Origins of Agriculture

World Anthropology

General Editor

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CHARLES A. REED

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to

Robert and Linda Braidwood

General Editor's Preface

The growing threat to the planet and to humanity caused by the over-success of technology has generated severe doubts as to the entire notion of progress so popular in the Western world. But the idea, which seems to have been legitimate throughout our first three million years and which has become questionable only during the last fifty, has been one of species success in accumulating scientific knowledge and technology. And the largest of the giant steps forward, only 10,000 years ago, was the development of agriculture. To find out how that happened — to pull together what all the scientific disciplines have learned of the events and processes — was the object of the major conference reported so well in this book. The conference in turn was occasioned by an unusual congress which brought together scholars from every continent.

Like most contemporary sciences, anthropology is a product of the European tradition. Some argue that it is a product of colonialism, with one small and self-interested part of the species dominating the study of the whole. If we are to understand the species, our science needs substantial input from scholars who represent a variety of the world's cultures. It was a deliberate purpose of the IXth International Congress of Anthropological and Ethnological Sciences to provide impetus in this direction. The *World Anthropology* volumes, therefore, offer a first glimpse of a human science in which members from all societies have played an active role. Each of the books is designed to be self-contained; each is an attempt to update its particular sector of scientific knowledge and is written by specialists from all parts of the world. Each volume should be read and reviewed individually as a separate volume on its own given subject. The set as a whole will indicate what changes are in store for anthropology as scholars from the developing countries join in studying the species of which we are all a part.

The IXth Congress was planned from the beginning not only to include as many of the scholars from every part of the world as possible, but also with a view toward the eventual publication of the papers in high-quality volumes. At previous Congresses scholars were invited to bring papers which were then read out loud. They were necessarily limited in length; many were only summarized; there was little time for discussion; and the sparse discussion could only be in one language. The IXth Congress was an experiment aimed at changing this. Papers were written with the intention of exchanging them before the Congress, particularly in extensive pre-Congress sessions; they were not intended to be read aloud at the Congress, that time being devoted to discussions — discussions which were simultaneously and professionally translated into five languages. The method for eliciting the papers was structured to make as representative a sample as was allowable when scholarly creativity — hence self-selection — was critically important. Scholars were asked both to propose papers of their own and to suggest topics for sessions of the Congress which they might edit into volumes. All were then informed of the suggestions and encouraged to rethink their own papers and the topics. The process, therefore, was a continuous one of feedback and exchange and it has continued to be so even after the Congress. The some two thousand papers comprising *World Anthropology* certainly then offer a substantial sample of world anthropology. It has been said that anthropology is at a turning point; if this is so, these volumes will be the historical direction-markers.

As might have been foreseen in the first postcolonial generation, the large majority of the Congress papers (82 percent) are the work of scholars identified with the industrialized world which fathered our traditional discipline and the institution of the Congress itself: Eastern Europe (15 percent); Western Europe (16 percent); North America (47 percent); Japan, South Africa, Australia, and New Zealand (4 percent). Only 18 percent of the papers are from developing areas: Africa (4 percent); Asia-Oceania (9 percent); Latin America (5 percent). Aside from the substantial representation from the U.S.S.R. and the nations of Eastern Europe, a significant difference between this corpus of written material and that of other Congresses is the addition of the large proportion of contributions from Africa, Asia, and Latin America. "Only 18 percent" is two to four times as great a proportion as that of other Congresses; moreover, 18 percent of 2,000 papers is 360 papers, 10 times the number of "Third World" papers presented at previous Congresses. In fact, these 360 papers are more than the total of ALL papers published after the last International Congress of Anthropological and Ethnological Sciences which was held in the United States (Philadelphia, 1956).

The significance of the increase is not simply quantitative. The input of scholars from areas which have until recently been no more than subject matter for anthropology represents both feedback and also long-awaited theoretical contributions from the perspectives of very different cultural, social, and historical traditions. Many who attended the IXth Congress were convinced that anthropology would not be the same in the future. The fact that the next Congress (India, 1978) will be our first in the "Third World" may be symbolic of the change. Meanwhile, sober consideration of the present set of books will show how much, and just where and how, our discipline is being revolutionized.

As well as a volume on the origins of plant domestication in Africa (edited by J. R. Harlan, J. M. J. de Wet, and A. B. L. Stemler), readers of the present volume will find especially interesting at least twenty-five books in this series on culture theory, adaptation and evolution, and on the archaeology, history, and ethnology of several areas of Asia, Africa, Oceania, and the Americas.

Chicago, Illinois SOL TAX
September 7, 1976

Preface

My decision to add a final chapter to this book, in an effort to pull together the data and thoughts of the individual authors of the several chapters, has delayed publication by at least a year, and I owe this note of apology to those authors, who will have waited for more than three years from the time of their writing to the day when they can see their articles in print. The only excuse for the delay is one which is true for any professor with a rather heavy load of teaching and advising, plus other academic duties: what he appears to be doing is only the tip of an iceberg; there is much to do and little time for the doing.

If this book serves to clarify the issues and present the problems of agricultural origins for botanists, zoologists, anthropologists, demographers, agricultural historians, and paleo-environmentalists in such a way that new students become fascinated with the challenges, it will have accomplished its purpose.

Many persons contributed to the success of the meeting held at the Woodstock Conference Center and to the subsequent work which went into the book that emerged rather slowly from the proceedings of that conference and from the chapters submitted by the individual authors. Ms. Judith Krysko was the efficient secretary for the conference and has continued her interest and active participation by typing and sometimes retyping many of the chapters and bibliographies during the intervening years. Others, too, have been called upon for typing and retyping; of these numerous helpers I signal out Ms. Elvira Bayod, graduate student in anthropology at the University of Illinois at Chicago Circle, for her particular industry. Ms. Adina Kabaker, who was a member of the conference and is also a graduate student in anthropology at Chicago Circle, spent more than one hundred hours doing proofreading, besides being helpful in many other ways. Ray Brod, cartographer for the Department of Geography, Chicago Circle, was the skillful artist who

drew the figures for the last chapter. Karen Tkach, of Mouton's editorial office in The Hague, was responsible for patiently (and sometimes impatiently) pushing the often-inert editor toward finishing the last chapter.

Finally, a great debt of thanks — a debt hitherto unknown to all members of the conference other than myself — is owed to my wife, Lois Reed, for without her efforts no conference could have occurred. Conferences cost money: people must be transported from various parts of the world, housed, fed, entertained to some degree, and sent home again. All of my own efforts to find the several thousands of dollars necessary to have a conference were unavailing; I spent two summers trying, and failed. My wife then succeeded, by an appeal to a donor who wishes to remain anonymous.

Chicago, Illinois CHARLES A. REED
July 19, 1976

Table of Contents

Introduction

CHARLES A. REED

This volume has resulted from a conference on the origins of agriculture held prior to and in conjunction with the IXth International Congress of Anthropological and Ethnological Sciences.

The conference has had its historical origins in my shared field experiences in the Near East with Robert and Linda Braidwood,[1] Bruce Howe, Herbert Wright, Hans Helbaek, Jack Harlan, Patty Jo Watson, Kent Flannery, Frank Hole, Charles Redman, and numerous others. One of the avowed purposes of the expeditions of the Prehistoric Project of the University of Chicago's Oriental Institute, expeditions organized and directed by R. Braidwood (1972), was the exploration, through field archaeology and studies in biology and environments, of the origin and early history of agriculture, with an emphasis on the Near East. At several informal meetings, mostly at the Braidwoods' pleasant country home in Indiana, numerous people interested in Near Eastern prehistory continued their discussions; these discussions, our field experiences, and our various stimuli from teaching and reading have broadened our horizons. My own interests, at first concentrated upon problems of domestication of animals, expanded to include the fascinating problems involved with the changing pace of cultural evolution, the origins of plant agriculture, and the changing pattern of the environment, not only for the Near East (where I am most knowledgeable) but for the whole world.

The path leading to this conference became more definite following a

[1] The Braidwoods, in their influence on their students and then both indirectly and directly upon their students' students, fit well the felicitous phrase, "The lengthened shadow of a man and his wife," used so admiringly by Needham (1946) in his short biography of John and Anna Comstock of Cornell University.

meeting at the Oriental Institute in October 1969, when several of us involved in the studies of the late prehistoric period in the Near East presented short public talks to an interested audience. My own unexpressed feeling that we were being quite superficial was, unknown to me at the time, shared by Wright, who subsequently circulated by mail a mimeographed outline (Wright 1970), in which he stressed his firm belief that, at least in the Near East at the end of the Pleistocene, cultural evolution in general and agricultural origins in particular were closely correlated with changes in environment and probably dependent on these. Such environmental determinism was not popular then, nor is it any more so now, but Wright has stuck to his thesis, as can be seen by his paper in this book.

At the time, Wright and I shared duties on a committee that met once or twice a year in Washington, D.C. As a result, we also shared waiting time and talking time in airports, and the idea for this conference was born directly from the resultant conversations.

Antecedent to the history outlined above was my experience as a farmboy. Hardly more than a toddler, I hunted for hen's eggs, rode the horses home from the field, slid on the hay in the barn, watched the rooting of pigs and helped with their feeding, played in the water of irrigation ditches, and pushed my way through fields of grain higher than my head. Later I harnessed the horses, plowed the furrows, planted and harvested the hay, dug the ditches to control the water, milked the cow, picked the fruit, butchered the pigs, fertilized the fields with the animals' manure, and prepared and tended the garden. The toil was long, hard, and financially unrewarding, but I became steeped in the knowledge and the emotion that comes to man with the growing of plants and animals, the annual cycle of the life of a farmer. When I reached the Near East I was once again with farmer folk, who functioned as they and their ancestors had for millennia, and as we excavated in the prehistoric villages — Jarmo, Sarab, Banahilk, Gerikihaciyan, Çayönü, and others — I felt kinship with the people of those villages; they too had lived the annual cycle of planting and reaping of plants, of births and deaths of animals, gaining their bread and their meat by the sweat of their brow.

A man is led to wonder, each of us at our conference has come by his own path to wonder: Why and how did man and his wife begin their farming? Why, after millions of years of hunting and gathering — with emphasis on the gathering — did man become a settled farmer? Why — the major question, why, why — did man in several parts of the world begin growing plants and domesticating animals at nearly the same time? After millions of years of hunting and gathering, the beginnings of domestication of plants and animals occurred within a period of only four thousand years in the Near East, southeastern Asia, northern China, south-central Mexico, and highland Peru (Table 1). Are these situations

Table 1. Chronology of earliest evidences of agriculture

Near East	?Domestic sheep, 10,750 B.P., Zawi Chemi Shanidar; cultivated emmer and einkorn from base of Çayönü, 9000+B.P., domestic sheep by 9000 B.P.
Southeastern Asia	Taro and rice by 8000 B.P. (or earlier?)
Northern China	Millet and pigs, 6000 B.P. (or earlier?)
Middle America	Summer squash, possibly by 9790 B.P., certainly by 9300 B.P., at Guila Naquitz Cave, Oaxaca
Peru	Gourds, beans, and guinea pigs by 7000 B.P.?; major domestiaction of squashes and cotton soon after 5000 B.P.

Dogs are not included in this table as dogs were domesticated by hunters and gatherers, independently of the processes involved in the origins of agriculture. Dogs may well have become domestic in southwestern Asia by 12,000 B.P., but we do not know how widespread domestic dogs may have been at that time.

related or independent? If related directly by cultural diffusion (see the paper in this volume by Carter on "a single origin of agriculture"), how did man, with the primitive technology most of us think typical of nine or ten thousand years ago, cross Great Ocean, the fearsome wide and lashing sea, from one hemisphere to another? If independent phenomena, what were the factors, presumably similar factors, which separately led people of diverse races and cultures to the same principles of preparing the ground, planting, harvesting, storing the seed, preparing the ground, and planting again? Was there something mystic, preordained, the farming following inexorably when a particular (but hitherto unexplained) level of cultural complexity had been reached? Can we rely upon such an obvious explanation as the general warming and associated environmental transformation at the end of the Pleistocene, with new opportunities offering a challenge and certain cultures having the necessary complexity to effect a response — the response being toward agriculture? Is challenge-and-response a valid principle in prehistory? What were the responses to such environmental changes at the end of the Pleistocene of the people who did not become farmers?

Why farm at all? Farming by hand, or even with the help of domestic animals, has always been hard work, and mostly dull, whereas hunting-and-gathering — as numerous authors delight in telling us — provides the necessities of life with much less exertion, and, at least for the masculine hunters, is often exciting and also the stuff of which myth is made.

We find the contrast embedded in English mores and English literature; compare for instance:

Homeward plods the weary ploughman[2]

with the excitement of the hunter in pursuit of his quarry:

The stag of warrant, the runnable stag,
The runnable stag with his kingly crop,
Brow, bay and tray and three on top,
The royal and runnable stag.[3]

The farmer can only be exhorted to be steadfast, and plan his life with care:

Ye rigid ploughman bear in mind
  Your labour is for future hours:
Advance — spare not — nor look behind —
  Plough deep and straight with all your powers![4]

But the hunter ever exults in his skill:

An archer keen I was withal,
  As ever did lean on greenwood tree;
And I could make the fleetest roebuck fall,
  A good three hundred yards from me.[5]

The pattern continues; a recent author makes the contrast on opposite pages (Shepard 1973: 154–155):

Although it has long been fashionable to describe it so, the world of the hunting and gathering peoples is not a vale of constant demonic threat and untold fears. It is a life of risk gladly taken, of very few wants, leisurely and communal, intellectual in ways that are simultaneously practical and aesthetic. Most pertinent to our time, it is a life founded on the integrity of solitude and human sparseness, in which men do not become a disease on their environment but live in harmony with each other and with nature. The ways of the hunters are beginning to show us how we are failing as human beings and as organisms in a world beset by a "success" that hunters never wanted.

On the next page, the same author continues:

The capacity to learn to do brain surgery or play the piano may represent final touches added by our species. If so, the activity responsible for that final and delicate perfecting is probably the making and using of tools by early man for

[2] Thomas Gray, "Elegy Written in a Country Churchyard."
[3] John Davidson, "The Runnable Stag."
[4] Richard Henry Horne, "The Plough."
[5] Thomas Love Peacock, "Friar's Hunting Song."

killing, dissecting, and utilizing animals. Tools associated with the seasonal harvest of domestic grains and preparation of plant materials are gross by comparison. The "good hands" of the hunter is not a familiar image, yet he is the surgeon. Tuber grubbers and soil tillers have hands calloused, arthritic, swollen, and otherwise deformed by their work.

But for the man who thinks and writes thus, the mystique has indeed worked magic in his mind; no population dependent upon hunting and gathering ever produced a culture with brain surgeons or piano players. Only agriculture, with its pattern of population growth, urbanization, and economic surpluses has produced civilization. Our brain surgeons and our pianists come straight from generations of farmer folk, as did I myself, with my dissector's hands.

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SECTION ONE

General Principles

The Origins of Agriculture: Prologue

CHARLES A. REED

The process of living involves the directed control of the acquisition and use of energy. In the long history of the cosmos, energy flows from centers of concentration to regions of diffusion, but in the process, as Homer Smith (1932) so succinctly said, life is a temporary eddy in the second law of thermodynamics, a temporary — but only a temporary — reversal of entropy.

Each protoplasmic entity must find its own energy or, as with a green plant, be placed in such a position that energy comes to it. Animals find energy by finding food; they eat (*sensu lato*). An amoeba surrounding and ingesting another protozoan, an octopus catching a crab, a cow grazing in a pasture, a fox eating mice, and a man picking and eating wild berries are all akin; they are using their individual protoplasm, their own private protoplasmic system, to provide themselves with the energy necessary for their life processes. Each is a "primary energy trap," not in any sense in relation to its position on the food web — that concept is not involved — but simply because it acquires energy via food by no other means than its own protoplasm. Also, to the degree that it maintains its own continuing individuality (escapes enemies, conserves energy) by using only its own protoplasm and its own cellular system, it functions as a primary energy trap.

Most organisms, whether prokaryote or eukaryote, plant or animal, unicellular or multicellular, are simply primary energy traps; the function without accessory nonprotoplasmic devices. Evolution is adaptive, obviously, and a considerable variety of complex structures has evolved which, while not strictly living, are still an integral part of the protoplasmic system. Bone, for instance, is internal, replaceable, repairable, porous, filled with tissue fluid and permeated by cells, and at the molecular level is a dynamic part of the biochemical system, even though the actual crystals and spicules of bone are extracellular. Bone — even the bone of an armadillo's "shell" — is a functioning part of the inner animal, and is thus part of the primary energy trap.

By contrast, a variety of noncellular structures or merely things, produced by the organisms or existing naturally in the environment, is utilized by animals for the acquiring or conserving of energy, and these can be called "secondary energy traps." (Tools, to be discussed later, are a special kind of secondary energy trap.)

A total catalog and discussion of secondary energy traps would fill a volume larger than this book, but some examples are: secreted, noncellular and nonliving external tests or shells, such as those of Foraminifera and Mollusca; natural cover, holes, crevices, etc., sought and utilized by animals for protection or by a predator for concealment, or similar structures (burrows) constructed by an animal for the same purposes; external secretions (mucus, silk, perspiration, body oils, a variety of toxins); tools, either objects naturally occurring in the environment and used without modification or those shaped and thus manufactured; social behavior, whereby the energy expended by the individuals of a group is pooled between them and benefits can accrue thereby which would be impossible for the lone operator (group hunting, food sharing, systems of communication, aid from kinfolk, economic networks, etc.).

Different animals accomplish the same ends differently, some with primary, some with secondary energy traps. A few such contrasts, as examples, are outlined in Table 1.

As typical of evolutionary sequences, a structure evolved in correlation with one function may be modified by secondary or tertiary adaptations as the result of subsequent natural selection. Thus, the silk produced by spiders, ancestrally used as guidelines and to line burrows, in most groups also became a food-catching device, and in some the web then also serves as a channel of communication by which a male may approach a female without being attacked; he must activate the web in a way that a struggling insect would not. Another species of animal, only distantly related, may use some part of the web as a secondary energy trap of its own, to become cross hairs in a transit; this same species takes the stuff of which cocoons are made, a secondary energy trap produced by an insect larva, and modifies that silk into its own secondary energy traps, to win a mate or buy an emperor's favor.

Modifications of behavior, as with the actions of the male spider mentioned above, are often part of such continuing evolution of secondary energy traps; they serve to continue the animal's energy system, which otherwise might be abruptly terminated. Thus, many animals secrete noxious substances to protect themselves from predators. Such poisons are obviously secondary energy traps, protecting the animal's own energy system from oblivion. Some animals, however, improve their efficiency by modifying both behavior and morphology; thus, the poison may be sprayed over an attacker (the bombardier beetles), or an appendage is used to wipe the poison on aggressors (one species of harvestman, or daddy longlegs; Eisner, et al. 1971). Many types of behavior, that of both solitary and social animals, are further examples of the principle that secondary energy traps are subject to natural selection; they have evolved and continue to do so.

From the viewpoint of reproduction, parents (and often other adults) are secondary energy traps, providing at the minimum some food in an egg, and often additional food and many services (protection, teaching) to the young. A nursing mother, to a baby, is a secondary energy trap. Many and complex are the secondary energy traps by which genetic (and in some cases cultural) endowments are passed from one generation to another.

These examples undoubtedly can be found in all phyla of animals — I can think of one such in the Protozoa, for instance — but mention of a few cases among the vertebrates will illustrate the principle. Among the primitive jawless fish, male lampreys prepare a depression ("nest") on the stream bottom in which the female spawns (Brigham 1973); salmon and many other bony fish do the same. This minor depression is some protection to the eggs and thus, to the hatching young, is a secondary energy trap. The eggs of many frogs are laid in a mass of noncellular jelly — a secretion of the female's reproductive system — which inhibits predation. The young of reptiles and birds are protected by an enveloping shell, and usually are deposited in nests, some extremely elaborate. Universally among mammals and almost universally among birds, the young are furnished food, and in many such animals are additionally given the time and effort of teaching. Among a few animals, humans for instance, the expenditure of energy by parents on the young continues long after the latter have reached reproductive age. Sometimes, one thinks, parents are little more than secondary energy traps for their offspring.

Tools, however defined, are all secondary energy traps. In dictionaries and in the numerous articles on tool using among animals, one finds lack of agreement on the precise meaning of the word "tool" (Alcock 1972). I do not intend here to pursue this semantic topic; van Lawick-Goodall (1970) has discussed the problem to some degree in her excellent summary

Table 1. Some comparisons between primary energy traps and secondary energy traps

Animal	Action or function	Primary energy trap	Secondary energy trap
Most predators of ostrich eggs	Breaking of egg of ostrich	Use of body parts	
Man and Egyptian vulture[a]	Breaking of egg of ostrich		A rock, thrown at the egg
Whalebone whales	Filtering krill	Use of whalebone[b] and tongue	
Most filter feeders[c]	Filtering plankton	Use of part of body as filter	
Some filter feeders	Filtering plankton		Mucous traps as filters[d]
Robber fly	Catching insects	The robber fly is a direct predator	
Orb-weaving spider	Catching insects		Use of spider web as a trap
Trap-door spider	Catching insects		Prepared, silk-lined burrow with hinged door[e]
Polar bear	Protection against cold	Fur[f], fat, warm blood	Seeks or prepares shelter, particularly for sleeping
Man	Protection against cold	Fat, warm blood	Clothing, dwelling, fire[g]
Some insects	Protection between stages	Pupa case[h]	
Some insects	Protection between stages		Cocoon[h]
Most crabs	Protection against aggressors	Use of claws; escape	Threat, a direct communication
Hermit crab *Dardanus*	Protection against octopus		Sea anemone *Adamsia*, placed on adopted shell[i]
Hermit crab *Pagurus*	Protection against larger hermit crabs	Tolerance of commensal hydroids which live on some abandoned snail shells[j]	
Eolidoid nudibranchs	Protection against aggressors		Discharge of stinging cells (nematocysts) derived from consumed coelenterates[k]

[a] J. van Lawick-Goodall and H. van Lawick (1966); see also Chisholm (1954) for reference to a similar practice of Australian buzzards in breaking emus' eggs.

of tool using among vertebrates. She regarded a tool as an object necessarily manipulated; thus, a rock thrown at or dropped on an egg is (in her opinion) a tool, whereas a rock against which an egg is thrown or upon which it is hammered is not a tool; knitting needles are tools, whereas yarn or the sweater produced by the knitting are not tools; a twig used by a chimpanzee to pull termites from a termite mound is a tool, but the nests the chimpanzees make — or that birds or other mammals make — are not tools.

Considering the problem from the viewpoint of secondary energy traps, the differences between these categories of tools and nontools (as used by

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[b] In the mysticete whales, the filter (the so-called "whalebone") is an epidermal structure, and, thus, cellular and originally living tissue.

[c] The filter, whether antennae or mouthparts or other, is a part of the cellular structure of the animal's body.

[d] Mucus is a secreted, noncellular, nonliving substance. Even the mucus of pharyngeal filter feeders such as tunicates and *Branchiostoma* (= *Amphioxus*) amongst chordates is a secondary energy trap, since the whole of the digestive cavity of any metazoan is not INSIDE the animal but is merely a part of the external universe that is surrounded by the animal. The fanciest filter feeder that comes to mind is that of a marine pteropod (a particular kind of free-swimming, shell-less snail) which spreads a filmy net of mucus in seawater and then consumes it along with the trapped plankton (Gilmer 1972).

[e] The "home" of the trap-door spider is of course a secondary energy trap in that it hides and protects the spider, thus conserving its energy and continuing its being, but the "home" is more; as a camouflaged lair from which the spider can spring upon its prey, the "home" is a basic part of the feeding pattern, thus having a double function as a second energy trap.

[f] Hair, like whalebone, is composed of cells; these, once alive, are a part of the body, not a noncellular secretion from it.

[g] Fire was the first chemical reaction (oxidation) used as a secondary energy trap by man. Although man was the first to use fire, the slower oxidation of rotting vegetation has been used for millions of years by various other animals to control temperatures of domiciles (as with some ants) or nests of eggs. See Clark (1964) for a most interesting case among birds.

[h] A pupa case is a part of the animal's body, but a cocoon, like a spider's web, consists of a nonliving, noncellular secretion.

[i] Reilly and Stone (1971). The abandoned snail's shell which the hermit crab adopts is also, of course, a secondary energy trap for the crab using it, as it previously was for the snail.

[j] *Pagurus*, where in competition with populations of larger hermit crabs, may have his adopted snail shell taken from him by one of the larger hermits. If, however, an individual of *Pagurus* can find an empty snail's shell with hydroids growing on it, he can probably occupy it safely, as the populations belonging to *Pagurus* have evolved a natural immunity to the poison of the hydroids, an immunity lacking in the species of larger hermit crabs, which sometimes attempt to occupy such a shell with hydroids but are soon forced to leave (Wright 1973).

[k] The nudibranchs (shell-less, noncoiled, surface-creeping marine snails) feed on hydroids, jellyfishes, sea anemones, and corals; the delicately triggered nematocysts of these coelenterates are passed intact through the wall of the digestive tract and then through the tissues of the nudibranch, to be stored in special sacs in spurs on the back, discharging finally against aggressors attacking the nudibranch (Zeiller 1971).

van Lawick-Goodall and by Alcock) are not so important. All of the examples listed above are secondary energy traps in that the various objects provide means for utilization or conservation of energy which would not be available to a particular animal were it limited to the use of its own body without the additional help of the external object.

Unless one wishes the meaning of the word "tool" to include all secondary energy traps, a definition of the subcategory intended is obviously necessary, and probably the concept agreed upon by van Lawick-Goodall and Alcock is the easiest, even if to my own mind unduly restrictive. I myself have always automatically thought, in agreement with Lancaster (1968), that nests of birds in particular, but also those of such mammals as build nests (chimpanzees, for instance) are tools; they are built by the animal of objects manipulated to form a structure, and are used for a definite purpose. However, once one has crossed the line of the definition established so succinctly by Alcock — "Tool-using involves the manipulation of an inanimate object, not internally manufactured, with the effect of improving the animal's efficiency in altering the position or form of some separate object" — one would find difficulty, I can see, in locating another definitional boundary.

This digression into the use of the word "tool" is necessary in this introductory chapter because cultivated plants and domestic animals have sometimes been regarded as living tools of humans. By the above definition they would generally be excluded. A harness is a tool if used to pull a wagon which moves an object in a way that increases human efficiency; in this operation the wagon is also a tool, but the horse that wears the harness and pulls the wagon is not a tool. However, for the man involved, all three — horse, harness, and wagon — are secondary energy traps. Glue in a pot is not a tool (although presumably the pot is), but the same glue actually used in the process of manufacturing becomes a tool — or does it? Alcock stated that a tool must be an object, and perhaps glue is only a substance. A domestic animal would become a tool if one used a dead chicken to beat another chicken to death and then ate the second chicken. Such gentle chiding aside, the concept of tool use probably does have value in that manipulation of objects (and/or substances) producing changes which increase the user's efficiency would have selective value and thus may well be evolutionarily important. For the purposes of the discussion being presented here by me, the concept of secondary energy traps seems more fundamental.

With regard to feeding and food getting, an efficient technique for making energy available would be for the feeder to have control over the supply of food, whether that control be called husbandry, gardening, horticulture, herding, food production, or agriculture in general. Any action by the feeder which increases the yield of a food in a given area over the natural yield turns the particular plant or animal being fostered into a secondary energy trap for the feeder — the one who then utilizes the additional energy produced. The dominant population has, thus, evolved a mechanism for utilization by itself of a greater part of the energy available in a given environment than was available to its ancestors; the general trend that results is that the population increases over that of the ancestors.

Symbiosis and other kinds of mutualism are not rare in the living world; even such closely related intermutual benefits as the combination of an alga and a fungus to produce a lichen, or the combination of termite and intestinal flagellates to produce a wood-utilizing animal are not so rare. However, the propagation and protection of one species by another, to the benefit of both, ARE relatively rare in the biological world.

The leaf-cutting ants (genera *Acromyrmex* and *Atta*, of the tribe Attini) (Weber 1972) are the most specialized and the most successful of the "gardening" or agricultural ants. The ants cut leaves, blades of grass, or flowers, carry them to underground nests, clean them and chop them, force the pieces into prepared ground, and then transplant mycelia of a particular fungus onto the pieces so implanted. (In competition with human farmers, the ants are sometimes more successful, stripping fruit trees of their leaves.) The fungus grows luxuriantly on the rotting pieces of vegetation, and the ants thrive by eating the fungus. In parts of South America these ants plus the termites comprise the greater part of the animal biomass.

Other kinds of attine ants behave similarly, but use insect droppings or pieces of already-decayed vegetation upon which to grow their fungi. As termites are to flagellates, so are the attine ants to their fungi; neither insect can survive without its symbiotic organism. Each insect has its neural system programmed to maintain the symbiont; the termite, stripped of its necessary fauna by a molt, will beg and receive from a coworker an anal drop teeming with the necessary protozoans, and the ants instinctively accomplish all of the necessary complex activities to maintain their gardens. The difference, and the reason we call the ants agricultural, is that they prepare the soil, maintain proper temperature and humidity, and plant their fungus. In neither instance can one of the partners survive without the other, but the ants — instinct bound as they seem to be — are not without some modicum of versatility; a population of ants in the laboratory, denied vegetation but furnished with nutrient agar, utilized the unaccustomed substance, planted their mycelia, and successfully reared both fungus and a new generation of ants. Another group, denied their own species of fungus, adopted another species which before had been grown only by another kind of ant.

Numerous other kinds of ants have domestic animals; none of these ants are also horticulturalists, nor do any of the fungus-growing attines keep livestock. Thus, no ant is a complete agriculturalist; only man has achieved that unique capability. All of the ants' livestock (aphids, leaf-hoppers, and scale insects, mealybugs and other coccids) belongs to sucking insects of the order Homoptera. Each homopteran inserts a hypodermic-like proboscis into the phloem sap of a plant, and sucks much more fluid than it can use. The excess, sweet and nutritious because of contained sugars, fats, and proteins, is normally ejected in jets or droplets from the hindgut. (Dried, this plant sap becomes the "manna" of Exodus, and is supposedly still gathered and eaten by the Bedouins of Sinai.) In both liquid and dried form this "honey-dew" is utilized by many kinds of insects, but certain ants have entered into productive symbiosis with certain homopterans. Most such ants are restricted to one species or a few related species of such sucking insects, but many and strange are the intermutual adaptations (Michener 1951; Sudd 1967; Wilson 1971).

In general, the ants tend, guard, defend, and sometimes transplant their livestock. In return, the aphids particularly, but some of the other homopterans as well, learn not to eject their liquid or kick it away with their hind feet (their typical solution of an obvious problem of sanitation), but instead to wait for a herder, and, being stroked by the ant's antennae, let a drop ooze out gradually, to be sucked up by the ant. Indeed, if the ant is disturbed at the feast, an aphid will pull the drop back in. (Cows, which function on a different principle, cannot do this.)

The ants drive off predators, and sometimes, from earth and plant debris, build protective sheds or tunnels for their charges. Greater care is given by ants which nest underground to aphids which feed on rootlets; here the ants care for the eggs of the livestock, as they would for their own, maintain optimal temperature and humidity, and when the aphids hatch, a cleared area is prepared around a rootlet and the aphid is carried to the spot. Indeed, these underground ants are reported sometimes to clip the wings of the sexual, migratory generation of aphids, thus keeping the eggs in the nest.

Certain ants keep scale insects instead of aphids; of these, those in Java move their livestock as desired on their own backs; at a given tactile signal the tiny coccids climb nimbly aboard. A new queen, leaving the nest to start a new colony, will be carrying one or more of the scale insects from the parent colony.

While several examples are known of Homoptera which have not been found except in ants' nests, only one case seemingly is known where both the ant and its domesticate are completely dependent upon each other (Flanders 1957). This population of ant, which lives in Colombia, keeps a particular scale insect; neither ant nor coccid is ever found separately. The nests, which are underground, are always small, and both sanitation and increases of population present potential problems; the ants have solved these problems by rotating the scale insects at the feeding stations (rootlets); typically only 30 percent of the livestock is allowed to feed at one time. When a new queen leaves, she carries a scale insect gently in her jaws as she flies; without the proper "cow" the new colony would be a failure.

Thus, we see that while some ants are agriculturalists, profound differences exist between such ants and men: the ants, although capable of some learning by experience (as tested in the laboratory, Sudd 1967), generally function at an instinctive level; only a few kinds of ants, of one tribe, are gardeners, but many kinds of several subfamilies keep livestock. By contrast, all men belong to but one species, and many if not most human farmers keep one or more kinds of domestic animals while at the same time cultivating plants. In that practice of mixed farming, man is unique in the animal kingdom. Ants are generally limited to the agricultural practices of their nearer ancestors, and must depend upon the slow mechanisms of evolution for any change, while man is culturally adaptable.

Aside from man and the relatively few kinds of attine ants, horticulture is unknown among animals (insofar as I am aware), with the possible exception of the curious case of a marine amphipod (Crustacea), *Dulicha rhabdoplastis*. This tiny animal, a small relative of the better known beach-hopper or sand flea, builds its own elongate, cylindrical, diminutive "farm" on the tip of a spine of the giant red sea urchin *Strongylocentrotus franciscanus*, living on the bottom of Puget Sound, Washington, United States (McCloskey 1971). The farm consists of the tiny amphipod's own feces, carefully placed and glued into position to make an elongate rod, from 2.5 centimeters to almost 4.0 centimeters long. In the summer this rod supports a luxuriant growth of diatoms, a form of unicellular plant, upon which the amphipod feeds, keeping the population spaced by the eating of the larger individuals. Other organisms, which could foul the surface, are carefully removed. The amphipod is not an obligate farmer, for during the winter and in part during the summer it is a filter feeder, ascending to the tip of its rod and spreading its two elongate, multisetaed antennae at right angles to the bottom current. Bits of plankton caught in the intermeshing setae are scraped off as each antenna is drawn through the mouth.

We do not know that *Dulicha rhabdoplastis* is a true horticulturalist, for the incomplete studies to date have not produced evidence that the amphipod plants the diatoms; instead the situation seems to be more similar to several examples known from ethnographic studies, where man weeded and might otherwise protect a patch of esteemed natural vegetation. The amphipod, however, has gone a step beyond this simple pre-agricultural situation, for he carefully prepares an environment which is not only his own home but is an optimum place for the growth of one of his favored foods.

Although man has used individuals of his own species as slaves, in agricultural work and otherwise, thus converting them to secondary energy traps on an economic and social level with domestic animals, slavery is not necessarily correlated with agriculture, either among humans or other animals. Some ants are slaveholders; they raid the nest of certain other species of ants, capture the inert pupae, and bring these back to their own nests. The ants that emerge from the captured pupae then become slaves, procuring food for their masters and feeding and otherwise caring for their larvae. The slaves are not themselves used for food, nor are they bred in captivity. The populations from which the slaves are captured are not dependent upon the slaveholders, but the slaveholders cannot survive without slaves. In this case, the slaves are obviously involuntary secondary energy traps for their masters, but the situation does not involve agriculture; instead it is more akin to the case of men in parts of southeastern Asia who train macaques to climb coconut trees and loosen and drop the nuts for the men to collect (Bertrand 1967). The master gets the coconuts, while the slave, belonging to another species, escapes punishment and is fed as the reward for his success. Agriculture may be but is not necessarily involved, nor are the macaques domestic. They are wild animals that are tamed and trained, as also are elephants who are caught and trained to work.

Agriculture, which includes in the broadest sense the domestication of either plants or animals (or both), is not a common phenomenon. Horticulture amongst the ants was probably innovative in the population ancestral to the tribe Attini, and the practice there has had its own adaptive radiation, coincident with that of the several genera and species of that tribe. The case of the diatom-feeding amphipod, *Dulicha rhabdoplastis*, shows a possible avenue toward true horticulture among non-human animals, particularly arthropods.

With men as with ants, a plant or animal which is protected, reared, and maintained (whether truly captive or not) is a secondary energy trap if it yields a return in energy. Insofar as men or ants furnish labor or protection or food or fertilizer for their charges, the dominant species serves in turn as a secondary energy trap for the domesticate! Man, however, is a canny beast; he will not long serve as a secondary energy trap for a domesticate if the return be less than the investment. Ants, of course, will not do so either, but the pattern is different; man would shift his ground, growing a different crop or quitting the soil for city life, but the ants (if obligate agriculturalists) simply starve to death, as do men sometimes in similar circumstances.

Domesticates which are totally dependent upon ants for survival undoubtedly have been changed genetically by the selective pressures of life under the restrictive conditions of the care by the ants. For similar reasons, plants and animals which have been domesticated by man have almost always been changed genetically — sometimes purposely, but often not; some (hexaploid wheats, maize, bulldogs) have undergone more change, and others (two-rowed barley and cats) less.

Domestication is not a clean-cut concept, and the word is difficult to define. I have become lost in this semantic bog before, and so avoid the morass now. The truth is that all situations are known to occur, from the free-living "wild" animals and plants, through such cases as animals of zoos and circuses (animals which often breed in captivity under conditions of controlled mating), to semidomestic or recently domestic species (white rats, "domestic" cats), to the typical domestic plants and animals (barleys, wheats, oats, rye, millets, etc.; sheep, goats, cattle (*sensu lato*), pigs, horses, guinea pigs, camels, llamas, etc.), to those forms which cannot survive without the assistance of man (maize, Ancon sheep, numerous toy dogs). These and any other categories, however, always have multiple exceptions. And additionally, one is always faced, at the one extreme, with the relative ease of taming some "wild" animals (American bighorn sheep, wolves, pigs), and at the other, with the ease with which many, but not all, of our well-established "domestic" animals (pigs, dogs, horses, water buffaloes) become successfully and even fiercely feral.

Each population of plant and animal that we call domestic — each of the many kinds involved in the topic agriculture — is a subject of its own. Yet agriculture is certainly a unit — the totality of the human practices involving those living secondary energy traps which man plants, breeds, nurtures, grows, guards, preserves, harvests, and prepares for his own use.

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The Earliest Farming: Demography as Cause and Consequence

BENNET BRONSON

INTRODUCTION: DENSITY AND HUSBANDRY

I propose to discuss here the association between agriculture and population size in very early times in the millennia that separate the Palaeolithic from the first appearance of cities and states. That some such association exists, and that the association is in part causal, can hardly be doubted. But its precise nature remains elusive, the subject of many vexed and convoluted debates from Malthus' and Ricardo's time down to the present day.

Can the apparent explosion of population in the time of the early states be explained entirely by the Neolithic Revolution? Obviously not: the revolution precedes the explosion by several thousand years. Can the one partially explain the other, by saying that food production (i.e. agriculture) is a PRE-CONDITION though not a sufficient explanation for demographic expansion? Perhaps. Such explanations are a staple of elementary textbooks in anthropology (and nowadays even history) and may well be limitedly valid. But they suffer from a number of practical defects. They are no longer as productive of important new hypotheses and stimulating research as they were in the 1930's and 1940's .Moreover, they are not so subtle as to inspire anyone with respectful surprise. Such defects may contribute to the relative eclipse of agriculture-as-cause formulations among modern theorists.

On the other hand, the idea that the causality is reversed, with expansion in population supplying the motive force behind agricultural progress, is presently enjoying a modest vogue. One of the principal proponents of this position is the agricultural economist Ester Boserup (1965), who has elaborated a typology of agricultural stages arranged in order of increasing intensity (by which she means increased frequency of land use) which evolve from one to another under the influence of the exogenous variable, population density. An attractive feature of the Boserup formulation is that it does not simply assume that more intensive farming appears because increasingly dense populations need it but instead provides a mechanism to explain the changeover. This mechanism depends on the reasonable assumptions that (1) the technology for some kind of agricultural intensification is readily available to most peoples, and (2) that the average farmer is inhibited from employing this technology by the fact that the more intensive systems are also the most labor demanding in terms of output per man-hour. Hence, all agricultural regimes, even if intrinsically quite intensifiable, will remain in the most extensive state possible until the farmers are forced to change through the pressure of population and an increasing scarcity of land.

This straightforward but novel view of agricultural evolution has been received by some (P. Smith and Young 1972; P. Smith 1972) with enthusiasm and has even been extended back into the prehistoric period (Cohen, this volume) on the grounds that hunting and gathering is still more economical of labor than the most extensive forms of true farming — thus, the very existence of agriculture is seen as a response to demographic factors. Others (Bronson 1972) have questioned the applicability of the Boserup model as originally presented, pointing out the lack of empirical evidence for the central proposition that extensiveness and labor efficiency are really correlated and suggesting that there are strong theoretical reasons for doubting that agriculture actually did evolve along a single track as Boserup proposes.

However, the details of this and similar models of agricultural change are not immediately germane. My concern here is to discuss the abstract issue of demographic explanations of subsistence systems, with reference not to recent alterations in farming methods but to the origins of farming itself. Can it be said that population pressure is a sufficient, or necessary, or even plausible precondition to the Neolithic Revolution? More importantly, is it an explanation? Do we gain anything in the way of theoretical rigor or predictive power by postulating a single demographic prime mover to explain all the manifold subsistence choices made by early man?

To answer these questions, I must redefine (or select among the definitions of) several concepts and reconstruct several models of subsistence economics and ancient demographics. Such concepts and models will be found to occupy the major portion of this paper.

DEFINITIONS AND RESTRICTIONS

Several terms in the following pages are likely to cause confusion unless the meanings assigned to them here are described. Among these are "efficiency," "intensiveness," and "permanence" as applied to subsistence regimes, and the more general terms, "agriculture," "cultivation," and "domestication."

"Efficiency" in this paper can be understood in two ways. When prefixed by the word, "labor," it refers to the success of a given subsistence method in minimizing the number of man-hours required for each unit of production. Prefixed by the word, "land," on the other hand, it refers to a related but sometimes opposed kind of success, the extent to which the subsistence method minimizes the quantity of land required for each unit of production. One could also evaluate efficiency according to other criteria (for instance, by social utility or effectiveness of capital utilization) but land- and labor-efficiency are what will mainly concern us here.

The concepts of "intensiveness" and "permanence" also have potential for causing misunderstanding. Both are applied to land use, but while the former is essentially a synonym of land-efficiency, the latter refers only to the relative frequency with which a plot of land is exploited. We know of subsistence regimes which are at once permanent and extensive (e.g. medieval plow farming — see Homans 1970 and Slicher van Bath 1963) and others which are intensive in spite of their impermanence (e.g. Ibo swiddening [Morgan 1955] which has a higher carrying capacity than many permanent regimes). The confusion between intensiveness and permanence is built into a good deal of the traditional terminology with which non-Western agriculture is described. Such terms as "shifting agriculture" have therefore been used sparingly here. They impute an excessive importance to simple permanence of field location, a datum which has only a limited relevance to demographic questions. We are far more interested in the land- and labor-efficiency of a regime than in whether its fields are in the same location from one year to the next.

"Agriculture" and "horticulture" are here treated as synonymous. Although some students of the subject have seen an evolutionary gap between *agris* and *hortus*, regarding one as an attribute of advanced societies and the other as intrinsically primitive, such an attitude is faintly ethnocentric. Many demographically successful modern peoples (e.g. the Javanese, Terra 1954) gain a major portion of their livelihoods from gardens, from plots of land too small and messy to be called, by our clean-cropping Western standards, "fields." Yet these plots may be centrally important from an economic point of view and, moreover, may be cultivated with a very high degree of skill, if not of hardware technology. My own feeling is that the owners of these plots are as much agriculturalists as any Ukrainian peasant or Nebraskan factory farmer. To treat horticulture as an essentially distinct system is misleading; it is equally misleading to talk as though horticulture is inferior from the standpoint of subsistence or necessarily early in an evolutionary sense. Some ancient farmers — wheat growers in the Near East, for example — undoubtedly possessed plots a Westerner would call a field as soon as they began to cultivate a staple crop. But others — root-croppers in South America and Africa, mixed farmers in eastern Asia — began with small gardenlike plots and have continued to depend on them down to the present day.

Agriculture is not, on the other hand, used synonymously with "cultivation," nor does either term necessarily mean "domestication." In the following pages, the term agriculture is reserved for contexts of substantial dependence on plants grown by humans, while cultivation denotes only that a useful species has been deliberately caused to reproduce by man. All agriculturalists are indeed cultivators, but a cultivator need not always be an agriculturalist; he may be just a gatherer (or a factory worker) who occasionally puts a seed or cutting into the ground with the expectation of using the result. This distinction is unorthodox but useful. One consequence of it is that cultivation is seen to be more elementary and perhaps older than agriculture, a theme which will be expanded in a later section.

"Domestication" is used here in the strictly biological rather than partly cultural sense, referring not to taming, growing or other patterns of regular human utilization but instead to the genetic effects that sometimes accompany that utilization. The term is arbitrarily restricted to effects produced specifically by human use. Even though one can easily conceive that plants might become adapted to, and undergo genotypic changes because of, preferential utilization by cows, and even though one might plausibly call a plant adapted for growth in a field favored by cow manure a "bovine domesticate," our present interest is focused on the causal interactions between the natural environment and man.

Even within this limited sphere, under other circumstances it might be necessary to make a still narrower restriction in the meaning of domestication, confining it to meaning the effects produced specifically by cultivation and thus excluding the inherited phenotypic changes that frequently have attended the adaptation of weeds to human, but not necessarily subsistence-connected, habitats. Luckily, distinguishing between weeds and useful domesticates is not necessary to what follows. All that matters is to establish a conceptual separation between cultivating and domesticating and to observe that neither can be assumed invariably to accompany the other. Cultivation (and even agriculture) without domestication is perfectly conceivable; there is no reason why even repeated cropping should necessarily always produce a phenotypically distinctive population. Likewise, a process much like domestication, and perhaps cytologically and morphologically indistinguishable from it, can be assumed to have occurred in numerous species of weed and perhaps in some selectively utilized wild species as well; hence, quasi domestication without cultivation is also possible.

The conceptual distinction thus has a practical consequence. If we have no evidence but remains of plants, we cannot demonstrate conclusively that cultivation did or did not exist. The plant remains can of course indicate probabilities. I myself am inclined to feel that the abundant presence of domesticated characteristics yields a fairly strong presumption of cultivation and that their absence is indecisive, indicating no more than that the site in question MAY have been inhabited by pure gatherers. But in either case, plant remains by themselves are insufficient. Acceptable proof or disproof of cultivation requires the use of several additional lines of evidence.

A last comment should be made relative to cultivation and agriculture. Here, both terms are confined to plant growing. Animal husbandry is indeed an integral part of many agricultural systems and the histories of the domestication of plants and animals in many areas are inextricably intertwined. Nonetheless, I have excluded animals from the following discussion. The reason is simple: I have not yet sorted out in my own mind how herding is related to population growth or whether, except insofar as traction power and manure are necessary to agriculture and scavengers to public health, it is related to population growth at all. Certainly, herding seems an inefficient way of getting protein and a most wasteful source of calories. In some environments it may be adaptive enough, but in others the idea of replacing the efficient wild fauna with domesticated animals seems demographic madness. If a causal relationship exists between population growth and herding, and if the adoption of herding is not due to quite different motives, that relationship is subtle and complex indeed.

With these distinctions and definitions in hand we can now return to the central subject, the association between population and agriculture. We must necessarily consider three sets of models before any conclusions are reached. The next three sections, accordingly, treat (1) the beginnings of cultivation, (2) the beginnings of staple agriculture, and (3) the history of population density.

BECOMING A CULTIVATOR

The proposition to be presented here is that the beginning of cultivation — that is, of the habit of deliberately growing useful plants — was neither a unique nor a revolutionary event. It probably happened repeatedly in different places, starting at a very early date. Its causes may have been comparatively trivial. And, for a period perhaps as long as ten or more millennia, it may have had few discernible social or genetic effects. The proposition is supported by the following arguments.

To begin with, cultivation is not in essence either a complex idea or one difficult to develop. True farming — committing one's resources to the establishment of an artificial ecosystem to yield a staple food supply — may be filled with subtle risks and calculations, but small-scale non-staple cultivating is elementary, so much so that it is not beyond the inventive reach of almost any human being. We can be quite sure that activities resembling cultivation go far back into the Palaeolithic. By the time a modest degree of intelligence had appeared in the human stock — certainly by the late Pleistocene if not before — extensive and in some cases massive interference with the habitat of certain selected species must have already begun. Even non-human predators (e.g. cows) are often observed to feed with discrimination, singling out a small number of species for special attention. But when the predators are intelligent and use fire, the potential for sustained, focused, and drastic selective pressure is clearly increased by several orders of magnitude. Through field fires lit by humans and intelligent concentration on selected food sources, numerous species must have been virtually exterminated long before the famous extinctions of big game during the terminal Pleistocene. Numerous others must have begun their adaptation to microhabitats influenced by humans such as refuse piles and fire clearings, and thus started to become quasi domesticates. It should be remembered that domestication as defined above is not necessarily a consequence of cultivation. Moreover a few species must have been deliberately favored by man. Many recent gatherers are reported to intervene extensively in the life cycles of wild species, going so far as to replant them (wild yams among the Andaman Islanders, wild rice among the Great Lakes Indians) or even to irrigate them (among the Paiute). Ancient gatherers surely were also given to this sort of intervention. One can easily imagine that a Neanderthaler had the foresight to spare a fruit tree growing near a regular camping spot, or that an Upper Palaeolithic *sapiens sapiens* had the intelligence to remove weeds from a bed of useful perennials.

It seems most realistic therefore to envision the process of human adaptation in the late Pleistocene as forming a continuum of selective exploitation, intervention, near-cultivation and quasi domestication. Somewhere in this continuum the first act of deliberate cultivation must have occurred, without fanfare, or important consequences, or awareness that anything new had been done. The contemporaries of the pioneer among all cultivators were surely as aware as he or she that seeds sprout and planted cuttings become new plants. Accidental planting and subsequent utilization must already have occurred numberless times. The only new aspect of the situation was the element of deliberation, the decision to plant a seed or cutting with the intention of using the result.

We may assume that this first of cultigens had the following characteristics: (1) it was of a kind necessary or strongly desirable in the eyes of a group with a rather simple lifestyle; (2) it was in short supply within collecting range of this group's usual camping places; (3) it was not a major staple — if it had been, then planting a few individual plants would not have solved the problem of scarcity while planting a whole field full would probably have seemed to the group a dubious investment of their labor; they could far more easily have moved to an entirely new area; and (4) the plant may have been perishable, or rare everywhere in the region, or distributed in what the ecologists call a "fine-grained" fashion: that is, spread evenly over the landscape rather than in widely separated but easily harvested patches. This last set of characteristics would make resupply difficult even if the group should resort to the strategy of detaching a large part of its labor force to concentrate on long range foraging expeditions. If the plant is hard enough to procure even under those conditions then its labor-cost will be unacceptably high. The group will have no choice but to do without or to learn to cultivate.

Under the assumptions that this protocrop was highly desirable, quantitatively unimportant in the everyday diet, locally scarce, and difficult to keep in adequate supply even when areas outside the local zone were exploited, one might venture an *a priori* description of the plant. It should be native to a fine-grained environment (like a tropical forest) or to an environment of low species and individual density (like a desert). It should have an annual habit and other traits that will make it likely to die off under careless exploitation (unlike a fruit tree or a grass). And it should contain some substance rarer than standard proteins, fats, sugars, and starches — perhaps an ester flavoring, an alkaloid stimulant, a glycoside poison, a fiber, or a dye. The theoretically ideal protocrop would be a non-staple plant with several important potential uses, such as flax, hemp, areca nut, turmeric, or the fruit banana. And empirically speaking, it is of interest that plants with these qualities are quite often found archaeologically in protoagricultural contexts — chile and agave in Mexico at Tamaulipas in the Infiernillo Phase (Mangelsdorf, MacNeish, and Willey 1964: 430) and at Tehuacàn during the El Riego phase (C. E. Smith 1967: 232); nuts of *Piper* and areca in the lowest levels at Spirit Cave, Thailand (Gorman 1973: 100); and cotton and *Lagenaria* in early South America (Pickersgill and Heiser, this volume).

But detailed speculative models of this kind are a luxury at this early stage of prehistoric research. What matters more at present is to produce general models, and such a model can be abstracted from the preceding paragraphs. The probability of an early hunting-and-gathering group becoming cultivators is seemingly controlled by only four sets of factors:

1. Pre-existing technical knowledge — that is, familiarity with certain aspects of plant reproduction.
2. Sufficient rationality to be capable of acting for the sake of remotely rather than immediately anticipated gains.
3. A moderately strong locational constraint, which may be either positive or negative. It may be either (a) a focus of attraction, perhaps a natural resource that is difficult to transport and constantly used (e.g. a water source or a concentrated supply of a staple food) or a cultural resource with the same qualities (a defensible locale or, conceivably, a shrine); or (b) a circumscribing zone of negative attraction, rendered marginal by such factors as environmental poverty, climatic discomfort, military danger, or disease.
4. A botanical commodity which is both highly desirable and scarce, scarcity being defined in terms of the labor cost of collection when the collecting group is under a locational constraint.

Seen through the glass of such a model, the probability of early cultivation in any area might seem quite high. Certainly knowledge and rationality can be assumed to exist in some degree even in remote prehistoric times, and the coincidence of locational constraint with scarcity must be a well-nigh universal condition. We might therefore conclude without further ado that the inception of cultivation should itself be a near-universal. But first a few comments on the role of population density are in order.

The main effect of the model in this regard is to reduce the role of demographic pressure to that of one among several factors producing scarcity. Perhaps the commodity in question has become scarce simply because the increase of population has outrun the ability of the local habitat to maintain the commodity in steady supply. In such a case, demography is one of two producers of scarcity, the other being the always-necessary factor of locational constraint — if no constraint exists, and the group is free to wander anywhere in search of what it needs then "scarcity" can hardly exist. But the commodity may be unobtainable for reasons other than straightforward population growth. Perhaps a small and non-increasing population has eaten all the commodity up over the years it has remained in a certain locality. Here, demographics remains a factor but in a rather less decisive way. Conceivably the commodity may never have existed in adequate quantities within foraging range of the place where the population is constrained to live. The population might have migrated to that place and brought their knowledge of the scarce plant with them, or might have acquired a taste for a previously unknown plant through chance discovery or trade. A case in point is the interest in and subsequent cultivation of tobacco among the Northwest Coast Indians during the eighteenth and nineteenth centuries. Tobacco can be said to have become scarce among the Kwakiutl as soon as they discovered its existence, but in "scarcity" of this kind demographics plays no role at all.

A last point to be considered is that resource scarcity (and for that matter, population pressure) is a highly subjective matter as far as causation in human societies is concerned. Whether a commodity has really become scarce and whether it really is necessary to survival are not entirely relevant when we seek to explain actual human decisions and actions. As modern specialists on agricultural development have begun to emphasize (e.g. Found 1971) what counts most in subsistence decisions is PERCEPTION. If a technique is perceived to be laborious then it will be resisted even if, from the standpoint of an outside observer, it is convenient and economical. And if a commodity is perceived to be scarce, even though it may in actuality be abundant enough, then appropriate action will be taken. Possibly the original cultivator decided to plant his crop because he wrongly evaluated the difficulty of finding the plant growing wild.

But if we ignore this problem of perception for the moment, we can arrive at four interim conclusions. First, cultivation of an elementary kind should be extremely old; there is no reason why it should not have come into being quite far back in the Pleistocene. Second, this rudimentary cultivation need not have had any decisive genetic effects on the plants involved. If only a few individuals were grown at once and especially if the parts of the plant utilized were not the flowers or the seeds, a protocultigen might be indistinguishable from its wild congeners. Third, early cultivation need not have had much effect on human populations. Perhaps it enabled a few groups to lead more comfortable lives and encouraged some to slow their wanderings, but it may not have contributed directly to any kind of archaeologically discernible increase in population. And fourth, increase in density of population need not have played a decisive causal role. Although a plant may have occasionally come into cultivation as a response to demographically induced scarcity, there are many alternative routes to that result.

All these conclusions are of course predicated on the notion that cultivation and agriculture, a substantial dependence on cultivated plants, are quite distinct institutions. As will be seen in the following sections, the interconnections between staple agriculture and demography are of a rather different kind.

BECOMING A FARMER

At this point we should inquire why the appearance of staple crop farming was delayed so long. Even if we reject the almost unprovable possibility of a Pleistocene origin for casual plant tending, we must still account for the fact that full-scale dependence on agriculture lags a surprising distance behind the known beginnings of cultivation. In Mexico, Peru and Southeast Asia, although perhaps not in the Near East, this time-lag seems to amount to at least several thousand years. What is the reason for such a long delay?

Several explanations can be invented. One of the most attractive is a hypothesis based on Boserup's model (see above) of agricultural development — that just as "extensive" agriculture is less labor demanding and therefore preferable to "intensive" agriculture, so gathering is still more economical of labor than agriculture itself. There is even some empirical evidence for such a hypothesis. Sahlins (1972: 1–39) has pointed out that many hunters and gatherers, contrary to what once was generally believed, are comparatively affluent. Both the Hadza (Woodburn 1968, 1972) and the !Kung Bushmen (Lee 1972a, 1972b) are said by their ethnographers to lead an easy life, devoting no more than a few hours a day to subsistence activities even (in the case of the !Kung) in distinctly marginal environments. Thus one can argue that the apparent reluctance of early gatherers and casual cultivators to convert to true farming may have been due to a simple lack of incentive. Before the appearance of the incentives of the later prehistoric period — denser populations, markets, perhaps government persuasion — remaining a gatherer may have been the economically rational course.

However, the labor-saving explanation is difficult to accept as a universally applicable rule. As I have argued elsewhere (Bronson 1972), a great many other factors enter into decisions concerning subsistence besides labor-efficiency: considerations of security, of prestige, of comfort, of health. For instance, nomadic gathering usually seems to exact a rather high price in natural and induced mortality among the very young and very old. It also limits substantially the possibilities of owning weatherproof dwellings, of developing non-subsistence technologies, and of storing food against times of scarcity. One is not convinced that the desire to do as little work as possible will invariably offset such considerations as these. It is far from certain, in fact, that gathering is always less work than some kinds of farming. Numerous food-production regimes, both shifting and permanent, require no more than a few hours' work each day in order to keep a family in food; most known hunter-gatherers (including the Hadza and !Kung) work at least this much, particularly when the labor cost of trekking from camp to camp is counted in. The labor-efficiency of gathering is indeed a factor to be considered, but it is not adequate as a full explanation for the apparent fact that substantial dependence on cultivation appeared so tardily.

An alternative explanation is that time was required for productive and trustworthy staple crops to evolve, and that the delay in the appearance of farming was thus due to a built-in lag in genetic possibility. But this explanation seems weak. Except for a few especially intractable species (perhaps maize), few staple crops can have needed more than a century or two of human attention to reach an adequate level of productivity.

A third explanation, which might be called the "naive-demographic" model, depends heavily on the idea that the development of farming was a straightforward adaptive response to the development of large, dense populations. Thus, it could be argued, true agriculture did not appear earlier simply because it was not needed until the time it did appear. But there are a number of serious objections to such a baldly eufunctional proposition, among them the fact that demographic development on a local scale is inherently too fast-moving to explain a series of events that extends over several millennia. As will be pointed out in a succeeding section, if demographic necessity were the only cause, agriculture would have appeared much more quickly than it did.

The fourth explanation that suggests itself has to do with the minimization of risk. When the casually cultivating hunter-gatherer turned to farming, he may not necessarily have had to work harder, and he may have obtained a number of benefits from the settled life that then was possible. But it is undeniable that he took a considerable gamble. He committed a substantial amount of labor to a course of action from which he could receive no immediate return. Indeed, in those days of pristine farming when no one had successful agriculturalist neighbors to observe, he could have reasonably doubted that he would receive any return at all. Even nowadays crops frequently fail, and still more frequently return no profit on the labor and capital expended, in spite of some nine or ten millennia of agronomic experience. Back in the days when farming began, the risk must have seemed and been very great indeed. While other factors may have contributed, simple caution is an almost adequate explanation for the reluctance of early cultivators to engage in full-scale farming.

The problem that remains is to find a model to explain why agriculture came to exist at all, why men everywhere, perhaps through judicious use of infanticide, war, and other fertility-controlling measures, did not remain casually cultivating hunter-gatherers down to the present day. The model that seems most useful is described below.

In its most generalized form, this model has a good deal of similarity to the one presented in the preceding section for the probability of becoming a cultivator. Again one must postulate a locational constraint and a scarcity of an important commodity. But here the commodity must be essential rather than simply desirable — that is, a staple food. And the question becomes more acute of why the proto-farmers stayed put when faced with this scarcity rather than just moving on. The risk they took by staying and attempting to grow the commodity was, as has already been pointed out, considerable. We must therefore assume that the locational constraint was very strong.

A number of more detailed submodels can be generated by considering the possible nature of this constraint.

The first submodel is a classically simple one — an island or otherwise circumscribed environment from which, for reasons of military danger, epidemiology, or sheer physical impossibility, the inhabitants cannot migrate. Within such an area it is plausible that population densities will increase quite quickly beyond the point where a hunting and gathering way of life can be sustained. In later times, such densely populated enclaves have been observed often to produce strikingly land-intensive agricultural systems, even in the midst of regions where most subsistence is of a very extensive kind. Numerous examples of what Clark and Haswell (1967: 50) call "societies under siege" occur in East and West Africa, Central and Southeast Asia, the Pacific, and the New World (see also Bronson 1972: 216). Since these isolated enclaves are often rather idiosyncratic in terms of the intensive farming technologies they use (e.g. the Haya — Allan 1965), one concludes that many of these technologies have evolved *in situ* in response to the fact that no one could migrate out when land grew scarce. But if constraints on out-migration can thus render inefficient farming systems efficient, then why could they not at an earlier date make a casual cultivator become a full-scale agriculturalist? Many of the same constraining forces were as operative in the early Neolithic as in recent times. It seems plausible that they could have had similar effects.

A rather more complex submodel is generated when the constraining forces are considered to be centripetal and positive: when, for instance, a population is drawn to a given place by the abundant presence of a second staple commodity different in kind from the one which is becoming scarce. A fishing lagoon on an otherwise unproductive coast would meet these requirements, as would a water source in a generally waterless region. The attractions of an abundant supply of protein or water might easily counterbalance the disadvantages of a shortage of a starchy staple in the eyes of a hunting-and-gathering group, causing them to attempt to raise that staple rather than move on to another place.

It will be observed that this two-staple model is a generalized version of two well-known theories of the origin of food production. The idea that the first agriculturalists may have been fishermen was originally suggested by Sauer (1952: 23) and has been subsequently taken up by several more recent authorities (e.g. Adams 1966: 40–41). The latter sources emphasize the importance of sedentarization as a factor in the decision to plant a staple crop; here, the conflict between two separate locationally fixed staples is assigned the central role. Sedentarization undoubtedly predisposes to agriculture but is not a necessary precondition in this model's terms. A conflict between the need for fish and the need for grain could result in the adoption of agriculture even in the absence of a settled life. The fishermen could use the vicinity of the lagoon only seasonally, planting a (necessarily pest-resistant) crop and then continuing on a gathering circuit for the remainder of the year.

The water-source-centered version of the model rather resembles the somewhat discredited "oasis theory" of agricultural origins, whereby the first domestication was assumed to have occurred within oases isolated by increasing regional desiccation. The main difference between this model and the oasis theory in its more highly elaborated form (e.g. Peake 1928) lies in the way the future farmers are assumed to get into the oasis in the first place. While Peake and Pumpelly postulated that the farmers had to be trapped there by a vast climatic change, here no catastrophe is necessary. Many nomadic pre-farming groups must have stayed within oases for long enough to consume most of the food supply inside the watered area and within the exploitable zone surrounding the oasis. That the group would always attempt to farm rather than move on to another oasis is of course unlikely. But, given a sufficient scarcity of water elsewhere and perhaps a reluctance to split the group into smaller units, it is entirely plausible that agriculture would sometimes have been the result.

A last submodel worth considering is the most diffuse and indeterminate of all. Let us assume that most of the conditions laid down previously do not always hold — that under some circumstances agriculture is less risky and easier than collecting, that no locational constraint exists, and that the desired staple commodity, although in short supply, is not necessary to survival. A wandering band of gatherers in an almost deserted rainforest will serve as an example. What is to keep them from cutting down a few trees and planting a moderately large crop of, say, manioc? The labor investment need not have been excessive. If they girdled the trees they would have had to do little cutting and if the forest was deserted, and hence primary, the undergrowth would have been minimal. A quarter hectare of cleared area might have needed no more than two weeks' work and could have produced, in the case of manioc, enough calories to live on for a year. Moreover, since manioc has few natural enemies, the members of the band would not have been obliged to wait around until harvest time; they could have gone off and gathered wild foods elsewhere in the forest while the crop took care of itself. The band thus took no risk, made little commitment, and enjoyed a greatly increased level of security — if the supply of other staples failed, it could always have fallen back on the manioc, which can be expected to remain in edible condition in the ground for several years. Whether such farming as this is theoretically significant — whether it would ordinarily lead to any kind of sociocultural or demographic progress — may seem questionable. But that it IS farming cannot be denied. Agriculture in some instances can have evolved for reasons which are both unrecoverable and trivial.

In summary, one can produce a number of quite disparate models of agricultural origins, varied according to the constraints and commodities assumed to be necessary. I myself see little to choose between them. Any could have happened. If we assume that agriculture was independently "invented" often enough, then all of these causal sequences should have unrolled at least once somewhere in the world.

As for the role of growth of population, this clearly varies from case to case. In the model of the population under siege, it is always present but not, as will be pointed out shortly, as a truly independent variable. In the two-staple model, increasing density may or may not be present, and demographic causation is entirely absent from the model of the part-time forest-farmers. But before demographic issues can be dealt with properly two observations on that subject must first be made.

THE NATURE OF POPULATION PRESSURE AND INCREASE

The two observations in question have to do with (1) the *a priori* probability of being able to project demographic growth curves into the past and so to make assumptions about the size of ancient populations, and (2) what does and does not constitute demographic pressure.

Increase Curves and Frame-Dependence

It is usual, when discussing the influence of demography on societal and economic development, to consider that long-term population growth is represented by the familiar exponential curve (Figure 1).

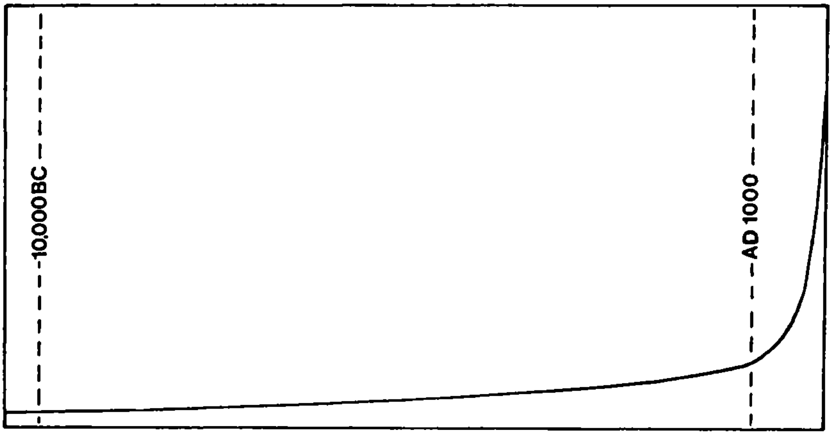

Figure 1. Model of population increase on a worldwide scale

A curve much like Figure 1 would, with somewhat varying parameters, be accepted by most specialists as a fair model of worldwide demographic trends between the Palaeolithic and the present. It would be accepted validly; that international population growth has actually followed such a curve is not open to doubt. But its usefulness is quite another matter. There are reasons for questioning whether the exponential-curve model has any explanatory relevance to early socioeconomic evolution.

The main reason is that socioeconomic events do not (or did not until recently) happen on a worldwide scale. They take place instead within restricted blocks of area measuring at most a few hundred miles on a side, and have their roots in causes which operate within a similarly reduced frame. If we are interested in demographic causation then densities of continental populations are of no interest to us; such data are meaningless abstractions. And if we come to consider the probable history of populations within restricted regions and localities, the exponential-curve model becomes unsatisfactory as a predictor of demographic density.

Empirically speaking, it is difficult to find a single example of a regional or local population before the era of modern medicine known to have followed a steady pattern of exponential increase for longer than a few centuries. Virtually every population of this kind for which we have long-term documentary records can be shown to have undergone substantial fluctuations. If we consider only the period before A.D. 1800, taking the diffusion of the Jenner vaccine as the cut-off point for the beginning of demographically effective medicine, we find that the late eighteenth century rarely marks the known apogee of any regional population. Northern Europe may be an exception, but in most regions the premedical peak was reached long before 1800 and was followed by a considerable decline afterwards. Aztec Mexico, Byzantine Anatolia and Egypt, pre-Mongol Persia, and perhaps Sung China and Roman Italy and northern Africa are examples of such early peaks. And in areas smaller than regions and nations, the short-term fluctuations must completely overwhelm any secular trend toward gradual increase. Seen within a frame of this size, the exponential curve cannot be expected to resemble the actual histories of populations except in a small fraction of cases.

The theoretical explanation for the "frame-dependence" of demographic models is obvious and need not occupy much of our time. Human populations are capable of intrinsically high rates of increase — even under premedical conditions — of a doubling rate of less than fifty years. The inhabitants of a given locality should therefore be able to fill it solidly with human bodies within the space of one or two millennia. The probability that a population will actually sustain such an increase rate over a large area is of course vanishingly small, but as the spatial frame shrinks the probabilities change. If the frame is a region of 10,000 square kilometers, possibly this regional population has at some time in its history remained free from excessive mortality for long enough to produce a substantial population boom. And if the frame is a locality measuring only 500 square kilometers in size, the probability approaches certainty. Given a moderate reduction in mortality, the likelihood of in-migration, and the absence of controls over fertility that is almost universal among modern peoples, we may assume that almost all 500 square-kilometer local populations have undergone a number of extreme fluctuations during the last ten millennia. The actual population curve for a locality of such a size would probably resemble Figure 2 more closely than Figure 1.

Figure 2. Model of population increase on a local scale

One interest of this indeterminate population model is that it frees us of the need to find mechanisms by which low densities of continental populations can be assumed to exert significant pressures on resources of land and labor. There is no need, for instance, to postulate that pre-Neolithic gatherers were driven to adopt a major subsistence change because of crowding at high relative densities of several persons per hundred square kilometers. If high absolute local densities are needed for a hypothesis, then they can be assumed to have existed almost anywhere and at any date.

But this conclusion has a corollary, and the corollary is of equal interest and importance: high densities of population do not invariably lead to the adoption of agriculture. Density-induced resource scarcity must have occurred in numerous localities during the late Pleistocene; even though these densities must sometimes have been considerable, not once are they known to have resulted in the large-scale cultivation of staple crops. In the early Holocene, such densities must have occurred at numerous times and places again; yet in only five or ten small regions can they be shown to have led to farming. Demographic pressure is thus a most inefficient cause. When response follows the presumed stimulus only once in each ten thousand trials, one is justified in doubting the adequacy of that stimulus as an explanation.

Density versus Pseudo-Density

The second observation that should be made has to do with the nature of population density. From the standpoint of possible socioeconomic consequences, what does and does not constitute a "dense" population? No difficulties arise if we envision a classical situation of an increasing number of inhabitants, fixed renewable resources, an area finite in size, and a static exploitative technology; the population becomes dense and begins to experience scarcity at or rather below the point where the rate of consumption equals the rate of renewal of resources. A slight complication that also causes no real difficulties appears when consumers and resources are distributed unevenly within the area. A fine-grained distribution of resources can be expected to result in a lowered threshold of scarcity and population density. The same effect should follow if the consumers are distributed in a coarse-grained fashion. The maximum carrying capacity of a locality is reached only when resources are clustered into easily exploitable nodes, and when the exploiters are spread out as evenly as possible.

On the other hand, there are some sorts of complications which cause real difficulties for the concepts of density and scarcity. One is the probability that any resource which is not necessary for survival can be exhausted eventually by a bare handful of consumers, just as long as these consumers are sufficiently omnivorous, determined, and improvident. Into this category fall almost all individual species of plants and animals. The consumers can always fall back on other species once the preferred ones grow scarce or have been exterminated. The category also includes all non-staple species as a class; a few consumers can exterminate these without suffering any consequences except perhaps for a certain regret at the disappearance of a favored condiment. Hence, we cannot always glibly say that some sorts of scarcity are due to population pressures; they may be due to simple overconsumption. Population pressure is not a meaningful concept except when referred to a critical class of resources, so critical that increasing scarcity can be presumed to bring Malthusian demographic checks into operation.

Another serious complication arises when we consider more carefully the subject of population distribution within the local area. Let us imagine, for instance, an underpopulated valley inhabited by a number of households which are relatively dispersed but focused around a single non-subsistence feature in the center, such as a shrine or defensible hilltop. The resulting settlement-pattern might resemble Figure 3. The issue here involves the problems of supply faced by households located at differing distances from the focal feature. The household marked B on Figure 3 is out at the edge of the settled area; its inhabitants may be far from the population focus but are otherwise in an advantageous position, closer to their fields, to wild resources, and to most other things necessary to the household economy. As a consequence, B's resupply costs, measured in time and effort of transportation, are relatively low. Household A, on the

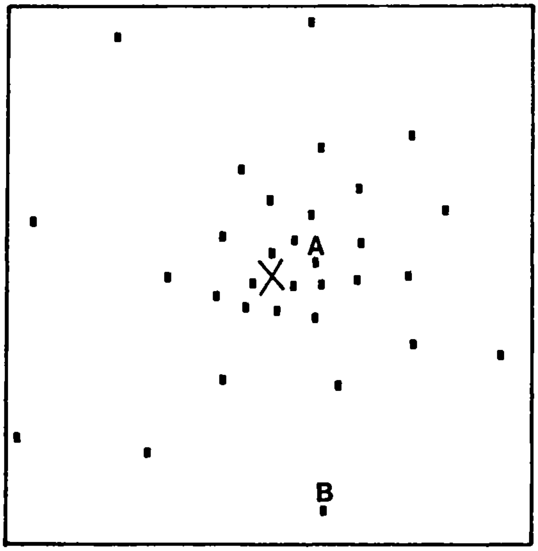

Figure 3. Pseudo-density caused by locational factors

other hand, is in a high-cost location. It may gain some advantages from its proximity to the center, but few of these advantages are economic. It is further from most resources and must regularly expend more time and labor in obtaining them, in spite of the fact that the valley contains adequate land and that no resource is scarce in an absolute sense.

The model being constructed here is only a restatement of the familiar "Isolated State" model of Von Thünen (Chisolm 1967; Chayanov 1966), whereby locational considerations — factors of distance and ease of transportation — are said to be decisive in optimizing the choice of crops and farming techniques in a marketized regional economy. However, this particular version of the Von Thünen model is being used to point up a somewhat different conclusion: that even in an unmarketized pure subsistence economy, locational variations will still produce cost differentials and hence exert the same kind of economic pressure as does genuine scarcity. From an operational point of view, cost (in this case, labor cost) is the only meaningful measure of scarcity. That a given resource is actually common somewhere within the valley makes no difference to the inhabitants of Household A. For them, the resource is hard to get, and they are therefore under economic pressure, a kind of pressure which is most difficult to distinguish from the pressure caused by overpopulation.

Analogous forms of "pseudo-populational" pressure can be presumed to exist at some level in all societies for whom the choice of a place to live is not dictated by the location of a single food resource. If the location of the settlement, whether temporary or permanent, is chosen partly on the basis of defensibility, sociability, or the presence of a second critical resource, then some members of the society will be under appreciable pseudo-pressure. Depending on the keenness with which this pressure is felt, those members will be more or less receptive to the idea of new subsistence alternatives.

The last complication for the concept of population pressure has already been discussed, the fact that increased density must first pass through the filter of cultural perception before it is likely to have any socioeconomic effect. In human as distinguished from animal populations, pressure, scarcity, and stress are to a considerable extent states of mind. A group which feels itself in need of *Lebensraum* may take steps to solve the problem even though, by a more objective measure, the shortage of living space is largely imaginary. Likewise, the scarcity of a resource is not measured in actual labor cost but instead in terms of PERCEIVED labor, and this will clearly depend on a whole host of variables besides caloric expenditure and man-hours worked. In all probability such perceptual factors will usually tend to lower thresholds of pressure and scarcity rather than raise them. But how much these thresholds will be lowered in any particular case is impossible to predict. Thus it follows that an appreciable percentage of ancient subsistence changes will not be explainable by objective economic and demographic factors. Repugnant though it is to our nomothetic instincts, we must consider the possibility that some changes, including some instances of the inception of agriculture and cultivation, may have been caused by a perceptual mistake.

THE LIMITS OF DEMOGRAPHIC EXPLANATION

None of the foregoing is meant to deny the validity of some demographic explanations. Unquestionably population pressure has been significant and sometimes decisive in documented cases of recent alterations of subsistence patterns. The twentieth century intensification of farming by the Ibo (Netting 1969) is quite clearly a more or less direct effect of the recent population boom in Eastern Nigeria, and a whole series of historic shifts in English agriculture have been convincingly tied by Slicher van Bath (1963) to price fluctuations and, through this intermediary market mechanism, to long-term national demographic changes. But there are also numerous countervailing examples. Many of the pre-modern agricultural innovations in Tokugawa Japan (T. Smith 1968) seem to have been accomplished through administrative fiat, because of a concern for increased productivity on the part of landlords and tax collectors. Similar incentives to agronomic change are also known to have been present in eighteenth-century England and in Rome at the time of Virgil. Many modern changes of subsistence in Africa are better interpreted as responses to market development than to increase in population — witness the appearance of land-extensive commercial agriculture among the Gishu (Allan 1965) and Kofyar (Netting 1968). A twentieth-century farmer in New Jersey or Kent selects techniques and crops with regard only to input costs and output prices; he (and, one imagines, his counterparts on the outskirts of any ancient city) farms in a singularly labor- and land-intensive fashion because land is dear, transport to urban markets cheap, and prices for perishable produce high. Whether the total population within the city's hinterland is dense or sparse makes no difference to his choice of farm technology. If he is close to the city, even though that city may be in the midst of a fertile and uninhabited wasteland, he will be an intensive farmer.

Numerous other examples could be cited but there is no need. It is absurd to maintain that, in the modern and recent world, simple demographic density is invariably the prime mover of subsistence change. It may be important in many cases and decisive in some, but too many other factors affecting subsistence exist — market forces, administrative controls, limitations on information, differences in perception — for one to conclude that population pressure alone is an adequate explanation for the majority of ethnographically and historically known instances of the intensification of subsistence methods.

Perhaps it may seem that the early days of farming represent a more pristine and simpler pattern, when the primacy of population pressure should emerge more clearly. However, as the preceding pages have tried to show, this commonsense expectation encounters a number of theoretical difficulties.

The inception of cultivation (as distinguished from full-fledged farming) would seem to have a most tenuous *a priori* connection with increase in population. This follows directly from the postulate that the first cultivated plants need not have been staple crops. If they were not staples, or were not treated as staples, then they can hardly have begun to be grown because the growers were faced with imminent starvation. One of the archetypal instances of pre-agricultural cultivation is the El Riego phase at Tehuacán, where the ordinary diet, on the evidence of coprolites, is said by MacNeish (1972: 71) to have contained between 0 and 6 percent of cultivated plants. Now, this quantity of food may have made a considerable difference to the comfort and even nutrition of the ancient Tehuacanos. But it did not save anyone from starving to death. One cannot believe that the Tehuacanos began cultivating in order to obtain 6 percent more of a staple, or because they sensed that a decline of 6 percent in gathering output meant future disaster. Whyever they began, there was no perceptible wolf at their door.

The beginnings of agriculture, on the other hand, may have had a firmer relationship with demographic factors. If the knowledge of cultivation was already widespread, it is entirely plausible that a population crisis could have turned a group of hunter-gatherers into farmers almost overnight. But one can think of other equally plausible reasons for taking that drastic step — locationally generated pseudo-pressure, conflicts between positionally fixed resources, the social benefits of sedentism, perhaps even at times the increased ease and diminished risk of farming as against gathering. It is true that these other reasons may have had a demographic component, but then demographic causes themselves must always have been much diluted by other factors. The model of straight population pressure is inadequate as an explanation even of situations where a marked demographic increase can be shown to precede staple agriculture, for of necessity the increased population must be a purely local phenomenon which cannot exist without factors — called here, "locational constraints" — that keep the excess people from wandering off into the surrounding emptiness. And so which is the independent variable, the population increase or the constraint?

To my mind, such questions are both unanswerable and unnecessary. What we are dealing with is a complex, multifaceted adaptive system, and in human adaptive systems (as in real natural and human systems of any kind), single all-efficient "causes" cannot exist. True, it may be advantageous occasionally to construct models of such systems in which a single factor is given paramount status. But in the case of this particular system, the heuristic value of a simple model is most doubtful, perhaps especially when the paramount factor is to be demography. Population pressure is not the only possible explanation of farming. Nor does it invariably lead to farming. As pointed out earlier, high local densities must have occurred very early in man's history and with great frequency; only in a small percentage of post-Pleistocene cases can these have led to the adoption of large-scale food production. Thus, increase in population is neither necessary nor sufficient as an explanation. It is also among the most difficult of all data to recover archaeologically, depending as it does on excavations on a tremendous scale and on datings of an improbable accuracy. Even if it were true that in a given case a rapid increase in population had immediately preceded and thus presumably caused the appearance of true farming, that fact would be most difficult to demonstrate through any conceivable excavation. And, as I say, the farming may have many other explanations. The population-centered model of subsistence evolution may be pedagogically useful but it is of doubtful value as a research guide.

Much more satisfactory is the rather subliminal model that seems actually to guide much of the research on post-Pleistocene adaptations, whatever the explicit theoretical orientation of the individual researcher may be. The leading characteristics of this model are complexity, factor feedback, and instability. A great many agencies — sedentariness, epidemiology, genetics, environmental structure, technologies of subsistence and non-subsistence, political evolution, economic development, warfare, the density and distribution in space of populations — are recognized as potential influences, without seriously contending that any necessarily have priority. The relationship between each pair of these is visualized as one of feedback; the chicken-and-egg quality of interactions between adaptational factors has long been recognized by most specialists. And the rather Augustinian notion that all recent (i.e. post-Pleistocene) adaptive patterns are intrinsically unstable is gaining ground again after a brief setback during the heyday of functionalism. Change, driven by the sheer impossibility of keeping so many interacting factors out of disequilibrium, is a normal condition. What requires explaining is stability, not change.

Under the influence of this implict model, a considerable quantity of significant work has been done. Indeed, in spite of the regrettable lack of detailed and overt consensus, the model has probably generated as much useful research as has the average paradigm of one of the Kuhnian "normal" sciences. It is too good a model to be replaced casually.

However, beyond question it will be replaced. The appearance of numerous proposals for new explicit models of socioeconomic evolution, together with a growing feeling that the field is on the edge (or over the edge) of a breakthrough, signals the old model's approaching demise. As yet the few comprehensive models that have been attempted have not been unqualified successes. But a number of partial models, focused on disentangling only a few strands of the web of factor relationships, have done quite well in terms of generating research hypotheses that are at once testable, non-trivial, and interesting. It would seem that studies of ancient demography could be aimed best at producing partial models like these, at clarifying the connections among a small number of precisely defined and quantified variables of which one is size of population. Such an aim may seem dishearteningly modest when compared with the dimensions of the overall problem of why the long Pleistocene stasis did slip over into a disequilibrating mode and produce the world as we now know it. But a sharply limited approach is the only one that is likely to be productive. Testable explanations for grand patterns are not necessary for research, nor are they practicable in the present state of the art.

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The Concept of Environmental Determinism in Cultural Evolution

PHILIP L. WAGNER

The natural environment undoubtedly exerts an all-pervasive influence on human life. Environment conditions every social, cultural, and economic situation. Abundant instances in history attest to nature's intervention in the fates of men and nations.

These generalities are not at issue here. Taking them as indisputable, we further may acknowledge that cultural evolution always must itself proceed under heavy influence and tight constraints imposed by the environment. This idea is central to, and quite inseparable from, what we mean by "evolution"; there would be no point, and indeed there would be error, in talking about cultural evolution without the core idea of change toward greater efficiency, for evolution is directional. Cultures may properly, if speculatively, be thought of as "adapting" by slow cumulative change to their particular environments. We may, according to our tastes, conceive of given environmental circumstances as either permitting, or favoring, or demanding, or evoking some particular direction of cultural development, and thus we may attribute change in culture to processes of "adaptation." Such reasoning is ecological and evolutionary, and is therefore much in vogue. It has a venerable history as well.

The history and substance of the various forms of environmental determinism have been discussed at length by many writers (Callot 1952: 275–360; Freeman 1961: 74–82; Fuchs 1966; Lowie 1937; Tatham 1951: 128–162; Thomas 1925), and it would be superfluous to recapitulate them here. Sorokin (1928: 99–103) and Hartshorne (1939) have contributed extensive critiques of the doctrine on logical and empirical grounds. The ensuing discussion, however, does not constitute an attempt to summarize those critiques. It seeks only to clarify some of the chief implications of the thesis of environmental determinism, and to assess its potential relevance to contemporary culture history. How useful is the concept in the explanation of cultural evolution? Can it lead to fruitful redirection of research? These questions will be foremost. We may begin with some approximations.

Environments, as well as cultures, are engaged in never-ending change. Evolution, then, can hardly be a static concept. Instead of seeking some advanced, perfected form of life, it could be interpreted in this light merely as a ceaseless striving to keep up with change. Yet this striving does appear to induce "progress" in a species, and perhaps in a society — that is, to lead to cumulative increase in their security and ecological efficiency. This fact implies, in turn, that changes in environment, at least over relatively great spans of time, follow a consistent trend; for otherwise, any momentary adaptation would probably prove fatal within a rather short span. Three notions are involved here: (1) consistent, cumulative change within societies; (2) similarly regular and incremental changes in their environments; and finally, (3) some kind of orderly relationship between the two developments.

We know empirically not only that a culture may reflect environmental states and changes, but also that all environments inhabited by man are prone to culturally guided transformations. This latter fact has major implications for our topic, but we must explicitly ignore it for the moment, and suppose that only natural environmental influence on man is relevant. In such a case, it is possible to conceive of an orderly relationship like the one mentioned above, which holds between ongoing environmental change, and coordinated with it or even subordinated to it, changes in society and culture. This conception is the central feature of environmental determinism.

The doctrine emphasizes order and not accident. Whereas human events and mankind's evolution must, by any reckoning, fall subject now and then to interferences of nature, it is not the exceptional, spectacular occurrence that impresses the environmental determinist, but the regular and gradual effects that he supposes are exerted by any environment upon any society. Accidents themselves, in fact, may hardly interest him. They are matters for historians. Determinism is a systematic and, in principle, predictive doctrine.

PHILOSOPHICAL DETERMINISM

The general concept of determinism holds that "for everything that ever happens there are conditions such that, given them, nothing else could happen" (Taylor 1967: 359). This concept has a lengthy history within philosophy and has found varied expression and application among philosophers. These can probably be fairly represented as falling into three categories. One group, embracing in modern times such leading thinkers as Leibniz, Hobbes, and most of the "logical positivists," would hold that all events and phenomena are determinately caused, or at least can best be explained by deterministic assumptions. A second group, typified by Kant and perhaps most existentialists, would regard determinism as a just hypothesis concerning certain classes of phenomena, but would reserve a place for the undetermined sphere of human freedom. The third viewpoint, perhaps best exemplified by thinkers like Hume and Wittgenstein, would eschew causality as an epistemological and ontological principle, and so reject determinism. There is no doubt at all that for a great many philosophers — analytic, existential, or other — deterministic explanation has become a losing game.

These philosophical differences, of course, go far beyond the special problem of environmental determinism. They relate to the question of whether or not ANY complete causal explanation of anything is possible. Furthermore, some of the most important perplexities in this regard revolve around whether or not anything whatever is possible, i.e. they affect the possibility of knowing discrete entities or particular events. If the flow of reality cannot be subdivided, how can anything be extricated as the effect of given "causes"? This notion translates into doubt that the character and delimitation of historical events may be important for the problems to which the environmental determinists address themselves. Such perplexities have entertained philosophers as far apart as Wittgenstein and Husserl.

The doctrine of determinism has a reciprocal predictive form, deriving known effects from known determinate circumstances. The proposition may be turned around so that successful prediction implies cause. Finding expression in contemporary probabilistic thinking, this idea permits an event predicted successfully, but NOT on the basis of strict (deterministic) causality, to be assigned *a posteriori* to a series of imputed causes. In effect this tends to make the "laws of nature" mostly retroactive! Such a rather fictive form of determinism may provide explanatory frames, if not the only rational account of things. Determinism and "destiny" display some common features here, as if myth and science had met. This viewpoint may or may not help in discovery procedures or experimental design.

The logic of explanation and prediction does not inherently depend upon deterministic assumptions. As Curry (1966) has argued, landscape change can be explained by chance events, operating cumulatively. Relations of possibility and likelihood, as well as necessity or certainty, can be subjected to successive transformations and summations; falsification serves as a criterion perhaps as good or better than confirmation for evaluating hypotheses (Popper 1962). There are manifestly full resources in philosophy to provide for logical analysis of problems of cultural evolution, even if deterministic doctrines now enjoy less currency.

As for the doctrine of environmental determinism, the decline of philosophical determinism may simply leave the controversy where it has been; for if nothing is determined, strictly speaking, then NEITHER environmental determinism nor any of its actual or potential rival theories can explain phenomena deterministically. As a doctrine concerning the tendency of evolution, even if not as a causal doctrine, environmental determinism may still attract adherents. It belongs to the wider tradition of determinism in history which, given mankind's unfortunate inability as yet to conquer circumstance, is likely to persist for a while.

DETERMINISTIC VIEWS OF HISTORY

Many peoples have regarded human history as dependent on the wills and whims of gods. Fatalism, an extreme determinism devoid of methods of prediction, has been widespread since ancient times, often in conjunction — as among the Aztecs or the Greeks — with some idea of either periodic cycles of creation, or successive and distinct epochs of development. Prescientific observation and interpretation of natural phenomena have given models for speculation about the course of human events, represented, for example, in the wide variety of concepts known collectively as totemism and animism. Astronomical, even more than biological exemplars, often having application to the notion of man's destiny, have figured prominently in the speculative determinisms of different peoples, from the Sumerians and probably the men of Stonehenge down to the mathematical macrocosmologists of the present day.

Astrology, unrivaled as a popular determinism, looks to astral controls on human life, and the deterministic view of history, first expressed in modern times by Vico, also rested in good measure on stargazing. For Vico, history manifested an overall direction of progress followed by decline. Like many subsequent conceptions, his bore a moral tone. That moral tone was even stronger in the doctrines that descended from theology. Christian eschatology continued to impart a teleological cast to views of history right down to at least the end of the nineteenth century in Europe. These Christian versions in their more predestinarian varieties presented nature and the human race as the abject executors of God's inexorable will and plan. Their dualistic struggle of the spirit with the flesh may echo Manichaean thought. Calvin brought this tendency in Christianity to its most categorical expression. The more or less self-conscious agents of the divine plan, the "saved," could be regarded as fulfilling ordained necessity — which, in Calvin's own time, operated through capitalist production and commerce.

Seldom have determinisms so specified their individual human agents; the "great man theory" of history is most commonly at odds with them. But one other great determinist system in which the roles of given individuals can be known is Marxism. According to Marxism, history is progressive and can be predicted through scientific laws. Class analysis permits progressive forces in society to be identified. Not nature, but the productive system of society itself, is for Marxism the locus of historical determination; cultural and social evolution is interpreted as man's progress in "the struggle with nature."

In all advanced modern societies, what is perhaps a more fundamental determinism inheres in practical decision and the public rationales of policy. It dates back at least to the physiocrats and English "classical" economists. The idea of progress, moral as well as material, is well bound up, in contemporary thinking, with what is often seen as a necessary order of development within technology. Popular thinking finds nothing astonishing in the conquest of the world by the industrial, commercial, urban order. Thus, all of history is sometimes seen as the growth of rational efficiency, full employment, and economies of scale. Strongly influenced by certain conceptions of the growth of science and by current scientific principles applied to social issues, this is a central tenet of both capitalist and socialist belief.

VARIETIES OF DETERMINISM IN ENVIRONMENTAL EVOLUTION

One rationalist view of history sees progress as the discovery and application of the laws of nature and society. Nature is constant, but knowledge of it grows; man does not evolve organically (for the time considered), but comes to know himself. Society does change, and culture — as the correct understanding and implementation of the principles of order in the universe — does grow and is dynamic. The modern temper finds congenial this particular materialism, sometimes expressed in what is loosely called a "cultural determinism" (cf. White 1949), in which nature takes a passive role, while shared, transmitted human knowledge (itself determinate, of course) evolves in its determined order, and in doing so transforms the world. The conception is a realist conception: the truth is "there," behind the principles we seek to grasp. The cosmic reality itself, if anything, is static; change does not proceed in consequence of a changing fundamental structure of reality, but occurs as human knowledge reaches ever deeper toward a comprehension of that fundamental structure, and becomes diffused increasingly among humanity. Environment, in turn, is changed as culture, and in this sense, evolves.

Cultural determinism on this realist basis has been vigorously challenged by such philosophers of history as Collingwood and Croce. Their idealist conceptions represent another kind of cultural outlook, in which no accessible underlying world structure compels recognition and obedience. Human evolution is for them a human creation-invention, rather than the product of discoveries. Implicitly, this outlook calls for a rejection of determinism in regard to human history. The most explicit opposition to deterministic philosophies of history has perhaps been that of Popper (1957), who has argued that the astronomically inspired model of a universe of order, on which these philosophies rest, itself is suspect. The apparent motions of the heavens are explained upon the basis of advance agreement on a model, then confirmed in observation. But no such agreed-upon model has developed for history. The appeal of Marxist thought, of course, must rest in large part precisely on possession of just such a model; but Popper impugns the model of that historical materialism on the epistemological ground that history can teach no lessons and reveal no order of its own, since fundamentally the very concepts of "event" and "tendency" and "cause" remain unclarifiable.

Another, rather odd, determinism is expressed in racial theories of history. Writers like Gobineau and Chamberlain have claimed innate superiority for certain groups, and attempted to explain most great historical achievements as the work of some single biological stock. Purity of race becomes for them the key to progress. Almost always chauvinistic, racial doctrines well exemplify the invidious way in which most determinisms single out particular social elements for a uniquely positive, progressive role.

The concept of inevitable developmental stages of historical development was widely prevalent in nineteenth-century European thought. It came to figure, through the influence of Lewis Morgan, in the stages of productive relationships adopted by Marx and Engels, as well as in the idea of inevitable stages of technological development so commonly still encountered among prehistorians and ethnologists (cf. Lowie 1937). A notion of this same sort is also inherent in the psychological determinism propagated by the school of Freud. Psychoanalysis interprets not only individual life histories in terms of unconscious motivations and images fixed in childhood, but history, also, as the enactment of the fundamental psychic dramas. Freudian concepts are vulgarized in visions of Oedipal confrontations that overthrow patriarchal systems, or of capitalism as an "anal" stage of social development. The symbols and encounters that make history, and all the underlying behavior, are, according to this viewpoint, just projections or realizations of the underlying psychic reality, obedient to its own "natural laws." Another immanence is apparent here, although its spelling out has remained only fragmentary.

The foregoing instances suffice to show the range of deterministic viewpoints in recent historical thought. All of these disparate doctrines emphasize the immanence of one or another supposedly immutable order: the divine plan, the laws of historical materialism, physical (and economic) law, the structure of the unconscious, or genetic inheritance. Each of them in its own way presents human cultures as dynamic applications of some fundamental principles that govern transformation of the world. Man, if not determinant, is agent and executor of underlying forces, and social evolution is PROGRESSIVE realization of those principles. For all these doctrines, man's evolutionary role is central; they are all more or less anthropocentric and some are also teleological.

GEOGRAPHICAL AND ENVIRONMENTAL DETERMINISM

Thoughtful men have always remarked not only differences in custom and appearance among the variously situated human populations, but deeper differences of character and temperament that seem to coincide with geographical location and, perhaps in consequence of these differences, great contrasts in the constitution of society and the political order. A plausible geography of character, as we should call it now, suggests itself to many discerning observers. Why should personal and national traits exhibit so much geographic regularity? Montesquieu and Buckle gave the classic expression to the idea that character and social forms agree with some environmental factors — an idea still vigorously advocated by some few writers today. Whatever cause may be adduced, it becomes but reasonable to investigate the geographic distribution of the causal factors indicated. Geographical determinism generally means "deterministic reasoning applied geographically," and an injunction to consider a phenomenon within its geographic context. Even in the absence of strict causal reasoning, and apart from any doctrinal position, this particular notion is unexceptionable.

One objection does arise, however, even at a most general level, to geographical determinism. For a dubious assumption may be hidden in this reasoning, to the effect that whichever causes (or, if "cause" is abdicated, then "associated circumstances") might account for the presence of a given phenomenon in a certain place, these causes will themselves be found in that place. It is unwarranted to assume that the geographical distributions of effects and "causes" must necessarily long remain congruent. Therefore, the simple comparison of distributions cannot ever lead to certainty concerning "cause."

Strictly speaking, environmental determinism ought probably to be distinguished as a special case of the more general geographical determinism just alluded to. The distinction does not, unfortunately, hold consistently in the literature, but it may help clarify some issues. The meaning of environmental determinism, as such, revolves around the common usage of the term "environment." In particular, it concerns itself with factors of the natural environment.

"Environment" is one of those words that seem to have an obvious intuitive meaning — until considered carefully. Let us examine some of its possibilities:

SPATIAL: Environment means the spatially contiguous surroundings — simply whatever exists in the vicinity of something. The spatial extent remains unspecified: how far away from a point is the perimeter of its environment?

SENSORY: Environment refers to the normal field of a subject's perception. The boundary of a person's environment lies at the limit of his sight, hearing, and so on. (The heavens make up, spatially, most of our environment. Or do they?)

DYNAMIC: Environment connotes the zone of interaction between a body and the forces impinging on it. Invisible microscopic pathogens infest it; distant nuclear explosions intrude themselves into it. Most restrictedly, "environment" in this sense stops at the surface of the skin and somewhere in the inner pulmonary sacs; most broadly, it includes whatever can touch or be touched, affect or be affected.

HABITUAL: Environment, as enduring and developing reality, includes the whole potential range in which an individual functions. It coincides with his daily round of places visited and occupied, or with seasonally various haunts. For mobile creatures, an environment cannot be (meaningfully) just a single point in space. (When is an "environment" a lifetime's range of wandering?)

CAUSAL: Environment signifies the physical (and perhaps symbolic) conditions associated necessarily with the presence or persistence of a given phenomenon. It is the characteristic situation in which a given phenomenon can occur. Yet other definitions, suitable for certain purposes, have been proposed. Clearly, "environment" can be conceived in many different ways. The causal viewpoint required by the idea of environmental determinism — some combination of immediate environmental conditions necessary and sufficient to produce a given phenomenon — in the present case becomes not merely a logical but also a geographical concept.

One of the implied premises of traditional environmental determinism holds that the environmental conditions presiding over human destiny or character are those conditions that are not man-made. Nature dominates mankind. Since physical rather than some kind of "mental" causality is almost always contemplated — even if supposedly it operates through physical effects upon mentalities — the relevant environment must be "physical." But more particularly, "physical environment" in this context emphatically does not include everything in the spatial, sensory, or dynamic fields that is physical; it carries the implication of the "natural" elements alone, and so in effect becomes the "natural environment" as against what I have called (Wagner 1960) the "artificial environment." Furthermore, according to this theory, even if the natural environment does at times allegedly affect mentalities, it operates primarily in a directly physical and not a symbolic fashion. Whatever environmental determinism offers, it is not a theory of perception.

The factors of the natural environment that have figured in the arguments of the environmental determinists include particularly spatial properties — e.g. contiguity; concentration or dispersion; climatic features, especially the seasonal march of temperature and rainfall; soil and mineral resources; and general surface configuration.

At its simplest, environmental determinism attempts to find an explanatory correlation between the areal patterns of physical and those of cultural geography. The relationships discovered have not always been envisaged as specific processes, which, as will be apparent, constitutes one of the fundamental weaknesses of this whole thesis. In more evolved form, the intimately related concepts of environmental influence and environmental adaptation, applied to historical developments in the context of environmental change, sometimes allegedly permit investigators to identify key processes. The concepts of environment evoked vary not only according to the several categories outlined just above, but also in their temporal reference. This permits four possible modes of relationship between society and (natural) environment: (1) gradual "adaptation" by society to constant environmental conditions; (2) continual adaptive "response" by society in consequence of continually varying environmental conditions; (3) fixed, unchanged "adjustment" to unchanged conditions; and (4) established "accommodation" of a society that serves to cope with considerable ongoing variation of environmental conditions.

VARIETIES OF ADAPTATION AND ADJUSTMENT

Every human society must somehow come to terms with its environment, and every such society must deal with some inconstancy in its surroundings. The amplitudes of variations in the social and environmental domains, as well as systematic correlations linking them, provide the basis for distinctions drawn above between what I shall call (using terms never well standardized in the geographic literature), "adaptation," "adjustment," "response," and "accommodation." The ideal of a society and its environment remaining constantly in close adjustment oversimplifies the case; however, we may assume that many a society becomes sufficiently accommodated that its normal functions adequately cope with modest amplitudes of environmental change. This situation in fact better represents a typical "adjusted" state than does an ideal thoroughly immobile, static case, wherein neither domain exhibits change. The relationship of society and environment, however, may become more or less stabilized, either when both remain comparatively constant or when both are changing but in phase with one another. Potential instability develops when environmental change occurs to which society is not accommodated, and the necessary response is long delayed; this may bring on cataclysm.

A balanced statement on adaptation and adjustment must make allusion to a formidable complication of the argument, to wit, the fact that when society "accommodates," "adapts," "adjusts," or "responds" to environmental influence, it does so, partly if not wholly, through transforming the environment itself. This is a salient universal characteristic of human societies. A view directed only toward the evolution of society and culture, therefore, may misread the history; alteration of the environment claims equal relevance.

A second complication that generally applies is the possibility of geographical mobility. A potent influence from the environment may fail to operate in the expected fashion because the society withdraws from that environment. This, too, is response. Such an eventuality gives rise to a special kind of environmental determinism — it has been contended that particular outbreaks of migration have ensued upon the occurrence of certain crucial changes in the environment. As developed especially by Huntington (1907, 1922, 1945), this has counted among the most striking and influential environmental determinist theses.

Having taken account of these reservations, we may consider both environmental and social or cultural change as capable of a varying degree of consistent directedness and continuity. The manifold number of possibilities expands: we can conceive now of societies possibly remaining altogether static in the face of either an unchanging environment, an environment of sporadic and disarticulated variability, or an environment that evolves progressively in some direction. Alternatively, we may entertain the corresponding possibilities of social systems undergoing articulated or chaotic change as a reflection of environmental influence of any one of these kinds.

MAN AND THE ICE AGE

A temporal coincidence between the reconstructed history of glaciation in the Pleistocene and what was known of mankind's record struck many people long ago. Although discoveries have demonstrated that the earlier episodes of human history took place outside the glaciated zones themselves, climatic, topographic, and biotic conditions differing substantially from the present ones presumably occurred in the intertropical and subtropical zones (apparently the scene of critical stages in mankind's development) concurrently with what was happening in higher latitudes. Hence a temptation arises to interpret mankind's cultural and physical development as "determined" by environmental changes during the Pleistocene.

If man "evolved" at all, as almost all authorities would nowadays acknowledge, it meant a process of becoming ever better fitted to survive and multiply within the conditions of environment prevailing at a given time. Consistent upward reassessments of the age of man imply that not only nonglacial but preglacial environments may have been of consequence for early man. The theme of human physical evolution lies outside our scope, but we ought to remark upon the seeming lack of a convincing correlation between certain critical periods in that development and the most impressive changes of environment of which we know. And when we come to the story of Neanderthal and diverse other mid-Paleolithic physical types, relationships with environmental change are somewhat puzzling, to say the least.

The actual environmental picture anywhere during Pleistocene time is still unclear enough that any close connections with man's cultural development are necessarily uncertain. Some broad outlines do suggest themselves, however. Simultaneous physical, social, cultural, and ecological distinctiveness among the recognizable ancestors of man began at the latest in the Lower Pleistocene and probably before, well outside the zones affected by the ice sheets. At least in Africa, several different environmental niches permitted, or encouraged, the development of divergent types of men. Climatic change may have exerted increasing selection pressure on the heavier, more vegetarian population of *Paranthropus* and ultimately brought about its extinction, while hunting bands evolved among its relatives, accentuating human sociability and encouraging some kind of culture.

The mastery of fire and possibly the invention of clothing may have given men the means of seizing ecological dominance themselves by the time of the Mindel glacial period, and may have allowed them to colonize and endure in much colder northern latitudes. Men did not, however, move toward a physical adaptation to cold environments in one zone, to warm wet or dry ones in another, and so on. The concomitant of climatic, vegetational, and faunal change seems not to have been physical specialization into very closely adjusted local stocks, leading toward new species, but rather development into a culturally accommodated, dominant stock, able, because of its versatility and artificial influence on environments, to survive virtually everywhere except on the ice caps. Nothing is perceptible physically, even in Neanderthal men, that is clearly and irretrievably cold-adapted; nor was their Mousterian cultural equipment obviously so adapted. Apparently man did not adjust to one or another environment as such, but to the very fact of secular climatic change with all its consequences. Finding nature's climates unreliable, he undertook to make his own.

Once the overthrow of the pressure of natural selection is accounted for, this interpretation of our evolution need not absolutely presuppose determinism. Insofar as man's physique was concerned, nothing henceforth needed to become a fixed commitment to a certain climate. Whereas until then mutation had been the source of evolutionary adaptation, after this point invention had to fill its place.

There remains the problem of the possible determination of inventions by environment. It is easy to admit selection pressure as applying equally to cultural innovations and to genetically induced novelties, once they have appeared. But whereas no one seems to question the virtual randomness of the process of genetic mutation in a living population — apart from some few mechanical, chemical, and radiational effects that bear on the frequency but not the character of mutants — random evolution of a culture is perhaps intuitively less acceptable. Culture, and emphatically its technical and organizational aspects, appears to show consistent growth and improvement (cf. Kroeber 1944, 1952), and to achieve them almost unerringly. It seems, to date at least, as if societies do not make as many blind mistakes as nature does in the course of producing and testing innovations. Hence we have to ask whether progressive culture change reflects particular determinate controls.

MENTAL ACTIVITY AND ENVIRONMENTALISM

Bodily adaptation and accommodation to the natural environment count for much less now than in the early Pleistocene, at least in the aggregate, despite the multiplicity of possible effects (e.g. of climate) enthusiastically alleged by Markham (1942), Huntington (1915), and others. The adaptation to natural conditions of man-modified environment itself, specifically technics, is of greater consequence. Technical systems of objects, in and of themselves, may well respond to advancing change in their surroundings, or they may accommodate over limited amplitudes to fluctuations therein, but they do not exactly "evolve." We should not think of artifacts as adapting or adjusting. Technology does not command its own development, nor has it, strictly speaking, any pattern of internal descent from form to form. Its continuities reside entirely in the concepts of observers.

But human populations, and their habitats, evolve by regular descent and truly cumulative change. Ideas, too, may well exhibit a genuine evolutionary pattern — one notion growing from another toward a given culmination. This evolution of ideas is what generates the changes or initiatives for change within the technological domain. Is it legitimate to see the application of ideas within technology as consequent on a genuinely evolutionary process external to technics? In what sense might this evolution be determined by environment?

Any idea evolves within an individual mentality. It can never be otherwise. Yet in order to belong to culture, and to contribute to the evolution thereof, we can agree that an idea must be diffused and therefore shared. Finally, if applied, it may enter into technics or some kind of group behavior. The evolution of culture involves a more complex process than simply a generation of ideas, but the character of culture must still depend on ideas, for the origin of which we may seek an explanation. Whereas a physicalist outlook, based upon the philosophical position that all physical phenomena can be explained by purely physical determinants or influences, suffices to account, say, for the effects of climate on the individual organism or on its living space and livelihood, the physicalist conception of mind is vague, and except for a rare minority of pathological subjects, is useless for explaining the genesis of a particular sort of idea.

What governs mental life? If chance alone determines it, we find an analogue to mutation in biology. But if it has a pattern that reflects some regular relationship with something else, that pattern possibly may prove intelligible. One potential source of mental patterns is the natural, and generally the physical environment. A conception of mankind as rational, even if incompletely so, entails the supposition that men learn — that is, that mind responds adaptively to environment. In recent years, many geographers — such as Lowenthal (1961, 1967), Brookfield (1969), and Saarinen (1969) — have reviewed the evidence for geographical perception as a factor in behavior. The concept of culture, more particularly, has emphasized learning as a sharing of experience and insight among men. As earlier proposed, cultural evolution means adaptive change; that position would imply that such change occurs in consequence of insights into man's environmental circumstances. By definition, adaptive evolutionary change reflects its environmental circumstances, and so our mentalism ought to be environmentalist as well.

Apart from physicalist climatic theories of environmental determinism which have to do with births and deaths and health (including mental health and vigor), the related doctrine of possibilism associated particularly with the French school of Vidal de la Blache, provided geographers with a view of man as rational within a world of intelligible possibilities — a view that is environmentalist, as Lewthwaite (1958) remarked, and also mentalist. For possibilism, cultural change is seen to follow upon discovery of inescapably obvious ideas through contemplation of the environment. Adaptation and adjustment through cultural means, in this view, require insight and the recognition of environmental possibilities or pressures. Reflection and deliberation are implied, and choice involves conjecture and prediction. The origin of such a technique as fire making, by this reasoning, might be explained by imaginative imitation of observed events in nature, or even by deliberate research in semiplayful contexts. A complex such as domestication likewise might have come about through some premeditated course of action, although not necessarily with practical intentions.

The possibilist conception of cultural evolutionary theory permits of various motivations in man's adaptation to environment. It need not presuppose a purely utilitarian intention. If men adapt to their environments by making more or less conscious and purposeful choices, their consciousness and purposes may well include symbolic, ritual, and ceremonial features. Cosmologies, as comprehensive explanations of human experience of environing reality, perhaps prescribe their own adjustments. The ritual origins of such evolutionary landmarks as animal domestication, urged by Hahn (1896), and urbanism, recently proposed by Wheatley (1971), command a certain plausibility. Methodologically, the importance of this version of possibilist environmentalism — in contrast to environmental determinism — lies in its indication of a source of explanation in the progress of cosmological speculation, for which, unfortunately, material evidence is often meager. The growth of cosmologies may often be slighted in the reconstruction of prehistory.

SITES AND ROUTES

Environments affect diffusion of ideas. Two kinds of influence apparently are relevant: those that confirm or refute, establish or extinguish, the idea itself, and those that favor or obstruct its physical communication. Consider first communication. The contacts among peoples play a major role in spreading new ideas, and so it must have been for ages past. A kind of environmentalism is inherent in the idea, so prominent in Ratzel's (1882, 1891) anthropogeography, that topographical configuration or proximity and distance must condition cultural development. Most of Ratzel's ideas — not nearly so mysteriously environmental-determinist as is sometimes alleged — lend themselves to statement as hypotheses about communication and its technical necessities, and in that guise call for no particular assumptions about controls of another order inherent in environments. Differences between Old and New World civilizations, or between Black Africa and the circum-Mediterranean world, are easiest explained with the help of such hypotheses. The known spatial patterns of the spread of many great inventions unmistakably attest to some necessities of spatial order. Little controversy need arise about the validity of these ideas of Ratzel, nor do they in themselves clarify the problem, strictly, of the origin of innovations. But nonetheless they have great logical bearing on the problem. Diffusion may figure as the alternative in a given place to independent local evolution or invention.

Historically, extreme differences of opinion have arisen among ethnologists, geographers, and prehistorians upon this issue. If true invention be regarded as a very rare event, and the geographic process of diffusion as a commonplace one, then most if not all cultural evolution anywhere can be explained, as Graebner (1911) and other *Kulturkreis* spokesmen would have it, by relatively simple stages in a universal process of diffusion of a small number of basic inventions. All of cultural evolution might constitute a series of emanations from one great dominating center of invention. For such theories, the original inventions could stand as utterly unique and probably inexplicable events, all importance then attaching to the routes, times, and means of their diffusion.

Demonstrable relationships preclude alternative hypotheses. A known diffusion obviates environmental deterministic explanation of local processes of cultural innovation. We should not forget that the immense majority of local cultural developments at any time empirically show obvious derivation from elsewhere, and close resemblance and connection with adjacent situations. Only rarely can an actual invention be even a possibility. Furthermore, the laborious quest for absolute beginnings may in fact be futile, yielding scant enlightenment about the course of cultural development, even if initial accidents or small determinate responses can be found. Their propagation is what counts.

The interesting cases, therefore, present themselves when very similar new traits arise in places isolated from each other and with little possibility of intercourse. As wandering and intermarriage militate against genetic isolation of the local human group, diffusion and related migratory movements intrude upon autonomous adjustment of local cultures to environment. Experience and ingenuity are shared. Trade equalizes resource allocation. Accordingly, one of the most fertile kinds of speculative explanation by determinists has focused on the predisposing circumstances for migration or diffusion. Much better arguments can be made for environmental limitations and incentives for communication and migration than for any environmental impulse toward invention. The crux of the question then becomes how conducive environmental conditions are to spatial propagation of the innovation.

POTENTIALS FOR DIFFUSION

Conditions for diffusion may serve to explain attested innovations introduced to given places, if not their ultimate beginnings. The category of conditions that impinge upon communication of a new idea, or actual movement of people, permits of probabilistic interpretation; conditions governing the establishment or extinction of the idea in any given spatial phase — and therefore continuity or interruption of the spatial process of its expansion — are capable of possibilistic assessment. In neither instance does a strict determinism seem to recommend itself.

Cultural evolution as exhibited by apportionment of great world areas at any period of history to culture regions reflects an unmistakable conformity to major geographical controls. Before the maritime age, the major boundaries ran through oceans, deserts, and high mountain ranges. Cultural communities conformed to topographic sectors like the valleys of great rivers. Connections now express the existing maritime and interurban networks of communications.

Thus, in regard to cultural evolution, the possibilities of diffusion should first be judiciously canvassed, and the relative ease and likelihood of intercourse, according to the transportation media peculiar to the time, should be considered. A certain net propensity to travel (and in consequence, communicate) applies in any situation where a group of human beings move about. On a probabilistic basis, the random spatial movements of all individuals within a given population, over a sufficient time, will tend to establish a pronounced pattern of circulation that describes the mean relative cost-distance values from all their points of origin.

Suppose, instead of delving into mentalistic theories, that we merely assume that innovative thoughts occur at stated rates, and that they spread according to "potentials" in a field of forces that reflects the cost in trouble, time, or resources, of crossing any given distance, topographically differentiated. Cost-distance governs the degree of interconnectivity. It is furthermore legitimate to suppose, in probabilistic terms, that cultural resemblance between any two selected points is *ceteris paribus* a function of the subsisting interconnections patterned by cost-distance. The problems of "inspiration" or "creativity" that bedevil mentalism are then obviated. We simply postulate that new ideas are very frequent and occur at approximately the same rate everywhere. Attention shifts to their dissemination. Such a viewpoint is reasonable, in the absence of any indication of geographically differential creativity, such as would arise from particular superiorities of climate, race, or other elements.

The peculiar sort of retrospective, pseudocausal reasoning mentioned earlier that reads back from a known result toward its initial conditions, can apply to the search for cultural initiatives. A known distribution in time and space, considered in the context of a differentiated spatial field (especially when we know it at several of its stages), will tend to reveal the sequence and direction of diffusion, and the points whence given movements came. The "naïve" space of ordinary distance does not serve such purposes, however, for the "distances" involved must register the complications and constraints produced by many other factors. Nor can the conclusions from such reasoning attain to certainty, for they must remain expressions of the probable and nothing more.

A good example of the application of distributional analysis to the reconstruction of a putative diffusion pattern, Sauer's study (1952) of agricultural origins and dispersals, has recently received from the research of Solheim and Gorman in southeastern Asia what may be independent confirmation of its startling claim for early planting cultures in that region. The thesis formerly had lacked all archaeological support (see Gorman's review elsewhere in this book). This instance demonstrates how altogether different lines of reasoning, employing quite distinct assumptions and a different sort of evidence, converge at times on certain problems and reinforce each other's results.

ENVIRONMENTAL SCREENING

An idea's viability, of course, depends not on the means of its communication alone. In the process of diffusion, described formally as a succession of temporal-spatial phases, the idea has to pass at each phase from the donors to the receptors accessible to them. We may calculate the probability of acceptance and further propagation of an idea on the part of the respective receptors — the transition probabilities — according to the compatibility of the idea with local cultural and natural circumstances and the dispositions of the receptors. If the innovative impulse is repeatedly presented to a representative variety of possible receptors, then this probability becomes fairly specific and reliable.

Although cost-distance may express a moderate diversity of variables, it shows an overall consistency. But factors that contribute to environmental screening take a great variety of forms. In formal terms, given an initial innovative impulse and the potential spatial field for its communication, what may be called the net environment for each point of reception (once cost-distance values are assigned) either rejects or accepts the impetus. Environment, in this case, includes personal and social as well as natural conditions. This concept of diffusion does not require complete, determinate articulation in each phase between the idea diffused and the circumstances of receptors. Some degree of latitude applies to possibilities for acceptance and incorporation of the new idea into the environment. Environmentally screened transitions decide what portion of the potentially accessible cost-distance field is attained in a given phase by the diffusing impulse.

The voluminous literature on diffusion surveyed by various geographers of late (Gould 1969; Brown and Moore 1969; Hägerstrand 1967; Hudson 1972) provides a wealth of formulations of the process, but no easy rule for coping with the problem of transitional probabilities. In fact, the same formidable issues that becloud environmental determinism might appear to arise here again. A combination of probabilistic and possibilistic reasoning avoids these pitfalls. The adoption of the innovation will presumably ensue, sooner or later, according to this approach, if environmental possibilities are such as somehow to allow it. Various recipients may be contacted in turn, and their several dispositions may affect the outcome, so that if one or more possibilities for it do exist, over some time random processes of introduction and attempted application of the new idea effectuate its establishment in one or a number of different versions in the new location. The propensity of human beings to travel, invoked above, insures repeated random exposures of potential receptors to the innovation, and guarantees in turn that once naturalized, the idea will henceforth undergo a further propagation at the hands of donors from the new area, again exhibiting the probable effects of random but repeated onward contacts and environmental screening. A random process operating in a field of possibilities thus explores and, given time, will actualize some of those possibilities, if not all of them.

Consider the examples furnished historically by the spread of grain crops. The diffusion of grain growing saw the introduction of the various wheats and barleys, from different but related origins, to peripheral regions, and at certain margins the gradual screening out of these in favor of some associated weeds like oats and rye, or conceivably certain millets. Sometimes climatic barriers intervened, but sometimes cultural preferences appear to have decided the election of a certain species or variety for planting. The possibilities were multiple, and active interchange involving numerous and various encounters prompted exploitation of them, until the middle latitudes of the Old World had acquired a well-diversified pattern of grain agriculture, sensitively adjusted to environments and cultures. Something similar must have happened, also, in the case of rice or maize in other areas, and in countless other cases of diffusion.

Environmental screening of diffusions carries with it another important effect, as the foregoing instance also shows. Innovations change en route. Ideas evolve while they move, and successive passages from one kind of environment to another may subject emerging variants to a powerful selection process leading to considerable differentiation as the impulse spreads. Slight random changes cumulate when shaped into a trend, and various environments eliminate some features altogether while preserving or intensifying others. Conjointly, the originally distinct diffusions of a number of ideas (or genetic lines of crops) may, under the pressures and incentives of particular environments, result in fusions and mutual modifications. Such a situation appears most clearly in the case of linguistic material, notably words that incorporate more than one remote influence. (The evolution of a language furnishes a wealth of suggestive indications concerning the possible course of cultural evolution in general.)

ENVIRONMENTAL PREDISPOSITIONS

The behavior that operates in the finding of compatible conditions for phenomena takes place within a realm of possibilities, some of them of natural provenance and some of them in whole or part man-made. If we know in advance (or retrospectively) the character and requirements of a given innovation as it moves, we may discern those situations where, once it arrives, it may become acclimatized. The prediction of this kind of fitness of a place to new ideas must depend upon the identification of minute particulars of both the innovation and the environments, but sometimes this procedure does prove feasible. More frequently than otherwise, the stipulated limiting conditions for the occurrence of phenomena provide a means of such prediction. Thus, a simulation of the spread of grains might be effected by compiling data on the geographic distribution of relevant factors, such as critical isotherms for the species concerned, crucial soil properties, or existing techniques of cultivation. The field of possible expansion for each crop appears quite clearly in such simulations where, of course, this geographic information has in fact assisted in determining the very limits here invoked; the reasoning is often circular, predicting what has already happened, on the basis of its having happened.

The probabilistic concepts introduced above allow for chance occurrence of ideas at equal rates at any point. They further postulate a fixed propensity to travel and exchange. Cost-distance sets controls on the diffusion of ideas in differentiated geographic fields, and continuity in diffusion rests upon the receptivity to the impulse of given environments reached by it. If a particular notion — quite hypothetically — has arisen here and there repeatedly throughout the time of man on earth, it necessarily has had to wait for certain propitious conditions in order to diffuse and become widely established. At first environmental barriers may have contained it; then environmental change facilitated its dissemination. Although the assumption that all ideas exist in embryo through all the ages, or arise repeatedly, may be unrealistic, this issue is irrelevant. What matters is the postulation that the spread of new ideas attests a changed environment.

Types of limiting (and facilitating) conditions vary. The crucial changes opening the way to new disseminations may involve: alterations in the mode of perception on the part of a people, consequent on cultural developments; modifications of technics, again a cultural acquisition; transformations of environment by man; variations in the natural conditions, either casual or secular — and of course, mutations in the idea that diffuses, or in the ways of implementing it. The foremost influence for receptivity to change, accordingly, appears to stem from previous diffusions that have modified the local culture, or through it, the environment. Only rarely, seemingly, would natural environmental change contain the key.

Environmental determinism, therefore, after having been watered down to such a minor theme as the foregoing one of favorable natural conditions opening the way for new ideas, remains a part of valid reasoning. Certain times occur in history when circumstances change in such a way as to favor rapid evolution. Progressive change takes place, we might infer, when natural conditions come about that favor the acceptance and establishment of new ideas — not necessarily of local origin — that institute a better adaptation of a culture to its environment. But redoubtable objections to a purely automatic supposition of this kind abound, as will be evident. Only rarely will environmental determinism offer an attractive explanation of events. Its fallacy lies in unwarranted metaphysical assumptions that induce a specious confidence in full foreknowledge or retrospective certainty.

With all due caution, we may embrace a functional interpretation of the growth and geographical adjustment of human cultures that almost (but only almost) demands the invocation of environmental determinism. The structural-functional anthropology that followed the pronouncements of Radcliffe-Brown (1948) was predisposed to this expedient, because it looked for close, or "best," adjustments of societies to habitats. Ideas emerge in abundance; only the impediments of nature (i.e. cost-distance) prevent their penetrating everywhere; where they reach, they are selected insofar as circumstance requires — not merely allows — them to be adopted. Whatever their inception, their vocation is assured and understandable.

Even the nonfunctionalist schools of anthropology regard some cultural features of a people as less successful than others, or less adaptive; some make a place for "irrational" behavior and beliefs. Such categories are a pale but true reflection of environmentalist ones: adaptation is their criterion and cue. Is it realistic to deny that house types, livelihood activities, or clothing tend to show conformity to climate, raw materials, and other geographic features? Hardly so! Culture does consult environment. The primary objection to the doctrine of environmental determinism rather cites the multiplicity of rational adjustments possible to given concrete habitat conditions. Ethnographic evidence profusely illustrates the multiplicity of viable adjustments to a given habitat. There are better and poorer adaptations, to be sure, but more than one of each can be conceived, and often demonstrated. As rationale, environmental determinism furnishes a useful literary tool, but as a method of exact prediction it deceives.

THE FEASIBILITY OF CHOICES

A negative determinism, in accord with limits known for given distributions, would arouse no controversy. An impulse spreads until it reaches limits. The possibilities for positive incorporation of a new idea within existing habitats are so diverse and numerous, however, that the stipulation of limits seldom proves convenient. Nor, given numerous conditions that distinguish a particular environment, can some uniquely suited innovation be foreseen. Always more than one solution to a problem may develop, more than one reaction may exploit an opportunity presented.

The defect of environmental determinist reasoning, even at its best, concerns its basic logic. A set of conditions necessary for the occurrence of a certain phenomenon may or may not also be sufficient for it. Furthermore, a condition that suffices to produce (or guarantee occurrence of) a phenomenon may or may not invariably co-occur with it, that is, count as a necessary condition for it. Environmental determinism never could attain to statements of the type "if and only if ...". Accordingly no test of constancy of co-occurrence of "effects" and "causes" could develop. Concretely, the doctrine could not assert empirically vulnerable (testable) claims of necessary implication such that a particular environmental conjuncture would hypothetically be accompanied, wherever and whenever it appeared, by some specific cultural result. The environmental determinists did not promise to discover what conditions would always insure the invention of agriculture, or the discovery of fire, or the preference for warm clothing, or human slavery. Nor did they ever describe in detail the processes supposedly sufficient to implement the influence they claimed.

In fact, the chief proponents of this thesis fell back on mentalism — either proposing that certain climatic conditions (for instance, frequent cyclonic storms) stimulate mental activity, to which they attributed all differential progress; or that particular conditions revealed themselves at crucial moments in a way that forced insight on men, and so led to inventions (such as the putative domestication of wild animals, confined along with men by the secular increase of regional dessication into rare oases).

From a logical standpoint, the possibility remains that for every situation (or rather, for every long chain of successive situations), one particular adaptation (or again, a chain of such ordained ones) will in the end prove to have served maximum advantage. But optimality is always relative. There exists no reason to suppose that long-run maximum advantage dictates always what the short-run largest gain demands, nor any indication that what happens in reality will correspond to either one. Evaluations of "success" will differ in accordance with the time span contemplated. The doctrine that whatever happens is determined by all that precedes it leaves us without much practical understanding, because we cannot know the infinite details of what has gone before. But even if we did, we have no reason to suppose that what preceded an event would always manifest the most efficient, economical determinants at every point most adequate to bring about the current state of things. So even a comprehensive and absolute environmental determinism, like any sort of strict determinism, might not enlighten us much, whether it be "true" or not.

The choices that become "inventions" may not figure as momentous in the minds of those who make them. A small idea succeeds in practice, spreads and still succeeds, and metamorphoses to some degree while spreading. At last it reaches limits set by some environment, or fails to find new pathways for diffusion. Entangled with a host of other novel notions, it does not impress receptors as exceptional, but somewhere it, or some associated new idea, becomes the key to a protean achievement like "toolmaking" or "agriculture" or "irrigation" or "the internal combustion engine." These entities may often not exist for their inventors. Nor, perhaps, do the moments of the vital transfers of ideas involved have any prominence as "historical events." All transpires in the flow of life and time.

The invention or discovery of anything important, then, must emerge out of small acts and happenings, reflecting very minor, often unobserved environmental changes. Recognition of importance in a new idea or artifact or method constitutes, itself, inventive insight. The small events concerned, however, fall increasingly within the sphere of culture; as human groups condition their surroundings more and more, ever greater influences issue from their handiwork to bear on further evolution of their culture and environment. The role of nature in the determination of their progress lessens as their own proficiency increases. But the cultural effects facilitate development in just as complicated and obscure a fashion as do the natural ones.

The crux is this: if every moment is determinate, its determinants are infinite. How can the course of history be known beforehand?

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SECTION TWO

Worldwide Concepts

Cultural Evolution in the Old World and the New, Leading to the Beginnings and Spread of Agriculture

JOSEPH R. CALDWELL

The problems approached in the present volume concern an important series of innovations: the domestication of certain plants and animals and the establishment of these domesticates in economic systems. This paper will begin with a discussion of some still earlier events for, in the present state of our knowledge, some of these earlier developments appear as preconditions for domestication. Such widespread events as the establishment of a degree of sedentary life and of hunting-gathering economies (as distinguished from more specialized hunting economies) may have helped set the stage for initial domestications at various times and places.

We shall, therefore, be interested in the kind of cultural milieu in which the first domestications would seem most likely to have taken place. While admitting that this is far from a complete explanation of the origins of agriculture, I shall offer some suggestions about general and special conditions facilitating or inhibiting agricultural innovation. In this connection I shall also reject some recent views attributing plant domestication to demographic or other kinds of stress.

Finally, one can argue that in our concern with the origins of agriculture, there has been some neglect of an equally important series of problems — the steps by which domestication of plants and animals became increasingly important in economic systems until, ultimately, they became the mainstay of the most successful systems. Probably archaeological, botanical, and zoological studies can provide more specific data on these matters than on the events leading to the initial domestications themselves.

CULTURE-HISTORICAL DEVELOPMENTS PERHAPS LEADING TO AGRICULTURE

What in general do we know about developments prior to and possibly leading to food production? We know that from Middle through Upper Paleolithic times there was a notable increase in cultural complexity. This is evidenced by more specialized tool kits, by storage pits, discernible dwellings, and such large permanent settlements as are found in the eastern Gravettian-Pavlovian of Moravia and the Kostenki-Borshevo occupations in the Soviet Union (Klima 1962: 193–210; Klein 1969). We also find systematic burial practices, personal ornament, sophisticated art, and perhaps calendrical notation (Marshack 1972). In the long run, there was an increase in population.

We also know that prior to the warming trend that ended the Pleistocene there were in Europe and the Americas and, judging from tool types, perhaps in northeastern Asia and northeastern Africa, many societies that emphasized the hunting of large mobile game. I shall refer to these as specialized hunting societies. From various lines of evidence we believe that meat formed the greater part of their diet and that cooperative and sophisticated hunting techniques were used. Examples of these, in addition to the Pavlovian and Kostenki-Borshevo groups, are the Upper Paleolithic hunters of Western Europe and some of the Paleoindians of the Americas. There were other societies at this time whose hunting practices were not so well developed, or at least not so archaeologically recognizable as such. Some of the latter could probably be characterized as foraging or generalized hunting-gathering groups. We ought to make a special effort to look for these on all continents. They deserve more attention in the literature.

We also know that in Europe, northeastern Africa, eastern Asia and North America the age climaxed by specialized hunting societies was followed by warmer climates, changes in vegetation and sea level and a range of societies adapting to forest and waterside environments (Waterbolk 1968; Caldwell 1958; Treistman 1972).

The archaeology of these groups also reflects subsistence changes. There is now a greater emphasis on fishing, fowling, and the harvesting of shellfish and wild plants. The scene is set for societies which, while continuing to hunt rather smaller game, were also developing sophisticated food-collecting techniques. We find a whole series of devices which are not so characteristic of earlier times: traps, snares, boats, sleds, digging sticks, querns, and sickles. Some of these artifacts were later to be used in the technology of food production. Examples of such peoples are the mesolithic societies of Europe, the Natufians of the Levant, some contemporary peoples of Japan, and representatives of the Desert and Eastern Archaic cultures of North America. There were many others.

This is not to say that these major changes in subsistence necessarily meant the total disappearance of the old specialized hunting way of life. That way of life may only have become less characteristic. Specialized hunting under worsened conditions continued on the North American plains and I would be afraid to argue that the recently discovered permanent hunter's village of 8500 B.P. at Suberde (Perkins and Daly 1968) was a completely autochthonous development.

I find interesting the obvious conclusion that the broad sequence of economic adaptations noted above should have appeared on the major continents and, with the possible exception of Africa south of the Sahara, at roughly the same time. The widespread occurrence of the earlier specialized hunting societies is not difficult to accept, given the continuous Eurasiatic land mass and our present belief that a substantial number of the early immigrants to the Americas had arrived before the end of the Upper Paleolithic. Nor is it difficult to see how the Americas should happen to have turned, like Eurasia, to more diversified hunting-gathering economies including a similar emphasis on the use of shellfish where these were available. With many species of game moving northward or becoming extinct innovations of subsistence would have been toward diversification of resources of wild foods. Under such conditions the nutritious and readily available shellfish could hardly have been overlooked as a staple food supply.

With a world occupied by collectors who had developed an intensive and pragmatic interest in wild plants and their properties, I believe that, inevitably, some would have begun to develop the techniques of food production. We have, at least, nicely documented instances at Tamaulipas and Tehuacan (MacNeish 1958, 1964) of societies depending chiefly on wild plants and the slow appearance of cultigen after cultigen while wild plants continued to make up the bulk of diet. Jarmo, Deh Luran, and other sites in the Old World also show a combination of wild and domesticated plants of similar species, but these sites are evidently systadially more developed toward agriculture than the earlier levels of the Middle American sites.

Binford (1968) has proposed that food production originated under demographic stress. I cannot believe this. If I were hungry, I would not put seeds into the ground. Rather, I would eat them as many starving peoples have done. Nor would I experiment with the development of a complex technology. Rather would I turn to marginally nutritious wild foods, as in fact, some starving agriculturalists have also done. To suppose that agriculture could be developed under the stress of demographic pressure is to suppose, impossibly, that the first hesitant experiments with planting would have yielded an immediate and efficacious bounty. To the contrary, I rather think that the demographic stress would have been settled one way or another long before a primitive agriculture could have reached a point where it could provide any relief at all.

On the other hand, peoples ALREADY practicing a paleotechnic agriculture might turn more and more to planting as wild foods became increasingly inadequate. It is here that a theory of demographic stress would be more appropriate, and it is perhaps for this reason that the Near Eastern and Middle American regions of heavy dependence on domesticated plants are found in semiarid lands where wild foods were presumably far less abundant than in such areas as Western Europe and eastern North America. This would not have been the case with the tropical agricultures of southeastern Asia or South America, but in those areas there may well have been a longer and more substantial dependence on hunting and gathering.

I wonder if it might not be possible in the next few years to approach the origins of agriculture from another point of view in addition to those we have held in the past. We are dealing here, after all, with problems of innovation. If archaeology, botany, and zoology have not yet provided, or may never provide, all the answers we seek, perhaps the special insights of cultural anthropology may be of assistance. I am proposing here that we should make an effort to discover some systematic and ultimately testable propositions about innovation as a process. Such propositions might well provide some additional ways to interpret archaeological evidence bearing on the origins of agriculture. Moreover, more understanding of innovation as a process is a pressing need in anthropology today.

I propose that one general condition for innovation is an appropriate cultural milieu. This would include a cultural focus, in the sense of Herskovits (1951) — a strong concern with a particular kind of activity. Although the concept of cultural focus does not of itself entail receptivity to innovation — other conditions would have to be specified here — I do not think major innovations would be made when a cultural focus was absent. And, indeed, most students of the origins of agriculture look to a cultural context of ancient hunting-gathering societies showing great dependence on foods derived from wild plants.

Such societies, we suspect, would — like some recent hunter-gatherers — have an abiding and pragmatic interest in wild plants and their properties.

Another general condition is that innovations arising out of an activity should be congruent with other aspects of a society or culture in the sense of Radcliffe-Brown (1935). Innovations should not, at least in the beginning, require extensive rescheduling of other activities. They should not, at least in the beginning, be opposed to existing value systems. In either event they would not be readily accepted. We know of some recent hunting-and-gathering societies which either did not accept agriculture, or accepted it very slowly, and I think a very good case could be made that this was one of the reasons.

Finally, we might also think of a general condition in which innovations would be redundant. Little possibility exists of innovations being accepted when whatever benefit they might confer is already satisfied by another, better established, activity. Some years ago I argued that acceptance of agriculture in prehistoric eastern North America was slowed down because hunting-gathering activity had already been developed to such a degree of efficiency that the adoption of paleotechnic agriculture would have seemed irrelevant (Caldwell 1958).

In illustrating the propositions advanced above I am suggesting that some hunter-gatherers have accepted agricultural innovation while others have resisted it. This apparent contradiction could be understood in terms of these same propositions if we suggest the role of timing. A cultural focus on wild plants ought to facilitate innovations involving plants. On the other hand if there has been enough time to elaborate hunting-gathering efficiency to a point where domestication would seem redundant, then agricultural innovation would be resisted. We may note here that in southwestern Asia the first domestic plants may have appeared within two thousand years, or less, of the time when the wild progenitors of wheat and barley were available in the area (see Wright and also Reed, this volume). In eastern North America, where resistance seems to have been a factor, no less than eight thousand years elapsed between the appearance of hunting-gathering systems (e.g. the Modoc Rock Shelter in Illinois; see Fowler 1959) and the first domesticated plants (e.g. in Kentucky and Tennessee; see Yarnell, this volume).

In addition to these general propositions there are also others specifically relevant to agricultural innovation. A high degree of sedentary existence would also be a condition of the cultural contexts for the first domestications. Another special condition is the apparent propensity of wild plants to develop special characteristics as a result of interaction with man.

The exact processes of the first domestications may forever elude us, but I am partial to the view of Anderson (1952) that disturbance of habitats through human clearings may in some instances have caused inadvertently one of the processes which we would perform deliberately if we wished to domesticate a plant, i.e. take it out of competition with other species. I am equally partial to the suggestions of Kent Flannery and others that the collecting process itself will ultimately bring about certain selections in the direction of domestication. Nevertheless, I leave these matters to plant geneticists and others wiser than I.

The foregoing statements about general and special conditions of innovation say nothing about the efficient causes for the first domestications. I see these as necessary, but not sufficient, causes — as part but not all of the explanation for the origins of agriculture.

THE ESTABLISHMENT OF AGRICULTURE IN ECONOMIC SYSTEMS

The developments leading to agriculture did not end with the planting of the first seed. The establishment of domestic plants and animals in various local economies probably proceeded slowly and at different rates. In southwestern Asia, for example, if such sites as Çayönü and Deh Luran show domesticated plants before 9000 B.P., there are certainly other sites that do not. In the Levant, Wright (1971) has regarded the early occurrence of domesticated grain at Beidha, marginal to the Mediterranean zone, as congruent with Binford's model, but it is in fact also congruent with a view of greater resistance to food production in the Mediterranean zone of greater natural resources. The spread of food production within southwestern Asia will have to be worked out site by site and region by region. But it is apparent that hunting-and-gathering had some persistence in southwestern Asia, and much ink has been spilled over Jericho by those who cannot believe that a substantial town could be built by collectors hearding a few goats. The seeds which may have been kept in the Jericho storage bins do not need to have been domesticated. Curiously, there have been fewer demands that the large permanent stone settlement of Khirokhitia on Cyprus be considered an agricultural community, and I understand that no grain has been found there either. I am less impressed by the cultigens and possibly domesticated cattle found at Çatal Hüyük (Mellart 1967) than by the amount of animal art and symbolism suggesting the importance of hunting. Indeed, hunting seems to have been the mainstay at the nearby contemporary site of Suberde (Perkins and Daly 1968).

Quite possibly we have erred in assimilating the Levant to southwestern Asia instead of to the Mediterranean Basin of which it is a part. The "Perrot proposition" (Perrot 1968) and the revisions of western Mediterranean dates proposed by Renfrew (1971) are long steps in the recognition of the culture-historical importance of the Mediterranean Basin, which too long has been regarded as an almost passive recipient of Near Eastern diffusions. I think that additional studies would show the importance there of collecting societies with notable innovations being made by these collectors. In an even more speculative vein, one may wonder if the cultural precocity of Jericho, Çatal Hüyük and Khirokhitia may not derive from being in a position to receive diffusions from the nascent food producers of the Zagros and Syria and sophisticated collectors of the eastern Mediterranean and the Levant.

INNOVATIONS IN COLLECTING SOCIETIES

In the past we have been too prone to credit agricultural societies with innovations which may well have been made by collectors. Thus Brea (1956) has argued for a Near Eastern origin for early Mediterranean impressed pottery and shipping even though the former occurs most abundantly in the central Mediterranean and is being made by collectors whose domesticates may have included only sheep and goats.

The present emphasis in the Near East on the achievements of the early food producers *vis-à-vis* collectors reminds me a little of the situation which prevailed in eastern North America a few years ago when large mounds and other earthworks were regarded as evidence of a "social surplus" which could only be obtained through agriculture. Although we know that some cultigens were present in the Ohio and Tennessee Valleys (Yarnell, this volume) at this time and earlier, there can be little doubt that the bulk of foods came from hunting and gathering. The crucial point is not when cultigens first appear, but rather when they become an essential economic staple.

Outside of the "centers" of agricultural origins, collecting societies persisted for thousands of years and continued to make important innovations including substantial buildings, earthworks, elaborate tombs, some of the so-called Neolithic arts — pottery, weaving, the polishing of stone — and in North America, at least, even a kind of metallurgy.

There are several examples of cultural climaxes achieved by collectors in which a considerable surplus was obtained to be disposed of in curiously lavish ways. In addition to the Adena and Hopewellian societies of eastern North America (Caldwell 1958) we have such groups in central California and the American northwest coast, the prehistoric fishers of Lake Baikal in Siberia and others.

In a few instances we can trace the innovations through which collecting efficiency was achieved. People living at about 10,000 B.P. at the Modoc Rock Shelter in southern Illinois tapped all nearby sources of food (Fowler 1959). Later the site became a hunting station where parties of males came only to hunt deer. During this later period other sites in the region reflect similarly specialized activities. The Ferry site on the Wabash River shows thousands of charred acorn fragments, large burned areas where they were roasted, and dozens of querns for grinding the meal. Other sites in northern Kentucky were utilized for the collection of shellfish and still another for shellfish collecting and fishing (Fowler 1959). Thus we see the development of scheduling and procurement systems necessary to efficient collecting. Similarly, on the coast of Georgia at 8000 B.P. shell middens show no foods which could not be obtained in the nearby marshlands (DePratter n.d.). By late prehistoric times there was a much more diversified diet and historical accounts exist of whole communities moving in season into the woods to secure other kinds of foods.

In the overall view, collecting societies in one place or another achieved nearly all the things which we ordinarily attribute to agricultural peoples: permanent settlements, substantial architecture, craft excellence and sophisticated arts, and a notable amount of leisure time. We can further suppose that the systems of scheduling and procurement necessary for efficient collecting imposed a discipline not inferior to that necessary to early agriculturalists. With such accomplishments in mind, resistances to the diffusion of food production are not surprising. In eastern North America, for example, there was a thousand years between the appearance of the first cultigens in the Midwest and the use of maize as an economic staple. There are indications of similar resistances in prehistoric Europe and Manchuria. Other examples could be given. In hindsight, we know of only two cultural-historical disadvantages of the collecting way of life. There was usually a limit to population concentration, and above all there was a limit to cultural evolutionary potential. No collecting society has ever achieved a civilization in the sense of being a technologically complex society.

THE SPREAD OF FOOD PRODUCTION

I have mentioned some instances of resistance to spread of food production. There are other cases where the spread seems to have been rapid. In the first case we may suspect that diffusion is taking place. In the second case the first inference ought to be a migration or explosion of peoples already practicing agriculture. Examples of the latter would be the spread of the *Bandkeramik* peoples from central Europe to the Netherlands and the spread of Prepottery Neolithic B assemblages from Syria to Jericho and the Jordan Valley.

I have been a farmer and know that even paleotechnic agriculture was a complex thing. Complex technologies do not readily diffuse. Nor, may I repeat, could they be invented under demographic pressure. Here I will suggest additional reasons for agricultural resistances. It was not just that collectors were content with the good life and would have considered the introduction of agriculture unnecessary or irrelevant. There are also deep-seated cultural reasons militating against fundamental change in any society. One thing most anthropologists believe is that if we analyze cultures into parts, then we must conclude that all of these parts are related to a greater or lesser degree. In the words of Radcliffe-Brown (1935), who preferred the term society rather than culture, each part contributes to the maintenance of the whole. Fundamental innovations, if adopted, could not be confined to such a single part of culture as its techniques of subsistence, but would have immediate effects on the whole way of life. Therefore, fundamental innovations are adopted very slowly or not at all. Into the fabric of a culture are also woven its values. Hunters and warriors do not easily give up their statuses and personal *raisons d'être* to become agricultural laborers. Worse, they may regard this as women's work. I could give recent American examples. It does not surprise me, therefore, that sometimes the spread of agriculture has been so slow. Other things being equal, I would also expect most of the earlier agricultural innovations to have been made by women. Most primitive societies contain two economies: men's work and women's work. Women, collectors *par excellence,* could cultivate a few plants without disturbing either the fabric of society or their own self-esteem. Were it not for this, agricultural diffusion might in many cases have been impossible.

CONCLUSION

In looking over the above remarks, it appears to me that some of our thinking about the developments leading to the origin of agriculture could be characterized as the kind of catastrophism which prevailed in geology two centuries ago. In opposition to this I would suggest that the Pleistocene hunting societies did not invent collecting under the stress of changing environments. As do all modern hunting societies, they had been collecting all along. A long-term response to changing climate could be reasonably seen as a gradual augmentation of collecting activities with which they were already familiar. As collecting became more and more a cultural focus, we would expect collecting innovations to be made in terms of new sources of food, scheduling, food preparation and storage.

Similarly, I cannot believe that food production was invented under the stress of demographic pressure. It is just as complex a technology as food collecting. What may have happened in the Near East and Mesoamerica is that peoples already using some domesticated plants would turn more and more to these as sufficient supplies of wild foods became more and more difficult to procure.

In the foregoing pages are outlined some of the earlier cultural developments which may have served as preconditions for the origins of agriculture. I also attempted to specify certain conditions of innovation in general and agricultural innovation in particular. The establishment of domesticates in various local economic systems was identified as a problem amenable to archaeological solutions. In this, developments of food collecting in the eastern Mediterranean Basin might be of considerable concern. Some specific innovations and continued achievements of the later food-collecting societies were noted, and the spread of agriculture outward from its apparent centers was seen as a series of interactions between food producers and the later hunter-gatherers. I have suggested that these hunter-gatherers were probably responsible for some innovations usually credited to food producers, and, generally speaking, they do not usually accept agriculture without some degree of resistance.

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A Hypothesis Suggesting a Single Origin of Agriculture

GEORGE F. CARTER

> Nimble thoughts can jump
> both sea and land.
> WM. SHAKESPEARE

We can easily get stuck in a rut, going round and round, reinforcing our preconceived notions by the happy process of talking only to those who agree with us and avoiding any dangerous new thoughts that might expose us to critical comment. Surely this is one of the greatest blocks to the creative process that should infuse the scholarly world. But look, if you will, at the many conferences made up only of "accepted" scholars who blandly sweep over the most fundamental questions and plunge on with their "accepted" lines of inquiry. Note for instance the sweeping under the rug of any possibility of consideration of an overseas origin of the Olmec civilization in America. "There have been, over the past century, a number of arguments made for Old World germinating contacts, either by way of the Pacific or from Africa by way of the Atlantic. None of these OPINIONS in my judgment are to be seriously considered" (my emphasis). I will not identify the source since my purpose is not to ridicule anyone, but to exemplify a situation. How revealing that the carefully compiled evidence of real contact can be swept under the rug as mere opinion! Or, as an example of name calling, consider the coining of the term hyperdiffusionists for those who disagree with the independent inventionists on the extent of diffusion in culture history. And it IS only a matter of extent. No one argues that diffusion does not occur. On the contrary, it is accepted that diffusion is immensely more frequent than invention. On the other hand, without invention there would be nothing to diffuse. There is little room for dogmatism, and great need for open-ended consideration of the evidence, and the many possible conclusions our tenuous evidence allows. Instead of this we find a dogmatic defense of the anthropological Monroe Doctrine: there were no ideas diffused to America, at least no important ideas, and anyway, American agriculture is utterly distinct. On a worldwide scale we find much the same thing. Independent agricultural origins are postulated not only for eight or more areas but also for the same genus of plant; e.g. *Phaseolus* is not only accepted without question as independently domesticated, but even as independently domesticated four or more times in the New World. These are extraordinary conclusions, for most of culture history shows that totally independent inventions are exceedingly rare. The gist of my argumènt will be that there is little reason or evidence to show that agriculture is different from other cultural systems, and that one might expect but one origin.

I am not really insistent on just one. Perhaps there were two: roots versus seeds. Or maybe there were four: two in the Old World and two in the New World. But, I will present a hypothesis suggesting a single origin. This will be one step beyond Carl Sauer's suggestion of a dual origin: root crops and then seed crops in both the Old and New Worlds, though with characteristic boldness he even suggested that the New World agriculture might stem from southeastern Asia (Sauer 1952: 40).

I would prefer to continue to mull over this idea of the single origin of agriculture for a few years and then someday present a book-length study of the problem complete with exhaustive botanical and historical arguments. At present it is still a hypothesis and one that is not at all developed to the degree that I would like it to be. It is presented now in hopes that it will forestall freezing of opinion too rapidly in the field of inquiry into the origins of the agricultures of the world, for it seems to me that we must keep our options open yet a while.

The gist of my thesis will be that the evidence is more in favor of an invention, dependent on individual genius, than on an inevitable process. Granted that genius functions in a particular cultural setting complete with antecedents and so forth, still there is a difference between emphasis on processes which will produce the end result by some inexorable functioning versus the flash of creative genius. Emphasis on process leads to an explanation favoring numerous cultures arriving at similar solutions, at various places at different times. Emphasis on invention leads, as with all fully documented inventions, to the expectation of single occurrences. So far as I know no one can actually prove a single case of absolutely separate multiple invention of anything. The zero, one of the perennial cases put forward, is certainly a poor one, for the idea of the zero far precedes the alleged Hindu invention, and appears in America in cultures which we have ample reason to believe were either Asiatic transplants or greatly influenced from Asia and the Mediterranean. Note for example the recent publication which called attention to a forty-year-old article suggesting a Chinese origin for the dot and bar mathematics that is the basis for Olmec-Mayan mathematics (Kraus 1971; I have been unable to get the original article). Needham (1959), however, shows a magic square where six is shown by such a system and illustrates bars used with the value of five in Chin and Han times. Let us return from this aside to the problem of time.

Man and his ancestors undoubtedly gained their daily sustenance by being primarily gatherers of vegetation — leaves, buds, bark, seeds, and fruit — for some thirty million years or more; the anthropoid primates — monkeys, apes, and man — had their evolutionary origins no later than the end of the Eocene, and the dentition of all of them indicates their predominantly herbivorous diet from that time to this. The essence of the act of gathering is aided by the grasping hand, which, in coordination with superb stereoscopic vision in color, allowed (and allows) complete discrimination in the process of gathering. As gatherers, the earliest hominids, man's more immediate ancestors, became bipedal on the ground fifteen or more million years ago. One of the major sources of food at that time for this population probably was the hard seeds of many glade- and plains-living plants, including the grasses (Jolly 1970).

Better known than the obscure hominid ancestors of fifteen million years ago are the gracile australopithecines, of which the earliest known fossil is now dated at five to five and a half million years (Howell 1972). In spite of much imagery expended on the supposed hunting prowess of those gracile australopithecines (Ardrey 1961), they more probably depended for food almost entirely upon gathered plants. If, as advocated by some anthropologists (Robinson 1972; Reed, personal communication), we include these gracile australopithecines in the genus *Homo*, as *H. africanus*, then we can state validly that the earliest representatives of our genus were primarily gatherers and — looking at their magnificent molars — we can guess that, as has been true of hominids before and since, at least some of their daily gathering (depending upon season) was of hard seeds. Present evidence indicates that not until the time of the differentiation of that population we call *Homo erectus*, not more than a million years ago, did humans become competent as hunters of medium- to large-sized game. Even so, as with many hunting populations of the present or documented past, the majority of the calories available to the people in tropic and temperate regions came from plants. On the long