GUILÁ NAQUITZ

Archaic Foraging and
Early Agriculture in Oaxaca, Mexico

Edited by

Kent V. Flannery

Museum of Anthropology
University of Michigan
Ann Arbor, Michigan

1986

1

The Research Problem

Kent V. Flannery

We save the *chipil* seeds to plant for the time when there are none in the fields so we will
have greens to eat.

Zapotec informant,
speaking to ethnobotanist
Ellen Messer, ca. 1972
(Messer 1978:88).

INTRODUCTION

During the late Pleistocene period, at a time roughly
equivalent to the Wisconsin glacial advance in North
America, the climate of central Mexico seems to have been
colder and drier than today's (Bradbury 1971). Areas such
as the Tehuacán Valley of southern Puebla were inhabited
by a late Pleistocene complex of animals that today would
be found on the cooler and more arid plains of Coahuila and
southern Texas (Flannery 1967). As the Wisconsin glacia-
tion drew to a close, temperatures in central Mexico rose until
the mountain valleys of Puebla and Oaxaca had been taken
over by a rich forest of cacti and legume trees, giving way
at higher elevations to oak and pine. This transition to a
semiarid, semitropical complex of vegetation, not unlike to-
day's, appears to have been well along by 8000 B.C.

Sometime between the establishment of this post-
Pleistocene vegetational regime and the start of the fifth
millennium B.C., the Indians of this part of Mexico began
the cultivation of a series of plants, some of them native
members of the Holocene thorn forest. For centuries the
prehistoric inhabitants of Puebla, Oaxaca, Morelos, Guer-
rero and the Basin of Mexico had lived off the land, learn-
ing the secrets of the wild vegetation. They had learned how
to roast the maguey, or century plant, to make it edible, how
to make wooden tongs to pick off the spiny fruit of the organ
cactus, how to cook up a syrup from the pod of the mes-
quite, how to leach tannic acid from bitter acorns, and how
to find wild onion and runner bean flowers in the piedmont
underbrush and to predict when they would be ready to

harvest. They had developed a technology that included pro-
tective maguey-fiber sandals, the atlatl, or spear-thrower, the
fire-making drill, the coiled basket, the net carrying-bag, a
variety of snares and traps, the one-hand mano for seed grind-
ing, and the boulder mortar. Camping sometimes in caves,
sometimes in the open, for hundreds of years they followed
a strategy in which they moved to the wild resources as the
latter came ripe, dispersing into smaller groups or coming
together in larger ones as the seasons of abundance permit-
ted. By 7000 B.C. they were carrying their drinking water
with them by means of the bottle gourd, a domestic plant
whose early history is still mysterious and which is not
thought to be native to Mexico.

Finally, the Indians of central Mexico added another
strategy—the manipulation of certain edible plants by selec-
tion and planting. Three of the most important of these were
squashes (*Cucurbita* spp.), beans (*Phaseolus* spp.), and maize,
or Indian corn (*Zea mays*), all of which are discussed in this
volume. These cultivated plants made their first appearance
as relatively minor components of a diet still dominated by
wild species, but their importance was to increase with time
as favorable genetic changes made their cultivation an even
more attractive activity.

AGRICULTURE: "HOW" OR "WHY"?

The origin of agriculture was an important moment in
cultural evolution, and the question most frequently asked
about it by anthropologists is, "Why did it happen?" I would

3

like to reflect on that question for a moment, because it is so different from the one usually asked by paleontologists about biological evolution.

For example, the weight of fossil evidence has convinced paleontologists that birds evolved from reptiles, but one rarely hears a paleontologist ask, "*Why* did birds evolve from reptiles?" Rather, one usually hears the question, "*How* did birds evolve from reptiles?" To a paleontologist, "why?" is a teleological question; it implies that something was done for a purpose. Evolution, on the other hand, is generally considered to be opportunistic: Changes take place not because evolution has a goal to reach, but because opportunity is present and a favorable mutation takes place in the right place at the right time.

Paleontologists will tell you that there are two major competing theories about *how* birds evolved from reptiles (Lewin 1983). According to the "arboreal" theory, the intermediate stage was a reptile who became adapted to gliding at the end of a leap from a high place. According to the "cursorial" theory, the intermediate stage was a reptile who developed "lift" that allowed it to increase its foraging range while running and leaping. However, the question, "*Why* did birds evolve?" is unlikely to elicit the answer, "Because reptiles wanted to catch more insects." Some paleontologists would probably say, "We don't yet know." Still others might answer, "Because certain mutations took place at a time when conditions were right, and natural selection favored those individuals." This is not a specific explanation for the origin of birds but a reference to an underlying principle of evolution.

I have always been struck by the fact that while paleontologists most frequently ask *how*, anthropologists most frequently ask *why*, since it underlines a basic difference between the two fields. Paleontologists are concerned with evolutionary mechanisms, and they try to answer the *how* question by studying the intervening fossil forms from an evolutionary transition. Anthropologists are concerned with human aspirations, with what Rindos (1980) has called "intentionality," and they try to answer the *why* question by studying culturally patterned behavior. Paleontologists do not picture reptiles saying, "Let's turn into birds," but many anthropologists do picture hunter–gatherers saying, "Let's plant these seeds on the talus slope below our cave."

Nor is this an outrageously unscientific thing for anthropologists to picture. Let us consider, for a moment, the wild herb *Crotalaria pumila*, a member of the legume family, which is known as *chipil* in the Valley of Oaxaca. While not considered a cultivated plant, the *chipil* has a symbiotic relationship with man; it is one of the first second-growth weeds to invade fallow cornfields in the region, and its leaves are gathered, along with wild *epazote* (*Chenopodium* sp.) and *quintonil* (*Amaranthus* sp.), to be used as flavoring in tamales or stews. Ethnobotanist Ellen Messer (1978) found some Zapotec-speaking villagers saving the seeds of *Crotalaria*, and when she asked why, she was given the answer seen at the start of this chapter: "We save the *chipil* seeds to plant for the time when there are none in the fields so we will have greens to eat."

There are several points that can be drawn from this example. First of all, human cultures do display intentionality, and it may be completely rational and practical. They have perfectly good reasons for planting *chipil*, although those reasons would not necessarily be recoverable by an archaeologist 5000 years from today. The Zapotec certainly do not see themselves as occupying a stage of "incipient *chipil* cultivation", but if favorable genetic changes were to appear in certain *Crotalaria* plants and the Zapotec were to select for those traits when saving seeds, there could one day be a domestic strain of *chipil*. Botanical colleagues might be able to explain to future archaeologists how the change had taken place, but not necessarily why. As in the bird evolution example above, they could refer it to an underlying principle of genetic change and selection, leaving the problem of intentionality aside.

Anthropological archaeologists, however, would continue to kick intentionality around, and there is a good chance they would try to explain *chipil* domestication in terms far more deterministic than Messer's informants. Based on some of the models we examine later in this chapter, they might argue that the Zapotec were forced to augment their diet with *chipil* cultivation because of (1) climatic changes that reduced their maize crop, (2) population increases that made the maize crop inadequate, (3) greater production demands made on them by a local government, or (4) some nutrient missing in maize but present in *Crotalaria.*

While all these explanations are plausible, the quotation from Messer's informant suggests a much simpler and less frequently cited reason for planting *chipil*. The Valley of Oaxaca is a semiarid, semitropical environment in which many variables—most notably rainfall—show great fluctuation and unpredictability from year to year. While annual rainfall usually ranges between 450 and 650 mm, over the period from 1930 to 1970 there were 4 years with less than 400 mm, 5 years with over 700 mm, 2 years with less than 300 mm, and 1 year with over 1000 mm (Kirkby 1973:Fig. 58). In very dry years, the *chipil* may not survive at all, while in very wet years it may be flooded out by standing water on the fields. The Zapotec deal with this unpredictable annual variation by saving *chipil* seeds to plant whenever nature does not provide enough greens. It is not a response to overpopulation, crop failure, or political pressure, but to uncertainty.

This is perhaps the most general point to be drawn from our *Crotalaria* example. The origins of agriculture involve both human intentionality and a set of underlying ecological and evolutionary principles. When we, like paleontologists, ask *how* a particular case of domestication happened, we are probably going to be confronted with a biological principle such as mutation, natural selection, or (as we shall see below) coevolution. This is probably the level on which the most universal aspects of domestication lie. When we, like anthropologists, ask *why*, we may find ourselves confronted with reasons that are local and can only be modeled after we have done our best to reconstruct the local cultural pattern.

Small wonder, then, that anthropologists have had so little success producing universal explanations for the origins

of agriculture. Concerned as they are with human aspirations and intentionality, they have focused on what is probably the most regionalized and least universal aspect of agricultural origins. No matter how elegant a hypothesis of population pressure or climatic stress one could propose for preceramic Mexico, if we could go back to 8000 B.C. and interview the first cultivators in that region, we might simply be told, "We save the bottle gourd seeds to plant for the time when there are none, so that we will have something to carry our water in."

Of course, we can ignore human intentionality and concentrate on the underlying biological aspects of domestication, producing a universal model as Rindos (1980) has recently done (see below). The trouble is that while such a model may satisfy biologists, it often does not satisfy anthropologists (see Ceci 1980; Cohen 1980). The reasons for this lack of satisfaction are not hard to discover: Anthropologists want cultural explanations for cultural behavior and are not usually satisfied when culture is reduced to biology. Anthropologists know that human hunter–gatherers are mammals, primates, and predators, but that is not what anthropologists find most interesting about them. It is no accident that so much of recent writing on hunter–gatherers deals with "decision making," "strategy," "consensus," "information flow," and the application of models drawn from economics (Keene 1981a; Moore 1981; Reynolds 1978; Reynolds and Ziegler 1979; Winterhalder and Smith 1981).

Recently, Wilson (1979) presented a diagram showing the different ways in which biologists, primatologists, anthropologists, and sociologists view human social behavior; a modified version is reproduced here as Fig. 1.1. As one moves down the diagram toward biology, one gains perspective and loses resolution; as one moves up the diagram toward sociology, one gains resolution and loses perspective. Reducing the origins of agriculture to a case of plant–animal symbiosis or coevolution achieves biological perspective but lacks resolution for Mexico or the Near East. Focusing on human decision making in Mexico may provide high resolution for that area yet be inapplicable for the Near East. Perhaps no model will achieve much acceptance until it effectively includes both the universality of the biological principles and the specificity to tie it into the human cultural pattern for a given region.

The research problem we have chosen to attack in this volume is essentially that: to develop a model for the origins of agriculture in Oaxaca, Mexico, that deals not only with some of the underlying and more universal processes of domestication but also with the specific events of the Oaxaca cultural sequence. Our goals in this first chapter, however, are more limited. We deal first with the empirical data on early agriculture in Mexico as they are currently understood. Following that, we examine recent theoretical frameworks for the origins of agriculture as they relate to the Oaxaca–Puebla region.

THE PALEOBOTANICAL EVIDENCE

It is unlikely that we will ever know the precise chronology of early domestication in Mexico; preceramic sites are too
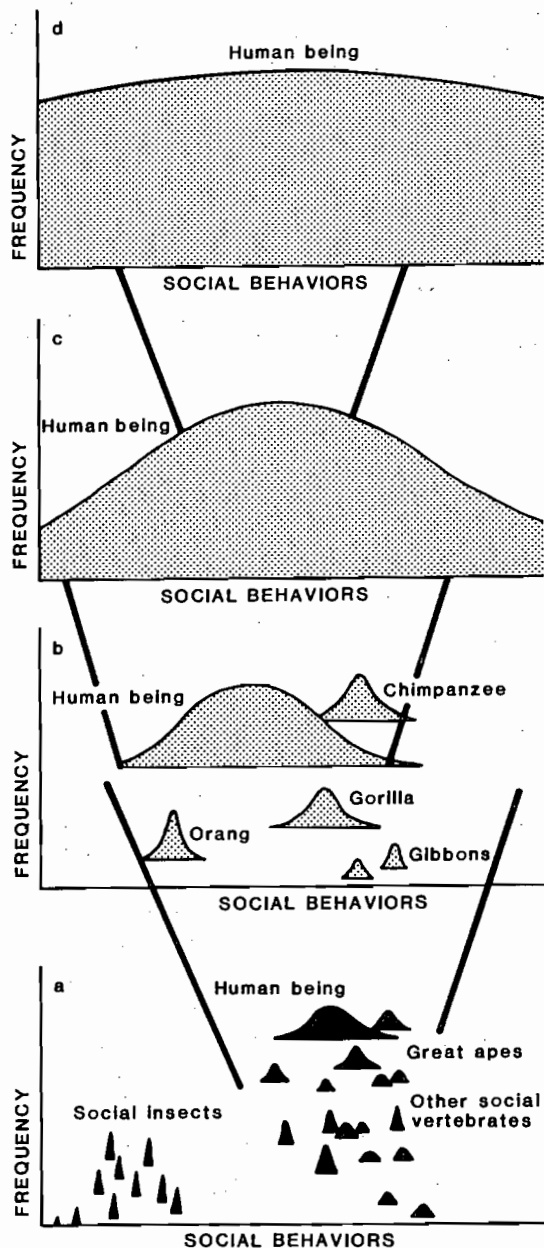

FIGURE 1.1. Wilson's (1979) diagram comparing the ways social behavior is focused on by (a) biologists, (b) primatologists, (c) anthropologists, and (d) sociologists. In going from a through d, we gain greater resolution but lose perspective, and vice versa. (Redrawn, with modifications, from Barash 1982:Fig. 7.2)

scattered and too infrequently excavated, conditions for plant preservation too unpredictable, and radiocarbon dates too frequently ambiguous. In Table 1.1, I have listed the earliest known occurrences of gourds, squashes, runner beans, maize, and teosinte, but the table only underscores how many more excavations we will need before we have an adequate sample. All dates are given using the original 5570-year half-life of ^{14}C as recommended by the journal *Radiocarbon*.

TABLE 1.1
The Oldest Archaeological Occurrences of Some Early Mexican Domesticates and Their Wild Ancestors[a]

Name of plant	Oldest archaeological occurrence	Comment	Subsequent archaeological occurrences
Lagenaria siceraria (bottle gourd)	Rind fragments in Zones C–B1 of Guilá Naquitz, Oaxaca (7400–6700 B.C.); 7000 B.C. levels at Ocampo, Tamaulipas.	Rinds are good evidence, but seeds are better.	One rind in Zone XIII of Coxcatlán Cave, Puebla (5050 B.C.).
Cucurbita pepo (summer squash)	One "pepo-like" seed in Zone D of Guilá Naquitz (8750–7840 B.C.); larger sample at 7000 B.C. at Ocampo, Tamaulipas.	Seeds are fairly good evidence, but peduncles are even better.	14 seeds and peduncles in Zones C–B1 at Guilá Naquitz (7400–6700 B.C.).
Wild runner beans	100+ in 8750–6700 B.C. levels at Guilá Naquitz; 14 between 7000 and 5500 B.C. at Ocampo, Tamaulipas.	Tamaulipas specimens are a wild ancestor of domestic runner bean; Guilá Naquitz specimens are not.	One runner bean from roughly 200 B.C. at Coxcatlán Cave.
Zea mexicana (teosinte)	Pollen grains in Zones C–B1 of Guilá Naquitz (7400–6700 B.C.).	It is still disputed whether teosinte pollen can be distinguished from that of maize.	Waterlogged seeds from 5090 B.C. level at Zohapilco, Basin of Mexico.
Zea mays (maize)	18 cobs in Zone XIII of Coxcatlán Cave, Puebla (5050 B.C.).	Mangelsdorf says "wild"; Beadle would say "domestic" (see text).	74 more cobs in 5000–3000 B.C. levels of Tehuacán (Puebla) caves.

[a] Source: Flannery 1973: Table 2.

At the time I began work in Oaxaca, virtually all our paleobotanical evidence for early domestication came from three areas studied by MacNeish: (1) dry caves in the Sierra de Tamaulipas (MacNeish 1958), (2) dry caves in the Sierra Madre near Ocampo, Tamaulipas (Mangelsdorf *et al.* 1964), and (3) dry caves in the Valley of Tehuacán, Puebla (Byers 1967). Since then, additional data have come from (4) Niederberger's carbonized or waterlogged material from the open-air site of Zohapilco (Tlapacoya) in the Basin of Mexico (Niederberger 1979), and (5) my own excavations at Guilá Naquitz and other sites in Oaxaca, reported in this volume. Bearing in mind how limited an area these five excavation projects cover (and the fact that in some cases the earliest evidence consists of one seed, one pod, or some equally small sample), let us now briefly consider three of the most important kinds of early cultivated plants.

GOURDS AND SQUASHES

Perhaps the first plants domesticated in the New World were members of the family Cucurbitaceae, a group that includes not only the squashes and pumpkins but also the bottle gourd *Lageneria siceraria*. This gourd, as botanist Charles B. Heiser has pointed out, "must have been one of man's most important plants before the invention of pottery," and may even have been one of the world's most widely distributed plants in the era of preceramic nomadism (Heiser 1979:71). Especially in arid and semiarid environments, foragers needed some way to transport drinking water. The !Kung Bushmen of the Kalahari and the Natufian hunters of the Negev used ostrich egg shells, some Near Eastern hunters may have used goatskin bags, and the Indians of Mesoamerica and the Andes had the bottle gourd.

Since this gourd is one of the very first cultivated plants to appear in the New World archaeological record, many researchers suspect that it may have been the first plant domesticated there. Lathrap (1977) has suggested that in addition to its use as a water bottle, the gourd's suitability as a net float led to its worldwide dissemination along coasts. One of the intriguing aspects of seeing the gourd as the New World's first domesticate is that it removes the origins of agriculture from the realm of food stress and population pressure, making it a technological breakthrough. Hunter–gatherers moving into an environment where bottle gourds did not occur naturally needed only to carry along some seeds to provide themselves and their descendants with a supply of water bottles. Like the origin of ground-stone tools, this is a technological change that could have happened at any population density and under a wide range of environmental conditions. Moreover, once cultivation of gourds had begun, it could have acted as a preadaptation for the growing of other plants of the same family: Wild squashes, whose seeds are edible (though their flesh is bitter to nonexistent), would have been instantly recognizable as something easy to grow, store, and transport.

A problem in any reconstruction of gourd domestication, however, is the uncertainty about its wild ancestor. The bottle gourd is supposed to be native to Africa, but as Heiser (1979) points out, the evidence for its homeland is far from conclusive, and the plant was widespread in Asia and America in prehistoric times. It has been shown that gourds can float in seawater for at least 347 days—"ample time to have crossed the Atlantic," according to Heiser—and still have viable seeds. In fact Heiser (1979:114) suspects that the bottle gourd floated to America many times, perhaps as much as 50,000–100,000 years ago, and may have washed up anywhere from Brazil to Mexico.

Once established on the New World coast and used by man, the bottle gourd could have become a commensal plant whose seeds would sprout naturally in human campsites. Thus, "there is no real need to postulate the existence of a wild gourd being found outside of man's habitations at any time in the Americas" (Heiser 1979:116). Heiser could picture the gourd originally spreading as a weed rather than as a domestic plant, achieving a truly symbiotic relationship with humans—the latter dependent on gourds for water bottles and net floats, the gourd dependent on human settlements for its survival—which was followed eventually by domestication. This model of mutualism is not unlike the approach taken by Rindos (1980).

If the bottle gourd was Mexico's earliest domesticate and served as a preadaptive model for cultivating the other cucurbits, this might eventually be confirmed by the archaeological record. At the moment, there are simply too few archaeological occurrences to show this, but we can say that both gourds and squashes are early in the Archaic record (Table 1.1). Rind fragments of bottle gourd occur in MacNeish's caves near Ocampo, Tamaulipas by 7000 B.C. (Whitaker et al. 1957) and in Zones C–B1 of Guilá Naquitz by 7400–6700 B.C. (Chapter 20). Fourteen seeds and peduncles of Cucurbita pepo occur in those same levels at Guilá Naquitz, and one pepo-like seed was found in zone D (8750–7840 B.C.). Guilá Naquitz also produced specimens of Apodanthera, the coyote melon, showing that local wild cucurbits were being collected as well (Chapter 20). Finally, pollen studies by Schoenwetter and Smith (Chapter 15) produced one cucurbit pollen grain from Zone B3 of Guilá Naquitz, identified as Cucurbita cf. moschata. This squash, not represented by macroscopic remains from Guilá Naquitz, was previously documented by five specimens from Coxcatlán Cave, Puebla, in the 4500–3000 B.C. time range (Cutler and Whitaker 1967). Perhaps future excavations will more clearly establish a temporal priority for bottle gourds over edible cucurbits in Mexico.

BEANS

Three species of Phaseolus—common beans (P. vulgaris), runner beans (P. coccineus), and tepary beans (P. acutifolius)—have wild ancestors in Mexico. The oldest archaeologically documented specimens are wild runner beans from MacNeish's caves in Ocampo, Tamaulipas (Kaplan 1965) and from Guilá Naquitz (Chapter 21). The 14 specimens from Tamaulipas are wild Phaseolus coccineus, the ancestor of the domesticate, and date to 7000–5500 B.C. The 100-odd specimens from Guilá Naquitz, dating to 8750–6700 B.C., are small black runner beans belonging to a wild species that has left no domesticated descendants. Their unusually high frequency, relative to other plants, makes it seem possible that the occupants of Guilá Naquitz were experimenting with growing them near the cave (Chapter 24), but if this is so, it was a strategy that was ultimately abandoned. The history of incipient cultivation may one day show many attempts to grow plants that were

eventually given up in favor of more responsive species; if Callen (1967b) was correct, Setaria, or foxtail grass, may be another example of this phenomenon.

TEOSINTE AND MAIZE

Maize (Zea mays) has by far the most enigmatic and controversial origin of any major cultivated plant. This can be directly attributed to the fact that no wild race of maize currently exists. This raises two possibilities: Either "wild maize" has become extinct or maize descended from a different wild plant.

The theory perhaps best known to archaeologists is that of Paul C. Mangelsdorf, who along with R. G. Reeves proposed in 1938 that cultivated maize descended from a now-extinct wild pod-popcorn in which individual kernels were enclosed in and protected by chaff rather than by a cupulate fruit case (Mangelsdorf 1947, 1958; Mangelsdorf and Reeves 1938, 1939). Most archaeologists are familiar with this view because Mangelsdorf, together with Walton Galinat, has been involved in analyzing virtually every important collection of early archaeological maize from Mexico, including the famous collections from the Tehuacán caves (Mangelsdorf et al. 1967).

A different theory, originally proposed in 1877 and vigorously revived in the 1970s by Beadle (1972, 1977), Galinat (1970, 1971), and de Wet and Harlan (1972), holds that maize may be descended from the widespread Mexican grass, teosinte (Zea mexicana). If this theory is correct, no "wild maize" ever existed, and maize and teosinte may in fact be the same species. This theory is supported by anatomical studies that claim to show a "clear-cut connecting link between the maize cob and the cupulate fruit case of teosinte" (Galinat 1970) and by scanning electron microscope studies that show no significant differences between maize and teosinte pollen (Banerjee and Barghoorn 1972). However, despite Beadle's suspicion that no more than 6–10 independently inherited gene differences separate maize and teosinte, Mangelsdorf (1974) can still argue that no genetic manipulation has yet shown how one plant could have given rise to the other.

I summarized the evidence on both sides of the controversy a decade ago (Flannery 1973:290–296), and there is no point in repeating it here. Since that time, however, Iltis et al. (1979) have discovered a perennial teosinte, Zea diploperennis, growing in Jalisco. Mangelsdorf (1983:94) has now argued that "the ancestry of corn is biparental, with Zea mays and Zea diploperennis playing coequal roles: the former contributing the botanical features that corn has had in all stages of its evolution under domestication; the latter providing many modern races with vigorous root systems and resistance to diseases." From this perspective, the annual teosintes would be recently evolved weeds that played no role in corn's ancestry. Unfortunately, Mangelsdorf goes further than this, stating that "the entire array of archaeological evidence throughout Middle America clearly suggests that teosinte itself has probably never been used either as a

cultivated or gathered fruit crop. Teosinte fruit cases are distinctive, durable, highly archaeologically preservable, and yet virtually never found prior to or during incipient agriculture" (Mangelsdorf 1983:89).

This statement overlooks the fact that Chalco-type teosinte has been found at the archaeological site of Zohapilco (Tlapacoya, Basin of Mexico) in levels dating to 5090 B.C. (Lorenzo and Gonzales 1970; Niederberger 1979). While this antedates by only 40 years the radiocarbon date for the earliest known corncobs from Coxcatlán cave in the Tehuacán Valley, it is a discovery that can hardly be ignored. Small *Zea* pollen grains, believed by Schoenwetter to belong to teosinte, occurred in a sample from Zone C at Guilá Naquitz, dated to 7450–7280 B.C., well before any macroscopic evidence for actual maize (Chapter 15). Of course, the mere presence of teosinte pollen does not prove that teosinte was being eaten. Indisputable use of teosinte, however, went on in Formative Oaxaca (1500–500 B.C.), where it occurred intermixed with maize kernels in carbonized seed samples from Tomaltepec (J. E. Smith 1981) and San José Mogote (C. E. Smith, Jr., unpublished data, 1980). At Fábrica San José, Ford's ethnobotanical study of the corncobs showed introgression with teosinte "to such an extent that one must conclude that this annual wild grass was growing in the corn fields themselves" (Ford 1976:265). In other words, Mangelsdorf may have underestimated the use of teosinte in early Mesoamerica, especially since we have so few excavated sites from the most relevant periods.

Mangelsdorf is quite right, however, in pointing out that no site yet excavated documents a gradual genetic change from teosinte to maize. Whether this means (1) that such a change did not take place or (2) that we have too few sites in too few areas depends on which of the two theories you like. Most recently Iltis, one of the discoverers of *Zea diploperennis*, has proposed a new and controversial reconstruction of the origins of maize. According to Iltis (1983), a "catastrophic" sexual mutation converted the male teosinte inflorescence (tassel) into the female ear of maize. By "modifying Beadle's teosinte hypothesis to accommodate some of Mangelsdorf's criticisms," Iltis believes, the catastrophic sexual transmutation theory can "lay to rest over 40 years of debate" (Iltis 1983:893). If Iltis is correct, the transition from teosinte to maize may have happened so fast that it will be very hard to detect in the archaeological record.

Unfortunately, Guilá Naquitz contributes no new information to this debate, presumably because its preceramic levels are too early to catch the domestication of *Zea*. The youngest preceramic level at Guilá Naquitz dates somewhere near 6700 B.C., while the first maize cobs from Tehuacán date to 5050 B.C. (Table 1.1). Hence, cultivation of *Zea* may not have taken place until some two millennia after bottle gourds and squash had come under domestication.

We did recover four small, primitive-looking maize cobs at Guilá Naquitz, but their provenience was such that little can be concluded from them. All occurred in small lenses of ash that lay stratigraphically above Zone B1 (the youngest preceramic level) and below the isolated scatters of Formative

sherds that preceded the deposition of Zone A (early Postclassic). Since these lenses of ash had no artifacts, all we can do is guess that they date somewhere between 6700 B.C. and the period of the earliest Formative sherd scatters, 1000 B.C.(?). The four specimens (Fig. 1.2) have been examined by Richard I. Ford and George Beadle, and both agree that, depending on which side of the maize–teosinte argument you prefer, they could be considered either (1) maize–teosinte hybrids or (2) primitive maize that shows strong teosinte influence in its ancestry. In view of the specimens' undated stratigraphic position, there is little more to say at this time.
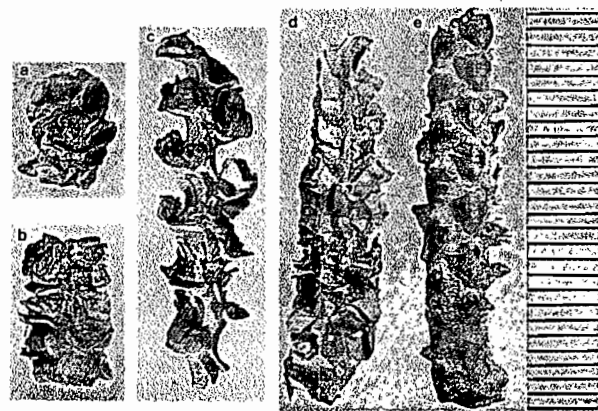

FIGURE 1.2. Primitive-looking maize specimens or maize–teosinte hybrids from undated ash lenses resting on the upper surface of Zone B1 at Guilá Naquitz (see text). a, b, two fragments of the same cob, Square C9; c, two-rowed "cob" from Square D10; d, e, two specimens from Square D7. (Scale in millimeters.) Specimens are now in Departamento de Prehistoria, Instituto Nacional de Antropología, Mexico City.

THE MAIZE–BEAN–SQUASH ASSOCIATION

Every textbook on Mesoamerica points out that corn, beans, and squash were an "agricultural triumvirate" for the Indians of that area. Indeed, it is sometimes suggested that one of the Indians' major contributions was the discovery that all three plants could be grown together in the same field. Not only do beans contribute nitrogen to the field as corn extracts it, the combination of the two crops in the diet produces a more complete plant protein. This is because beans are rich in lysine, an amino acid deficient in corn (Kaplan 1965).

It would be wonderful if we could attribute this triumvirate to the genius of the Mesoamerican Indian; but, in fact, nature probably provided the model. The annual teosintes are weedy, pioneer plants that colonize natural scars in the landscape, such as fallow cornfields, scree slopes, or areas opened by fire. In thorn-forest areas of Guerrero and the Michoacán–State of Mexico border today, abandoned milpas are rapidly taken over by massive stands of teosinte up to 2 m high (Flannery and Ford 1972). Wild runner beans and wild squash occur naturally in such fields, with the bean twining
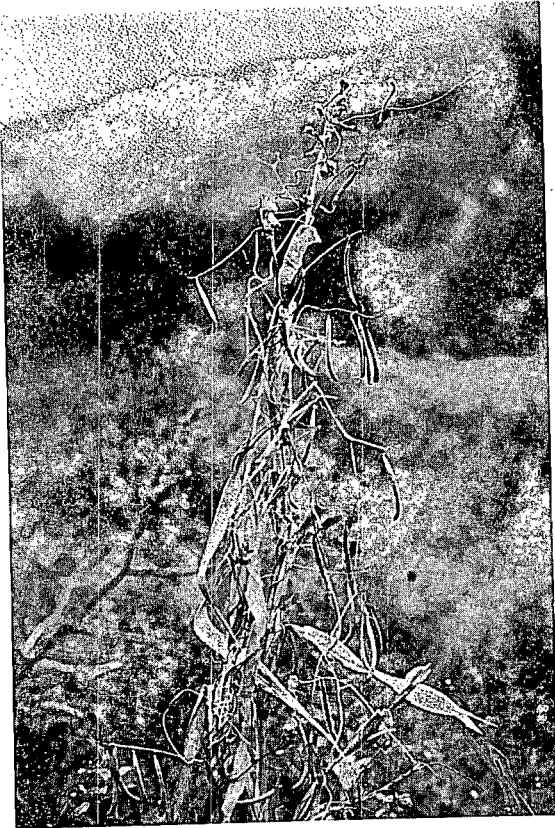
FIGURE 1.3. Wild runner beans twining around stalk of mature teosinte near Chilpancingo, Guerrero, 1971.

around the stalks of the teosinte (Fig. 1.3) and the cucurbits growing among the bases of the same stalks.

The Indians could hardly have failed to notice this naturally occurring weedy association whenever they cleared the thorn forest in such a region, and they eventually duplicated it in their cultivated fields. This may be yet another case of the kind of mutualism pictured by Heiser (1979) and Rindos (1980), with man providing the environmental disturbance and *Zea, Phaseolus,* and *Cucurbita* providing the economically useful plant association. However, in terms of the approach I outline in Chapter 2 of this volume, it is also an example of information flow: Nature, in effect, demonstrated to incipient cultivators the format of the multispecies milpa.

MODELS AND THEORIES FOR EARLY DOMESTICATION

Years ago it was assumed that early domestication in Mexico had resulted from a "discovery"—humans, after some prolonged period of living off the wild vegetation, discovered that seeds thrown away from one of their meals had sprouted, thereby providing them with the knowledge necessary for cultivation. Frequently, it was suggested that this had taken place on the talus slope below an occupied cave or rockshelter.

It is true that some plants are "weedy camp followers" that grow readily on the talus slope of a cave, or any other area disturbed by humans; as we have seen, teosinte and the wild curcurbits are noted for this. However, it is unlikely that domestication resulted from such a discovery. We know of no group of hunter–gatherers so ignorant that they are unaware that plants grow from seeds. Those who rely heavily on wild plants as food usually have extremely detailed knowledge of the habits, life cycle, and environmental requirements of a wide variety of plants, and in spite of this they do not cultivate them.

Today one almost never hears domestication proposed as a discovery or an invention. Rather, the last few decades have seen the emergence of a whole new series of models and theories, far more ecologically based and carefully thought out. Each of these models has something to recommend it, yet each runs into problems with the empirical archaeological data, and the latter—although far from ideal—provide one test for any model of agricultural origins. In this section we see what useful insights we can derive from each of these models.

Climatic Change

We have already mentioned the late Pleistocene and early Holocene environmental changes that took place in Mexico not long before cultivation began. It is no surprise that as the evidence for these changes has increased, so has archaeologists' curiosity about the role they may have played.

According to one model, the late Pleistocene occupants of Mexico lived primarily by hunting large game such as mammoth, mastodon, or native American horse. With the extinction (or northward migration) of many large species at the end of the Pleistocene, hunter–gatherers were forced to rely increasingly on small game and plant foods; and their increased dependence on the latter led to agriculture. According to another model, the climate of Mexico was thought to have gone through a hot, dry period between 5000 and 3000 B.C. (called the "Altithermal" in North America) that might have influenced agriculture. However, archaeologists were not in agreement as to whether this hot, dry period had (1) forced people to turn to agriculture by depleting their other food resources or (2) delayed the success of early agriculture until after 3000 B.C. For example, in a 1966 synthesis, Willey (1966:83) considered both dry and wet periods as possible stimuli:

> With the increasing heat and dryness of the Altithermal climatic era, after 5000 B.C. this casual, minimal cultivation seems to have been taken more seriously. . . . Then, with the return to the cooler and more moist conditions of the Medithermal, after 2500 B.C., plant cultivation apparently "exploded."

Let us now bring up some complications with these models. First, the late Pleistocene stratigraphic levels from Coxcatlán Cave make it clear that the Indians of that era did not rely solely on large fauna but hunted dozens of small species and even engaged in communal drives of jackrabbits (Flannery

1966, 1967). Moreover, the late Pleistocene climate was, if anything, drier than the early Holocene, and probably significantly cooler. Once the Holocene climate had been established in regions such as Oaxaca, pollen studies do not reveal a period so hot and dry that the post-Pleistocene vegetational communities would have been eliminated, although there were climatic fluctuations during the Archaic that might have caused those communities to migrate upslope or downslope (Schoenwetter and Smith, this volume:Chapter 15). In short, I see no evidence for climatic changes, either at the end of the Pleistocene or between 5000 and 3000 B.C., that would have forced the adoption of agriculture.

We cannot rule out, however, the possibility that climatic changes slowed down or speeded up incipient agriculture. This possibility is examined in Chapter 31 by Reynolds during the course of a computer simulation of incipient agriculture in the Valley of Oaxaca. Having simulated adaptation under today's climatic regime, Reynolds runs the program again—first under conditions wetter than today's, then under conditions drier than today's. This allows us not only to see whether such changed conditions would speed up or slow down incipient agriculture but also to examine the program output to find out how they did so.

What is the most important insight we might retain from these various climatic change models? In my opinion, it can be found in an article by H. E. Wright (1977) entitled, "Environmental Change and the Origin of Agriculture in the Old and New Worlds." Wright discusses the fact that the late Pleistocene climate of the Zagros–Taurus Mountains in the Near East was so cold and dry as to leave the area treeless and severely reduce the potential of the mountains for supporting wild cereals. He then adds, "With the end-Pleistocene climatic change about 11,000 years ago, a new suite of plants immigrated into the region, including the wild cereal grains. These and other wild plants were added to the diet of man" (Wright 1977:297). In other words, emmer wheat cultivation in the mountains of Iraq was a very unlikely occurrence prior to 9000 B.C.

This does not mean that end-Pleistocene climatic change *caused* agriculture, or that agriculture might never have begun without it. After all, even during the peak cold of late Pleistocene times, there were more sheltered localities, such as the Ghab Basin of Syria, that probably provided a refuge for some wild cereals (Niklewski and Van Zeist 1970). However, the range of the wild cereals expanded enormously during the early Holocene, making them accessible to thousands more people and increasing the likelihood of their use as food.

Much the same could be said of Mexico. End-Pleistocene changes did not cause agriculture, but the early Holocene saw an enormous expansion of the thorn–scrub–cactus forest in which so many of the wild ancestors of the early domesticates grew. Theoretically, agriculture might have begun without these changes, but it would probably not have taken place as early in Tehuacán and Oaxaca, to name only two regions.

Population Pressure

Another model that has risen in the past, and then fallen, and lately risen again is one in which humans were forced to adopt agriculture when their population reached the point where wild plants and animals were an insufficient source of food. There are two variants to this model. In one, the pressure for agriculture comes from local imbalances in population "density equilibrium" (Binford 1968); in the other, "overpopulation" is seen as a worldwide phenomenon that underlies all early cases of food production (Cohen 1977).

In a now-classic paper on post-Pleistocene adaptations, Binford (1968) proposed a density equilibrium model for the adoption of agriculture, which I later applied to the Near East (Flannery 1969). Binford argued that adaptation would change only in the face of some disturbance in the equilibrium between population and environment, such as (1) a change in the physical environment that would bring about a reduction in the density of chosen plant or animal foods or (2) a change in population density or distribution that would raise human populations too close to the carrying capacity of the immediate area. Following the end-Pleistocene climatic changes described by Wright (above), the Near East would have become a mosaic of "favorable" habitats (e.g., oak–pistachio woodland) and "marginal" habitats (e.g., gravel desert or arid steppe). The wild cereals also have definite "optimum" zones, in which they grow densely, and "marginal zones," in which they do poorly. Following Binford's model, the optimum habitats should have been the centers for population growth ("donor areas"), with the marginal habitats ("recipient areas") receiving the emigrant overflow. It is in these marginal zones that man–land disequilibrium would have been felt first. Thus, in 1969 I suggested that farming might have begun not in the optimum areas of wild cereal growth (where wild wheat and barley already grow as densely as they would in a cultivated field), but around the margins, where it would be necessary to cultivate cereals in order to achieve the same densities that occur naturally in the optimum zone.

There may be parts of the world where this process actually took place. Particularly in the Levant, where sedentary life based on the collection of wild cereals may actually have preceded agriculture, there may have been a mosaic of regions with great disparities in population density. Unfortunately for the model, however, available archaeological data (such as they are) do not show strong population increases in optimum areas, such as the southern Zagros or the Lebanese woodland, but the very opposite—some of the most striking end-Pleistocene increases in numbers and sizes of sites are in marginal habitats, such as the Negev (Marks 1971). While I am not prepared to discard this model out of hand, I also feel it needs further refinement because it comes too close to making climatic change and population growth into prime movers. Let us suppose that population growth in the end-Pleistocene Levant was a product of life in sedentary communities, which can shorten birth spacing and reduce infant mortality (Binford and Chasko 1976). Let us further

suppose that sedentary life began not as a response to optimal habitats but as part of a shift from foraging to collecting in the sense that Binford (1980) has recently defined those terms (see below). Suddenly we have a model that is substantially more complicated, and with causality not nearly as clear-cut. (In other words, our nice, neat, density equilibrium model has become a typical anthropological problem.)

Before discussing the difficulties of applying the local density model to Mexico, let us consider the far more ambitious and global model of Cohen (1977), who actually uses the term "overpopulation" in his subtitle. Cohen begins by asking two questions: (1) Why would human populations successfully adapted to a hunting–gathering life-style (thought by some to be mankind's most stable adaptation) abandon it in favor of farming? and (2) Why would so many of the world's people adopt agriculture at roughly the same time? Cohen concludes that agriculture would only have been adopted if the demand for calories was out of balance with the productive potential of wild foods and that "only one possible explanation—actual population growth—could account for an ecological imbalance that affected so many parts of the world." Cohen (1977:279) argues that hunting and gathering populations "had saturated the world approximately 10,000 years ago and had exhausted all possible (or palatable) strategies for increasing their food supply within the constraints of the hunting–gathering life style," thus leaving themselves no alternative but agriculture. Cohen realizes that this implies that human populations, unlike those of any other mammal, grow continuously without stabilizing at some carrying capacity; and he accepts this implication.

I think the two questions asked by Cohen are good ones, particularly the second. It does seem to be the case that agriculture began independently in the Near East, Southeast Asia, Mesoamerica, and the Andes sometime during the period 10,000–5000 B.C. Given the whole of human prehistory, that is a relatively brief period of time. To be sure, there are occasional claims for agriculture in earlier times, but most rest on shaky stratigraphic or radiocarbon evidence. The most recent of these to be brought back in line with the 10,000–5000 B.C. period is barley, which had been tentatively radiocarbon dated to 19,060 B.P. at Wadi Kubbaniya in Egypt (Wendorf et al. 1979). New analyses of the actual barley grains themselves give a date of 4850 B.P., suggesting they were not actually associated with the charcoal that yielded the earlier dates (Wendorf et al. 1983).[1] I expect similar future readjustments for most other cases of cultivated plants that are currently dated before 10,000 B.C.

Cohen is therefore quite right when he says that any universal theory for the origins of agriculture must explain not only how it began, but also why it began at that particular time. The end-Pleistocene climatic changes mentioned previously

could have influenced this, but surely there had been warm interglacial periods earlier in human prehistory. Many authors have speculated on why agriculture did not begin in one of those earlier periods, several of them suggesting that human populations "had not reached a cultural stage of a nature appropriate to take the necessary steps to domestication" (Wright 1977:296). Braidwood and Howe (1960:180) have pointed to a "regionalism" in tool assemblages that was not seen before the late Paleolithic; perhaps, they once argued, this reflects a degree of "settling into" the regional environment that was necessary before agriculture could begin. Now Cohen has suggested, in effect, that world population would have been too sparse during earlier interglacials to exhaust the wild food resources.

The main problems in applying either Cohen's overpopulation model or Binford's density equilibrium model to Mexico are presented by the empirical archaeological data. Populations in highland Mexico were so small when agriculture began that phrases such as "overpopulation," "food crisis," and "exhaust all possible strategies" appear to be exaggerations. Elsewhere (Flannery 1983:35) I have estimated that early Archaic populations in the Tehuacán–Oaxaca valleys consisted of no more than 1 person per 9–29 km², and "this figure includes only the valley areas, not the surrounding mountains" where so much foraging was done. In Chapter 24, I calculate that a catchment circle with a 5-km radius would contain more plant food than the occupants of Guilá Naquitz could eat in their lifetimes. As if this were not enough, it is even possible that agriculture in Mexico began with a plant that was not even domesticated for food. If the bottle guard was the first domesticate, the causes had nothing to do with food shortage; it was a technological breakthrough, equivalent to assuring oneself a reliable supply of flint.

On a deeper and more theoretical level, I share with Hassan (1981) a certain reluctance to see human population growth as an independent variable, unrestrained by the factors that govern other mammalian populations. In zoology, population increase is generally taken as the sign of a successful adaptation or as evidence that a favorable evolutionary advance has already been made, rather than as a cause for further evolution. In paleontology, I cannot think of a single new species believed to have arisen because its fossil ancestors became too numerous; becoming numerous is a mark of fitness. What increasing population growth often does do is to bring about *density-dependent* changes in behavior, but these changes are usually reversible when population declines.

In his book on demographic archaeology, Hassan (1981:Fig. 12.3) combines all available "best estimates" on Pleistocene world population increase into a single diagram, which is reproduced here as Fig. 1.4. Note that world population first began to climb steeply about 100,000 years ago, at the start of the Würm glacial advance, when the transition from hand-axe users to Middle Paleolithic flake-tool users was virtually complete. These flake-tool users ("Neanderthals" *sensu latu*) gave way to Upper Paleolithic *Homo sapiens sapiens* around

---

[1] Wendorf, Close, and Schild are to be commended for the dedication with which they pursued the true date of this barley, subjecting it to Electron Spin Resonance Spectroscopy and Linear Accelerator Radiocarbon Counting. Most archaeologists would have let the joy of the early dates overrule their own scientific curiosity.
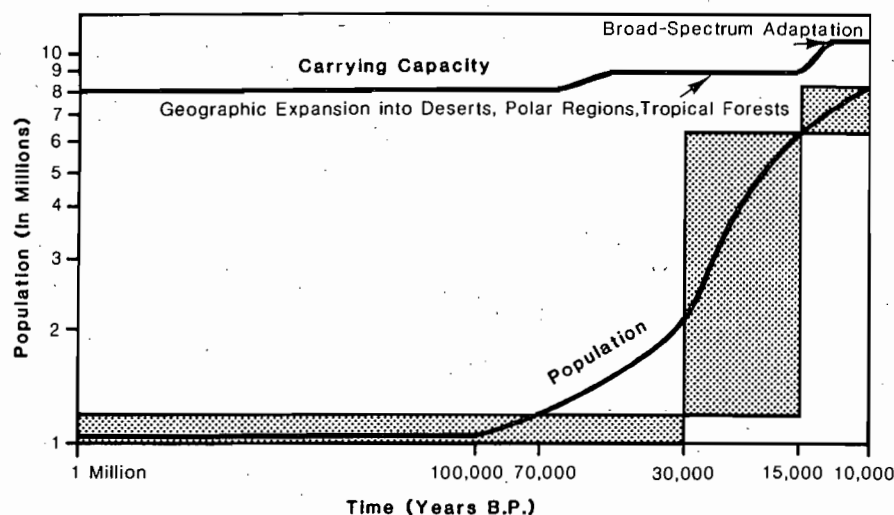
FIGURE 1.4. Reconstructed increases in world population and carrying capacity for humans during the Pleistocene. (Redrawr Hassan 1981:Fig. 12.3.)

30,000 years ago, setting off a precipitous population increase that saw "fully modern" humans continuing an expansion into deserts, polar regions, and tropical forests of the Old World, as well as colonizing the Americas.

Hassan's graph shows the carrying capacity for end-Pleistocene hunter–gatherers rising above 10 million worldwide as the result of the "broad spectrum adaptation." The latter is an empirically detectable phenomenon for which I suggested a name in 1969 and about which I have more to say below. The most significant feature of Hassan's chart is that it shows carrying capacity moving ahead of population as the result of changes in foraging efficiency that preceded (and made possible) population growth. This is, I believe, more in line with the way zoologists view evolution and population growth. The steep rise of Hassan's population curve after 100,000 B.P. is almost certainly associated with a major evolutionary change, perhaps of an irreversible (genetic) type. The approach of the population curve to carrying capacity after 15,000 B.P., on the other hand, might have set off behavioral changes in response to increased competition and decreased mobility (see below), but most of these changes would be reversible should population density fall for any reason.

Hassan's chart ends at 10,000 B.P., near the start of the period singled out by Cohen as crucial for incipient agriculture. What happened next, of course, was that successful agriculture was in turn followed by an enormous rise in population in those areas of the world where it developed. Significantly, however, this happened because agriculture raised the carrying capacity of those areas to the point where environmental resistance to human population growth was substantially lower, not because of an irreversible (genetic) change such as the one we have proposed for 100,000 B.P. This is an important point to remember when we consider Rindos' (1980) coevolution model below.

What are the most important insights we might retain the various population pressure models that have beer posed? One of them is surely Cohen's insistence that v count for the events of the period 10,000–5000 B.C. I would like to do, however, is drop the terms "overpo tion" and "food crisis" from his hypothesis and rephr this way: By 10,000 B.P., all the world's major landn had been populated to the point where (even though tl tual densities might not have been that high) emigratio been considerably reduced as a strategy for adjusting : land relationships.

For most of the Pleistocene, emigration had undoul been one of the easiest and most frequently used options to early humans, whether to correct for unpredictable tages or to resolve conflict (Lee and deVore 1968). By 1(B.C., however, most human groups would have had neig on all sides, making it increasingly necessary for them to their problems on a local basis. The late Pleist(regionalism of which Braidwood and Howe speak is or of evidence for this, as is the broad spectrum adapt discussed below. Complicating this picture still further possibility that the end-Pleistocene climatic changes des(by Wright may have increased, rather than decreased, tl vironmental variation with which hunter–gatherers h deal. As Hassan (1981:206–207) has expressed it:

By the end of the Pleistocene, the climatic fluctuations a panying the transition from glacial to postglacial condition great. These conditions would have enhanced group mc However, because the overall population density during tl minal Pleistocene was much higher than it had been (esp(in favorable habitats) and because contiguous groups pr(a barrier against territorial expansion or long-range migr localization of population and regional differentiation w tensified. Localization also enhanced the exploitation of ; range of resources which served as further protection agair predictable fluctuations.

This passage by Hassan effectively synthesizes the most defensible parts of Wright's climatic change model, Cohen's population growth model, Binford's optimal habitat model, and Braidwood and Howe's notions of regionalization. What Hassan does *not* do is accept Cohen's notion that end-Pleistocene hunter–gatherers had "exhausted all possible strategies" short of agriculture. Post-Pleistocene hunter–gatherers in many parts of the world opted for sea mammal hunting, salmon fishing, intensive seed collecting, or some other option instead of agriculture. Not all these strategies had as their goal "an increase in calories per unit space." What many seem to show—perhaps on the global scale that Cohen insists we should seek—is a penchant for dealing with uncertainty, or unpredictable variation, at a level of competence shown by no previous society.

Broad-Spectrum Adaptation

Since Hassan's model incorporates the notion of a broad-spectrum adaptation, this would seem to be the place to bring that concept up to date. In the same paper in which I tried to apply Binford's density equilibrium model to the Near East (Flannery 1969), I pointed out that early agriculture in that region was preceded by a trend from exploiting a more "narrow spectrum" of environmental resources to a more "broad spectrum" of edible wild products. In addition to the ungulates that had so long been a dietary staple, Near Eastern peoples now ate progressively larger amounts of fish, crabs, water turtles, molluscs, land snails, partridges, migratory waterfowl, and perhaps even wild cereal grasses. The same period was characterized by several preadaptations for agriculture, such as the development of ground-stone technology and storage facilities. While ground-stone tools appear to have originated as tools for milling red ocher, the technology was available when humans turned to extensive use of cereal grains. As for storage pits, their use expanded so rapidly that one site in Israel dating to 10,000–8000 B.C. contained more such facilities than all previously known sites in the Near East put together (Flannery 1969:78, 1973:281).

The use of storage facilities by hunter–gatherers is a topic that promises to shed considerable light on their strategies for coping with uncertainty. Binford (1980) has recently suggested that most hunting–gathering societies occupy a position along a strategic continuum that reflects differences in mobility and subsistence patterns. At one end of the continuum are *foragers*, the most mobile, and at the other end *collectors,* the most nearly sedentary. To use Binford's phrase, foragers "map onto" resources through residential moves and adjustments in group size. As resource opportunities change throughout the year, foragers travel to where the food is, and their settlement pattern becomes dispersed or aggregated according to whether resources are dispersed or aggregated. Collectors, on the other hand, supply themselves with specific resources through logistically organized task groups. That is, the band as a whole tends to remain in one spot while smaller groups go out and bring back resources; there may

be men's deer-hunting trips or women's fruit-collecting trips, but these differ from whole-scale residential moves like those of foragers. As Binford (1980) and Thomas (1983) have stressed, most groups fall somewhere between the two extremes, and there are foragers such as the !Kung San Bushmen who "map onto" *mongongo* nut groves while organizing their big-game hunting logistically through all-male hunting parties.

While both foragers and collectors may practice storage, the extent to which it is used can affect settlement and subsistence strategies. Storage, as Binford (1980:15) points out, is a way of extending the time of use of a specific resource beyond its period of natural availability in the habitat. Yet storage reduces the mobility of a group, so that "with increases in storage dependence there will be an expected increase in the logistical component of a settlement system." If, as Hassan suspects, group mobility was reduced during the terminal Pleistocene because contiguous groups provided a barrier to unrestrained emigration, we might expect the period 10,000–5000 B.C. to show both (1) an increase in storage features and (2) an increasing number of shifts from mobile foraging to more sedentary collecting. And Binford (1980:18) adds that still further increases in logistically organized procurement strategies should be expected with shifts toward agricultural production.

Binford also argues that since the purpose of storage is to extend the season of availability, then the shorter the growing season, the greater the reliance on storage. In an interesting graph (Binford 1980:Fig. 4), he compares the storage dependence of various contemporary hunter–gatherers with the "effective temperatures" of their regions, showing that groups in colder climates rely more on storage. Given the regions where Binford has done fieldwork, it is understandable that he sees temperature as the critical limiting variable; however, this would not work for us in Tehuacán and Oaxaca where growth is limited more by deficient moisture. It is not so much frost that ends the growing season at Guilá Naquitz but rather a 5-month drought with mean monthly rainfalls below 10 mm (Kirkby 1973:16–17). Hence, the key to sedentary collecting in the Tehuacán–Oaxaca region was to find something that could be stored from November until the rains began again in April. In some higher-altitude localities, such as Guilá Naquitz, it is clear that an effort was made to store acorns (Chapter 5). In lower-altitude localities, such as Coxcatlán, one sees few candidates for storage until maize came along.

We have also seen, in our discussion of *Crotalaria* at the start of this chapter, that the November-to-May dry season is not the only problem in the Oaxaca region. Harsh as it is, the dry season is at least a predictable problem, since the Indians know it is coming every year. On the other hand, the annual variation in rainfall—which can range from drought to flood—follows no observable cycle and is therefore unpredictable. Kirkby's (1974) work on individual and community responses to rainfall variability in Oaxaca suggests, at least to me, that the Zapotec response is aimed at reducing the differences between wet and dry years. In this regard, I am struck by the analogous behavior of the Tewa Indians

studied by Ford (1968). Like highland Mexico, the Pueblo region of the Southwest is an area with great contrasts in productivity between rainy and dry years. Like the Zapotec, the Tewa are not so much interested in controlling nature as in making it more predictable; both in their technology and in their ritual, they seek not to maximize but to reduce the extremes and thereby lessen variation.

In light of all these additional lines of data, I would like to rephrase the broad-spectrum adaptation in the following terms. The period 10,000–5000 B.C. witnessed annual and seasonal climatic variation at least as great as today's, perhaps even exacerbated by the fact that in many parts of the world the late Pleistocene vegetation was being replaced by Holocene floral communities that included the wild ancestors of many eventual domesticates. In the past, emigration had been one way of dealing with unpredictable variation, but now mobility had been reduced by the societal barriers of an expanding hunter–gatherer population. During 10,000–5000 B.C., therefore, humans had to learn how to cope with unpredictable environmental variation without leaving their own region, and they did so on a scale never before seen. Diversification of the subsistence base to include many more species on lower trophic levels was only one strategy in a milieu that included the escalation of storage, the development of ground-stone tools, improved fishing technology, and an increasing trend from foraging toward logistically based collection. What all these alternatives seemingly shared was a goal of resiliency, risk reduction, amelioration of environmental extremes, and an increase in resource predictability. From the standpoint of human intentionality, the cultivation of a series of weedy annual plants that like to pioneer disturbed habitats may simply have been one of a series of strategies that, like storage, were designed to help even out the differences between good and bad years. And if collectors "move goods to consumers with generally fewer residential moves" (Binford 1980:15), then agriculture was the ultimate collecting strategy.

Human societies are particularly well suited to dealing with long-term seasonal, annual, predictable, and unpredictable variation because they have a multigenerational "memory" and can share information on the success of past subsistence-settlement strategies in ways no other creature can. In Chapter 31 of this volume, Reynolds explores the use of a long-term, memory-using, efficiency-improving model for simulating the activities of incipient cultivators in the Valley of Oaxaca. I do not want to get too far ahead of our story here, but Reynolds' results suggest that a great deal of the prehistoric record can be understood as an effort to cope with uncertainty, even at relatively low population densities and in relatively benign environments.

Agriculture: Coevolution and Symbiosis?

As end-Pleistocene populations grew and diversified the plant species they were using, they undoubtedly disturbed their habitats more and more. Human modification of the environment, even such a simple act as clearing a campsite in the thorn forest, increased the prevalence of a second-growth species at the expense of primary vegetation. Even before agriculture began, as Heiser (1979) has stressed in the case of the bottle gourd, certain plants had achieved a symbiotic relationship with human hunter–gatherers.

In a recent article and an even more recent book, Rindos (1980, 1984) has developed this concept of plant–human symbiosis into "a new model for the origin and development of domesticated plants and agricultural systems" (Rindos 1980:751). I hope that my brief summary does not do too much violence to the model, for it is too lengthy to consider here in all its details. Basically, Rindos sees domestication as a symbiotic interaction between man and the plants on which he feeds, the product of long coevolution between the two. Citing numerous cases from the animal world, he discusses the symbiotic relationships between ants and acacias, termites and their fungus gardens, and squirrels and oak trees. Squirrels, viewed from Rindos' perspective, are one of the oak trees' mechanisms for dispersing their acorns; thus man becomes a mechanism by which maize disperses its seeds. As Aschmann (1980) puts it, Rindos "has taken the capability of choice or decision making in evolving agricultural systems away from humans and given it to the plants." Indeed, Rindos makes it clear that human intentionality is unnecessary to his model and may parsimoniously be set aside. Consistent with this view, Rindos see the morphological changes characteristic of domestic strains as having been favored by the symbiotic relationship, not selected by man, and he cites examples of plants unable to survive in the wild without their animal partner.

Agriculture, in Rindos' model, *follows* domestication; it is "a set of integrated activities that affects the environment inhabited by the domesticated plant throughout its life cycle" (Rindos 1980:752). Even here he insists that "agriculture cannot, in any meaningful way, be restricted to the human-plant interaction. If we try to find any activity or collection of activities to define the man–plant relationship, we are at the same time defining other animal–plant relationships" (Rindos 1980:754–755). Small mammals, he argues, accidentally "plant" seeds in nitrogen-rich soils near their nests. True, but how many small mammals deliberately hand-sort and plant large seeds, as the Mesoamerican Indians do, or make decisions about which species to plant "after a subtle evaluation of costs, yields, and markets related to the particular environment at the farmer's disposal" (Aschmann 1980)?

Obviously, I am sympathetic to many aspects of Rindos' model. After all, in 1968 I argued for a relationship between preceramic foragers and prickly pear cactus (Flannery 1968) that sounds like the one Rindos is proposing, and in Chapter 2 I provide some quantification for that relationship. My main worry about Rindos' model is that, in the process of finding some analogies for agriculture elsewhere in the animal kingdom, he has concluded that human domestication of plants can be filed away as just one more case of a well-known biological process. In 1957, when I was a zoology student at the University of Chicago, it might have seemed like a good idea, but now that I am an anthropologist I have reservations.

Is agriculture really the same kind of plant–animal coevolution displayed by ants and acacia trees? Let us consider the

interaction between early Archaic foragers and *Phaseolus* beans. Wild beans have two genetic characters that aid in propagation: (1) Their pods twist like a corkscrew when ripe, shattering on drying in such a way that the seeds are well dispersed, and (2) their seed coats are so impermeable that they will sprout only after the rainy season has begun in earnest, rather than opening prematurely after an aberrant early thundershower. Both characters were favored by natural selection, but both reduce the beans' attractiveness to humans; the former character makes them harder to harvest, while the latter requires extensive soaking in water to make them edible. In spite of these characters, it is clear that phenotypically "wild" beans were extensively used by Archaic foragers in both Tamaulipas and Oaxaca (Table 1.1). There is even some circumstantial evidence, summarized in Chapter 24, to suggest that the phenotypically "wild" runner beans at Guilá Naquitz were having their density artificially increased by cultivation.

Ultimately, a domestic strain of bean developed in which (1) the pod became limp, straight, and nonshattering, and (2) the permeability of the seed to water became greater (Kaplan 1965). Both these changes benefited humans but were deleterious to the bean's seed dispersal mechanism. If I read Rindos correctly, he would argue that these changes were produced by natural selection as the bean entered into a coevolutionary relationship with humans; and because they prevented the bean from reproducing adequately, became the reason humans were later drawn to cultivate *Phaseolus*. This is an interesting thought, but I doubt that many botanists would believe that a strain of bean with such a deficient seed dispersal mechanism would be able to survive unless man was already cultivating it, in defiance of natural selection.

Moreover, all the archaeological data at our disposal suggest that cultivation began before such morphological changes. In Mexico, Callen (1967b) found statistical evidence for incipient cultivation of *Setaria*, including the selection and planting of larger seeds, in the absence of any genetic changes that would make the plant dependent on human intervention. At Guilá Naquitz, Whitaker and Cutler (Chapter 20) have identified cultivated squash in levels with rind fragments that show that edible flesh had not yet developed. On the fringes of Mesopotamia, Helbaek (1969) spoke of "cultivated wild barley," a crop that could be shown to differ from true wild barley by its average seed size but that had not yet undergone genetic change in the direction of the nonshattering rachis.

Yet another archaeological example comes from the village of Tell Mureybit on the Euphrates River in northern Syria (van Loon *et al.* 1968, 1970). Dating to 8200–7500 B.C., Mureybit produced a series of earth ovens, used for the parching of grain, and some two dozen flotation samples totalling several thousand carbonized seeds. Van Zeist and Casparie (1968) describe the collection as dominated by einkorn wheat (*Triticum boeoticum*) of the large-seeded variety *thaoudar*. This is a cold-tolerant wheat that thrives in the Turkish mountains 100–150 km north of Mureybit but cannot be found in the lower and warmer environment of the preceramic site. The inescapable conclusion is that the Mureybit villagers had removed einkorn from its native habitat and were growing it on the Euphrates floodplain, where they harvested it with flint sickles and ground it in limestone mortars after parching. Two-row barley (*Hordeum spontaneum*), lentils (*Lens* sp.), and bitter vetch (*Ervum* sp.) were also present, giving Mureybit an early version of the same complex of cereals and legumes that characterized later Near Eastern villages.

Most significantly, none of the cereals studied by van Zeist and Casparie were phenotypically domestic; they lacked the tough rachis and fragile glumes characteristic of domestic einkorn (*Triticum monococcum*) and barley (*Hordeum vulgare*). Obviously, therefore, their cultivation began before the kind of genetic change that ultimately made them dependent on humans for survival. The alternative possibility— that Mureybit's main food was brought in by the ton from wild cereal stands located 100–150 km away—seems remote on the basis of ethnographic data. Nor do the sickles, grinding stones, and parching pits make it seem that intentionality played no role in the cereal use of the Mureybit villagers.

In other words, while I like many aspects of Rindos' model, the archaeological data strongly suggest that the crucial genetic changes resulted from human selection *after* cultivation of phenotypically wild plants had begun. And the latter is not true coevolution; albeit less sophisticated and intentional, it is more analogous to the genetic engineering that now goes on in laboratories all over the country. Features such as the corncob are monstrosities, artifacts of human intervention that could not survive in nature. When man selected for limp bean pods or pendulous chile pepper fruits, he was working against natural selection, not with it; to borrow Dawkins' (1976) phrase, the "selfish gene" met its match in "selfish man." And in contrast to biological coevolution, humans do not seem to have undergone *genetic* change— whether morphological or behavioral—as a result of their interaction with corn, beans, or squash. There is no gene for agriculture; any significant change the incipient cultivators underwent was cultural, and it was reversible. The ethnographic record provides us with cases of previously full-time agriculturalists who have largely reverted to hunting and gathering under changed conditions (see Lathrap 1968), just as a number of mammalian density-dependent behaviors may be given up when densities change.

Not surprisingly, Rindos' 1980 article ran into some strenuous objections from anthropologists. Cohen (1980) was quick to point out that if domestication was simply a case of symbiosis, we might expect it to be strung out through all of human prehistory rather than clustered so significantly between 10,000 and 5000 B.C. Ceci (1980), like Aschmann, was disturbed that "deliberation is taken away from man or, conversely, granted to all living things." Shaffer (1980) argued that "any model ignoring the different human responses to plant and animal domestication has limited value for assessing domestication's impact on the history of world flora and fauna or on the history of human behavior."

Rindos seems puzzled at this response. While acknowledging that his model "is not subject to empirical verification"

and that he has "identified no clear causes for agriculture" (which he believes "has no single cause"), he is "forced to wonder" why such an unnecessary variable as human intentionality "should cause such a fuss" (Rindos 1980:769–770). "Human genetics," he points out, "proceeds well without recourse to intentionality." Unfortunately, Rindos could not have picked a worse example. While it may tell us a great deal about blood types and eye color, human genetics has not told us much about the differences between hunter–gatherers and agriculturalists; nor is it likely to.

I suggest that what we have here is a classic case of scholars from two different disciplines talking past each other. We have already discussed Wilson's (1979) diagram showing the differences in resolution and generality between anthropologists and biologists (Fig. 1.1). Rindos, trained as a botanist, is content with reducing domestication to a biological model that is admirably general but (by his own admission) lacks the resolution to explain the origins of agriculture in any area. He apparently does not realize that anthropologists, trained to seek explanations in terms of cultural, social, political, and economic decisions, are most interested precisely in the testable, high-resolution explanations that his model fails to provide. The fact that there are biological analogies for human behavior is as big a bore for anthropologists as human intentionality is for Rindos. This is especially true since archaeological data suggest that, contrary to his expectations, intentional cultivation seems to have preceded the genetic changes leading to domestication.

So far, every model or theory we have considered has provided us with some insights we can defensibly maintain. In Rindos' case, I would like to preserve the notion of plant–animal symbiosis, but I feel it applies more accurately to the period of hunting and gathering than to later times. The mutualism of man and wild bottle gourds; the affinity of teosinte, wild runner beans, and squashes to areas of human disturbance; and the dispersal of prickly pear seeds by man (see Chapter 2) all seem to me acceptable cases of symbiosis. What happened next was a series of changes in cultural behavior, which produced genetic monstrosities in opposition to natural selection. This was not coevolution in the biological sense for it did not involve genetically programmed behavioral changes in the animal, nor were the genetic changes in the plant induced by natural selection. As a cultural phenomenon, it must be attacked by a science that focuses on culture rather than genetics.

Multivariate Models

It will be clear by now that if we save the defensible insights from each of the theories and models discussed above, we have a multiplicity of factors to deal with rather than a single, neat, prime mover explanation for the origins of agriculture. This is the situation Hassan (1981) found himself in when discussing incipient agriculture in the Near East, and his solution was a multivariate model that considers technocultural preadaptation, population density, sedentariness, climatic fluctuation, and many other factors (Hassan

1981:Fig. 13.3; Fig. 1.5, this chapter). Without question, many scholars would prefer to have a parsimonious algebraic formula—to be able to say simply, "Agriculture will begin if and only if x has reached a certain level y and z takes place." Every such attempt I have seen, however, is either contradicted by the archaeological evidence or leaves a great many important questions unanswered.

During the course of this volume we develop our own multivariate model for the origins of agriculture in Oaxaca, testing it not against some abstract logic but against the archaeological record. The biggest danger in such an approach is that the closer one comes to providing an appropriate model for incipient agriculture in Oaxaca, the less he has to say about the universal aspects of agricultural origins. For this reason, we incorporate into the model as many of the universal processes discussed above as we find convincing.

UNIVERSAL ASPECTS OF THE MODEL

In our model, the end-Pleistocene climatic changes described by Wright and the growth of world population described by Cohen and Hassan combined between 10,000 and 5000 B.C. to bring about a density-dependent shift in human cultural behavior over much of the world. Emigration and high mobility declined in importance, and strategies for dealing with predictable (seasonal) and unpredictable (annual) variation on a local basis began to emerge. The regionalism in tool kits mentioned by Braidwood and Howe and the broad spectrum expansion of lower trophic level foods I once described were but two aspects of this shift. Storage became more important, and the diversity of foraging and logistic collecting strategies proposed by Binford become more apparent in the archaeological record. So also do Binford's differences in population among the mosaic patches of the post-Pleistocene environment.

Humans are well suited to the working out of resilient strategies for environmental variability, since they possess multigenerational memory and have unique methods for exchanging information and establishing cooperative relationships over long periods. Their successful adaptation to uncertainty during the period 10,000–5000 B.C. was not the result of a change in genetically programmed behavior, and no anthropologist is likely to be satisfied with a model that treats it as barely different from ant–acacia coevolution.

Humans did, as Rindos suggests, establish mutually beneficial relationships with a number of plant and animal species in the post-Pleistocene environment, but these relationships did not all lead to domestication. Human environmental disturbance certainly favored second-growth species over others, but as late as A.D. 1800 there were still many areas of the world where successful human adaptation did not include agriculture. In Mexico, agriculture may have begun as one of many strategies aimed at reducing the differences between wet and dry years, at making nature "more predictable" in Ford's terms. And it may have begun with a plant that, although symbiotic with man according to Heiser, was used as an artifact rather than a source of food. Having
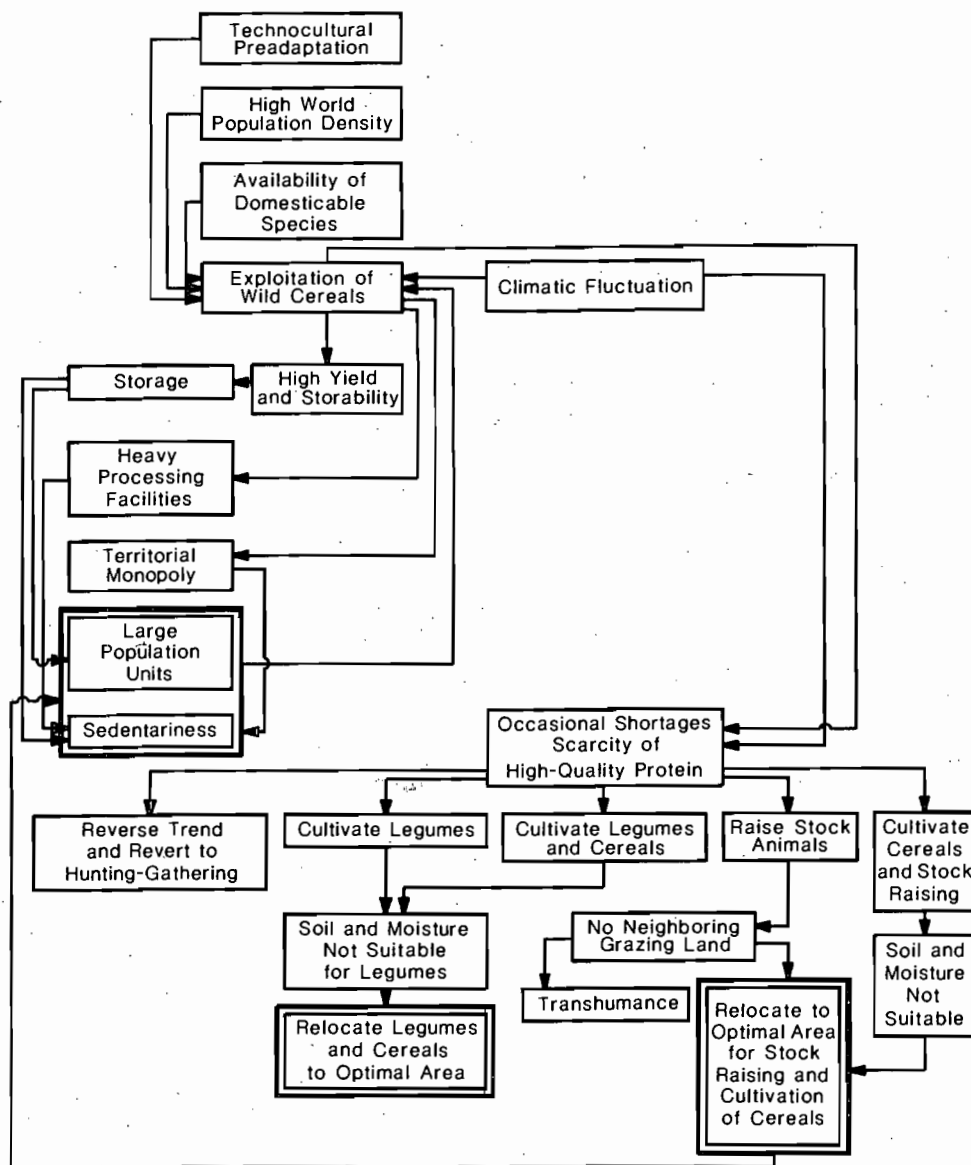
FIGURE 1.5. A multivariate model for agricultural origins in the Near East. (Redrawn from Hassan 1981:Fig. 13.3.)

disturbed the environment unintentionally for centuries, man now began to disturb it intentionally so he would have enough bottle gourds. And if bottle gourds, why not their relatives, the squashes? And why not store them, as Binford suggests, to shorten the dry season? In other words, it is not necessary to use terms such as "food crisis" or "overpopulation" to explain early agriculture in Mexico. By increasing the density of storable plants, you shorten the dry season, lessen the necessity to move, favor the shift from foraging to collecting, and make it easier to deal with uncertainty without being forced to leave your region. Thus, the specific steps taken in Oaxaca may be examples of the more universal hunter-gatherer behaviors recently codified by Binford (1980).

Why did agriculture "take off"? It did not always, and many possible early attempts, such as those proposed for *Setaria*

and the "wild" Guilá Naquitz runner beans, must have ended in failure. In 1968, I suggested that most of the attempts that "took off" did so because of genetic changes that reinforced the payoff for the cultivator. (Rindos discusses many of those same changes, except that in his model they *precede* cultivation.) While occasional mutations favorable to human collectors must have existed in preagricultural times, I follow the archaeological record in suggesting that not until cultivation had begun was there a mechanism—namely, human selection and planting—that would increase the frequency of such variants in spite of natural selection's tendency to eliminate them.

In the case of the squash mentioned above, Whitaker and Bemis (1975) suggest that only the seeds were edible at first. After cultivation had begun, humans selected for (and

planted) those that had the largest numbers of seeds. Selection for greater numbers of seeds led to the emergence of flesh. Further selection for less-bitter flesh led eventually to the domestic varieties we see today, whose large size and palatable flesh make them even more attractive to cultivate than the *Cucurbita pepo* at Guilá Naquitz—which had phenotypically domestic seeds but still had no flesh. I have argued that such favorable changes kicked off a positive feedback loop between cultivator and plant, which increased the investment of time in cultivation (Flannery 1968) but would not have been as difficult to reverse as a true case of coevolution (see Gilbert and Raven 1975).

EPILOGUE

We have indicated that the research problem we chose for ourselves at Guilá Naquitz was to develop a model that would not only deal with some of the underlying and more universal aspects of early domestication but also tie that process into the specific cultural pattern for the Valley of Oaxaca. By far the bulk of this volume is devoted to a case study that deals with what happened in Oaxaca between 10,000 and 5000 B.C. It should be clear from this chapter, however, that we do not see events in Oaxaca as independent of the more universal factors we have discussed. These included end-Pleistocene climatic changes; end-Pleistocene world population growth; plant–animal symbioses prior to agriculture; a widespread, probably density-dependent adaptation to uncer-

tainty and variation on the local level; worldwide broad-spectrum dietary expansion; and a different kind of coevolution, following the adoption of agriculture, that was genetic for plants but cultural for humans.

When we turn to the specific Oaxaca case, however, we want the "resolution" anthropologists like to get from their data. In Chapter 2, therefore, we discuss a model that is specific to Oaxaca, and in Chapters 3–29 we present the "fossil record" from Guilá Naquitz. Over the course of those chapters, we attempt to reconstruct the preagricultural lifeway of the eastern Valley of Oaxaca, seeking to understand the extent to which incipient agriculture was an outgrowth of that lifeway. Using the archaeological and environmental data presented in Chapters 3–29, Reynolds (Chapter 31) has designed a computer simulation to see whether we can model (and hence understand better) the way the Guilá Naquitz foragers added agriculture to their subsistence pattern. This simulation also allows Reynolds to test the effects of climatic change and population growth on the rate at which agriculture was adopted, allowing us to evaluate those factors in our final summation. In other words, we try in this volume to achieve some balance between perspective and resolution, between biology and culture, between universal process and regional intentionality. To be sure, the question "How?" will more often lead to a universal; but for anthropologists, who study rational, cultural, decision-making beings, the question "Why?" will just not go away.

Of course, anthropologists must be careful not to overlook an answer as simple as the one Ellen Messer got from her Zapotec informant.

Ecosystem Models and Information Flow in the Tehuacán–Oaxaca Region

Kent V. Flannery

> In short, we are describing a system in which everything affects everything else, and the complexity of the system of interlocking cause–effect pathways confronts us with a superficially baffling problem in scientific analysis. It is precisely this interlocking feature which is the most characteristic identifying aspect of a system.
>
> (Watt 1966b:2, reprinted by permission of Academic Press)

INTRODUCTION

It should be clear from our discussion in Chapter 1 that the origin of agriculture is a complex problem involving symbiosis, population growth, genetic change, technological change, human strategy, and cultural adaptation. It is, like the system described by ecologist Kenneth Watt in the quotation above, a complexity of interlocking cause–effect pathways in which every variable affects several others. My feeling is that a problem that involves so many mutual-causal processes is not profitably attacked by "linear-causal" or "prime mover" models; it requires a systems approach.

In 1968, in a volume published by the Anthropological Society of Washington (ASW), I presented the skeleton of a systemic, mutual-causal model for the origins and development of agriculture in the Tehuacán–Oaxaca region (Flannery 1968). This book attempts to put a little more flesh on that skeleton. It responds to one of the first comments on my ASW article by Ezra Zubrow, who asked me simply, "Why didn't you quantify your model?" I had to explain that since my own Oaxaca project had barely started, the model was based almost entirely on MacNeish's Tehuacán data, and we

had only recently begun to collect the information necessary to quantify and test it.

Perhaps the biggest mistake I made was to call the ASW paper "Archeological Systems Theory and Early Mesoamerica." This confused some critics, who assumed I was referring to Bertalanffy's (1962) "General System Theory" or Miller's (1965) "cross-level hypotheses of living systems." While I am sympathetic to the approaches of those investigators, the term "General System Theory" appears nowhere in my 1968 article. In fact, I explicitly stated that in the course of that paper I would attempt to apply, "on a prehistoric time level, the kind of *ecosystem analysis* advocated most recently by Vayda and Rappaport, with modifications imposed by the nature of the archeological data" (Flannery 1968:68, italics added).

As Watt's (1966a) volume suggests, systems models have long been part of the biologist's standard arsenal for the analysis of living communities of organisms. Had I called my ASW paper "Ecosystem Models and Mesoamerican Prehistory," many readers would probably have been less confused about what I was trying to do. As it is, Doran's (1970) and Salmon's (1978) critiques—which suggest that systems

19

approaches have relatively little to offer archaeology—do not really deal with the analysis of ecosystems. Rather, they concern themselves with whether General System Theory is really a "theory," with whether systems analyses are really "explanations," and with more formal and mathematical systems approaches, such as those used in engineering. Archaeologists have not really stampeded to such approaches.

The fact is that the kind of systems analysis outlined in Watt's volume is something much less ivory-tower: the modeling of "recurrence relationships" and "mutual-causal processes" in ecological systems. Watt (1966b) uses the example of a simplified, abstract forest ecosystem, including weather, trees, humans, insect defoliators, warblers, spiders, hymenopterous parasites, and insect diseases. In his example, an unusually warm spring season sets the trees to flowering faster and earlier. Insect defoliators respond by rapid growth, utilizing the unusual amount of available food. The warm spring brings warblers back in force to consume the insects, but it also depresses the rates of insect diseases, which do best in cool, damp weather. The more insects eaten by warblers, the fewer are available to spiders and to insect parasites. All these variables affect the population density of insect defoliators, who in turn regulate the probability of tree survival, and hence the economic life of the human lumbering community. If that human community chooses to use pesticides, they affect not only insect defoliators but also the warblers and spiders who consume them.

Watt points out that such a system can only be understood by viewing it as a whole, yet the whole is too complex for normal analysis. Hence, the analyst must break it down into subsystems, analyzing each without losing sight of the necessity to reintegrate the whole at the end of the analysis (Watt 1966b:3). Two basic concepts of Watt's analysis are (1) the notion of recurrence relationships, such as the relations between warbler populations and insect populations above, and (2) the concept of optimization, "the notion that the central aim of systems analysis is that of making the optimal choice from among an array of alternative strategies at each of a sequence of times" (Watt 1966b:4). For reasons that are more fully discussed in Chapter 30, in this volume I retain the concept of recurrence relationships but substitute the notion of "steady improvement over time" for "optimization."

Finally, Watt points out that computer simulation "figures prominently in the arsenal of tricks used by systems analysts." This is because "most systems optimization problems are so complex that they cannot be worked out in any straightforward fashion using paper and pencil" (Watt 1966b:5). Reynolds' simulation of the transition to agriculture in the Valley of Oaxaca (Chapter 31) is such a computer program, using the recurrence relationships discovered by our project and the notion of "steady improvement" mentioned above.

MATTER, ENERGY, AND INFORMATION

All ecosystems are characterized by exchanges of matter, energy, and information among their components (Flannery 1972:400). Most ecological studies monitor the transfer of

matter or energy, but leave aside the information exchang that regulate such transfers. This is particularly unfortuna in the case of human ecology, where the role of informatic is undoubtedly greater than for any other species. One stum ling block has been the widespread impression that whi matter can be measured in grams, and energy in calorie there is no workable unit of measurement for informatio Some archaeologists, however, have begun to consider tl use of the *bit*, defined as the smallest unit of informatic that can resolve the ambiguity between two alternatives. F example, the question, "shall we forage in that particul piñon nut grove?" can be reduced to the binary oppositic Yes/No, possibly resolvable by one crucial bit of inform tion passed on by someone who has recently walked throu; the grove.

Moore (1981) acknowledges that while cultural syste function through flows of matter, energy, and informatic researchers have only done a good job modeling the first tv "The ephemeral quality of information flows . . . has limit their study. Most theories have no explicit role for inform tion sharing and processing as a variable" (Moore 1981:19. Moore's paper is one of the few that attempt to build a mo in which human foragers' demonstrated need and tenden to exchange information about resources are given "equ time" with matter and energy. Significantly, he was forc to design his own computer simulation because he found acceptable set of computational formulae for informati flow. These are the same constraints that led Reynolds design his own program for this volume; at present, if y want your model to "learn" and "improve" through time, y will probably have to design it yourself. Reynolds was ide ly suited for this project because he had previously tried information-sharing, decision-making approach to foragi societies (Reynolds 1978; Reynolds and Ziegler 1979).

In addition to sharing information and making consens based decisions, human foragers can store informati beyond the lifetime of an individual. For example, in a la section of this chapter we discuss the fact that the preser or absence of purple bean flowers in the thorn–scrub–cac forest was a crucial bit of information that could set uj wild bean harvest months later. Consider the following formation transfers that could be based on the sighting such flowers:

1. "I saw bean flowers there last May."
2. "My cousin says he saw bean flowers there last Ma
3. "A microband from across the river says they saw be flowers there last May."
4. "During a dry year in my childhood, we found bea there."
5. "My grandfather said that in case of drought emergency, in his childhood you could usually fi beans there."

Here, then, is one of the ways human foragers differ fr the ungulates studied by optimal foraging theorists (Chapter 30): Not only can they make use of information l ed on the experiences of distant relatives and acquaintan-

they can store information on the availability of certain resources under specific climatic conditions and use it years later. This is how information on plant availability and foraging performance is used in the model that Reynolds presents in Chapter 31.

THE CHOICE OF AN ECOSYSTEM

The choice of the Valley of Oaxaca as an "open-air laboratory" for this kind of study was a relatively easy one. In 1964, MacNeish's Tehuacán Archaeological–Botanical Project (on which I had served as faunal analyst) was drawing to a close, and it was time to select a project of my own. I looked around for a valley (1) close enough to Tehuacán so that the artifact complexes, vegetation, and fauna would all be familiar to me, yet (2) not so arid and agriculturally marginal as Tehuacán. MacNeish had chosen Tehuacán for the dryness of its caves, and I realized that by opting for a less arid valley I might be sacrificing archaeological preservation. However, I was willing to risk this because I wanted to see what differences incipient cultivation might display in a valley that was less dependent on irrigation than Tehuacán.

The Valley of Oaxaca looked promising because its large early urban center, Monte Albán, had presumably been supported by successful agriculture. I also knew the valley had caves and rockshelters, because Lorenzo and Messmacher (1963) had recently located some near Mitla. In addition, MacNeish had examined projectile points in the surface collections of the Frissell Museum of Zapotec Art in Mitla and was convinced many of them were from preceramic periods. Several of Lorenzo and Messmacher's shelters lay only 150 km from Coxcatlán Cave, making it likely that any preceramic complexes I found could be directly compared with Tehuacán's. By Christmas of 1964, my preliminary survey had convinced me that at least some Oaxaca caves had plant preservation, although it seemed unlikely that there would be as many superb dry caves as Tehuacán had.

Hindsight, as a lovable old philosopher once said, is always 20–20; and with the wisdom of hindsight, I realized there were many things left undone in Tehuacán that I wished we had accomplished. Many of these things had to do with the quantification of subsistence data, and, as my old ASW model began to take shape, our need for quantification became even clearer. By the time the Oaxaca Project began, I had some of the following research goals in mind:

1. We needed to locate a series of preceramic sites occupied at different seasons of the year, keeping our fingers crossed that some would have good plant preservation. Luckily, Guilá Naquitz fulfilled this hope (see Smith, Chapter 19; Whitaker and Cutler, Chapter 20; Kaplan, Chapter 21).

2. Having learned from our dry caves which plant and animal species were important in the Oaxaca preceramic, we needed to undertake censuses of them in their native habitats in order to determine how many kilograms of edible portion were available per hectare. This would put us in a better position to quantify the flow of matter. Eventually, we realized that we needed to understand the differences in productivity between wet, dry, and average years, so the censuses took a decade to complete (see Chapter 18).

3. In order to monitor the flow of energy, we needed to have professional nutritionists convert our kilograms of plant and animal foods into calories and protein. (This is done by Robson and Elias in Chapter 23.)

4. Having quantified the present-day environment, we needed to find out whether it was sufficiently similar to the preceramic environment to serve as a guide for interpreting the past. This was accomplished by studies of pollen (Schoenwetter and Smith, Chapter 15) and microfauna (Flannery and Wheeler, Chapter 16).

5. Having assembled all these data, we wanted someone to "operationalize" them, to design a computer model that would simulate the foraging strategies of the Oaxaca Archaic. We wanted to model the flow of matter and energy, to be sure, but without forgetting the role of information and long-term memory in producing the "steady improvement [in adaptation] over time" of which we spoke earlier. The ultimate test of this model, of course, would be the extent to which it matched the archaeological record (see Chapters 30–32).

6. Having modeled the preagricultural pattern, we then wanted to simulate the adoption of incipient agriculture to see if it would provide insights into the way early agricultural strategies grew out of preexisting foraging strategies. I also hoped we could use the model to examine the importance of variables such as population growth and climatic change, which had been proposed as causal factors by previous authors.

7. Finally, since we knew that many aspects of our model would be open to debate, disagreement, and modification by our colleagues, we wanted the centerpiece of the volume to be a straightforward site report on Guilá Naquitz, which could stand alone and be useful even to archaeologists who did not share our point of view. In this way, we would at least succeed in accounting for the money the National Science Foundation and the Smithsonian Institution had spent on our research, although of course we hoped to do more.

CONSTRUCTING A MULTIPLE-LOOP MODEL

In the remainder of this chapter, we consider the form that an anthropocentric model for the preceramic ecosystem in the Tehuacán–Oaxaca highlands might take. As Watt's (1966b) hypothetical example suggests, we need a model composed of numerous subsystems, or "loops," each of which can be lifted out, studied, and eventually put back into the whole. Depending on the policies at a given moment, the recurrence relationships in such loops can be positive or negative, and they can either counteract deviations from the

previous state of the system or reinforce them. In the ASW paper I drew the framework for my model from "The Second Cybernetics," a seminal article by Maruyama (1963) that contains many more insights than I could use at the time. In my opinion, Maruyama's example of a multiloop system is still one of the clearest and most instructive in the literature.

In our Fig. 2.1 we reproduce Maruyama's Fig. 3, his simplified model for the operation of a hypothetical urban system. The seven variables used by Maruyama are linked to each other by a series of feedback loops, some positive and some negative. A plus sign on the arrow between two variables indicates that they change in the same way; for example, as P increases, so does G. A minus sign, on the other hand, indicates change in the opposite way—as S increases, D decreases. These pluses and minuses reflect the policies of the system, and a change in policy from plus to minus can have dramatic effects.

Negative feedback loops are ones that help to counteract deviations from the preexisting pattern and return the system toward its previous state. For example, consider the loop P–G–B–D–P. As the number of people in the city goes up, so does the amount of garbage per area, raising the bacteria per area, therefore raising the number of diseases and eventually lowering the number of people—returning the system toward the original population level, although not necessarily reaching it.

Positive feedback loops are ones that help the system diverge farther from the previous pattern, preventing its return toward the previous state. Consider the loop P–M–C–P. As
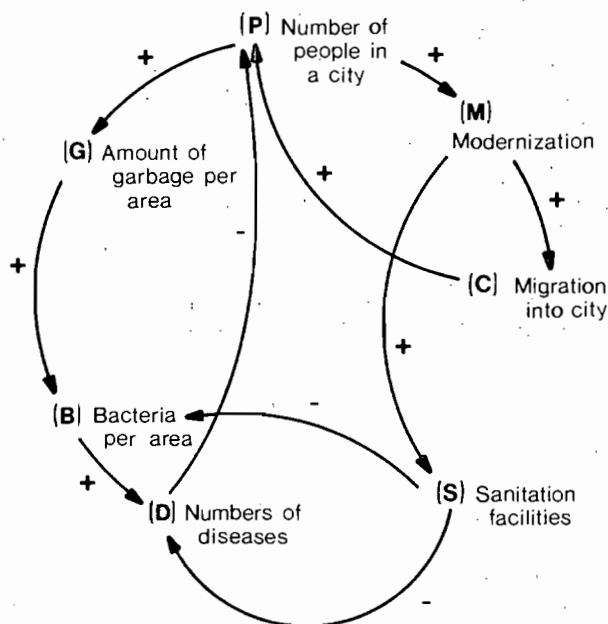

FIGURE 2.1. Simplified model for the operation of a hypothetical urban system. (Redrawn from Maruyama 1963:Fig. 3.)

the number of people in the city goes up, it stimulates modernization, which encourages migration into the city, further raising population and stimulating more modernization and more immigration. As the system diverges farther from its original condition, new loops can appear that cause major changes elsewhere. As an example of this, consider the arrow M–S that sets off the loop M–S–D–P–M. An increase in modernization can lead to better sanitation facilities, reducing bacteria per area, lowering diseases, and thereby partially canceling out the effect of negative feedback loop P–G–B–D–P.

What are the merits of such a model? First of all, even the simple act of putting in the pluses and minuses forces you to be explicit about the relationships among variables, rather than just listing them. A more sophisticated model would even quantify the rates at which variables change in response to others—for example, the number of kilograms of garbage per hectare per year that would be produced by each new person entering the city or the number of bacteria per hectare produced by each kilogram of garbage. Such quantification turns your work into the model of a real system, not just a "heap" (which is what systems analysts call a mass of variables with no explicit relationships). It also puts you in position to make your system "run," to simulate its ongoing progress as Reynolds does later in this volume.

Second, I believe that such a model deals with causality in a way that is more realistic than the linear-causality models sometimes used in archaeology. For example, which is the independent variable in Fig. 2.1? Many archaeologists would like it to be P, the growth of population in the city over time. As we have seen, however, the value of P is partly a function of immigration (C) and disease (D). Indeed, no variable in the system is wholly independent; while they are certainly not all of equal importance, each has contexts in which it is a cause and others in which it is an effect. Such a model may lack parsimony, but it more closely approximates the mutual-causal relationships archaeologists encounter in the real world.

Third, there is another advantage I see in the way systems models deal with causality. In any ecosystem, there are variables that are demonstrably associated. In the more deterministic models frequently used by archaeologists, these associations are often seized upon and proclaimed as "laws." For example, in Watt's model, "The higher the temperature, the lower the incidence of insect diseases"; in our model, "The greater the increase in seed permeability, the greater the likelihood that bean cultivation will be intensified." In a systems model these are not laws but simply the "recurrence relationships" of which Watt (1966b) speaks. They are important, but they are no more than components of a greater whole.

DERIVING A SYSTEMS MODEL
FROM THE TEHUACAN AND OAXACA DATA

In 1966, when I began to write my ASW paper, the Oaxaca project had barely begun. We had as yet no quantitative
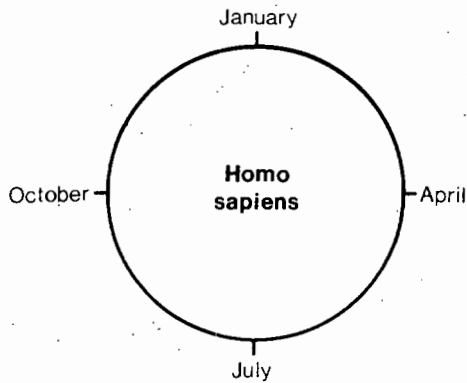
FIGURE 2.2. The annual cycle of activities of a microband of human foragers, to be used as the core of an anthropocentric ecosystem model. (Design by Richard I. Ford, unpublished, 1970).

analyses of cave remains, no decade of plant censuses, no nutritional studies of aboriginal food. Hence, most of my data came from MacNeish's Tehuacán project or from a study of the characteristics of its flora and fauna. In a very real sense, therefore, my ASW paper can be considered a preliminary model generated largely by the Tehuacán data—one that can now be tested against a whole new body of data derived from Oaxaca. For example, the plant census data we subsequently collected in Oaxaca now enable us to put some quantification into the model, which was impossible in 1966.

Since the model is a complex one, it cannot really be displayed in one single drawing. Rather, we must build it subsystem by subsystem, or feedback loop by feedback loop. We begin with the core of the model, which is shown in Fig. 2.2. In this diagram, Homo sapiens represents a microband[1] of human foragers in the Tehuacán–Oaxaca highlands, and the

[1]For the definition of microband, see Chapter 3.

encircling wheel represents their annual round of foraging activities, with January, April, July, and October serving as landmarks. To this core we can now attach some of the procurement subsystems I described in my ASW paper.

Maguey Procurement

As I suggested then, the preceramic forager "was not simply extracting energy from his environment, but participating in it; and his use of each genus was part of a system that allowed the latter to survive, even flourish, in spite of heavy utilization" (Flannery 1968:69). Figure 2.3 shows how this happened with the maguey, or century plant (*Agave* spp.), a tough and primitive member of the Amaryllis family that is available even at the leanest times of the year.

The wheel surrounding *Agave* is its life cycle. New magueys sprout and gradually grow to maturity over a 6–8-year period, at which point the inflorescence, or *quiote*, appears. The plant sends out its pollen and disperses its seeds, after which it begins to die. The seeds are not its main means of propagation, however, for mature agaves reproduce vegetatively and may be surrounded by suckers, or *hijos*, whose development is slowed by competition with the parent plant.

Today's Indians know that the best time to eat the agave is after it has bloomed, and the pod and *quiote* fragments in preceramic cave deposits indicate that the preceramic foragers knew that as well. The appearance of the inflorescence, frequently toward the end of the rainy season, provided foragers with the information that that particular agave was ready. They might cut off the *quiote* at this point, after which a natural fermentation took place in the moribund plant that softened it and increased its sugar content. Months later—even in the heart of the dry season, when few other plants were available—they could return to dig up that
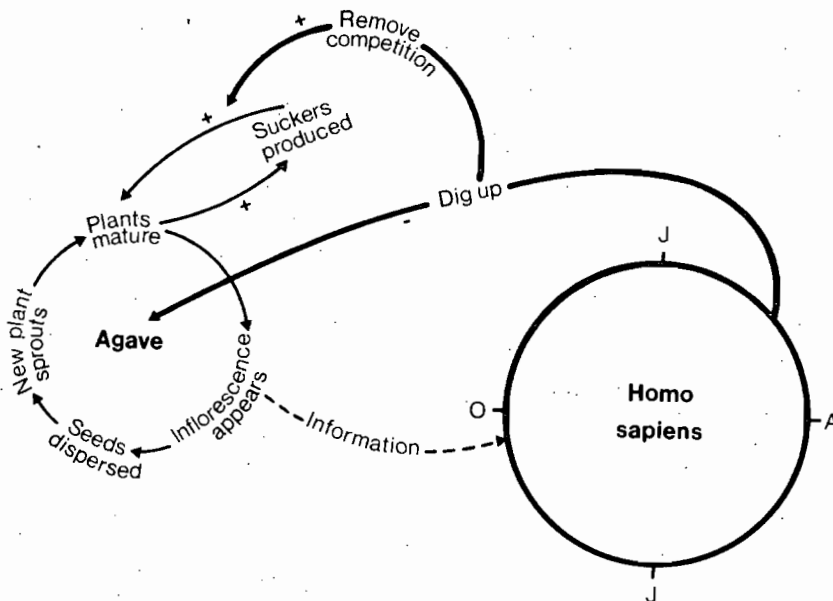

FIGURE 2.3. Maguey procurement in Archaic Oaxaca.

maguey and roast it in an earth oven for 24 to 72 hours, depending on its size. The digging up of the parent plant removed competition from the suckers or vegetative offspring, allowing them to develop into mature plants themselves and thereby keeping agave populations at roughly the same level.

The pluses and minuses in Fig. 2.3 indicate the variables that change in the same way (digging up eliminates one mature agave but may promote the development of one or more *hijos*). The diagram shows the critical point of information flow but not matter and energy. The latter variables, however, can now be partially modeled on the basis of data given in Chapters 18 and 24. For example, we can now suggest that there are an average of 122 inflorescence-bearing *Agave potatorum* per hectare near Guilá Naquitz; these agaves represent 341 kg of edible portion, providing 429,660 kcal/ha; and each occupant of the cave ate an estimated 140 g of agave per day during his or her stay.

Cactus Fruit Procurement

Organ cacti of at least four species were eaten in the Tehuacán caves. The variety was less in the somewhat higher Valley of Oaxaca, and at least one genus—*Myrtillocactus*—has no archaeological record of use because its fruits appear during February, when no one was at Guilá Naquitz.

Perhaps the most useful cactus in both regions was the prickly pear (*Opuntia* spp.), which was used both for its seasonal fruits (tunas) and its tender young stem segments (nopales). Nopales are available throughout much of the year and were despined and roasted over hot coals at Guilá Naquitz (see Fig. 10.5). They provided the preceramic foragers with an abundant (if less than delicious) vegetable.

The fruits, or tunas, are available seasonally, and the way they were used by the Indians may provide an example of the kind of symbiotic relationship proposed by Rindos (see Chapter 1). I described this relationship briefly in 1968 on

the basis of the Tehuacán data, and it is diagramed here in Fig. 2.4. On the average, prickly pear fruits from the eastern Valley of Oaxaca weigh 50 g and contain 50 seeds (Chapter 24), each of which has a tough outer layer that actually impedes the seed's chances of sprouting until it has been weakened by stomach acids. *Opuntia* remains in preceramic coprolites show that the seeds "almost invariably survive the human digestive tract and escape in the feces" (Flannery 1968:72). In other words, no matter how intensively tunas were harvested, the Indians were doing the plant a favor by weakening the protective outer layer and propagating the seeds.

As Fig. 2.4 suggests, the crucial information for the human population was the appearance of the flowers (and then the immature fruits) with the advent of the spring rains. This let the Indians know that they should return to the *Opuntia* groves in July to harvest tunas. This harvest had to be done in competition with fruit bats, birds, rodents, and collared peccaries, all of whom also like cactus fruits. Some of the Tehuacán caves contained wooden tongs for use in picking the spiny fruits off the stem. The spines could be singed off and the fruits could be split open and sun-dried to keep them from spoiling for several weeks. Ironically, as Fig. 2.4 shows, it is the fruits that the Indians *ate* that were more likely to have their seeds dispersed; fruits that were uneaten often went to waste (as, for example, when they were left behind in the cave to desiccate and be recovered by archaeologists). Thus the procurement of cactus fruits was an activity that was beneficial to both participants. It did not, however, produce a genetically different strain of *Opuntia* that was dependent on man.

Wild Bean Procurement

Wild beans (*Phaseolus* sp.) and wild onions (*Allium* sp.) were two plants whose presence in the Holocene thorn forest
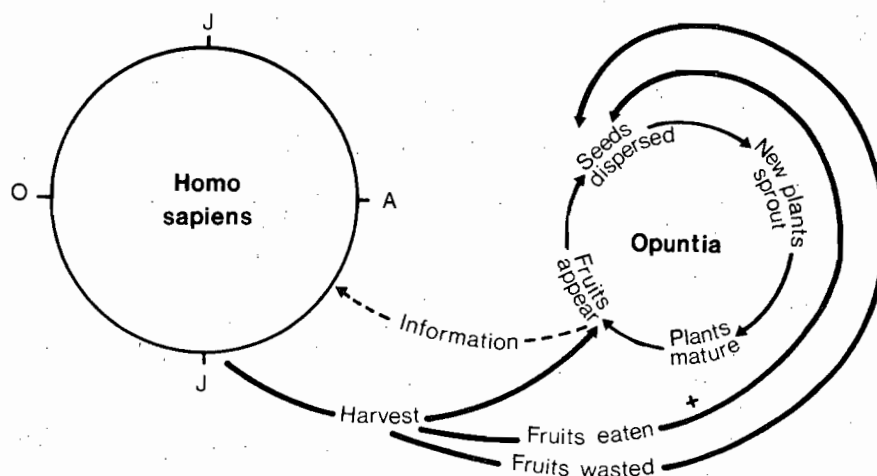

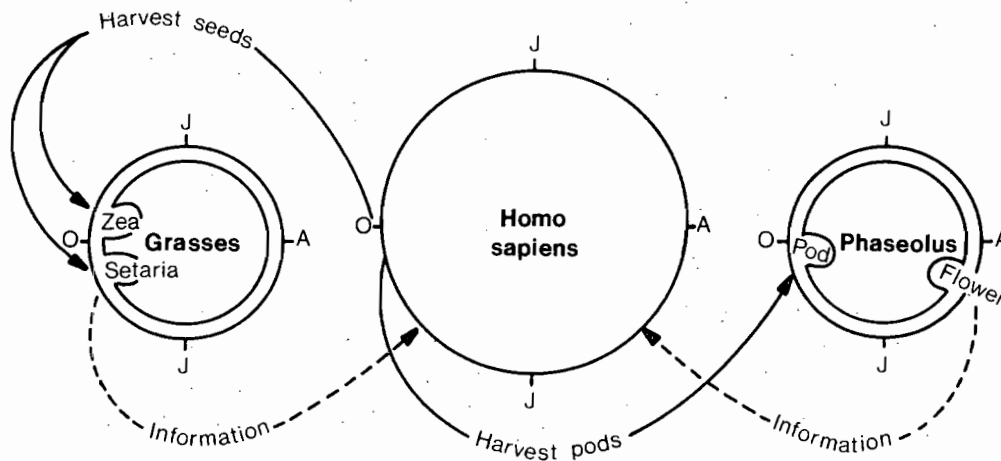FIGURE 2.4. Procurement of prickly pear fruit in Archaic Oaxaca.

FIGURE 2.5. Procurement of wild beans (right) and two genera of wild grasses (left) in the central Mexican highlands during the Archaic.

could be detected by their flowers. In the case of the wild beans of the Guilá Naquitz region (Fig. 2.5), the purple flowers appear in May during a normal year. These flowers provide the necessary locational cues so that foragers could return to the same area in September or October to harvest the plant, either for the beans in its pods or for its tuber (*jicamita*). In the case of the local wild onions, the flowers are white and tend to appear in damp microhabitats.

Beans are one of the plants that do not show strong evidence for a symbiotic relationship with humans. Their seed dispersal depends on a corkscrew-twisting pod that shatters easily, a genetic character that hampers harvesting. Archaeological data suggest that this character did not disappear until it was selectively bred out by humans following domestication; indeed, some beans may initially have been cultivated for the tuber rather than the seeds.

Wild Grass Procurement

The harvesting of wild grasses was one of the autumn activities of preceramic foragers in Tamaulipas, Tehuacán, and Oaxaca. Much of this activity took place in the tributary barrancas of the valley piedmont, a more humid habitat in an otherwise semiarid environment. Two of the important genera involved were *Setaria*, or foxtail grass, and *Zea*, which includes teosinte.

Setaria and teosinte share a number of biotopes, such as the Guerrero piedmont shown in Fig. 2.6. In such a habitat, *Setaria* comes ripe earlier, often in late September; teosinte comes ripe some weeks later, often in mid-October. As shown in Fig. 2.5, the gradually ripening heads of foxtail grass would provide the information necessary to bring foragers to the piedmont at the right time. By camping in the area for a month or two, they could harvest both grasses in succession, as well as any wild cucurbits and/or runner beans that were twining around the teosinte (see Chapter 1). No productivity figures are available for *Setaria*, but in 1971 Ford and I

collected data to show that teosinte could yield anywhere from 152.5 to 627 kg of edible portion per hectare (Flannery and Ford 1972).

Tree Legume Procurement

Several leguminous trees, including huizache (*Acacia* sp.) and *guaje* (*Leucaena* sp.), produce pods or seeds that could be used as food by preceramic foragers. By far the most important legume tree in the Tehuacán–Oaxaca environment, however, was the mesquite (*Prosopis juliflora* and related species). In their most favorable habitats in the eastern Valley of Oaxaca, mesquite groves can produce 109,960 pods (or about 183.6 kg of edible portion) per hectare. Such a harvest would yield 545,292 kcal and 10,465 g of protein (Chapter 18). Mesquite pods can be handpicked and probably were transported back to the Tehuacán and Oaxaca caves in the

FIGURE 2.6. Mature *Setaria* growing on the margins of a still-immature stand of teosinte near Chilpancingo, Guerrero, 1971.

many types of baskets and net carrying bags recovered from preceramic levels. Competing with deer, jackrabbits, coatimundis, ring-tailed cats, and other animals, the Indians harvested mesquite pods to cook up into a kind of *miel*, or syrup. Although the seeds can be toasted and eaten, archaeological data indicate they were often discarded by the thousands; this presumably aided in the dispersal of the plant by spreading the seeds over a much larger area than would have resulted from normal July–August tree fall.

ZEA VERSUS PROSOPIS:
AN EXAMPLE OF TWO GENERA LINKED
THROUGH THEIR RELATIONSHIP TO HUMANS

One of the characteristics of a multiple-loop system, as we saw in Maruyama's example, is that changes in one loop can have an impact on a distant loop in another part of the system. Frequently these loops are so widely separated in time and/or space that one's intuition would never lead one to expect their relationship. In this section I consider a relationship between the *Zea* and *Prosopis* loops in the Oaxaca model we are building. At the end of the Tehuacán project I did not have enough data to appreciate this relationship, and it was still undeveloped at the time of my ASW paper. By the time of my 1973 article in the *Annual Review of Anthropology*, some of the outlines of the relationship were emerging, and I elaborate on these here.

Under wild conditions, *Zea* and *Prosopis* do not generally occupy the same space, because the canopy of a good mesquite stand would shade out the teosinte. Mature mesquite groves occur with greatest frequency along the alluvium near streams on the valley floor, and the backslopes of the natural levees of suitable rivers. Teosinte is a pioneer weed of natural scars in the landscape, particularly clearings in the thorn-forested piedmont. Even the growth of agriculture during the period 8000–5000 B.C. did not initially move cultivation out of the piedmont barrancas and into the major river floodplains. Early agriculture of that period remained "barranca horticulture," as MacNeish has called it, possibly because the piedmont barrancas received higher rainfall than the valley floor and because the clearing of forested levees along the major rivers would have involved a great deal more work. Besides, some of those forested groves were producing 183.6 kg of mesquite pods per hectare.

For the purposes of this chapter it is not necessary for us to specify whether early maize was a domestic form of teosinte (as Beadle (1977), Galinat (1971), and Iltis (1983) believe) or came from a now-extinct wild corn (as Mangelsdorf (1974) believes). At 5000 B.C. it was a primitive cultivar with tiny cobs, easier to harvest than wild teosinte but probably no more productive.

> In many areas it would not have been worthwhile to remove the mesquite cover of the main river floodplains to cultivate such a plant. A more reasonable strategy would have been to leave the mesquite on the main valley floor to produce its 180 kg per hectare each year, while growing [*Zea*] in the piedmont barrancas where it was at home. . . . That such a strategy was used is suggested by the fact that for thousands of years after *Zea* had

been domesticated, no valley-floor villages appeared in Mesoamerica, although MacNeish (1964) suspects pit-house settlements in the tributary barrancas. (Flannery 1973:299)

This strategy was eventually altered, however, by gradual genetic changes that increased the length of the maize cob, its number of kernel rows, and the size of its kernels. We have some estimates of the related increase in productivity of maize, thanks to a linear-regression relationship between mean corncob length and yield in kilograms per hectare that A. Kirkby (1973:Fig. 48) recorded in Zapotec Indian cornfields in Oaxaca. Based on this relationship, and using figures on the mean length of corncobs recovered by MacNeish's Tehuacán excavations, Kirkby then calculated an "estimated yield" for various periods of prehistory.

The earliest cobs from Coxcatlán Cave (5050 B.C.) suggest a yield of only 60–80 kg/ha; later preceramic cobs from Tehuacán (ca. 3000 B.C.) suggest yields of 90–120 kg/ha. By 2000 B.C., maize reached an estimated yield of 200 kg/ha, surpassing mesquite in crude weight of edible portion per unit area (for the moment, we are ignoring all other differences between the two foods). By 1000 B.C., maize yields should have been up to 300 kg/ha or more.

A later study by Kirkby (1974) shows that Zapotec Indian farmers do not usually consider cultivation and land clearance to be worthwhile unless a yield of at least 200–250 kg/ha (shelled maize) can be expected. Taking all of the above observations into account, we can therefore propose the following possibility: Permanent, agriculturally based villages did not appear in the natural levee zone of the major river floodplains until maize productivity was high enough to make it worthwhile removing the mature mesquite cover of that zone. A possible corollary to this statement would be that the threshold for mesquite removal might have come (1) when maize surpassed mesquite in productivity (> 184 kg/ha), (2) when maize productivity reached the 200–250 kg/ha favored by today's Zapotec Indians, or (3) at some third value that can only be determined by future archaeological research.

In Fig. 2.7 we present a diagram of the situation prior to the reaching of the threshold described above. On the right we see the life cycle of the mesquite tree *Prosopis juliflora*, from the growth of new saplings to the maturing of the tree and the development of green pods in June. The appearance of these unripe pods was the crucial information transmitted to the human population, letting them know that mesquite would be ready to harvest by August. As mentioned above, seeds discarded when the pods were processed by the Indians probably had a better chance of sprouting (and certainly got wider dispersal) than those that remained in their pods on the tree to dry out.

On the left of Fig. 2.7, we see the annual cycle of early cultivated maize, gradually increasing from 50–100 kg/ha in its piedmont barranca clearings as the Archaic period progressed. Its yield was yet another source of information for the human population, who would certainly have had a set of policies concerning the amount of labor they were willing to invest in any given plant. At the time of Fig. 2.7 (perhaps 3500 B.C.), their policy was to cultivate *Zea* in the
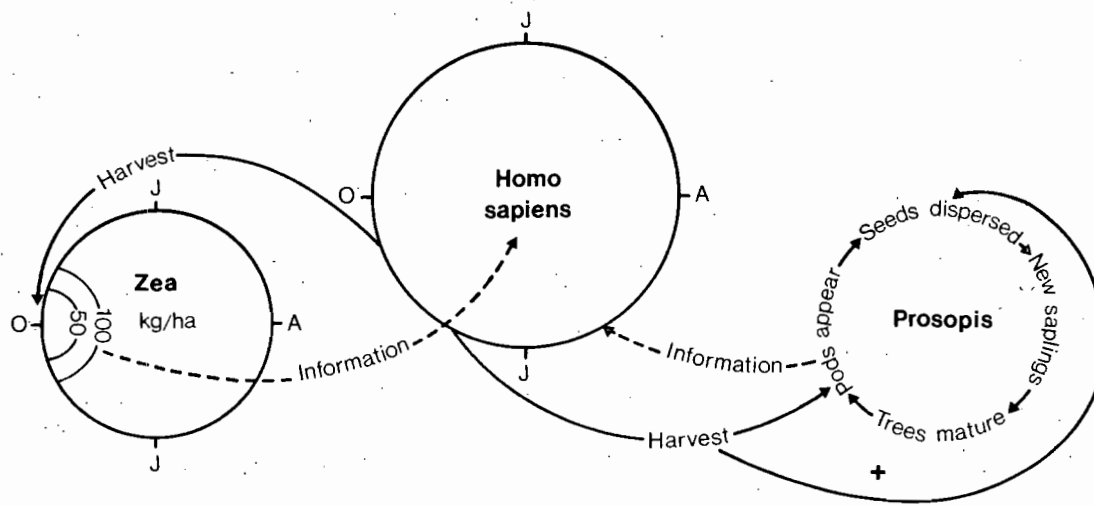
FIGURE 2.7. Human interaction with wild mesquite (right) and cultivated *Zea* (left) in Archaic Oaxaca.

piedmont and harvest *Prosopis* on the alluvium. A substantial increase in the productivity of maize, however, had the potential of providing new information that could change the policies for both maize and mesquite for years to come. In Chapter 32 of this volume, after examining both the data from Guilá Naquitz and the computer simulation by Reynolds, we present a second *Zea–Prosopis* diagram (Fig. 32.1) that we believe models the threshold of this policy change.

In Maruyama's terms, the favorable genetic changes leading to more productive maize may have been the "initial kick" that converted the *Zea* subsystem into a positive feedback loop. The increasing yield of maize, along with the greater ease of harvest of its tough-rachis, soft-glume cob, may have encouraged the Indians to spend more time cultivating corn and less time harvesting other, less responsive plants. If this is the case, the archaeological record should show not merely (1) an increase in commitment to maize but also (2) significant changes in the "mix" of wild species used as the human population changed its priorities. In other words, policy changes in one loop could have had profound changes in other loops.

The genetic mutations that contributed to these policy changes could not be farther removed from human intentionality; this is the biological component of the transformation. However, without human intentionality, mutations that so damage a plant's seed dispersal mechanism could not have survived, let alone had their frequency increased. Archaeological data suggest that humans had already made the decision to cultivate cucurbits and beans in Mexico, and wheat and barley in the Near East, *before* the crucial genetic changes that made the domestic varieties dependent on man. Our model must therefore take into account not only matter, energy, and information but also genetics and human intentionality. Systems models are well suited to this because, as Reynolds demonstrates in Chapter 31, they can be design-

ed to handle variables as different as kilocalories, search area, and decision making.

MODELING ENVIRONMENTAL VARIATION

We have already suggested in Chapter 1 that Archaic foragers had to adapt to two major sources of environmental variation. One of these sources, seasonal variation, was rhythmic and relatively predictable; in Oaxaca, the summer rains normally begin in May and end in November, with peaks in June and September. This means there will usually be an abundance of plant resources by the end of the rainy season and scarcity by the end of the dry season every year.

The second source, annual variation, is random and unpredictable. As indicated in Chapter 1, annual rainfall in Oaxaca averages 450–650 mm but may be as low as 300 mm or as high as 1000 mm. In rainy years, certain genera will produce a bumper crop; in dry years, yields of almost all genera will be reduced. Moreover, some trees, such as the piñon pine, may be so "exhausted" by a bumper crop that they produce substandard yields for the next few years regardless of rainfall (Richard I. Ford, personal communication, 1980).

In this volume, we do little to model seasonal variation since Guilá Naquitz seems to have been occupied only during the August–December period, and we therefore have inadequate data on activities of the January–July period. Future analyses of neighboring sites, such as Cueva Blanca and Gheo-Shih, may help to complete our understanding of the annual cycle of activities.

The modeling of annual variation, however, will be one important focus of Reynolds' simulation. In that simulation, an unpredictable sequence of wet, dry, and average years will be fed into the model in an effort to see how the Archaic foragers might have responded to such variation.

SCHEDULING

As I suggested in the ASW paper, so many possibilities for resource procurement were open to these ancient foragers that it would have been impossible to engage in all of them, even seasonally. "It happens that there are times of the year when a number of resources are available simultaneously, producing a situation in which there is some conflict for the time and labor of the group. Division of labor along the lines of sex, with men hunting and women collecting, is one common solution to these conflicts, but not all conflicts are so easily resolved" (Flannery 1968:75). The solution for more complex situations may be called *scheduling*.

In this volume, we investigate these solutions as follows. In Chapters 25–29, the pattern of debris on living floors at Guilá Naquitz is analyzed by Spencer, Whallon, and Reynolds, with an eye to determining (among other things) the extent to which sexual division of labor allowed the simultaneous performance of tasks that would otherwise have competed for the foragers' time.

In Chapter 31, on the other hand, Reynolds attempts to model the scheduling of plant procurement activities by the Guilá Naquitz population. A hypothetical band of foragers is presented with the 11 most common plants used in preagricultural levels of the cave, along with data on the environmental zone where each occurs, its density, and its nutritional makeup. It is up to our foragers to schedule the order in which these plant resources are procured, the environmental zone(s) visited, the area searched, and the amount collected. The efficiency is evaluated as they improve steadily over time while enduring their unpredictable succession of wet, dry, and average years. Eventually, our foragers are given cultivated squash, beans, and primitive maize to fit into their system of priorities, and we see whether their schedule of activities changes along the lines we have suggested.

SUMMARY

In other words, while the centerpiece of this volume is a site report on Guilá Naquitz Cave, we also use the volume to expand on, and test some aspects of, a model for wild plant gathering and incipient cultivation in the Valley of Oaxaca. At the core of the model is a small human population that moves through its annual wild plant harvests year after year for centuries. It is linked to a series of plant populations through exchanges of matter, energy, and information like those we saw in Figs. 2.3–2.7. Because there are so many of these linkages, we cannot represent them all in one diagram; we must imagine an *n*-dimensional space in which acorns, piñon nuts, prickly pear, mesquite, agaves, wild beans, squashes, and bottle gourds all intersect with the human population. Our human foragers cannot establish a static, unvarying relationship with these plant genera, however, because they are buffeted by a series of random variables, including a succession of wet, dry, and average years in which the productivity of the wild plants changes.

One of our suspicions—to be tested and clarified in this volume—is that our foragers dealt with both seasonal and annual variation in this ecosystem by working out a diver-sified strategy of resource scheduling, which was resilient enough to survive such perturbations over long periods of time. We hope that Reynolds' operationalized version of our model will tell us how this *could* have been done, but we also recognize that the archaeological record is the ultimate test of whether it *was* done in the way our model suggests.

Another of our suspicions is that incipient cultivation, as in the maize–mesquite example given earlier in this chapter, changed the information flows in the system in such a way as to alter its policies. To accommodate early agriculture, rescheduling should have taken place, with some wild plant genera rising or falling in priority according to their relationships with each other and with man. We hope that our model predicts some of the directions these changes might have taken, and we hope the archaeological record will allow us to see which of our predictions (if any) are correct. We also hope to be able to use the model to examine the extent to which variables such as population growth and climatic change would have affected the adoption of agriculture.

Incompleteness of the Model

Despite our best efforts, many aspects of my old ASW model remain unquantified even after this volume. Some of this can be corrected in future publications of the Oaxaca project, and some probably cannot.

For example, hunting and trapping were important activities for our preceramic foragers, and in 1968 I included the procurement of cottontail rabbits and white-tailed deer in my model. While animal procurement is discussed in Chapter 22 and elsewhere in this volume, it is not included in Reynolds' simulation. This is because we have decided to defer any major treatment of hunting until the publication of Cueva Blanca, a neighboring site that produced more abundant evidence for that activity than did Guilá Naquitz. Without hunting and trapping included, our model in this volume obviously falls short of completion.

In addition, we still cannot fully quantify the diagrams shown in Figs. 2.3–2.7 without additional data that would take years to collect. For example, to know exactly how human eating of prickly pear fruit would affect *Opuntia* densities in the Oaxaca piedmont, we would have to follow a sample of seeds all the way through their life cycle, from their deposition in coprolites until the point where we had figures on just how many of them would grow to be mature cacti. To collect the same data for *Agave*, we would have to dig up a sample of mature magueys and follow to adulthood those vegetative *hijos* that had flourished as a result. Until such data are collected, our model will continue to have some loose ends.

This should not be too discouraging, for it is likely that any model attempting to integrate as many variables as this one does will have areas of missing data. Building the model of any ecosystem is a slow process, and we have to expect that modeling a prehistoric ecosystem will be doubly hard. In the remainder of this volume, therefore, we try to think positively and concentrate on the data that *are* there, the insights that *are* forthcoming, and the flesh that *can* be added to our 1968 skeleton.