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KINSHIP AND THE SOCIAL ORDER

The Legacy of Lewis Henry Morgan

MEYER FORTES

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Foreword

LEWIS HENRY MORGAN WAS ASSOCIATED WITH THE UNIVERSITY OF ROCHESTER from its founding. At his death he left it his manuscripts and library, and money to establish a women's college. Save for a wing of the present Women's Residence Halls that is named for him, he remained without a memorial at the University until the Lewis Henry Morgan Lectures were begun.

These Lectures owe their existence to a happy combination of circumstances. In 1961 the Joseph R. and Joseph C. Wilson families made a gift to the University, to be used in part for the Social Sciences. Professor Bernard S. Cohn, at that time Chairman of the Department of Anthropology and Sociology, suggested that establishing the Lectures would constitute a fitting memorial to a great anthropologist and would be an appropriate use for part of this gift. He was supported and assisted by Dean (later Provost) McCrea Hazlett, Dean Arnold Ravin, and Associate Dean R. J. Kaufmann. The details of the Lectures were worked out by Professor Cohn and the members of his Department.

The Morgan Lectures were planned initially as three annual series, for 1963, 1964, and 1965, to be continued if circumstances permitted. It was thought fitting at the outset to have each series focused on a particularly significant aspect of Morgan's work. Accordingly, Professor Meyer Fortes' 1963 Lectures were on

kinship, Professor Fred Eggan devoted his attention to the American Indian, and Professor Robert M. Adams considered a particular facet of the development of civilization, concentrating on urban society. Professor Eggan's Lectures and those of Professor Adams were published in 1966. The present volume completes the foundation of the Lewis Henry Morgan Lectures, for although the series have been continued annually and are to be published, the first three years' Lectures have a special unity and importance as a whole, particularly in relation to Morgan's work.

The visit of Professor Fortes and his wife, Mrs. Doris Fortes, came when the Department at Rochester was just beginning its expansion. The informality favored by these circumstances enabled the Department's faculty to reap the maximum in pleasure and benefits from many unhurried conversations, seminars and evening gatherings, in which all were able to take part at one time or another.

In this greatly expanded version of his original Lectures, Professor Fortes has made it possible for readers who did not hear them or the discussions that went on daily to appreciate more fully (if still imperfectly) his impact on those who did. The Lectures, on which this book is based, were delivered at the University of Rochester on April 2 through April 18, 1963.

ALFRED HARRIS
Department of Anthropology
The University of Rochester

Preface

THIS BOOK IS AN EXPANSION OF THE LEWIS HENRY MORGAN LECTURES I WAS privileged to deliver at the University of Rochester in the spring of 1963. In thus enlarging what was originally a short course of lectures, I have had a special aim in view. It seems to me that the controversies among anthropologists of the past two or three generations relating to the subjects I deal with in this book have turned more often on misunderstandings, or even frank disregard, of the relevant source data than on conceptual inadequacies. The picture that has become traditional of Lewis Henry Morgan's contributions to our studies is a case in point. My thesis is that the structuralist theory and method of analysis in the study of kinship and social organization developed in modern British and American social anthropology stems directly from Morgan's work. This is not a novel point of view, but it has been smothered by the biased interpretations of Morgan's ideas and discoveries that have long prevailed. In order, therefore, to establish my thesis, I felt it to be essential to exhibit the evidence in full.

Here lies a difficulty. We do not, in social anthropology, have the notations and techniques to sum up complex researches and theories in compact formulas. Despite the valiant typological and statistical efforts of G. P. Murdock and others, we still have to go back to the monographic sources to test the value of a generalization or

the validity of an argument. In a short course of lectures one may be allowed the liberty of making the categorical pronouncements that flow so easily from the pens of most of us. When it comes to the permanent record, one has a choice. One can stay brief and leave it to the reader to search out for himself the evidence by which to check the argument, or, one can copy Morgan and, instead of a clutter of bare bibliographical references, lay out for the reader the data on which the analysis is based.

Rightly or wrongly, I have chosen the second road, setting out in detail the steps in my argument, and citing at appropriate length the evidence to which I could at best only allude in the lectures. My biggest dilemma has been how to take into account the many publications bearing on my subject matter that have appeared in the past five years. In the event, I have not hesitated to draw on such recent work where it has seemed particularly apposite and have reluctantly put it aside where it would make no difference to an already rounded-off argument.

The idea of tracing out systematically the connections between modern structural theory in the study of kinship and social organization and Morgan's investigations has long been in my mind. But I doubt if I should ever have ventured upon this daunting task if it had not been for the invitation to give the Morgan Lectures at Rochester. Bernard S. Cohn, at that time Chairman of the Department of Anthropology at Rochester, initiated it. I am deeply grateful for this. I am equally indebted to Alfred Harris, the present Chairman of the Department, for the considerate and patient friendship with which he has kept an eye on the progress of this work. The warmth and cordiality with which President Allen Wallis received and introduced me to the academic community at Rochester made the occasion truly memorable. I wish to thank him and his colleagues, in particular Provost McCrea Hazlett and Dean Arnold Ravin for honoring me with the invitation to give the first series of Morgan Lectures. Rella Cohn, Grace Harris, and many others gave generously of their time and hospitality to make my wife and me feel at home in Rochester.

This book is a tribute to Lewis Henry Morgan's abiding influence. If this were not so, I should have dedicated it to the great American foundations for the advancement of learning and human welfare. Tributes to the part they have played in sustaining the studies and researches of anthropologists have become a matter of routine. But it is not often realized how much their generous and disinterested support contributed to the survival and growth of British social anthropology in the critical years before the last war.

More particularly, I have a special debt to an institution that eloquently typifies this tradition of disinterested support for learning. The final revision of this book has been accomplished during my tenure of a Fellowship at the Center for Advanced Study in the Behavioral Sciences at Stanford, California. It must suffice to say that if it had not been for the freedom to concentrate on this book and the facilities to process it provided for me at the Center, its completion would have been yet further delayed. But the Center has played a bigger part than this, for it was during my earlier Fellowship at the Center, in 1959, that the principal ideas explored in this book began to take the shape they now have. My debt to the seminars and discussions I shared with G. P. Murdock, Fred Eggan, Raymond Firth and the other

anthropologists and sociologists who were in residence there at the time can only be acknowledged, not specified.

Nor can I state in detail what I owe to my colleagues in the Department of Archaeology and Anthropology at Cambridge, and to the postgraduate students who have, over the years, taken part with us in our research seminar. There is hardly a topic dealt with in this book that has not been debated and clarified for me in our seminar. I have drawn heavily on the publications of its members. Among others who have helped to clarify for me problems dealt with in this book, I must make special mention of Max Gluckman and Isaac Schapera. Discussion with them has helped me over some difficult hurdles in the development of the analysis, especially where it touches on the field of political and legal anthropology.

I am grateful to Ailsa Allan, Mrs. M. E. Molyneux, and, in particular, Mrs. Agnes Page for their patient and efficient secretarial services in preparing the typescript for publication, and to Mrs. Gail Petersen for bibliographical assistance. And I am under a very special obligation to Priscilla Jones for the vigilance with which she has scrutinized the text and checked the bibliographical references, to the reader's great advantage.

Finally, I wish to thank my wife Doris Y. Fortes for bearing with me at discouraging moments and for assistance in the task of revision.

MEYER FORTES

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The Opposition between the two viewpoints, the synchronic and the diachronic, is absolute and allows of no compromise.

—Ferdinand de Saussure: *Course in General Linguistics*

Reality is the embodiment of structure;
Structures are the embodiment of properties;
Properties are the embodiment of harmony;
Harmony is the embodiment of congruity.

—Kuan Tsi, Chapter 55, Section ix (Fourth Century B.C.)
Written by Ts'ao T'ien-ch'in, translated by Gustav Haloun (1951)

(Reproduced from Lawrence Picken: The Organization of Cells and Other Organisms, Clarendon Press, Oxford, 1960, by permission of the author.)

PART I

RETROSPECT

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CHAPTER I

Morgan: The Founding Father

I

LEWIS HENRY MORGAN'S SCHOLARLY INTERESTS, LIKE HIS PARTICIPATION IN PUBLIC affairs, had a wide range, but in this book I confine myself to his achievements as an ethnologist and his enduring intellectual legacy to us, his posterity in the discipline he helped to found.

History, we are often warned, is a fickle jade. Morgan was by all accounts as robust and uncompromising an American of his day as could have been found anywhere in the United States. Yet his ideas and discoveries, revolutionary as they were for the science of man, suffered eclipse in his own country at a critical time. Like the proverbial prophet, his following was greater outside than within his native land at that time. Nevertheless, you might well ask what special claims a British anthropologist could have to merit the honor of giving the first of these lecture courses dedicated to his memory.

It is due, I am sure, to a turn curious in the history of our discipline. It was a British anthropologist, W. H. R. Rivers, as all students of the subject know, whose rediscovery of Morgan restored him to his rightful place in the main stream of anthropological scholarship—and this was the beginning of a method and theory of research which took deep root in British anthropology. Our science is by its very nature incapable of existing as an insular study. Morgan's observations and theories

excited worldwide ethnological interest in his own lifetime, but leading authorities in the United States soon turned away from him, perhaps not without justification. For what chiefly attracted attention about his work was what we now know to be its ephemeral facade. His fundamental discoveries were either ignored or misunderstood. Then came their vindication by Rivers, and the discipline thus founded now stands at the center of anthropological science.

I am going to argue that the primary source of what is nowadays called structural theory in the study of kinship and social organization is to be found in Morgan's work, and this not only in the purely historical sense but also, and more significantly, in the conceptual sense. This is true almost in spite of Morgan himself. For one of the curious and instructive features of this story is that he himself overemphasized what eventually proved to be of merely incidental importance in his work, thus failing to make the most of what eventually proved to be his discoveries of lasting value.

I am concerned then with the emergence and growth of a discipline in the science of anthropology. "An academic discipline," says Robert Redfield, in one of those dazzling papers in which he blazed many a trail, "an academic discipline is at once a group of men in persisting social relations and a method of investigation" (1953: 728). Spelled out, his dictum applies to any autonomous branch of art or science or scholarship. It is marked, firstly, by the craft or skill or body of knowledge that is distinctive of it. But if it is a living activity, it can be equally well identified by the manner in which its practitioners are set apart from the laity, by which I mean all the rest of the world as far as the practitioners are concerned. They will have an organization that is exclusive, institutions that are peculiar to their community as a profession, distinctive customs and norms. In short a specific culture. This is how they appear to the outsider. He knows that they are different because he is unable to comprehend the idiosyncrasies of their activity even if he can to some extent appreciate the products of their labors.

But how do the insiders, the practitioners, represent the autonomy of their craft and their calling to themselves? How do they perceive their collective identity, as opposed to the uninitiated laity? As anthropologists, we know where to look for the answer. We may expect to find it crystallized in myth and pedigree and accounted for by tradition—that is the process of handing on from generation to generation. And we shall not be surprised to find this sense of in-group identity symbolized in figures of ancestors and heroes and their opponents, the false prophets and factionmongers.

II

I first heard of Lewis H. Morgan as one of these false prophets. It was in Malinowski's seminars in 1931. Morgan was presented to us as a regrettable example of deluded genius, personifying the Reign of Error in anthropology which functionalism had come to overthrow. He stood for many of the things that were anathema to Malinowski's view of human social life—the discredited and repugnant hypothesis of primitive promiscuity and group marriage, the preposterous scheme of stages of social evolution, the dreary addiction to kinship terminologies as an end in itself.

Rivers was Malinowski's *bête noire* and Morgan loomed behind him as the misguided inventor of primitive communism in women and in property and as the inspiration of his misleading emphasis on forms of marriage as the main causal factors in kinship institutions. Morgan's canonization, as Lowie has called it, by Marx and Engels, and the blind adherence of Marxists ever since to his theories was a further black mark. If it was not his fault, it was added evidence of the wrongheadedness and sterility of his ideas. The touchstone was elementary: Morgan's theories were all wild conjecture. They collapsed in the face of the ethnographic facts and of functionalist criticism. Lowie, who was more respected than admired by Malinowski, received praise for refuting Morgan's reconstruction of the origin of the clan, while Westermarck, hero of Malinowski's apprentice days, was held up for special honor because he had so early demolished the dogma of group marriage.[1]

At that point, then, Lewis H. Morgan was to me, and I suspect to all of Malinowski's pupils, one of the leading anti-heroes of our discipline. His theories, his methods, his whole approach, represented in starkest shape tendencies to which the new movement in social anthropology was most antipathetic.

III

I confess with shame that it was not till nearly a decade later that I first read Morgan's works with an open mind. My own field experience, illuminated by Evans-Pritchard's studies of Nuer lineage organization, had forced me to come to grips with kinship and descent theory. Happily for me, Radcliffe-Brown was there to show the way. He was critical of Morgan, indeed more so than Malinowski or most of Morgan's other critics, for the simple reason that he understood what Morgan was trying to do. He made a present to me, when he was disposing of his library, of the copy of *Ancient Society* (1877) which he had acquired as a student at Cambridge University in 1906. And one need only turn the pages and note the passages he marked to realize how closely he had read it and how he had penetrated to what was fundamental in Morgan's work.[2]

Coming to Morgan then, and reading him side by side with the works of his

1. I have discussed Malinowski's stand in kinship studies elsewhere (Fortes, 1957: 157–88). His antipathy to the approach which Rivers derived from Morgan, was noted in the position he arrived at in his first theoretical work, *The Family Among the Australian Aborigines* (1913). It remained fixed throughout his life. His main objection to Morgan was the current one based on Westermarck's criticism of Morgan's distorted theories of the nature of parenthood and marriage, and his alleged assumption that all kinship institutions expressed "ideas of community of blood through procreation" (*ibid.*: 168–69). What Malinowski specifically scorned was Morgan's purported disregard of the parental family as the source of all kinship ties and as the basis of social organization. (*Sex and Repression*, 1927: 223).

2. It is well to remember that Radcliffe-Brown was Rivers's student at Cambridge at the very time (1901–06) that Rivers was launching out on his study of kinship and social organization with Morgan's works to guide him. This was the beginning of Radcliffe-Brown's lifelong interest in kinship and allied aspects of social life. Professor Eggan tells me that Radcliffe-Brown had an exhaustive knowledge of *Systems of Consanguinity and Affinity of the Human Family* (hereafter referred to as *Systems* or as *Systems of Consanguinity and Affinity*) and used it as the basis of the researches on American Indian social organization which he and his students at Chicago initiated (see references in chap. IV below).

contemporaries and successors, put his contributions to the development of anthropology in a new light for me. I was reminded of this recently when reading an essay by Kroeber. In this paper Kroeber looks back from 1950, over the half-century since the official establishment of anthropology at the University of California. He divides the history of our subject into two major periods. He calls the first period "unorganized" and measures it from Herodotus, our quasi-mythological founding ancestor. This period ends, he says, with the revolutionary decade of the 1860's. Then began what he calls the "organized" portion of our history. It was, as he observes, the phenomenal decade which started with Maine's *Ancient Law* (1861) and Bachofen's *Das Mutterrecht* (1861) and ended with Morgan's *Systems* (1870) and Tylor's *Primitive Culture* (1871). Not only that, for Darwin's *Origin of Species* (1859) and *Descent of Man* (1871) appeared at the beginning and the end respectively of this intellectual explosion. It did not matter, Kroeber comments with characteristic wisdom, if the views held were right or wrong. "What did matter," he concludes, "was that there was a direction, an attitude, a notion of method, above all a set of problems" (1952: 144–45).

In retrospect, then, if we follow Kroeber (and other authorities support him) we see Morgan not as a false prophet but as one of the inspired band of mid-nineteenth-century pioneers who brought order and method into the studies that are now distinctive of our calling, where previously random speculation and casual curiosity had been the rule.

How does Morgan appear today in this light? To judge of this we must note, first of all, the controversies that blazed up at once around and among these innovators. Morgan in particular drew plenty of fire. In fact, he became one of the central figures in a debate that went on for half a century. But I must not linger over details of this, pregnant though it was for the future of our science. Morgan's contributions to this great movement of thought have been amply described in biographical and historical studies of his life and work, notably through the devoted scholarship of Leslie White. What we must remember is that the issues were not just academic in nature. The very foundations of the nineteenth-century conception of humanity were at stake. No wonder that passions flew high and that the most eminent scholars and scientists were engaged.

What chiefly aroused Morgan's contemporaries was his radical and grandiose vision of the origins and development of mankind's basic social institutions. I call it a vision rather than a theory for one cannot read his works, even from the sober perspective of modern anthropology and archeology, without being swept along by the ardor and enthusiasm that suffused them. Here is learning enough but it is not the dry erudition and the studied aloofness of Tylor. Nor is it the persuasive accumulation of detail that we find at the other extreme in Darwin. Nor is Morgan even in his most rhetorical and involved arguments carried away by the poetical licence of which Starcke (1889: chap. VII) accused Bachofen. Morgan often refers to his hypotheses as "conjectures." But when he presents them he does so in the spirit of a man who has made—to quote a favorite word of his—"stupendous" discoveries:

> . . . like some watcher of the skies
> When a new planet swims into his ken.

So he had, as was in due course recognized.

But what loomed foremost for him, and for his contemporaries, was not these discoveries, but as I have said, the extravagant vision of the development of human society which he derived from them. Historians of our science have shown how this came about. The zeitgeist demanded it. Darwin wrote *The Descent of Man* to demonstrate that man "like every other species is descended from pre-existing forms" and in order to refute those by whom "it had often and confidently been asserted that man's origins can never be known." Morgan's theme was as urgent for him, and as momentous, in his estimation, for the work of fashioning the new conception of mankind that was to replace "The theory of human degradation to explain the existence of savages and barbarians," which came in "as a corollary from the Mosaic cosmogony" (1877; 1878 ed.: 4–5). He set out, we remember, to show that "the history of the human race is one in source, one in experience and one in progress" (*ibid.*: vi), that it was created by a "common principle of intelligence" (*ibid.*: 533) and culminated in Civilization with all its promise for ages to come. He spoke for his time—and a De Tocqueville of the period would, no doubt, have added tartly, for his country's social philosophy too. And that was why it was his large generalizations, rather than the empirical and scholarly investigations with which he believed he was merely underpinning them, that caught most attention.

We must bear in mind, as his biographers have emphasized (Stern, 1931), that the hypothesis which he took over and built upon in elaborating what to us is his preposterous scheme of social stages, was widely current at the time.[3] Eminent scholars in Europe and America, as well as men of affairs, took for granted the notion of development by stages in the growth of society. Many, moreover, accepted in some shape or another, the assumption of a primordial stage of marital communalism coupled with matriarchy as the *terminus a quo* of the history of the human family.

In relation to this frame of thought, and more particularly to the nature and paucity of the observational data that could be drawn upon, Morgan's contribution was sensational. The criticism that broke loose, even from some who were broadly in agreement with his ideas testified to this. The story has been well told by others,[4] but what I wish to underline is that the focus of attention was then, and so remained for fifty years, Morgan's speculative hypotheses. The glaring fallacies in his reasoning and the shaky foundations of his conjectures were easily exposed. Darwin, arguing from the analogy of sexual selection and mating habits among the lower primates, was one of the first to express reservations amounting to a rejection of the

3. And, like his basic orientation and his method of "conjectural history," had a respectable scholarly pedigree going back to the social philosophers and historians of the previous century. As is well known, the method of "conjectural history" was first proposed by the eighteenth-century Scottish social philosopher, Dugald Stewart. Manifestly fallacious as it appears to us now in the light of modern knowledge, its historical importance should not be overlooked. As Teggart has shown, it was a "most serious effort to lay the foundations for a strictly scientific approach to the study of man" (Teggart, *Theory of History*, 1925: 87, quoted in Bryson, p. 112).

4. By Stern, 1931, and, among others, by Lowie, 1936; Tax, 1937; White, 1948. A recent biographical study by Carl Resek, *Lewis Henry Morgan: American Scholar*, 1960, also touches on the topic.

promiscuity and group marriage hypothesis (1871: vol. II, 358–63). But even he couched his objections in terms of deference to the prevailing ethnological opinion of his day. When in 1891 Westermarck stepped into the arena with his *History of Human Marriage*, it was with the primary, if not sole purpose of disproving the doctrine of original group marriage and its implications.

On the other hand, as others have recorded (Stern, 1931; Lowie, 1936; Tax, 1937; White, 1948), Morgan's speculative theories had partisans too. I am thinking not only of their adoption by Marx and Engels as the gospel source for their theory of the origin of the family and the state, nor of the cordiality of Maine or the enthusiasm of Bachofen (cf. Stern, 1931: 145 ff.). I have in mind rather such (from our point of view) more respectable and influential support as these views received from people like Lord Avebury (1870) and from Sir James Frazer even as late as 1910.

IV

To realize what this means in relation to our own times, let us remember that it was in the early 1900s that Malinowski fell under the spell of *The Golden Bough* (1890–1915) and that Radcliffe-Brown wrote the first draft of *The Andaman Islanders* (published 1922). By this time Boas was the unquestioned leader of American anthropology, Kroeber was already vigorously pursuing research, and Lowie was serving his apprenticeship with Wissler (cf. Lowie, 1959: chap. 2). We are by this date on the threshold of today, linked to Morgan by a brief intellectual pedigree, and by a tradition which is as green as that which binds us to our own parents and grandparents. Since then, the theory of primitive promiscuity and group marriage—outside the U.S.S.R. and apart from such Marxist scholars as Professor George Thomson (1949)—has been liquidated, helped by a strong push from Malinowksi with his study of the Australian family (1913). More significantly, the seemingly endless matriarchy controversy has been pulled down from the realm of inspired guesswork to the solid earth of ethnographical field research.[5]

It is this, particularly, that marks the big change since Morgan's lifetime in the direction and organization of anthropological research to which Kroeber referred. True, the main currents of ethnographical research in the first decade of this century, especially in the United States under Boas's influence (cf. White, 1948), moved strongly away from the beacon set up by Morgan; but his presence never ceased to be felt and soon it was authoritatively recognized. *The League of the Iroquois* (1851)[6] influenced ethnographic field research in America before Morgan became internationally known.

But what was more to the point was the recognition of Morgan as a discoverer, one not unworthy to be ranked with a discoverer of a new planet. And it is this that marked the real break between the period of anthropological history inaugurated by

5. Cf. the admirable evaluation of how the issue appears today by David F. Aberle, "Matrilineal Descent in Cross-cultural Perspective" in Schneider, and Gough (ed.), 1961: 655–727.

6. Full title, *League of the Ho-dê-no-sau-nee or Iroquois*; hereafter referred to as *The League* or as *The League of the Iroquois.*

Morgan and its earlier, amorphous anticipations; from this stemmed the shift in method and direction to which Kroeber drew attention.

By *discovery* I mean bringing to light previously uncomprehended or totally unknown facts and principles—in this context, facts and principles relating to human social life. What was significant for Morgan and critical for the subsequent development of anthropology was that his discoveries were made by direct observation in the field. Nor did he stop there. He added the necessary complement of theoretical interpretations which could be verified or falsified by recourse to further direct observation. This is something quite different from the reliance on speculation and conjecture which had prevailed before and which still held sway among the leading ethnologists of the day.

V

Morgan's greatest discovery, as every anthropologist knows, and as has often been stressed, was, in Leslie White's words, "the fact that customs of designating relatives have scientific significance." (1957: 257). Stated so modestly, its momentous importance would not be apparent to the layman. To us as anthropologists whose work it is to seek knowledge of the springs of man's social life, it has a clarion ring, for we know that it was from this discovery that some of the most far-reaching explorations of our subject matter first took their impetus. With all due respect to the memory of Malinowski, honor must be given in particular, as I have already suggested, to W. H. R. Rivers. He was, as Sol Tax reminded Radcliffe-Brown's followers "the founder of the modern study of social organization" (1955: 471). It is surely relevant that Rivers came to this study, as Morgan did, through direct observation in the field. In this lay the stimulus which led him to acclaim, to his everlasting credit, "the great theoretical importance" of what he described as Morgan's "new discovery" (1914a: 5).

We should remember that Morgan's analysis of classificatory kinship systems had been ignored for thirty years as the result mainly of McLennan's criticism. This explains why Rivers thought it necessary to refute McLennan's contention (1876; 1886 ed. p. 273) that classificatory systems (in Rivers's words) "formed merely a code of courtesies and ceremonial addresses" (1914a: 6). He rejected with equal firmness Kroeber's early (and subsequently discarded) thesis that they were "determined primarily by language" and "reflect psychology, not sociology" (1909: 84).

What Rivers did was to go back to Morgan. He commented astutely that the controversy about primitive promiscuity and group marriage had obscured the true import of Morgan's great discovery. Appealing to his own field observations, he emphatically confirmed Morgan's analysis of kinship and concluded that this—not his speculative theories—was Morgan's important and fundamental contribution.

If I might digress for a moment, the history of science offers many instances of revolutionary discoveries being temporarily smothered by the clamor of orthodoxy, or at best escaping recognition because the state of knowledge is not yet ripe for

them. Mendel affords a classic example. Morgan's partial eclipse in the last quarter of the nineteenth century was doubtless due in part to the rarity, at that time, of ethnologists with first-hand field experience. There are grounds for such a conclusion if we bear in mind how Rivers was led by his field experience to return to Morgan. For this empirical starting point, as I shall repeatedly argue, is crucial. Lowie, who surely deserves to rank beside Rivers as an architect of modern kinship studies, was also influenced in that direction by field research. It was his field work among the Crow Indians that opened his eyes to Morgan's prowess as an ethnographer.

Should we then conclude that Morgan's novel discoveries failed to take root in the ethnology of his time because the ground was barren through lack of the fertilizing waters of ethnographical field research? Here, I think, some caution is necessary. We need only recall Boas and Malinowski to see that empirical research, even when it is pursued with complete integrity, has provided material for the rejection, or, at best, the misinterpretation rather than the confirmation of the analysis of social organization put forward by Morgan. It depends, really, on the frame of mind, or rather the apparatus of theory and method with which the field work is conducted and interpreted. And here lies the crucial issue for a just assessment of Morgan's legacy to us.

Rivers's explanation, incidentally, is worth bearing in mind because I think it has a lesson for us. As I have already indicated, Morgan himself never doubted that what deserved pride of place in his investigations, what was, from his point of view, most conclusive, was what I have called his visionary conception of mankind's social progress. Yet this is the part which is now generally dismissed as nothing more than a fallacious excrescence on the discoveries of substance embedded in his two great treatises. This applies also to the more restricted pseudo-historical conjectures to which he yoked the enquiries recorded in *Systems of Consanguinity and Affinity*. What has remained of permanent value, both as a testimony to his genius and as a springboard for advances in knowledge, is the body of data and the ancillary interpretations which he thought of primarily as the raw material for his speculations.

Morgan is not unique in this respect. Other great men have fallen for phantom aims, only to reach spurious conclusions on the basis of major discoveries which they failed to assess rightly. Our branch of science, I fear, is peculiarly prone to attracting this type of intellect. Perhaps indeed there is no anthropologist, nor ever has been one, who is really free from this propensity. Perhaps this is because we are particularly vulnerable to the political, moral, and spiritual climate of our time and culture, and therefore succumb easily to illusory ideals about our work; or maybe at bottom it is simply the price we pay for our scientific underdevelopment—that is, for the insufficiency of our apparatus of theory and method—for the tasks of rigorous and disciplined research.

VI

Be this as it may, as regards Morgan, one consequence of the exaggerated importance he himself and his contempories—as well as many of his successors—gave to his

speculations is that, ever since, too much attention has been directed to them. Again, I am not here thinking of Engels and the other Marxist admirers and followers he has had. As I have mentioned, Malinowski exploited these extravagant hypotheses (and not without reason or precedent) to make a straw image of Morgan as a foil to functionalist theories. Even those who, like Rivers, Lowie, Radcliffe-Brown and, in our own generation, Leslie White and G. P. Murdock, have had the perspicacity to seize on the fundamental discoveries he made, have been hampered by this tradition. We see this in the paper I have previously cited which was contributed by Lowie to the Kroeber *Festschrift* in 1936, where he makes an assessment in retrospect of Morgan's total contributions to anthropology. Before paying his tribute to what he calls the "positive side" of Morgan's contribution, he finds it necessary to refute at length Morgan's evolutionist scheme and the deductions tacked on to it. Then only can he discuss what he elsewhere (1937; 1960, p. 62) describes as "Morgan's unique distinction . . . in literally creating the study of kinship systems as a branch of comparative sociology".

Morgan's field observations have been regularly taken into account for their ethnographical and historical bearing on later findings, as by Swanton, Lowie, and many others since. The revelation by Rivers of what was truly original and fundamental in Morgan's work on kinship and social organization has stimulated frequent reappraisal of his concepts and interpretations by later theoretical criteria. Yet the formative influence of his methods and ideas throughout the whole later development of the anthropological study of kinship and social organization has, to my mind, never been properly evaluated. Inquiry has been too much side-tracked by his evolutionist aberrations.

This formative influence was effective by degrees, making itself felt almost more by contagion than by willing acceptance. To appreciate it one has to go back to the sources. One has to immerse oneself particularly in the two great treatises, *Systems of Consanguinity and Affinity* and *Ancient Society*, without prejudice and, I should add, without impatience with their pedantic and repetitious form.

However, different ways of approaching the work of our predecessors are open to us. If we examine them in an historical spirit, we try to assess them in relation to the times in which they lived. In Morgan's case, biographers, critics and commentators, some of them already referred to, provide plenty of guidance and material. The revolutionary decade, the sixties of last century, singled out by Kroeber, is tremendously interesting to us today because of the parallels and contrasts in method, theory, and aims discoverable among the founders of our science in that period. Take for instance Sir Henry Maine and compare him with Morgan. In retrospect we can see that they exercised convergent influences on the birth of modern social anthropology. Jurist and ethnologist respectively, they started, as Stern notes, from almost opposite kinds of data and premises (1931: 88). They disagreed on what to them seemed to be basic conclusions, principally over the hypothetical issue of which came first, patriarchy or matriarchy. Now suppose we ignore this, realizing that it was the accidental result of Maine's absorption in the history of Roman and Hindu family law and of Morgan's Iroquois predilections. We cannot then fail to be impressed by the compatibility of their points of view

and their theories, and we can see why it is that modern structuralist anthropology has derived much of its inspiration from them. Whether or not, for example, Morgan borrowed the division he made between *societas* or gentile society, and *civitas* or political society, from Maine's celebrated contrast between status and contract, is immaterial. The fact is that he found it apposite to his thought and improved on it theoretically by lifting it from its narrow legal associations to the sociologically more significant political plane. Yet matchless scholar though he was, Maine's ethnological naïveté was exposed when he commented as follows on Morgan's analysis of classificatory kinship systems: "May I suggest," he wrote, "that it is at least worthy of consideration whether all or part of the explanation may not lie in an imperfection of mental grasp on the part of savages? . . . the comprehension of a large body of complex relationships demands a prodigious mental effort, even now requiring for its success the aid of a special notation." Perhaps, he added, classificatory kinship represents "a rude and incomplete attempt at the mental contemplation of a tolerably numerous tribal body" (1883: 289–90). Today we would not take this kind of explanation seriously.

On the other side, there was Morgan blinded, as Lowie pointed out in 1936, by his Iroquois experiences and his preconceived hypotheses to such an extent that he failed to grasp the relevance of Maine's analysis of the concept of the corporation to his own theory of the gens and its passage into political society. Today of course we can see that they were complementary to each other. Patriarchy versus matriarchy, *patria-potestas* versus the democracy which Morgan tended to overrate in the matrilineal gens, the principle of the corporation as a legal entity by contrast with the structure of the corporate gens as a political unit based on kinship—in all these respects their researches complemented each other.

Now compare Tylor, the true founding ancestor of what has come to be called cultural anthropology. Like Morgan and Maine, he was a social evolutionist. He believed in progress and his scientific aim was the recovery of the origins of social institutions. But his idea of the subject matter of anthropology, that is to say, of the stuff of human social evolution, was poles apart from Maine's and Morgan's. His disciple and biographer R. R. Marett (1936) testifies to Tylor's lack of interest in "sociological matters," in other words, in social organization and related institutions, until nearly the end of his career. Dominated by his image of the "complex whole," as he called it, to which he gave the name of Culture, and committed as he was to a psychological mode of explanation (derived straight from the eighteenth-century Scottish social and moral philosophers), he simply did not know what to make of the bones and sinews of social organization that support and direct the manifestations of Custom. In his first major work, which appeared in the middle of the formative decade of the sixties, he alludes to the little that was then known about rules prohibiting marriage between kin and comments on affinal avoidances. But he throws them into a grab-all chapter labeled "Some Remarkable Customs" and dismisses them with the reflection that they belong "properly to that interesting, but difficult and almost unworked subject, the Comparative Jurisprudence of the lower races"—in fact, to Maine's and Morgan's field (1865; 1878 ed.: 279).

This judgment, incidentally, shows that Tylor was well aware of the difference

between his interests and proclivities and those that were reflected in contemporary researches on kinship and marriage. His image of primitive culture as a jumble of variegated customs unified primarily by the superstitious beliefs generated by the all-prevailing principle of animism had no place for kinship and social organization. These topics are not even mentioned in his massive and famous book *Primitive Culture* (1871), whereas Morgan, in his work, reverses the emphasis and treats religious beliefs and ideas as secondary attributes of gentile and political structure.

We can see now that what Tylor lacked was the idea of a social system, of a society as a system of interconnected institutions that regulate social relations and embody norms of right and duty. I hope I will not be accused of invidious imputations if I add that this deficiency, transmitted through Frazer, Haddon, Boas, Kroeber, and others, has continued to characterize much of what goes by the name of cultural anthropology. Maine had the idea of a social system by way of the logical and analytical apparatus of Roman jurisprudence, though he too dressed up his findings in the positivist and evolutionist idiom of his time. Morgan came to it via the paradigm of formal regularity he discerned in kinship terminologies—influenced too, no doubt, by his thorough study of Roman civil law. Tylor, on the other hand, had only his evolutionist orientation and his comparative procedure to bring order into the atomistic assemblages of custom which he brought together. When, eventually, he came out with his famous paper on marriage and descent (1889), he still adhered to his method of dismembering institutional complexes and correlating (albeit now numerically) the traits he separated out. But the idea of a system, which Morgan and Maine had, is the key to the subsequent development, through Radcliffe-Brown and his successors, of our current structural theories.

These comparisons could profitably be extended but there is no need for this.[7] I have paused to consider them because I wanted to illustrate what I mean by examining our predecessors' work in an historical spirit. When we do so, we do not of course divest ourselves of all that we have learned since their day. It would be false, and indeed silly, to think that we can effectively put ourselves back in their place and time. We can only evaluate them fruitfully for our own instruction if we judge them frankly by our own standards of theory and practice.

This holds even more strongly if we approach their work in what I should like to call a genealogical spirit. I mean by this the frame of mind of someone who traces back his ancestry to see what he has got from it. As we all know, Radcliffe-Brown and Lowie, the two outstanding authorities in the generation just antecedent to ours on the subject which Morgan opened up for science, were inspired to study kinship and social organization by Rivers, who was thus passing on the insight he had derived from Morgan's original researches. If this is not tantamount to transmission "through the blood" (if I might be allowed to parody one of Morgan's more fanciful notions), it is, without disrespect, almost an apostolic succession.

To read Morgan in this spirit, but without renouncing our own criteria of theory

7. For example, by including Herbert Spencer, McLennan, and other nineteenth-century protagonists in the controversies concerning social evolution. For a concise assessment of their significance today see Evans-Pritchard's *Social Anthropology*. The intellectual climate in which their theories developed and the specific contributions of Maine, Spencer and Tylor, to evolutionist ideology are authoritatively analysed in J. W. Burrow's recent (1966) book.

and fact is no mere gesture of piety. For me it was a revelation. It made me realize that two distinct lines of descent are represented in the intellectual heritage of modern social anthropology.[8] I see one as going back through Radcliffe-Brown, Lowie, and Rivers, to Morgan and Maine in particular, and the other as going back through Kroeber, Malinowski, and Frazer, to Tylor and to some extent Boas. I see the first line as the source of our structural concepts and theories, the second as the source of our speciality in the study of the facts of custom, or culture. Naturally, I am oversimplifying, leaving out many names on the family tree that are no less distinguished and influential than those I have cited, not to speak of the collateral connections to Durkheim, Weber, and Freud. I do this to pin down my argument. But do let us remember that a double descent system specifies a person's place and capacity in society along both coordinates simultaneously. Even if a position has no value on one axis, it still signifies a stance in relation to what that axis stands for. We must never forget that "structure" and "culture" stand for indissociably conjoined frames of analysis in our studies. What matters is how they are mutually balanced in any particular kind of inquiry.

VII

In short, if we want properly to understand in what ways Morgan's work remains significant for us, we must look behind the intellectual conventions and the dominant forms of thought of his time, those in which he dressed up his observations and ideas, and we must try to see what he was really getting at. We must, as it were, read between the lines of his writings. For one thing, just as there was in the 1930's, and still is in some quarters, an anti-hero prejudice against Morgan, so there also was (and is) a hero-worshipping reverence for him in other quarters;[9] and both attitudes distort our judgment. For another, the historicist position[10] (better still, illusion, as Radcliffe-Brown used to say) has a mysterious fascination. When we try to understand human behavior in any of its manifestations, we are apt spontaneously to slip into looking for explanations of what is before our eyes—in the "here and now" of custom and behavior—in terms of sequences of antecedent actions and circumstances. I imagine that it is a tendency which is a projection outwards of the individual's experience of growth and change within the continuity of his personal identity. Professional historians have a good excuse for following this pattern, though among them too there are now many dissidents who have aligned themselves to all intents with functionalist and structuralist methodology (cf. Carr, 1961). At the other end of the continuum of human existence, in dealing with the phenomena of personality and individuality, psychoanalysis and genetic psychology have

8. As I pointed out in my Inaugural Lecture, *Social Anthropology at Cambridge since 1900* (1953a).

9. Again, I am not referring to his Marxist admirers but cf., e.g., Roland B. Dixon's "Some Aspects of the Scientific Work of Lewis Henry Morgan" (1919).

10. I use this term in the sense given to it by Professor K. R. Popper in his well-known work, *The Open Society and Its Enemies* (1945).

invested historically oriented theory with exceptional prestige.[11] In anthropology, however, it has been a perennial source of confusion, not only with regard to aims and methods but also with detriment to the tasks of empirical research. How it has also held back the advance of theory has been demonstrated in the generation-long debate begun with the joint assault by Malinowski and Radcliffe-Brown, fighting under the banner of functionalism, on the ultra-diffusionists, on the one hand, and on the Boasian empiricists, on the other.

If we now apply functional and structural criteria of analysis, we can see where the confusion resides and how Morgan himself was affected by it. It comes from failure to discriminate between synchronic and diachronic systems and relations. As a result, the things which are analytically discontinuous, but not dissociated within the synchronic frame of observation are interpreted as if they were diachronically related, therefore discontinuous in time, and therefore sequential. Maine had an inkling of this when he suggested, in criticism of Morgan, that it was not necessary to postulate that descent in the gens changed from the female line to the male line. "One of these two groups" he wrote "did not really succeed the other, but the two co-existed from all time, and were always distinct from one another. We must be careful, in theorizing on these subjects, not to confound mental operations with substantive realities" (1883; 1886 ed.: 287). A more familiar illustration of this point is Lowie's demonstration (1920) that territory and kinship are not successive but coexistent principles of political organization in simple societies. Morgan's supreme vice of method was to leap indiscriminately from what was effectively synchronic observation to pseudo-historical deduction. But his scrupulous regard for the facts as observed, coupled with his logical naïveté, make it quite easy to separate the one from the other.

This is the frame of analysis I shall apply in order to disentangle from his work and thought the ideas and conceptions which helped to shape our modern theories and methods. I shall examine *Systems of Consanguinity and Affinity* and *Ancient Society* by this procedure,[12] and I hope to show that, surprising as it may sound, if these works of Morgan are examined in a genealogical spirit and in the light of subsequent developments, they can be seen to constitute the basic charter of modern structural theory in social anthropology. My interest does not lie in the fact that

11. The conceptual and methodological parallels between historical and psychogenetic forms of explanation are classically exhibited in such works as Freud's *Totem and Taboo* (1913) as well as in Tylor's and Frazer's writings. It is interesting to find that Rivers was aware of this and even went so far as to argue that they are complementary. In *The History of Melanesian Society* (1914b: vol. II, pp. 6–7.) he declared that the "historical study of human culture" should go on "side by side" with its psychological study as each casts light on the other.

12. What I am here stating is a point of view often expressed by Radcliffe-Brown and expounded with his usual lucidity in a lecture on "The Development of Social Anthropology" which he gave at Chicago in December, 1936, and which was not, to the best of my knowledge, ever published. In the mimeographed record kindly made available to me by Professor Fred Eggan, Radcliffe-Brown explains how "pseudo-historical" theories are intelligible from a logical and scientific standpoint if they are thought of as "logical fictions" cast in a form that fitted in with the philosophical notions and intellectual conventions of the time. Eighteenth-century social philosophers, such as Hume and Dugald Stewart himself, were, he adds, quite aware of this. The confusion between suppositious history and true history seems to have developed in the nineteenth century, no doubt as a result of the coincident emergence of evolutionary biology and historical scholarship.

Morgan broke new ground ethnographically in the study of kinship and social organization, though I agree with those who consider *Systems* to be an imposing compendium of empirical field data judged by any standards. What I specifically wish to examine is an aspect of its construction that has hitherto been almost entirely ignored. To my mind this book (and *Ancient Society*, too) is remarkable for the intuitive analytical methods developed in it. It is a method which I claim to be thoroughly congenial to our current theoretical approaches and which indeed foreshadows them. It reflects a procedure and a theory which, in my view, are recognizably structural in our accepted sense today, if only embryonically so. Of course, there is all that bizarre nonsense about "streams of the blood" and all that fancy guesswork about the historical origins of the American Indian peoples. There is also the master hypothesis derided by Lowie, and curtly dismissed by Radcliffe-Brown as "one of the most fantastic in a subject that is full of fantastic hypotheses" (1941: 59), that is to say, the assumption that classificatory kinship terminologies are the precipitates of extinct forms of marriage and family institutions. It is salutary to remember, however, that this hypothesis was accepted by a number of highly qualified ethnologists, including no less a person than Rivers himself. It survives in Soviet ethnology as a fixed premise of kinship theory; and it is perhaps no exaggeration to see a trace of it in the priority given to the relations of marriage over those of descent in the theories of Lévi-Strauss and his followers today (Lévi-Strauss, 1949; cf. also Dumont, 1957). I mention this because we know well enough now that kinship terminologies do, in certain respects, reflect marriage rules and practices, though not in the way that Morgan asserted, and that the elucidation of these conceptions was in no small measure due to confronting his erroneous views with the facts of field research. This is a warning against the doctrinaire rejection of what are thought to be false theories which Malinowski, for example, was guilty of in respect to Morgan. Properly reinterpreted, they may stimulate valid discovery. It is my aim to demonstrate that Morgan requires and repays reinterpretation in the light of hindsight conferred by the later developments in anthropological theory and research. More precisely, what I propose to show is that the development in the study of kinship and social organization which culminated in what we today describe as structural theory has its roots and precedents in Morgan's work.

VIII

There are signs that we are coming to the end of this phase. New prospects are opening. Yet the continuity is, to my mind, unmistakable. I can easily imagine some future Kroeber—looking back fifty years hence on the first century of scientific anthropological research and theory in the field of kinship and social organization—seeing it as all of one piece. I can imagine him describing it as the Morganian classical period and setting its termination in our present decade. He will no doubt see it as subdivided by a succession of critical events. The first will be Rivers' reinstatement of Morgan as the discoverer of the key significance of kinship systems in human social organization. He will single out Rivers' insistence on Morgan's

principle that kinship terminologies and customs depend on social causes, have social functions, reflect socially ordained rights and duties, as against Alfred Kroeber's earlier view that their connotation is purely psychological and linguistic. This latter-day Kroeber would, I believe, next select Lowie's early work *Primitive Society* (1920) and show how its general theme and the particular topics he dealt with derive straight from Morgan. He will not fail to perceive that Lowie's great service to our science was to map out the subject matter that falls within the ambit of the anthropological study of kinship and social organization, delineate its constituent parts, and set out the problems to be investigated in this field. After Lowie he will surely regard as the next landmark Radcliffe-Brown's *Social Organization of Australian Tribes* (1930–31). He will be struck by the close parallels between Radcliffe-Brown's and Morgan's frames of analysis, noting in particular the crucial importance both attach to the concept of system in the study of kinship. I imagine him going on to the next landmark, which in my view, is the Chicago volume on *Social Anthropology of North American Tribes* (Eggan, 1937a), in which Radcliffe-Brown's conceptual framework and analytical methods are tested out in an ethnographic area that was peculiarly Morgan's. And here he will note how the separation of the synchronic dimension of social structure from the diachronic dimension, which—as I hope to show—was intuitively grasped by Morgan but smothered in his presentation, is made a basic rule of procedure. He could hardly avoid contrasting this line of development with what might seem on the surface to be the very different one best epitomized in Malinowski's slight but arresting study of the psychological and cultural dynamics of the matrilineal family in *The Father in Primitive Psychology* (1927b). But coming next to our own times he will find in Lévi-Strauss's ambitious work, *Les Structures Elémentaires de la Parenté* (1949), these two tendencies reconciled in a new leap forward and he will note the dedication to Lewis H. Morgan as testimony of the author's recognition of his scientific ancestry. From the point of view of such an historian, I think the conclusion will be inevitable that structural social anthropology has its roots and its origins, conceptually no less than historically, in Morgan's work.

I began with a reminiscence of how Lewis Henry Morgan was first presented to me and to many of my contemporaries as the arch-enemy of all we stood for, worthy only of ridicule and contempt for his preposterous theories. To counter blinkered prejudice with blind hero worship is absurd and I have no intention of playing this game. My position is, quite simply, that, far from being an anachronistic hindrance to the development of modern theory in social anthropology, Morgan's discoveries and procedures of analysis in fact foreshadowed it and set the course for it in important and fundamental respects. "As the births of living creatures at first are ill-shaped," Sir Francis Bacon wisely warns us (*Essays*, 24), "so are all innovations which are the births of time." We must not be deceived by the unprepossessing shape Morgan adopted for presenting his discoveries, and so fail to grasp their true significance.

CHAPTER II

The Line of Succession: From Morgan to Radcliffe-Brown

I

MORGAN HAS BEEN A CONTROVERSIAL FIGURE IN ANTHROPOLOGY, IN SOCIAL philosophy, and in political theory for nearly a century. A large and varied literature has accumulated concerning him and his theories. I have suggested that it would be profitable to take a fresh and unprejudiced look at his major treatises, ignoring his speculative hypotheses and making allowance for the backward state of ethnographical field research in his day. To do this it is necessary to put aside the critical, exegetical, and expository literature to which I have previously referred, and begin from the beginning. I say this in no spirit of disparagement of the many eminent scholars who have contributed to this literature, for, as stands to reason, some of my conclusions have been anticipated by them. My main thesis, however, has, I believe, hitherto escaped adequate recognition. I claim that Morgan's substantive discoveries and intuitively elaborated methods of analysis constituted the foundations of what we now call structural theory in social anthropology. I maintain that the analytical procedure implicit in his work foreshadowed and stimulated, in a striking manner, the development of theory which we owe above all to the lead given by Radcliffe-Brown. The proof, as I hope to show, lies in the Morganian sources.

It is essential to return to the sources because Morgan has been consistently

misunderstood, or rather misread, by generation after generation of scholars. I am not thinking here of the hypnotic attraction his conjectural stages and fanciful evolutionist ideas had—and still have—for many doughty and learned investigators. For this has been largely a matter of preconceived doctrine and ideology. (A good example is V. Gordon Childe's attempts to adapt and preserve Morgan's pseudo-historical framework.[1])

I am thinking rather of how Morgan's concepts and hypotheses in relation to kinship and social organization have been misconstrued by anthropologist after anthropologist in the past sixty years, not excluding Rivers himself. We owe a piquant example of this to Leslie White (1958). The distinction Morgan drew in exact and rigorous terms between classificatory and descriptive systems of terminology was and remains the linchpin of his and of subsequent kinship theory. Yet anthropologists of the highest repute and competence have, as White reveals, persistently misinterpreted it. His list—enlivened with telling verbatim quotations—of those who have made this mistake includes Kroeber, Lowie, Goldenweiser, and other leading anthropologists of their generation, and convicts even such contemporary authorities as Hoebel and Murdock of muddling the conceptual distinction. A common objection to it is that descriptive systems have classificatory elements; but, as White remarks, Morgan himself drew attention to this and explained it convincingly. It is not without significance for my theme that White specifically exonerates Radcliffe-Brown and his followers—among whom I am gratified to find myself named—from this error. He adds that, useful as some of the refinements of terminology proposed in recent studies may be, they do not cover or invalidate the distinction Morgan made, and I fully agree with him. In view of this widespread carelessness in the understanding of Morgan's basic concept, it is not surprising that the deeper theoretical implications of his work have escaped recognition.

II

Let us turn, then, to the first of Morgan's theoretical treatises, *Systems of Consanguinity and Affinity*. If I describe it as the basic charter of what subsequently became structural theory and method, this is not because it broke open for the first time in the history of the human sciences the golden vein of kinship and social organization for scientific mining. It is because of the procedure of exposition and method of analysis followed in the development of the argument. Crucial to the whole, and fundamental for later development, is the empirical basis of field observations, whether Morgan's own or those of his correspondents.[2] We must not lose sight of

1. Cf., e.g., Childe's *Social Evolution* (1951). I cite him because he was an erudite and balanced scholar distinguished among the archeologists of his time for his understanding of modern anthropological ideas.

2. Morgan's own description of how he first came upon the fact of classificatory kinship terminology and established that it was not a unique and peculiar Iroquois custom but probably general among American Indians, has often enough been referred to in anthropological literature to need no attention here. This applies also to his account of how he

this, nor of the fact I have previously emphasized, to wit, that Morgan worked with systems of relationships, not with traits of custom. He dealt with taxonomically distinguished units of human social organization, that is, societies, not with isolated properties or characters of man's behavior. But his investigations proceeded at two levels. At one, he was concerned with the internal constitution of systems and families of systems of relationships; at the other, he sought for the congruences and connections between systems and families of systems. But like Darwin and the evolutionary biologists before the advent of modern genetics, he failed to see that these correspond to both logically and analytically discrete dimensions of social organization. The result was the confusion of aims symptomatized in his pursuit of the chimera of pseudo-history.

Morgan introduces the concept of the *classificatory system* right at the beginning of the treatise (p. vi). Characteristically and significantly, it is presented in juxtaposition to the complementary concept of the *descriptive system*, which is defined as the "reverse" of it in "fundamental conceptions"; and each is said to contain "a plan, for the description and classification of kindred." After this, the master hypothesis of primitive promiscuity and group marriage is introduced; but it is tacked on, avowedly as a conjectural inference.

Morgan was a punctilious, even pedantic writer, as the much corrected and rewritten manuscripts of *Ancient Society* in the library of the University of Rochester testify. Now there is one thing about pedants which one must not overlook. They weigh their words carefully. Thus when Morgan writes of a "plan," there is a point to it which no other words would convey. This is evident from the way in which the statement I have quoted is repeated over and over again, like a formula, sometimes superfluously, frequently with the inconsequential corollary that "the important question . . . [is] how far these forms become changed with the progressive changes of society" (p. 13).

Let us consider a typical passage from the treatise in which he expounds the meanings he attaches to his key concepts. I quote the explanation which introduces the discussion of the "Ganowanian" system, but similar pieces abound *passim:*

In contradistinction from *descriptive* the term *classificatory* will be employed to characterize the system of consanguinity and affinity of the Ganowanian, Turanian, and Malayan families, which is founded upon conceptions fundamentally different. Among the latter families consanguinei are never described by a combination of the primary terms; but on the contrary they are arranged into greater classes or categories upon principles of discrimination peculiar to these families. All the individuals of the same class are admitted into one and the same relationship, and the same special term is applied indiscriminately to each and all of them. For example, my father's brother's son is my *brother* under the system about to be considered; and I apply to him the same term which I use to designate an own brother: the son of this collateral brother and son of my own brother are both my *sons*. And I apply

obtained his world wide sample of kinship terminologies (*Systems*, pp. 4–6). How he came to write the book is related Leslie White (1957). Who can fail to admire the rigor and caution with which Morgan checked his inferences and set up the comparative survey by which to test and validate his hypotheses?

to them the same term I would use to designate my own son. In other words, the person first named is admitted into the same relationship as my own brothers, and these last named as my own sons. The principle of classification is carried to every person in the several collateral lines, near and remote, in such a manner as to include them all in the several great classes. Although apparently arbitrary and artificial, the results produced by the classification are coherent and systematic. In determining the class to which each person belongs, the degrees, numerically, from *Ego* to the common ancestor, and from the latter to each kinsman, are strictly regarded. This knowledge of the lines of parentage is necessary to determine the classification. As now used and interpreted, with marriage between single pairs actually existing, it is an arbitrary and artificial system, because it is contrary to the nature of descents, confounding relationships which are distinct, separating those which are similar, and diverting the streams of the blood from the collateral channels into the lineal. Consequently, it is the reverse of the descriptive system. It is wholly impossible to explain its origin on the assumption of the existence of the family founded upon marriage between single pairs; but it may be explained with some degree of probability on the assumption of the antecedent existence of a series of customs and institutions, one reformatory of the other, commencing with promiscuous intercourse and ending with the establishment of the family, as now constituted, resting upon marriage between single pairs.

From the complicated structure of the system it is extremely difficult to separate, by analysis, its constituent parts and present them in such a manner as to render them familiar and intelligible without close application. There are, however, several fundamental conceptions embodied in the system, a knowledge of which will contribute to its simplification. The most of them are in the nature of indicative characteristics of the system, and may be stated as follows: First, all of the descendants of an original pair are not only, theoretically, consanguinei, but all of them fall within the recognized relationships. Secondly, relations by blood or marriage are never described by a combination of the primary terms, but a single special term is applied to each of them. . . (pp. 143–44).

What I wish to draw attention to is the plan of the argument. If we brush aside the pseudo-historical and pseudo-biological interpretations of the data and examine the analytical side of the argument, we cannot but be struck by the way it is presented. It is done in terms of a dichotomous opposition quite on the lines of the most up-to-date precepts of methodology in linguistics and the social sciences. We might almost say that "descriptive" and "classificatory" are presented as the two terms of a binary opposition. Moreover, this opposition is consistently followed through. "Primary terms" are counterposed to "categories," and "lineal" to "collateral" relationships; and what is most important, the coherent and systematic nature of the data comprised within the conceptual scheme is particularly emphasized.

Thus what we have here, so far, is the identification of a couple of paradigms which are claimed, on the basis of the empirical evidence, exhaustively to delineate the universe and to specify within it the distribution of all known forms of "systems of consanguinity and affinity." The parallel with classical biological taxonomy is obvious, though it goes no farther than a rudimentary distinction such as that of metazoa and protozoa.

III

So much for the form of the argument. But if we look at the substance, we can see that the data adduced and the distinction Morgan draws between the two kinds of system pertain wholly and strictly to their synchronic constitution. The "plans" he elicits are synchronic in their reference; and it is my contention that Morgan intuitively understood quite clearly what this implies.

The procedure he follows in order to reach the general propositions he aims at witnesses to this. The essential step is embodied in the concept of system, as I have already suggested. It is no empty word for Morgan. His introductory remarks (pt. I, chap. II) show him grappling with the problem of justifying its use. The critical observation is the one which established the standard form of presenting a kinship terminology with Ego at the center. The way Morgan put it (quotations in this and the following paragraph are from Pt. I, chap. II–III) was that every person can be thought of as "the central point, or *Ego*, from whom outward is reckoned the degree of relationship of each kinsman, and to whom the relationship returns" in other words, as standing at the center of a "circle or group of kindred." It is thus a self-balancing and, *eo ipso*, systematic arrangement. Morgan repeatedly insists that this is a "formal arrangement" not *ad hoc* usage, that it is a formal and systematic method for distributing kindred in accordance with "lines of descent" and for "distinguish[ing] one relative from another." Moreover, he regards these systems as so fundamental for human social life that he believes them to have been "one of the earliest acts of human intelligence." Put in more modern terms, we might say that he considers kinship systems to be so intrinsic to social organization that they must be accepted as irreducible components of it.

Now one of Morgan's preconceived notions reflecting the values of his own time and culture, was that the "marriage of single pairs," as he called it, was the most natural as well as the most refined and advanced form of human mating. Single-pair marriage, he contends, gives rise to "definite . . . lines of parentage" and so to a "natural system" of degrees of proximity among the descendants of one pair which can be numerically computed. He recurs to this theme repeatedly, though its true import is often obscured behind the fustian phraseology in which it is clothed. The point is that descriptive terminologies reflect one kind of "definite ideas," those enshrined in the "natural system" ensuing from single-pair marriage. Classificatory systems reflect another set of definite ideas. It is these which make them into systems and, what is more, make them all systems of that one type. Translating again, what the argument asserts is that the essence of the dichotomous opposition lies in the definite ideas.

One instance of this line of thought must suffice. It is reiterated with Morgan's customary prolixity. Summing up his gratification at finding complete coincidence between the Seneca-Iroquois system and that of the "Dakota nations" he writes:

It thus appears that every indicative feature of the Seneca system is not only present in that of the Dakota nations; but that they are coincident throughout. The diagrams used to illustrate the Seneca-Iroquois form will answer for either of the Dakota