH.D. THESSES

Theses awarded the Ph.D. in the Department of Sociology (and Anthropology) at the University of Chicago from 1895 to 1952

Raymond, J. H., 1895, 'American Municipal Government'
Sandeze, F. W., 1895, 'An Exposition in Outline of the Relation of Certain Economic Principles to Social Adjustment'

Thomas, W. I., 1896, 'On Differences of the Metabolism of the Sexes'
Vincent, G. E., 1896, 'Sociology and the Integration of Studies'
Miller, M. L., 1897, 'A Preliminary Study of the Peublos of Taos, New Mexico'
Clark, H. B., 1897, 'The Public Schools of Chicago: A Sociological Study'

Dorowa, D. P., 1897, 'The Ethnobotany of Conchile Indians of Southern California'
Howelth, I. W., 1898, 'The Social Aim of Education'
Fellows, C. A., 1899, 'Some Prolonguns of Social Psychology'

Gordon, W. C., 1899, 'The Social Ideals of Alfred Tennyson as Related to his Time'

Forrester, J. D., 1900, 'The Development of Industrial Organisations'


Gillette, J. M., 1901, 'The Culture Agencies of A Typical Manufacturing Group, South Chicago'

Steelman, C. J., 1901, 'A Study of the Stock Yards Community at Chicago, as a Typical Example of the Bearing of Modern Industry upon Democracy, with Constructive Suggestions'

Hayes, E. C., 1902, 'The Sociologist's Object of Attention'

Hawes, A., 1902, 'The Part of Invention in the Social Process'

Cressey, E. G., 1903, 'The Church and The Young Man'

Riley, I. J., 1904, 'A Study in the Higher Life of Chicago'

Adams, R. C., 1904, 'A Technique for Sociological Research'

Fleming, H. E., 1905, 'Some Phases of the Production and Consumption of Literature in Chicago'

Perkins, M. R., 1905, 'The Treatment of Juvenile Delinquents'

Borgman, E. W., 1905, 'Charities for Children in the City of Mexico'

Wood, E. B., 1905, 'Progress as a Sociological Concept'

Rhoades, M. C., 'A Casa Study of the Delinquent Boys in the Juvenile Court in Chicago'

Munford, E., 1906, 'The Beginning of Authority'

Dyer, C. W., 1907, 'Democracy in the South Before the Civil War'

Woodhead, H., 1907, 'The Social Significance of the Physical Development of Cities'

North, C. C., 1908, 'The Influence of Modern Social Relations Upon Ethical Concepts'

MacPherson, H., 1910, 'Co-operative Credit Associations in the Province of Quebec'

Fenton, F., 1910, 'The Influence of Newspaper Presentations Upon the Growth of Crime and Other Anti-Social Activity'

Bernard, L. L., 1910, 'The Transition to an Objective Standard of Control'

Reep, S., 1911, 'Social Policy of Chicago Churches'

Bagardus, E. S., 1911, 'The Relation of Fatigue to Industrial Accidents'

House, J. F., 1912, 'Purpose, the Variant of Theory'

Burgeon, E. W., 1912, 'The Function of Socialization in Social Evolution'

Steiner, J. F., 1913, 'The Japanese in America'

Sutherland, E. H., 1913, 'Unemployment and Public Employment Agencies'

Taft, J. J., 1913, 'The Woman Movement From the Point of View of Social Consciousness'

Ware, W. J., 1913, 'An Instrumental Interpretation of Social Theory: L'Ordre Naturel Et Essentiel Des Societes Politiques' de Le Mercier de la Riviere, Physiocrate'.

Elmer, M. C., 1914, 'Social Surveys of Urban Communities'

Coleman, G. I., 1914, 'The Transition From the Ideals of Personal Righteousness of the Seventeenth Century to the Modern Ideals of Social Science'

Eubank, E. E., 1915, 'A Study of Family Desertion'

Handman, R. T., 1917, 'The Beginnings of the Social Philosophy of Karl Marx'

Stone, R. W., 1919, 'The Origin of the Survey Movement'


Veyer, L. C., 1919, 'A Study of Wage-Payment to Prisoners As A Penal Method'

Queen, S. A., 1919, 'The Passing of the County Jail'

Kawabe, K., 1919, 'The Japanese Newspaper and its Relation to the Political Development of Modern Japan'

Blachley, C. D., 1919, 'The Treatment of the Problem of Capital and Labor in Social Study Course in the Chicago Churches'

Smith, W. C., 1920, 'Conflict and Fusion of Cultures as Typified by the Ao Nagas of India'


Horek, J., 1920, 'The Assimilation of the Czechs in Chicago'

Carroll, M. R., 1920, 'The Attitude of the American Federation of Labor Toward Legislation and Politics'

Bodenhaver, W. B., 1920, 'The Comparative Role of the Group Concept in Ward's Dynamic Sociology and Contemporary Sociology'

Sandercox, D. C., 1921, 'The Rural Community: A Social Unit'

Ratcliffe, S. C., 1921, 'Pauper Law and Institutions in Illinois'


McKenzie, R. D., 1921, 'The Neighborhood, A Study of Local Life in Columbus, Ohio'

Vincent, G. E. S., 1922, 'Nationalism and Language'

Dettweiler, F. G., 1922, 'The Negro Press in the United States'

Bickham, M. H., 1922, 'The Scientific Antecedents of the Sociology of August Comte'

Dawson, C. A., 1922, 'The Social Nature of Knowledge'

Hayner, N. S., 1923, 'The Sociology of Hotel Life'

Price, M. F., 1924, 'Race Prejudice'

Rosenau, W. H., 1924, 'Protestant Missions as Culture Contact'
Hauser, P. M., 1938, 'Differential Fertility, Mortality and Net Reproduction'
Glick, C., 1938, 'The Chinese Migrant in Hawaii'
Cox, E. C., 1938, 'Factors Affecting the Marital Status of Negroes in the United States'
Reed, F. A., 1939, 'Embezzlement: A Study of One Kind of Criminal Behavior with Prediction Tables Based on Fidelity Insurance Records'
Pierson, D. R., 1939, 'A Study of Racial and Cultural Adjustment in Bahia, Brazil'
Daniel, V. E., 1940, 'In Chicago's South Side Churches for Negroes'
Strong, S. M., 1940, 'The Social Type Method: Social Types in the Negro Community of Chicago'
Noss, T., 1940, 'Resistance to Social Innovations as Found in the Literature Regarding Innovations which have Proven Successful'
Hillman, W. A., 1940, 'Urbanization and the Organization of Welfare Activities in the Metropolitan Community of Chicago'
Schaufelner, W., 1941, 'The Suburbs of Cleveland: A Field Study of the Metropolitan District Outside the Administrative Area of the City'
Johnson, E. S., 1941, 'The Evolution of the Chicago Central Business District'
Jaffe, A. J., 1941, 'Urbanization and Fertility'
Gittler, J. B., 1941, 'Society's Adjustment to a Mechanical and Social Invention: A Study in Social Change'
Dunham, H. W., 1941, 'The Character of the Interrelationship of Crime and Schizophrenia'
Devine, L. H., 1941, 'Some Relationships Between Educational Achievement and Social Stratification'
Winch, R. F., 1942, 'Social Personality Characteristics of Courtship Revealed in Men'
Weinberg, S. K., 1942, 'Incest Behavior and Family Organization'
Wallin, P., 1942, 'The Characteristics of participants in a Social-Psychological Study'
Johns, E. D., 1942, 'Chicago's Newspapers and the News: A Study of Public Communication in a Metropolis'
Golchamer, H., 1942, 'Some Factors Affecting Participation in Voluntary Associations'
Baur, E. J., 1942, 'Voluntary Control in the Advertising Industry'
Alexander, C. S., 1942, 'Antipathy and Prejudice: A Study of the Distinctions Between These Two Phronemata'
Whyte, W. F., 1943, 'Street Corner Society: The Social Structure of an Italian Slum'
Dyler, M. D., 1943, 'Fertility and Migration of Kentucky Population, 1920-1940, as Related to Communication, Income and Education'
Symons, J. N., 1944, 'Utah Residence Types and Criminal Behavior'
Staunton, A. L., 1944, 'A Study of Three Psychological Factors Affecting the Choice of a Mate in Marriage'
Parrish, C. H., 1944, 'The Significance of Color in the Negro Community'
Hall, O. N., 1944, 'Informal Organization of Medical Practices Case Study of a Profession'
Mu, P. S., 1945, 'The Social Characteristics of Increasing, Stable and Decreasing Cities'
Weller, F. J., 1945, 'The Sect in Transition'
Walker, H. J., 1945, 'Changes in Race Accommodation in a Southern City'
Whitridge, E. R., 1946, 'Art in Chicago'
Sawyer, C. W., 1946, 'The Social Categories of Friendship'
Rose, A. M., 1946, 'Living Arrangements of Unattached Persons in American Cities'
Laviolette, F. E., 1946, 'Americans of Japanese Ancestry: A Study of Assimilation in the American Community'
Hill, M. C., 1946, 'The All Negro Society'
Rose, A. W., 1947, 'A Socio-psychological Analysis of the Ambition Patterns of a Sample of Industrial Workers'
Miller, V., 1947, 'The Areal Distribution of Tax Delinquency in Chicago and Its Relationship to Certain Housing and Social Characteristics'
Lee, R. H., 1947, 'Growth and Decline of Chinese Communities in the Rocky Mountain Region'
Freidman, R., 1947, 'Recent Migration to Chicago'
Clark, R. E., 1947, 'The Relationship of Occupation and Various Psychoses'
Campbell, L., 1947, 'A Scale for the Measurement of Acculturation'
Breese, G. W., 1947, 'The Daytime Population of the Central Business District of Chicago'
Bendix, R., 1947, 'The Public Servant in a Democracy'
Swanson, G. E., 1947, 'Emotional Disturbance and Juvenile Delinquency'
Beltzer, B. N., 1948, 'Preprofessional Career and Early Publication as Factors in the Differential Productivity of Social Scientists'
Klapp, D., 1948, 'The Hero as a Social Type'
Janowitz, M., 1948, 'Mobility, Subjective Deprivation and Ethnic Hostility'
Cohen, L. M., 1948, 'Factors Associated with Home Ownership in Twenty-two Metropolitan Districts, 1940'
Burnett, J., 1948, 'The Problem of Community Instability in East Central Alberta'
Bowers, L. E., 1948, 'The Measurement of Areas of Adjustment in Marriage'
Duncan, H. D., 1948, 'Chicago as a Literary Center: Social Factors Influencing Chicago Literary Institutions from 1885 to 1920'
Shibutani, T., 1948, 'The Circulation of Rumors as a Form of Collective Behavior'
Milone, G. S., 1948, 'Juvenile Delinquency and Youth Services in Wartime California'
Faw, V. E., 1948, 'Vocational Interests of Chicago Negro and White High School Junior and Senior Boys'
Thomas, J. L., 1949, 'Some of the Factors Involved in the Breakdown of Catholic Marriage'
Shunsky, E., 1949, 'The Personal Adjustment of Recipients of Old Age Assistance: With Special Consideration of the Methodology of Questionnaire [sic] Studies of Older People'
Dee, W. L. J., 1949, 'The Social Effects of a Public Housing Project on the Immediate Community'


Klassen, P., 1949, 'Internal Migration in Relation to Literacy and Language'

Lundey, G. A., 1949, 'A Study of Parole Prediction'

Clausen, J., 1949, 'Soldiers' Plans and the Prediction of the Post-Separation Activities of Veterans'

Colihan, E. C., 1949, 'Negro Stereotyped Conceptions of White People'

Stone, R. C., 1949, 'Vertical Mobility and Ideology: A Study of White Collar Workers'

Smith, H. L., 1949, 'The Sociological Study of Hospitals'

Nelson, C. W., 1949, 'Development and Evaluation of a Leadership Attitude Scale for Foremen'

Bonaccorsi, H., 1949, 'Paranoia and Paranoid Condition: A Social-Psychological Study of Paranoid Personality'

Duncan, D. D., 1949, 'An Examination of the Problem of Optimum City Size'

Wray, D., 1950, 'The Foreman and Managerial Functions'

Williams, J. J., 1950, 'The Professional Status of Women Physicians'

Reiss, A. J., 1950, 'The Accuracy, Efficiency, and Validity of a Prediction Instrument'

McKee, W. J., 1950, 'The Dynamics of the Childhood Families of Small Highly Selected Groups of Male and Female Schizophrenics, Behavior Problems and Normals'

Killian, L. M., 1950, 'Southern White Laborers in Chicago's West Side'

Hormann, B. L., 1950, 'Extinction and Survival: A Study of the Reaction of Aboriginal Populations to European Expansion'

Hale, W. H., 1950, 'The Career Development of the Negro Lawyer in Chicago'

Dalton, M., 1950, 'A Study of Informal Organization among the Managers of an Industrial Union'

Ramsy, N. C., 1950, 'Recent Trends in Occupational Mobility'

Reitzer, D., 1950, 'Collective Factors in Race Relations'

Quinn, D. W., 1950, 'Racial Attitude and the Conforming Personality'

Marcussen, S., 1950, 'The Prediction of Intermarriage'

Fitchett, E. H., 1950, 'The Free Negro in Charleston, South Carolina'


Ilke, F. C., 1950, 'The Impact of War Upon the Spacing or Urban Population'

Linn, E., 1950, 'The Military Mind and the Soldier Press'

Moltz, A. B., 1950, 'Conceptions of Martial Roles in Transition'

Rowenstein, J. S., 1950, 'Small-Town Party Politics'

Roes, A. D., 1950, 'Ethnic Relations and Social Structure: A Study of the Invasion of French-Speaking Canadians into an English Canadian District'

Schmidt, J. F., 1950, 'Patterns of Poor Adjustment in Persons of Later Maturity'

Lu, Y-C., 1950, 'A Study of Dominant, Equitarian and Submissive Roles in Marriage'

Lee, S-C., 1950, 'Social Implications of Farm Tenancy in China'


Harlaxton, N. H., 1950, 'Isolation and Conduct in Later Life: Study of Four Hundred and Sixty Four Chicoesma of Ages Sixty to Ninety-Five'

Goldman, N., 1950, 'The Differential Selection of Juvenile Offenders for Court Appearance'

Calhoun, D. W., 1950, 'The Reception of Marxist Sociological Theory by American Academic Sociologists'

Bogart, L., 1950, 'The Comic Strips and their Adult Readers: A Study of Male Workers in a New York City Neighborhood'

Star, S. A., 1950, 'Interracial Tension in Two Areas of Chicago: An Exploratory Approach to the Measurement of Interracial Tension'

Stafford, A. B., 1950, 'Trends of Invention in Material Culture: A Statistical Study of the Class-Wise Distribution of Inventive Effort in the United States as Determined by Patents Granted During the Period 1910-1940'

Pan, J-S., 1950, 'A Comparison of the Factors in the Personal Adjustment of Old People in the Protestant Church Homes for the Aged and the Old People Living Outside of Institutions'


King, C. A., 1950, 'Factors Making for Success or Failure in Marital Adjustment Among 466 Negro Couples in Southern City'

Schwartz, M., 1951, 'Social Interaction in a Disturbed Ward of a Mental Hospital'

Becker, H. S., 1951, 'Role and Career Problems of the Chicago Public-School Teacher'

Westley, W. E., 1951, 'The Police and a Sociological Study of Law, Custom and Morality'

Short, J. F., 1951, 'An Investigation of the Relation Between Crime and Business Cycles'

Lewis, H. C., 1951, 'The Social Life of the Negro in a Southern Piedmont Town'

Kitagawa, E. M., 1951, 'Differentials in Total and Marital Fertility, Chicago 1920-1940'

Hare, A. P., 1951, 'A Study of Interaction and Consensuses in Different Sized Discussion Groups'

Jackson, G. A., 1951, 'A Study of Evaluations of Nisei as Workers by Caucasian Employment Agencies'

Magers and Employers of Nisei' [12]

Haimowitz, M. L., 1951, 'The Development and Change of Ethnic Hostility'

Elkin, F., 1951, 'A Study of the Relationship between Popular Hero Type and Social Class'

Ireland, R., 1951, 'The Aging Industrial Worker: Retirement Plans and Preparation with Some Reference to the Meaning of Work'

Keyfritz, N., 1952, 'Urban Influence on Farm Family Size'

Blackiston, D., 1952, 'The Judge, the Defendant and Criminal Law Administration'

Edwards, G. F., 1952, 'Occupational Mobility of A Selected Group of Negro Male Professionals'

Benson, P., 1952, 'The Interests and Activities of Engaged and Married Couples in Relation to Their Adjustment'


Reeder, L., 1952, 'Industrial Location in the Chicago Metropolitan Area with Special Reference to Population'


Linn, E., 1952, 'The Correlation of Death Rates From Selected Causes with the Business Cycle, 1919-47'


NOTES TO APPENDIX 5

[1] Sources: A mimeographed list provided by Professor M. Janowitz, University of Chicago, dated 1968. The collection of theses lodged in the Social Science Research Building at the University of Chicago, Farias (1967), who has a list of doctoral theses up to 1935. Files of the Regenstein Library, University of Chicago. Computer search, Dialog File 35, Ph.D theses in sociology 1900-1920. Not all sources concur. Where any differences occur these are noted, details from the original theses or the most likely version are recorded in the listing. Various other commentators refer to Chicago Ph.D's, but where these are not in any of the above source lists they are not included. For example, Mullins (1973, pp. 54-57) lists Chicago Ph.D's who he included as symbolic interactionists, on this list is William Troyer (1942), but no other reference to this thesis could be located.

[2] This is the title on computer printout, it does not appear on Janowitz's list, nor in Farias (1967) nor was it located in the Social Science Research Building. On the Regenstein Library file the title was 'The Church and Young Men'.

[3] These theses only appear on the computer printout and in none of the other sources. It is possible that these were Henderson's students. Henderson was made head of the Department of Ecclesiastical Sociology in 1904, later (1913) Practical Sociology. This still seemed to be under the general rubric of the Department of Sociology and Anthropology at the time, see the Official Publications of the University of Chicago, although subsequent classifications of the theses may have detached them from the sociology output. Alternatively, they may just have been lost. Henderson died in 1914 and no further mention was made of Practical Sociology in the section of the Official Publications relating to the Sociology Department.

[4] This is missing from Janowitz's list, is entitled 'The Origins of Leadership' on the Regenstein list. A book by the author with this latter title was published by the University of Chicago Press in 1909.

[5] Janowitz's list, Farias (1967) and the computer printout have this reference. However, the computer printout also lists 1915, 'The Japanese Invasion: A Study in the Psychology of Inter-Racial Contacts'. The Regenstein list has no reference to either title. A book, entitled 'The Japanese Invasion' by Steiner with an introduction by Park was published by A. C. McClung and Co., in 1917. Raushenbush (1979) noted that Steiner was a graduate from Washington who wrote a thesis on the Japanese on the Pacific Coast in 1913.

[6] This is not in any source except the computer printout. In 1916 a book with the same title was published by the University of Chicago Press, sponsored by the Committee on Publications in the Physical Sciences.

[7] The only reference for this is on the computer printout.

[8] The subtitle is missing from the Regenstein catalogue.


[11] These theses are missing from Janowitz's list and Farias (1967) but are included in the Regenstein file.

[12] These are missing from Janowitz's list.

[13] The subtitle is missing from Janowitz's list.

[14] The title is missing from Janowitz's list.
The following table shows the extent to which different techniques were used in the theses.

More than one technique may have been used in a thesis. Major usage refers to a technique upon which the author primarily relied. Supporting usage means that the technique(s) were adopted to provide back-up information. Minor usage means that the technique was used but played no substantial part in the final thesis.

<table>
<thead>
<tr>
<th>Method</th>
<th>All Theses (n=42)</th>
<th>Pre 1940 Theses (n=22)</th>
<th>1940-1950 Theses (n=20)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Major</td>
<td>Support</td>
<td>Minor</td>
</tr>
<tr>
<td>Historical Analysis</td>
<td>10 (24)</td>
<td>9 (21)</td>
<td>6 (14)</td>
</tr>
<tr>
<td>Comparative Analysis</td>
<td>11 (26)</td>
<td>1 (2)</td>
<td>5 (12)</td>
</tr>
<tr>
<td>Literature Review</td>
<td>16 (38)</td>
<td>8 (19)</td>
<td>14 (33)</td>
</tr>
<tr>
<td>Life History</td>
<td>6 (14)</td>
<td>6 (14)</td>
<td>1 (2)</td>
</tr>
<tr>
<td>Participant Obs.</td>
<td>5 (12)</td>
<td>5 (12)</td>
<td>5 (12)</td>
</tr>
<tr>
<td>Document Analysis</td>
<td>11 (31)</td>
<td>2 (6)</td>
<td>6 (14)</td>
</tr>
<tr>
<td>Informal Interviews</td>
<td>11 (26)</td>
<td>4 (10)</td>
<td>3 (7)</td>
</tr>
<tr>
<td>Questionnaires</td>
<td>6 (14)</td>
<td>0 (0)</td>
<td>0 (0)</td>
</tr>
<tr>
<td>Scheduled Interviews</td>
<td>13 (31)</td>
<td>0 (0)</td>
<td>0 (0)</td>
</tr>
</tbody>
</table>

Historical Analysis:
10 (24%) Used historical analysis to determine categories, models or derive theories
5 (12%) Used historical analysis to provide a descriptive setting
10 (24%) Concentrated on analysis in terms of 'natural history'
17 (40%) Did not use historical analysis.

Comparative Analysis:
11 (26%) Made a comparative analysis between two or more counties, states or regions
6 (14%) Compared methods, cases etc.
25 (60%) Did not use comparative analysis

Literature Review:
14 (33%) Were based on sociological texts
9 (21%) Were based on a mixture of sociology and psychology literature
12 (29%) Were based on non-sociological literature
4 (10%) Used a variety of sources
3 (7%) Made no use of a literature review and analysis

Life History:
12 (29%) Were collected from subjects
1 (2%) Were based on case records
29 (69%) Did not use life histories

Participant Observation:
2 (5%) Complete participant observation
6 (14%) Partial participant observation
7 (17%) Used casual observation, past personal involvement or other observers
27 (64%) Did not use participant observation in any sense

Document Analysis:
5 (12%) Analysed newspapers
5 (12%) Analysed published autobiographies
11 (26%) Used letters, case documents, hotel registers or a variety of such sources.
21 (50%) Made no use of document analysis

Informal Interviews:
11 (26%) Used a systematic approach to in depth interviews (6 were guided interviews)
7 (17%) Used an unsystematic approach to in depth interviews
24 (57%) Did not use informal interviewing

Questionnaires:
4 (10%) Were administered to students
2 (5%) Were mailed to respondents
36 (85%) Made no use of questionnaires

Scheduled Interviews:
10 (24%) Used formal schedules
2 (5%) Used flexible schedules
30 (71%) Made no use of scheduled interviewing
### Table 2: Attitude analysis

<table>
<thead>
<tr>
<th></th>
<th>All theses</th>
<th>Pre-1940</th>
<th>1940-1950</th>
</tr>
</thead>
<tbody>
<tr>
<td>Made no attempt at attitude analysis</td>
<td>25 (60%)</td>
<td>16 (73%)</td>
<td>9 (45%)</td>
</tr>
<tr>
<td>Attempted some form of attitude analysis</td>
<td>17 (40%)</td>
<td>6 (27%)</td>
<td>11 (55%)</td>
</tr>
<tr>
<td>Of the latter</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Constructed some kind of attitude scale</td>
<td>6 (14%)</td>
<td>1 (5%)</td>
<td>5 (25%)</td>
</tr>
<tr>
<td>Used or adapted an existing scale</td>
<td>3 (7%)</td>
<td>1 (5%)</td>
<td>2 (10%)</td>
</tr>
<tr>
<td>Did not attempt to construct a scale of attitudes</td>
<td>8 (19%)</td>
<td>20 (90%)</td>
<td>13 (65%)</td>
</tr>
</tbody>
</table>

### Table 3: Case Study

<table>
<thead>
<tr>
<th></th>
<th>All theses</th>
<th>Pre-1940</th>
<th>1940-1950</th>
</tr>
</thead>
<tbody>
<tr>
<td>Analysis involved a case study approach</td>
<td>17 (40%)</td>
<td>13 (62%)</td>
<td>4 (20%)</td>
</tr>
<tr>
<td>Made no mention of a case study approach</td>
<td>25 (60%)</td>
<td>9 (38%)</td>
<td>16 (80%)</td>
</tr>
</tbody>
</table>

### Table 4: Discussion of Methods

<table>
<thead>
<tr>
<th></th>
<th>All theses</th>
<th>Pre-1940</th>
<th>1940-1950</th>
</tr>
</thead>
<tbody>
<tr>
<td>Extensive discussion</td>
<td>13 (31%)</td>
<td>6 (27%)</td>
<td>7 (35%)</td>
</tr>
<tr>
<td>Some discussion</td>
<td>19 (45%)</td>
<td>8 (36%)</td>
<td>11 (55%)</td>
</tr>
<tr>
<td>No discussion</td>
<td>10 (24%)</td>
<td>6 (27%)</td>
<td>2 (10%)</td>
</tr>
</tbody>
</table>

### Table 5: Discussion of Methodology/Epistemology

<table>
<thead>
<tr>
<th></th>
<th>All theses</th>
<th>Pre-1940</th>
<th>1940-1950</th>
</tr>
</thead>
<tbody>
<tr>
<td>Extensive discussion</td>
<td>11 (26%)</td>
<td>9 (43%)</td>
<td>2 (18%)</td>
</tr>
<tr>
<td>Some discussion</td>
<td>10 (24%)</td>
<td>1 (5%)</td>
<td>9 (45%)</td>
</tr>
<tr>
<td>No discussion</td>
<td>21 (50%)</td>
<td>11 (52%)</td>
<td>9 (45%)</td>
</tr>
</tbody>
</table>

### Table 6: Reformism

<table>
<thead>
<tr>
<th></th>
<th>All theses</th>
<th>Pre-1940</th>
<th>1940-1950</th>
</tr>
</thead>
<tbody>
<tr>
<td>Research directed to reformist concerns</td>
<td>2 (5%)</td>
<td>1 (5%)</td>
<td>1 (5%)</td>
</tr>
<tr>
<td>Some mention of reform concerns</td>
<td>3 (7%)</td>
<td>3 (14%)</td>
<td>0 (0%)</td>
</tr>
<tr>
<td>Clearly opposed to reform concerns</td>
<td>9 (21%)</td>
<td>8 (36%)</td>
<td>1 (5%)</td>
</tr>
<tr>
<td>No mention of reform concerns</td>
<td>28 (67%)</td>
<td>10 (46%)</td>
<td>18 (90%)</td>
</tr>
</tbody>
</table>

### Table 7: George Herbert Mead

<table>
<thead>
<tr>
<th></th>
<th>All theses</th>
<th>Pre-1940</th>
<th>1940-1950</th>
</tr>
</thead>
<tbody>
<tr>
<td>Mead's theories or approach utilised</td>
<td>4 (10%)</td>
<td>1 (5%)</td>
<td>3 (15%)</td>
</tr>
<tr>
<td>Mead mentioned but not utilised</td>
<td>8 (19%)</td>
<td>4 (18%)</td>
<td>4 (20%)</td>
</tr>
<tr>
<td>Mead not mentioned</td>
<td>30 (71%)</td>
<td>17 (77%)</td>
<td>13 (65%)</td>
</tr>
</tbody>
</table>

### Table 8: Charles Horton Cooley

<table>
<thead>
<tr>
<th></th>
<th>All theses</th>
<th>Pre-1940</th>
<th>1940-1950</th>
</tr>
</thead>
<tbody>
<tr>
<td>Cooley referenced</td>
<td>20 (48%)</td>
<td>13 (59%)</td>
<td>7 (35%)</td>
</tr>
<tr>
<td>Cooley not referenced</td>
<td>22 (52%)</td>
<td>9 (41%)</td>
<td>13 (65%)</td>
</tr>
</tbody>
</table>

### Table 9: William Isaac Thomas

<table>
<thead>
<tr>
<th></th>
<th>All theses</th>
<th>Pre-1940</th>
<th>1940-1950</th>
</tr>
</thead>
<tbody>
<tr>
<td>Thomas referenced</td>
<td>21 (50%)</td>
<td>15 (67%)</td>
<td>6 (30%)</td>
</tr>
<tr>
<td>Thomas not referenced</td>
<td>21 (50%)</td>
<td>7 (33%)</td>
<td>14 (70%)</td>
</tr>
</tbody>
</table>

### Table 10: Park and Burgess (1921)

<table>
<thead>
<tr>
<th></th>
<th>All theses</th>
<th>Pre-1940</th>
<th>1940-1950</th>
</tr>
</thead>
<tbody>
<tr>
<td>Major source text</td>
<td>10 (26%)</td>
<td>5 (28%)</td>
<td>5 (25%)</td>
</tr>
<tr>
<td>Referenced</td>
<td>8 (21%)</td>
<td>2 (11%)</td>
<td>6 (30%)</td>
</tr>
<tr>
<td>Not referenced</td>
<td>18 (53%)</td>
<td>11 (61%)</td>
<td>9 (45%)</td>
</tr>
<tr>
<td>Missing</td>
<td>4</td>
<td>4</td>
<td></td>
</tr>
</tbody>
</table>

### Table 11: Use of Official Statistics

<table>
<thead>
<tr>
<th></th>
<th>All theses</th>
<th>Pre-1940</th>
<th>1940-1950</th>
</tr>
</thead>
<tbody>
<tr>
<td>Extensive use for inference or description</td>
<td>5 (12%)</td>
<td>4 (18%)</td>
<td>1 (5%)</td>
</tr>
<tr>
<td>Some use</td>
<td>12 (29%)</td>
<td>8 (36%)</td>
<td>4 (20%)</td>
</tr>
<tr>
<td>No use of official statistics</td>
<td>25 (59%)</td>
<td>10 (46%)</td>
<td>15 (75%)</td>
</tr>
</tbody>
</table>

### Table 12: Chicago references in bibliography

<table>
<thead>
<tr>
<th></th>
<th>All theses</th>
<th>Pre-1940</th>
<th>1940-1950</th>
</tr>
</thead>
<tbody>
<tr>
<td>Up to 20% Chicago references</td>
<td>17 (47%)</td>
<td>12 (71%)</td>
<td>5 (26%)</td>
</tr>
<tr>
<td>Between 20 and 40% Chicago references</td>
<td>16 (44%)</td>
<td>5 (29%)</td>
<td>11 (58%)</td>
</tr>
<tr>
<td>Over 40% Chicago references</td>
<td>3 (8%)</td>
<td>0 (0%)</td>
<td>3 (16%)</td>
</tr>
<tr>
<td>Missing</td>
<td>6</td>
<td>5</td>
<td>1</td>
</tr>
</tbody>
</table>

* Appendix 7 contains a review of sources.
A NOTE ON DOCUMENTARY SOURCES

The primary documentary sources used in the thesis consisted of the following: published work of the Chicagoans and their contemporaries; Ph.D. theses produced at Chicago up to 1952; unpublished papers, research proposals, letters, minutes of meetings and other documents located in the personal papers of Chicagoans; the private journal of William Fielding Ogburn; minutes and papers of the Society for Social Research; transcripts of tape recordings of interviews conducted in 1972 by James Carey with twenty-five surviving Chicagoans of the 1920s; copies of correspondence between Fred Matthews and Chicagoans written during the 1970s. All sources directly referred to in the thesis are documented below.

Apart from the published material and the Ph.D. theses, the source material is all located in the Special Collections Department of the University of Chicago Regenstein Library. The examination of the papers in the Special Collections Department provided a general profile against which other retrospective accounts could be compared. Such accounts included some recollections in Ogburn's journal, the reflections of Anderson, and Cavan amongst others in Urban Life, 11, (1983), Matthews' letters (including correspondence with W. Fielding Ogburn and Louis Wirth), and an interview with N. Anderson, J. Bernard, and C. Herrmann) and, more importantly, Carey's interviewees. Carey interviewed the following in 1972 as part of the research for his book 'Sociology and Public Affairs' (1975): Barnhardt, Blumer, Cavan, Bartlett, Carter, Cottrell, Dollard, R. E. L. Faris, Hayner, Mrs. H. Jenson; G. B. Johnson, Kerpf, Kincheloe, McCluer, McKay, E. Mowrer, H. Mowrer, Nelson, Neumeyer, Newcomb, Pederson, Reckless, Stephan, Stonequist, Thompson, and Winton. (Full references in bibliography, by contributor, dated 1972).

The range of personal papers located in the Special Collections Department is extensive. The papers of William Fielding Ogburn, Ernest Watson Burgess and Louis Wirth were examined in some detail. These collections are very large and materials were systematically examined by the Special Collections catalogue which outlined the contents of different files in the collection. In the case of the Burgess papers, however, the catalogue was of limited use as the collection (in 1980) was only partially sorted and it was necessary to resort to a pseudo random selection of file boxes.

The three collections provided a great deal of useful information and as different items were pieced together, a general picture of the Department of Sociology at the University of Chicago from the 1920s to 1950 emerged. This picture was reinforced and given more depth by the extremely valuable and detailed accounts of meetings found in the papers of the Society for Social Research and by the overview of the research work of the members of the Society available in successive issues of the Bulletin of the Society.

The inspection of source documents was primarily directed to the period 1920 to 1950 as this emerged as the period in which there was the greatest conflict between the Chicagoans activities and the taken-for-granted view of their activities. Additional material on the research activities, social organization and wider context of the earlier period came from a number of well researched secondary sources including Sidney Blumer 1980, 1981, 1981a; Burgess 1952; Carey, 1975; Coser 1978; Dibble, 1972; Diner, 1980; Faris, 1967; Furner, 1975; Hinkles and Hinkles, 1954; Martindale, 1976; Matthews, 1977; Odum, 1951; Raushenbush, 1979; Schwendinger & Schwendinger 1974; Smith & White, 1929; Wirth, 1947. Additional material on this period relating to research practices and theoretical development came from published texts of the Chicagoans and Ph.D. theses (see references).

To augment the investigation of the development of sociological work at Chicago a sample of forty-two Ph.D. theses were selected at random from the list of theses completed between 1915 and 1952 and examined in detail (see Appendix 6). This source proved extremely useful and clearly showed the variety and trend of methodological approaches, typological procedures, theoretical orientations, epistemological underpinnings and extent of concern with reform. The progress and development of ideas in substantive areas (such as the sociology of race) were identifiable as a result of this analysis.

In reading primary sources one must be critical of both one's own interpretation and of the content of the material. First, such sources are not self-evident facts. Their sense and meaning are derived from their context and the researcher should be critical of avoiding dislocating text from its context. The context, of course, is, in part, created by the historian. Any documentary source must also be treated not as a static picture but part of a dynamic process. In short, one should not 'fix' any document with too rigid an interpretation, but should be constantly critical of the interpretation.

Second, the material itself may not be fully 'transparent'. For example, in the case of the Chicago material, minutes of meetings did not provide a verbatim report and may have concealed fundamental differences under a gloss of consensus. A precis of a speaker's presentation to a meeting, such as the Society for Social Research, may have tended to be complimentary irrespective of the quality of the contribution, and discussion sessions following such presentations seem to have been underreported. Applications for funding, too, tend to paint the institution in glowing colours and make the most of supporting evidence and on-going research whether or not it is particularly significant for the institution as a whole.

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