Donald W. Lathrap: approaches and contributions in New World archaeology

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Introduction

This essay is to honor the late Donald W. Lathrap, my maestro, friend and collaborator (Fig.1). Yet, I suspect, he would have cringed at the idea that the only objective of any essay in his honor would be to eulogize him. Therefore, it is appropriate in this paper to reflect upon Lathrap as a professor and to examine his contributions to New World archaeology. Although Lathrap had only briefly visited Venezuela once, his models provided new, challenging perspectives on Venezuelan culture history. For this paper, I have consciously adopted a blend of subjective, personal experiences with more objective assessments of Lathrap's work. I will be examining in some detail his understanding of the relationship between evolutionary and historical approaches in archeology with particular reference to the subjects of subsistence adaptation and the origin of agriculture. I do not, however, pretend this paper to be an exhaustive coverage of all of his contributions. An in-depth analysis of the enormous breath of Lathrap's work is a challenge to be met in due time.

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On May 1990, anthropology lost one of its most exciting and vocal generalists, and a great Americanist. Donald W. Lathrap was the best example in his generation of the Boasian tradition of working in all branches of anthropology. He was an innovative thinker and synthesizer; his impact can still be felt in the rupture of long-held paradigms about human and cultural potential for development in the tropical lowlands. He was a catalyst for a scientific revolution, changing forever our perceptions of lowland South American prehistory. Thanks to his work, the Amazon Basin and other tropical lowland areas of the New World can no longer be dismissed as cultural and technological backwaters, riding in the shadows of the advanced civilizations of Nuclear America. This paradigmatic shift has increasingly guided Venezuelan research toward a more integrated, truly continental perspective. The Circum-Caribbean is no longer to be the only productive theater of
archaeological endeavor for those of us working in northern South America.\footnote{Howard (1947) was a lone precursor, since he had presented what at the time was the first attempt to understand Venezuelan prehistory from a broad South American lowland context. Interestingly, Howard's work was done under the supervision of I. Rouse and C. Osgood, two important figures in Venezuelan-Caribbean archaeology (Rouse pers. comm. 1990). The next major work on Venezuelan archaeology, dominating the 1950's and 1960's, had generally restricted its discussion on questions of Venezuelan chronology and cultural historic relationships to the Circum-Caribbean area (Cruxent and Rouse 1958, 1961; Rouse and Cruxent 1963), leaving the possible Amazonian connections for others, like Meggers and Evans (1961) or Lathrap (1962a), to figure out.}

Without reservations, Lathrap acknowledged his intellectual debt to many great ones, among them Alfred Kroeber, Carl Sauer and Robert Lowie from his Berkeley years, and his Harvard professors particularly Gordon Willey, Clyde Kluckhohn, William Howells, Philip Phillips and Cora DuBois. He also regarded highly Alfred Kidder (the elder) and Edward Sapir for their stimulus in artifact analysis and the integration of linguistics and archaeology. He frequently spoke of the Baron Erland von Nordenskiöld as a major source of inspiration on his rethinking of Amazonian culture history, particularly the importance of Arawakan-speaking peoples. Not surprisingly, Lathrap's model of what an archaeologist should be was consciously patterned after the eminent Quechua and pioneering Peruvian archaeologist, Dr. Julio C. Tello, considered to be the greatest Peruvianist of his time.

Following the Boasian tradition, Lathrap required his students to master the historical insights that ethnography, linguistics, biological/physical anthropology, and cultural ecology had to offer in understanding human nature, and to control every facet and detail of the archaeological record. More important, he taught us to judiciously inter-relate all of these independent domains of anthropological analysis into a coherent whole. His students had to be literate in all four fields of anthropology. This conviction was far more than paying lip service to anthropology's long standing claim of being a holistic and integrative discipline. This is clearly reflected in Lathrap's trajectory as both an educator and a researcher.

While many would think that Lathrap's achievements are limited to his numerous publications, I believe that his most outstanding contribution to anthropology rests on his enormous academic productivity as a professor and advisor. Indeed, Lathrap directed, chaired and weaned a large crop of Ph.D. students all of whom represent in one way or another a continuation of his legacy, but in the best Darwinian sense of speciation, radiation and divergence. He always demanded his students to be independent, critical thinkers. One good example comes from the disagreements that former students had regarding Lathrap's hypothesis on the origin of the Shipibo Indians and their art (Lathrap \textit{et al.} 1985c, 1987a).\footnote{To avoid needless repetition, all of Lathrap's (and associates) references appear in the appended Bibliography of Donald W. Lathrap. The doctoral dissertations and master theses are also cited in the aforementioned bibliographic appendix. All other references appear at the end of this paper in the References Cited section.}
The diversity of topical interests of Lathrap and his graduate students is a reflection of his command of South American and New World culture history. For example, Lathrap chaired dissertations and master theses dealing with an economic model using domestic spatial analysis of Early Valdivia dwellings (Zeidler 1984), the ethnobotany and paleobotany of Formative Ecuador (Pearsall 1979), the cultural ecology of pre-Columbian Bahamas (Sullivan 1981) and the American Bottom (Chmurny 1973, O’Brien 1969), the implications of climatic change in coastal Perú (Richardson III 1969), the effects of volcanic activity in Formative settlements in Ecuador (Isaacs 1987), the trade network and symbolism of exotic mother-of-pearl artifacts of the Manteño culture in Ecuador (Mester 1990), the symbolic and social use of ritual and public space and the changes exhibited over time in the Valdivia settlement of Real Alto (Marcos 1978), the agricultural technology of raised fields with its demographic and economic implications for the rise of complex societies (Erickson 1988), and the formal, stylistic grammar and syntax of great Pre-Columbian art traditions such as Marajoara’s polychrome burial urns (Magalis 1975) or Taíno-Chican iconography (Oliver 1981) and the emic stylistic categorizations of the Peruvian Quinua pottery designs (Arnold 1969). Only two of his students took up the task of combining linguistic and archaeological data in modeling population movements in South America (Brochado 1984; Oliver 1989)—a subject to which Lathrap devoted much of his own energies.

True to his Boasian outlook, Lathrap also chaired dissertations in the field of physical anthropology, dealing with the subject of hominin dental evolution (Wolpoff 1969). He did not shy away from supervising theses dealing with the unglamorous but important analysis of pre-ceramic cultures in Ecuador (Spath 1979) and the Virgin Islands (Lundberg 1989). It is through his students that Lathrap’s intellectually diverse legacy to New World archaeology will continue to grow expand and diversify. I do not know of many thesis directors who would accept or feel comfortable supervising students exhibiting such a wide range of research interests and specialities. All he demanded was excellence.

Biographic sketch

Donald Ward Lathrap or Don, as his friends and associates called him, was born in California (1927), son of a prominent professor of Chemistry; a field in which Don excelled as an undergraduate student. In 1950, he obtained the degree of Bachelor of Arts in Anthropology from the University of California at Berkeley (UCB). At UCB Lathrap had studied with the eminent anthropologist Alfred L. Kroeber, from whom he first learned the art of interrelating and synthesizing diverse cultural phenomena into a coherent pattern and, perhaps more important, learned to recognize the power of history in understanding cultural development. It was also at UCB that Lathrap was first impressed by eminent geographer Carl Sauer (1952) and his theory on the origin and spread of agriculture. The latter subject would become a focal research interest throughout his career. In those early years, he was an Assistant Archaeologist of the University of California
Archaeological Survey team. The results of his early experiences in Californian archaeology were published in a number of articles (1950a, 1950b, 1952a, 1952b, 1955a, 1975b, 1975d).

In 1954, Lathrap became a Preparator at UCB's Robert E. Lowie Anthropological Museum. The latter experience was also to become another hallmark of Lathrap's future work. He was among those archaeologists who relished the challenge of reanalyzing --literally, redigging-- archaeological museum collections. He was always able to come up with exciting new discoveries and perspectives (e.g., Lathrap et al. 1984a). His enthusiasm spilled over many of his students, instilling a deep appreciation of the great potential museum research has in resolving current archaeological problems (e.g., Braun 1970; Magalis 1975).

That same year, 1954, Lathrap was admitted as a graduate student at Harvard University. Gordon R. Willey became his advisor and director of his dissertation. As a student, Lathrap seemed to have played Kent Flannery's "Skeptical Graduate Student" role to the hilt, which did not always endear him to his professors. Lathrap's writing style was found by his professors, such as Kluckhohn, to be "insufferably pompous; an opinion with which I now heartily concur..." (Lathrap 1976c: 495). An early example of his revisionist skills can be seen in a term paper for Philip Phillip's course on North American Archeology, Anthropology 239, where Lathrap evaluated the prehistory of the Lower Mississippi Valley and presented alternative hypotheses to those of Ford, Griffin, and even his own professors Willey and Phillips (Lathrap 1955b). Despite his skeptical, rebellious nature, Lathrap had only the highest regard for all his Harvard professors. More than once he told me that he had learned all the pedagogical "tricks" he knew and used in his seminars at Illinois from Gordon Willey.

It was during those Harvard years (1954-58) that Lathrap fine-tuned his methodological skills in the analysis and classification of ceramics. The underlying assumptions and implications of the analytical and taxonomical classificatory procedures for obtaining chronology and for defining discrete cultural complexes would become a dominant theme of his later work. Precision and clarity of conceptual and analytical categories, the manner in which these are operationalized, and the quality of stratigraphic provenience controls are at the core of all of Lathrap's numerous reviews (e.g., Lathrap 1964a, 1966d, 1970d; Lathrap and Oliver 1980b).

Lathrap enrolled at Harvard with the intent of specializing in Mayan archaeology (e.g., Lathrap 1957). Fortunately for us Willey cajoled Lathrap into accepting the challenge of doing his doctoral field work in the unknown "marginal" territory around San Francisco de Yarinacocha in the Eastern Peruvian Montaña. With a modest grant from American Museum of Natural History, and the encouragement of Harry Tschopik Jr., he embarked on an archaeological journey in 1956 that eventually led to his doctoral dissertation. It was his first experience in South American archaeology. The Shipibo Indians, the Ucayali River, along with the rain, oppressive heat, mosquitoes, and all the stereotypical tropical lowland "green hellish" scenario proved to be an irresistible charm: Lathrap fell into the trap,
devoting his life to the South American tropics. Lathrap also established a life-long, intimate relationship with the Shipibo people. Catalino Cumapa became not only Lathrap’s informant, but also his “compadre,” a kinship bond that both took very seriously. When he looked back on Mesoamerica or North America, it was almost always from a South American or an hemispherical perspective. As Donald Collier (1990: 5) wrote in his recent eulogy, Lathrap was an "Americanist extraordinaire and one of the last of the Hemispheric archaeologists, concerned with the culture history of the whole hemisphere, not just Nuclear America."

Clifford Evans and Betty Meggers, who began work in Marajó Island in 1947, deserve to be called pioneers in modern Amazonian archaeology. Nearly a decade elapsed before the next major lowland excavations by Lathrap. But his were to be at the opposite end of the Amazon River. It was his work that provided the first alternative and contrasting interpretation of Amazonian prehistory. The combined effects of this troika paved the way for new generations of archaeologists to come. Indeed, Anna Roosevelt (1988, 1990) and associates recently concluded archaeological research in the Island of Marajó and her current investigations around the mouth of Tapajós (e.g., Santarém, Taperinha sites) in Brazil are both, to a large degree, designed to test models and hypotheses initially developed by and debated between Meggers, Evans and Lathrap.⁴

In 1959, while still writing his doctoral dissertation, he accepted the position of Instructor in the newly formed Department of Anthropology at the University of Illinois. There he became part of a stellar cast that included Julian H. Steward, John McGregor, Dimitri Shimkin and Oscar Lewis. Soon thereafter, he was joined by R. Thomas Zuidema, a South Americanist ethnohistorian and ethnologist with a background in Dutch-European scholarship. The healthy exchange of ideas between Zuidema and Lathrap and their complementary approaches are largely responsible in building one of the top lowland South American anthropology programs in the United States. I would be remiss if no mention was made of the productive interaction between him and Mesoamerican archaeologist David C. Grove. Many of their ideas on the shared iconographic symbolism of Mesoamerica and South America were incubated in the hallways of Davenport Hall. Likewise, Chris Lehman (South East Asian ethnologist) and Rudy Troke (then in Bilingual Education at Illinois) both with a deep interest in linguistics provided Lathrap with a stimulating forum in which to discuss his views on prehistoric population movements.

During his first summer in the corn-fields of Illinois, in 1960, Charles Bareis (U. of Illinois) and Lathrap (1960a, 1962b) co-directed excavations in the Cahokia area of the American Bottom (Powell Tract site). Two dissertations, one dealing with the cultural ecology of Mississippian occupations (Chmury 1970) and another focusing on Cahokia’s ceramic chronology (O’Brien 1969), were supervised by

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⁴Roosevelt’s results on the Marajó Island are scheduled to be published in February 1991 by Academic Press. The work’s title is *The Moundbuilders of the Amazon: Geophysical Archaeology on Marajoara Island.*

Note: Marajó is the name of the Island; Marajoara is the name of an archaeological phase given by Evans and Meggers in 1957.
Lathrap. His involvement in the American Bottom prehistory led to an ongoing interest in the origin and diffusion of crops, especially maize, and their role in the development of chiefdoms in the Midwestern and Southeastern United States. Indeed, his last published paper (1987c) was devoted to this subject. This paper and his ideas on North American maize are just now beginning to have an impact on his colleagues (c.f. Riley et al. 1990).

Lathrap (1962a) earned his Ph.D. in 1962, with the thesis Yarinacocha: Stratigraphic Excavations in the Peruvian Montaña. Although still unpublished, it is a landmark of Amazonian archaeology that has currency today. In the summer of 1962, he returned to Yarinacocha to expand his survey and excavation programs, an event that was repeated in 1964 and 1965. Again, in 1967 as well as in 1968 he went back to the Ucayali area, but this time his research also concentrated on the ethno- and ethno-archaeology of the Shipibo Indians (Lathrap 1967a). Eventually this produced some of the earliest ethno-archaeological reports in lowland South America (Lathrap 1969c, 1970e, 1983; Lathrap and DeBoer 1979). In 1968, he became a Full Professor in Anthropology at Illinois. Lathrap's Peruvian Montaña field research period produced the first series of doctoral dissertations, beginning with Allen (1968), Richardson III (1969) and Myers (1970), and ending with Raymond (1972), Roe (1973) Yangüez-Bernal (1975), Weber (1975) and Isbell (1976).

In 1970, Lathrap (1970a) published his best known work, The Upper Amazon, a culmination of his Peruvian Montaña research. The broad, encompassing approach of this book, beyond the provincial interests of a few lowland archaeology specialists, catapulted Lathrap to the forefront of Amazonian prehistory. I suspect that Lathrap's demonstration of the relevance of Amazonia as an active participant in providing a good deal of the basic foundation upon which Andean high civilization was built is a major reason for this book's broad impact in the archaeological community. A second reason was that Lathrap's book made widely available a model which was in sharp contrast to the prevailing view, championed by Meggers and Evans, that portrayed Amazonia as a "counterfeit paradise" (Lathrap 1973g; Meggers 1971). That the Amazon could produce and sustain complex prehistoric societies and advanced art styles was, at the time, a revolutionary perspective.

Needless to say, Evans and Meggers' immediate response was to refute Lathrap's model by stating that "it is a truism of anthropology that language and culture are independent variables" and, therefore, Lathrap's correlations are unacceptable (Evans 1971: 1416). Moreover, Evans (ibid) argued that Lathrap's approach "reverses accepted scientific procedure by creating the model first and then accommodating the facts to its requirements" and concludes that Lathrap's work is no more than "an ingenious construct" with no empirical basis. This statement reflected the heated debates on methodology emerging between processual "new" archaeologists and their logico-deductive approach and the so-

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4 A copy of Lathrap's dissertation in Venezuela is available at the Biblioteca del Departamento de Antropología, Instituto Venezolano de Investigaciones Científicas (Altos de Pape, Caracas).
called "old-guard" archaeologists and their inductive approach. It is, however, interesting to note that Meggers-Evans, as much as Lathrap, elaborated models deductively and then proceeded to cite the facts that supported them (compare Lathrap 1970a with Meggers and Evans 1961, 1969, 1983). This and the many exchanges between Lathrap and Meggers-Evans gave rise to a dichotomous view of Tropical Lowland prehistory, but also provided the next generation of archaeologists with the ammunition to test and improve them. I will not dwell here on the details, but much of the results of recent in-depth investigations in the Amazon appear to be much closer to Lathrap's than to Meggers and Evans' model (e.g., Roosevelt 1988, 1990; Brochado 1984).

Despite Lathrap's accomplishments in restoring the cultural status of tropical South America, the conceptualization of the tropical lowland cultures as marginal to the achievements of the Nuclear civilizations (championed by Evans and Meggers) is so ingrained in North American academia that even today a noticeable number of anthropology departments in major institutions exhibit an archaeology curriculum that overemphasizes Nuclear American archaeology (the Peruvian Andes, Mesoamerica) at the expense of the apparently "irrelevant" lowlands. It is this sort of "elitism" that Lathrap fought tooth and nail throughout his life.

What is most telling is that Lathrap's *Upper Amazon* book was published when the New Archaeology revolution was reaching its peak. He was, indeed, an early advocate of this revolution, but as we shall see shortly with a rather personal twist: he never dismissed the earlier Culture Historic paradigm as a bankrupt enterprise. One of Lathrap's most explicit statements favoring the New Archaeology deductive approach comes from an oral presentation he gave at the Antillean Project Conference at the University of California Berkeley in 1980.

Irving Rouse (Yale University) in his writings on archaeological theory has suggested that one always starts with the primary data, the potsherds in the ground, usually out of a small excavation... and from there first classify this information into categories that can be compared, then map similar groups of artifacts, and only as a third or fourth or fifth level of interest (of inference) bring in the people, and people actually doing things. I believe [it is] more important to think about "doing things" [and bring them into] the picture [model first]. I think that most of the vast "woolly" of thinking that has [been] going on in American Archaeology comes from this emphasis. I think that the best scientific approach to this problem [population movement into the Caribbean] is to consider people "doing things" and people "thinking" about the solutions of their own day-to-day problems as the beginning of one's research, and only then one has a basis for looking [for] particular bodies of data as a specific test for one's continuous modelling and remodelling of what people are doing on the ground (Lathrap, unpublished transcripts, 1980; my clarifications in brackets).}

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5 An example of the critiques and replies to Meggers and/or Evans by Lathrap can be found in: Lathrap 1960b, 1962a: 571-ss, 1963d, 1963c, 1964a, 1966a, 1967a, 1972b, and 1973g (consult Lathrap's references on Meggers and/or Evans cited in these articles).

6 The New Archaeology revolution peaked about 1974.

7 It should be noted, however, that Rouse (e.g., 1986) does not reject a deductive approach or modelling as an invalid methodology. He does, however, prefer to proceed inductively from the lowest levels of inference—the artifacts themselves— to the highest, such as population movement.
While Lathrap advocated a deductive approach, he strenuously objected to what Kent Flannery labelled as the "Law and Order" New Archaeologist. The models developed by the latter explicitly adhered to the principles of linear causality (cf. also Reichel-Dolmatoff 1976: 318). Lathrap preferred a systems theory approach to explain historic and evolutionary culture change. He attributed the dynamics of change to slow deviations in one or various parts of the system that eventually resulted in major modifications of the system at large (cf. also Flannery and Marcus 1983: 1-4). The tendency of Law and Order archaeologists to oppose "real" science to history and to regard synthesis models involving language, archaeology and other domains ("parts of the system") of culture as a sterile approach are among the key elements that sharply distinguished them from Lathrap's view of the New Archaeology. As he stated recently "such studies [as Sapir's Time Perspectives mini-monograph] have been scorned by many of the New Archaeologists as not only ascientific but also antiscientific, a position with no rational justification." (Lathrap et al. 1985c: 42). Lathrap's brand of New Archaeology did not imply rejecting but rather adapting and maximizing the approaches developed by an even earlier generation of traditional culture historians, such as Sapir and Nordenskiöld. Thus, linguistic, cultural, and other distributional studies were not ignored but fully adapted to processual modelling in Lathrap's work.

The Upper Amazon, with its multifaceted approach, told a post New Archaeology generation --myself included-- that it was not only valid but also "scientific" and productive to pursue the dictum of a holistic, integrative approach in anthropology --in the tradition of Boas, Sapir and Kroeber-- where language, culture, human biology-morphology and environment could be brought to bear on the problems at hand (cf. also Lathrap 1963e) and, at the same time, it encouraged to focus on these problems from the perspective of evolutionary and historical processes. History was not to be opposed to "real science" (or evolution) and vice-versa, as the Law and Order New Archaeology seemed to imply. It also brought home the view that cultural processes could be best understood by visualizing them as a system in which diverse cultural and natural components interacted in complex ways, shaping the nature and direction of history. With Lathrap's utilization of linguistic models against which to test his archaeologically derived hypotheses on prehistoric population movements and its underlying economic-ecologic factors stimulated and renewed an interest in migration theories at a time when it, for all practical purposes, was out of style (at least in North America).

A couple of years after the Upper Amazon publication, Don and his wife Joan Lathrap suffered a devastating personal tragedy, which would remain with them throughout their lives. Their only child, Bonnie, died of leukemia. From all accounts, this sad event had a major impact in Don's personality. From an academic standpoint Lathrap's energy was, henceforth, completely focused on his work and his students. It is worth noting that Joan Lathrap was a close collaborator in his work, as attested by Don's many acknowledgements in his publications.

From early on, Lathrap (1960b, 1961a, 1962a, 1963b, 1966b, 1967a) had been profoundly interested in the early Ecuadorian Formative and its possible
correlations with the Upper Amazon complexes. In the early 1960's it meant dealing with one of the two oldest known New World ceramic complexes at the time: Valdivia. Lathrap (1962a: 516) was convinced that Valdivia culture was characterized by a Tropical Forest pattern—as he defines it—based on flood-plain agriculture, a view that sharply contrasted with the established interpretations of the time that suggested incipient horticulture with a strong dependence on maritime resources.\(^6\)

The Primer Simposio de Correlaciones Antropológicas Andino-Mesoamericanas, held in Salinas in 1971, gave Lathrap the opportunity to change his field work interests from the Upper Amazon to Ecuador (Figs 2 and 3). To test a wide range of hypotheses on Valdivia culture Lathrap directed, with Jorge Marcos, a large excavation project (ca. 1974-1977) at the Valdivian site of Real Alto, spawning a series of doctoral dissertations not only from Illinois, but also from other universities (e.g., Marcos 1978; Pearsall 1979; Damp 1979; Zeidler 1984). The Real Alto project also was a major field training program for a number of graduate and undergraduate anthropology students from the University of Illinois, many of whom later specialized in other areas and fields of inquiry.

But Lathrap's personal interest in the Ecuadorian Formative—which would continue through the 1980's—did not prevent him from maintaining research interests elsewhere. The year of 1975 was remarkable in that, in addition to two Upper Amazon dissertations (Weber 1975; Yangüez-Bernal 1975), Lathrap supervised two others dealing with the archaeology of the Colombian Amazon (Bolian 1975) and Brazil's Marajó Island (Magalis 1975). Also in 1975 Lathrap collaborated with Collier and Chandra in organizing the first comprehensive museum exhibit of the Ecuadorian Formative archaeology at the Field Museum of Natural History in Chicago (1975a). That same year Lathrap made his only visit to Venezuela at the invitation of the Instituto Venezolano de Investigaciones Científicas (IVIC). In this trip he was able to visit the Orinoco valley (Saladero area), Oriente and the Andean region as well. He also was invited to give a lecture at the IVIC, that stressed the relationships between Venezuelian and greater Amazonian prehistory (Fig. 4).

The latter half of the 1970's brought another generation of graduate students, many of whom became engaged in Ecuadorian archaeology, but this time the emphasis was on prehistoric cultures other than Valdivia; e.g., Spath's El Encanto archaic site (1979), Isaacson's highland Formative site of La Nueva Era near Quito (1987), Mester's Manteño coastal site of Los Frailes (1990) and McEvans's Agua Blanca site (in progress). Others were to become engaged in M. A. or Ph.D. research in the Bahamas, Puerto Rico and the Virgin Islands, Eastern Brazil, Western

\(^6\) Lathrap (1962a: 516) had already hypothesized that Valdivia's emergence "may be due to the expansion of an early form of Tropical Forest agriculture rather than a North [Mesoamerica] to South expansion of an essentially shell-fish gathering economy as [Meggers, Evans and Estrada] suggest." Here Lathrap is also referring to Willey's 1958 Pre-Agricultural Early Pottery Horizon Hypothesis (cf. Oliver 1989: 33-40) for a discussion). It was later shown that the maritime emphasis was the result of mixing, through arbitrary level excavations, of an underlying preceramic component with an overlying San Pedro-Valdivia ceramic deposit at the type site (Bischof and Viteri 1972).
FIGURE 2
VISIT TO REAL ALTO SITE, ECUADOR (APRIL 1975)
From left to right: Jorge Marcos, James Zeidler, the late Donald W. Lathrap, Irma Jarrín, the late Don Carlos Zevalllos, and Erika Wagner
(Photographer Alberta Zucchi)

FIGURE 3
DONALD W. LATHRAP, EXCAVATING AT THE SITE OF REAL ALTO, (APRIL 1975)
(Photographer Erika Wagner)
Venezuela and Perú. Throughout the rest of the 1980’s, Lathrap’s energies were devoted to the supervision of dissertations, writing, and teaching. The Ecuadorian Real Alto excavations along with the Daule River-Guayas Basin surveys were to be his last major field work projects (Lathrap et al. 1980a).

The sheer number of theses and graduate students being simultaneously supervised began to take its toll on Lathrap’s energy. I think that by about 1987, he had already worked out in his mind that henceforth he would concentrate his efforts on Californian archaeology and, thus, come full circle to where he began his career (cf. Lathrap and Troike 1983-84). Still, right down to the last minute he continued to write on South American culture history (1987c).
In 1989, Lathrap opted for an early retirement from academia. He planned to move back home to Surprise Valley, California. On May 13, 1990, while recuperating from abdominal surgery, he quietly passed away, victim of a heart attack, in Champaign, Illinois.

The Professor

During the years I was at Illinois, Lathrap taught a wide range of graduate level courses and seminars. The seminar meetings were almost always held in the evenings at either "Timpone's" or "Treno's," two traditional spots where one could munch on pretzels and gulp a pitcher or two of draft-beer. This was Don's favorite, relaxed setting and it is here where we learned not only about archaeology but also about archaeologists. Don had an encyclopedic knowledge of anecdotes about many archaeologists of his and prior generations, instilling in us a sense of history - as well as humor -- that no archaeology textbook could ever offer. His seminars were as much about archaeological content, theory and method as they were about the people behind the ideas.

Don's lectures on South American culture history were without parallel. This two-semester course focused first on the Tropical Lowlands and then on the Andes and Pacific watershed. It ranged from Paleoindian to the ethnohistorical present. His oral presentations were masterful, for he was able to provide the "Big Picture" with an amazing wealth of detailed information. He was blessed with a photographic memory, to the point that he could recall single potsherds stored in the deepest recesses of museums throughout the Americas. In lecturing, his most salient quality was his enormous capacity for synthesis and clarity of exposition. What most impressed me was his ability to lecture without ever bringing any kind of class notes or mnemonic devices. Moreover, he frequently would lecture with precise bibliographic references -- even to a page! He never repeated himself, and in the next meeting he would pick up the thread of history exactly where he had left it in the previous one. Few can deliver lectures precisely and exactly as if one were reading aloud a well edited text from a Teleprompter screen. Don excelled in this technique, which allowed him to captivate and transport his audience to the fascinating world of native South America. In fact, it was not uncommon for us, as graduate students, to audit the entire course for a second time just to keep our minds fresh and up-dated (and to increase our chances in the dreaded qualifying doctoral examinations!).

Among Lathrap's favorite upper-level seminars was one entitled "Problems in Chronology" where, as an expert forensic pathologist, he anatomically dissected a variety of classic monographic studies, such as Evans and Meggers' (1957) "Mouth of the Amazon" monograph, Willey, Strong, Ford and Evans seminal studies in the 1940's on the Virú Valley ceramic chronology, and Phillips, Ford and Griffin's (1951) landmark monograph on the archaeology of the Lower Mississippi. Here we learned all about diachronic/synchronic classification and analysis of artifacts, seriation, stratigraphy, methods of data presentation, etc. Most of all, we learned to
respect and admire the achievements of many great archaeologists, but were also forcefully encouraged to challenge the "great ones" with critical thinking, echoing Lathrap's own experience at Harvard.

He was also fond of teaching a course on "Art and Anthropology." It provided a forum in which to explore, test and refine his hypotheses on the rise of complex, great art styles in Pre-Columbian America. Here, following the footsteps of Boas' "Primitive Art" and Kubler's "The Shape of Time," we explored the mastery of the Pomo basketmakers, the Kwakiutl woodwork, Eskimo ivory carving, African and Iroquois masks, and even the art of bullfighting through the eyes of Ernest Hemingway's *Death in the Afternoon*. His own contributions are popularly known as the "Cayman Papers," since the central theme of indigenous aesthetic expression focused on this tropical lowland saurian (Lathrap 1970c, 1971a, 1973c, 1974b, 1982, 1985a). The Cayman papers convincingly argued for the Amazonian-lowland foundation of Chavín art and cosmology. Furthermore, Lathrap proposed that the various great art styles of Mesoamerica (e.g., Olmec, Izapa) and the eastern slopes of the Andes (e.g., San Agustín, Chavín) were ultimately derivable from a single ancestral South American Tropical Forest cultural pattern (as defined by Lathrap) that spread widely from a postulated early Neolithic expansion out of Amazonia (e.g., Lathrap 1974a, 1977a, 1982). This Spinden-like unitarian thesis is a perspective that Lathrap exploited later in his masterful study on the origin and evolution of agricultural economies (Lathrap 1977a).

He was most enthusiastic about his interdisciplinary seminar on the evolution of agricultural economies in the New World, which was taught jointly with the late C. S. Alexander and T. Hymowitz in the Department of Agronomy. Lathrap's lectures and the ensuing discussions and debates among guest scholars (including George Beadle, the "father" of the Teosinte Hypothesis) and students alike provided a forum in which he refined his unitarian model of New World Neolithic Revolution; however, the source of his ideas can be traced to Carl Sauer (1952) and a 1956 paper he wrote as a Harvard graduate student (more on this later). All of the details of his model had crystallized by 1973, shortly after Lathrap participated in the Conference on the Origins of Agriculture in Woodstock, Illinois (Lathrap 1977a: 713-14). Lathrap elaborated on the concept of the house garden, which he probably picked-up from botanists like Anderson (1960), as a means to explain the shift from hunting/fishing and gathering to a fully agricultural economy. It is in the house garden context where the conscious efforts of genetic manipulation and experimentation with plants initially took place, perhaps 16,000 to 14,000 years ago. By, 6,000 B.P. the Neolithic Revolution was fully established, the transition and expansion from hunting/gathering and house garden to a full-blown conuco agriculture was accomplished, resulting in a "bewildering" and "large arsenal of high calorie crops" (Lathrap et al. 1985c: 55-6).

What I find most astonishing are the accurate predictions emerging from Lathrap's Neolithic Revolution model. Two examples will suffice. As early as 1962, Lathrap suggested that by 3,000 B.C. a Tropical Forest Culture with an effective floodplain agriculture economy and pottery should be found fully developed in the
Colombian lowlands, specifically the Magdalena valley (Lathrap 1962a: 510, 1977a: 735, 740). Two decades or so later Reichel-Dolmatoff (1985) publishes his finds on the Lower Magdalena site of Monsú, with radiocarbon dates of ca. 4,000 B.C., with about 100 cm of the lower deposit still undated (i.e., Reichel-Dolmatoff's Turbana Period). Moreover, the predicted vessel form set, the so-called rim-less _tecomate ollas_ and shallow plates, also characterized the entire sequence at Monsú (1977a: 736).

Brochado and Lathrap hypothesized a six stage sequence of continuous development in the Central to Lower Amazon area, of which the second earliest --if found-- was predicted to date over 4,500 B.C. and be characterized by simple hemispherical bowls of relatively simple technology perhaps with simple incised decoration proto-types of the Mina tradition (1980 [1982 version]: 82-3; Brochado 1984: 316-7). Brochado and Lathrap (ibid) predicted that such stage should already represent a good step above the incipient agriculture phase of development. In 1986 at the mouth of Tapajós River, Roosevelt and associates obtained dates between ca. 5,000 and 4,500 B.C. for the Taperinha occupation associated with pottery described as a fairly plain culinary pottery with simple bowl shapes and rare incised decoration. Although we must await Roosevelt's final results with regard to the level of agricultural and social development as well as the specific details of pottery style it is evident that Lathrap's predictions are not a matter of chance and luck but the result of a viable model of cultural development and history in Amazonia.

His last memorable seminar co-lectured with Rudolph Troike in 1982 focused on the inter-relationships of historical linguistics and archaeology in reconstructing prehistoric population movements. The results are a matter of record published in a special issue of the _Journal of the Steward Anthropological Society_ (Lathrap et al. 1983-84a). In the editorial preface, Mester and McEwan noted that:

Throughout the semester the participants [of this seminar] were subjected to a curious disinterest and skepticism, at times bordering on hostility from faculty and students alike... The halls of the Department rang out with the warnings against the dangerous undertaking we were attempting. For many... the combination of archaeological and linguistic data approximated the volatility of matter/anti collision. (Mester and McEwan 1983-84: iv; comments in brackets mine).

Although these comments may sound acerbic my own qualifying oral examination revolved around the issue of why should no less than four ethnologists be involved in what was essentially an examination of an archaeologist. Lathrap took the opportunity to drill me on how and why ethnology is relevant to archaeology (especially the concepts of culture, society and ethnicity). My success depended on how well I could justify the presence of the diverse...

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* At the Annual AAA Meetings in Washington D.C., Roosevelt (pers. comm., 1989) noted that Taperinha's early pottery was reminiscent of the early Monsú pottery (Turbana) and the incised decorated style found at Agüetio site, Middle Orinoco (cf. also Roosevelt 1990; Zucchi, Tarble and Vaz 1984). (Since this paper was submitted, Roosevelt (1991) has reported an even earlier date of 7,000 BC associated with ceramics of very simple technology which I think appear to relate to Mira. In short, if anything, Lathrap was quite conservative in his predictions).
members of my committee and at the same time I had to do so without violating the integrity of each specialty. These internal debates including linguistics vs. archaeology, actually mirrored those in North American academia at large.

The resistance to integrated, multidisciplinary approaches, that were in force until the early to mid 1980's, however, have subsided. There now seems to be a realization that both New Archaeology strategies and Culture History are not only equally valid but logical steps in our understanding of past cultures and societies. Without culture history, middle range hypotheses on social behavior and environmental adaptation strategies, as stressed by New Archaeology and the Latin American movement of "Arqueología Social," could not be deduced: the chronological-cultural architecture must be in place a priori.

An increasing number of recent archaeological publications on Polynesian, West Indian, and Venezuelan prehistory, for example, attest to a renewal and revival of integrating models of archaeological, linguistic, ecological, etc. data sets (e.g., Arvelo 1987; Kirch 1984; Oliver 1989; Rouse 1985, 1986; Vidal 1987; Tarble 1985; Tarble and Zucchi 1984; Zucchi 1985, 1987, 1988, 1989; Zucchi and Tarble 1985). Although Lathrap is not directly or uniquely responsible for this shift, it is nevertheless interesting to observe that his long-held convictions are on the road to become once again acceptable and scientifically valid approaches that complement—not oppose—New Archaeology and Social Archaeology.

Lathrap vignettes

I still remember that morning in late August 1977 when I first met Don. I was at the footsteps of the entrance to Davenport Hall, where the Department of Anthropology of the University of Illinois is located. I was then a young, idealistic, impressionable sort of fellow, ready to conquer the world. I had just graduated from Miami University (Oxford, Ohio) and remembered why I chose to be on these steps. I had been strongly impressed by Lathrap's Upper Amazon (1970a) book, since it presented me with the most appealing alternative to the excesses of the then popular New Archaeology, the one that I thought to be most appropriate given the historical realities of archaeology in northern South America. In the Upper Amazon, for the first time, I was fully able to sense the "man behind the potsherd," to use José M. Cruxent's phrase.

That morning, Lathrap was walking alongside Professor Lehman (Southeast Asian ethnology), toward the footsteps of Davenport Hall. I approached them and began to introduce myself. "I'm your new graduate stu..." Quickly, Don interrupted me. "Yes I know," he said, "I expect to see you in class in ten minutes". He then quizically frowned --which was easy for him-- and said "You are in my South American Culture History course, aren't you?", which was more a statement than a question. Without allowing me to respond, he commented --his frown now deeper--"You have read [Sauer's] Agricultural Origins and Dispersals..." "Well...," I responded with ambivalence, "...I'm not sure..." He interjected --now visibly annoyed-- "And Chomsky's Syntactic Structures?" Before, I had a chance to reply, Lathrap
pronounced "Here! I've already given you your first assignment." And with that much said, he strolled into the 9:00am class, and I followed him hoping that the Earth would swallow me and mercifully put me to rest.

Just as Don's lectures were models of logical structuring and organization, his work space was a paradoxical example of maximum entropy. He was the kind of person who almost always knew exactly where everything was in an apparently chaotic office. This is all the more striking since his adjoined Chuck Bareis' office which still is a model of maximum order and, yet, they were the closest of friends. For this reason alone, students joked, we knew that the Second Law of Thermodynamics and Levi-Strauss had to be right! I distinctively recall that one day when Lathrap wanted to show to me a letter that Gordon Willey had written to him a couple of years before. Upon entering his office for the first time, I was amazed to find piles and piles of papers on the floor, on his table, on top of the desk, protruding just about from everywhere. As a good archaeologist he began to "dig" into stratigraphic layers of papers; lo and behold, there was the letter! The most amusing aspect of this was that in Don's file cabinets there was absolutely nothing. Well, not quite true, he did have a massive telephone directory and a bunch of empty brown paper bags—a true symbolic gesture of where he thought trivia ought to be; it's either in your head or nowhere. It also reflected Don's contempt for bureaucracy, which he consistently opposed to scholarship.

I have already mentioned Don's aversion to note-reading in his classes. Oral deliveries without note aids were a must. He always valued what was in your mind and not what you kept in your "file cabinets." He truly felt that if you had to open your files to sustain a dialogue with him, you simply had failed to grasp the essence of whatever the subject was. Partly, this stems from, the fact that Don suffered from a form of dyslexia, which gave him difficulties in reading and writing. Although he was a voracious reader, he did so slowly. He claimed that because of this handicap, he had plenty of "time" to digest and internalize all of the information that faster readers might pass-over lightly.

A memorable anecdote occurred in the Ninth International Congress for the Study of Pre-Columbian Cultures of the Lesser Antilles, held in Santo Domingo in 1981. I was sitting between the late Gary Vescelius and Don Lathrap, intently listening to a presentation on a Venezuelan ceramic seriation. The archaeologist (who will remain anonymous) was having some problems with the projection of a slide with a Ford style seriation of ceramic types. The slide was inverted and the lecturer asked the slide preparer to put it upright. The poor Dominican fellow repeatedly failed to discover which side was upright, while the speaker went from impatient to furious. The room was somewhat dark and the audience was evidently amused by this situation and increasing their expectations of the "truth" to be revealed. Finally, the speaker abruptly walked to the projector and got the slide to project correctly onto the screen. At this juncture, Lathrap, who had seriously attempted to make sense of the slide's ragged, amorphous bar-graphs in its various positions (the expected Fordian battleship curves nowhere in sight!), exclaimed—loud enough for all to hear—"It doesn't matter; whether down side up or up-side
down it still doesn’t make any sense!” The initial disappointment of the much awaited “truth” from the slide, led to hardly controllable laughter. The punch line is that the speaker, unaware of the activity, beamed with pride while admiring the “truth” emanating from the siliation slide. This also exemplified Don’s conviction that chronology could not be obtained via “cook-book recipes.”

Many have negatively viewed Lathrap’s aggressive style in his critical, revisionist writings. Lathrap tolerated what one might call “honest mistakes” but when intellectual sloppiness and laziness were in evidence, his critiques were frank, direct, and unmerciful. Despite the public image of “viciousness”, described by Anna Roosevelt as as “a terrible feud [that] scared everyone away,” Lathrap had often expressed to me a deep respect for Evans and Meggers and others who had felt the sting of his critiques. Indeed, in 1981 Lathrap was the keynote speaker and discussant at the Conference on Andean Archaeology organized by the Latin American Center of the University of Texas in memory of Clifford Evans.

Admittedly, Don’s aggressive style of criticism was not a judicious exercise in the art of diplomacy, nor was he interested in it since he saw no relationship between diplomacy and the advancement of science. For him, science thrived on confrontation and debate of opposite perspectives. I once complained about his lack of diplomatic skills and he simply answered something like “Demanding rigorous, high scientific standards are not subject to a Miss America Beauty Contest.” I frequently witnessed hotly contested arguments in conferences, immediately followed by a friendly conversation over dinner. It is interesting to note that what people remember most about Lathrap’s reviews are those which centered on controversial, opposite interpretations of the archaeological data; whereas, all of the many positive critiques are hardly remembered (e.g., Lathrap 1965b, 1972a, 1977b; Lathrap and Oliver 1987b). I must agree with Don that in an intellectual setting of complacency, the necessary stimulus conducive to new and better paradigms will most likely be short-circuited.

As students we were required not only to understand Lathrap’s perspective, but we had to master the opposing points of view to the last detail. In fact, we had to be prepared to defend hypotheses that squarely contrasted with Lathrap’s own interpretations. I still vividly remember my heart sinking into abyss when I read the first essay question in the final examination of his South American archaeology course. After Lathrap had spent nearly two weeks demolishing, bit-by-bit, Moseley’s (1975) hypothesis of the maritime basis for the rise of Andean civilization, we were asked to defend it! In short, Lathrap never selectively chose “targets” for criticism, since he applied it with equal detail and zeal to anyone, his students in particular. This was Lathrap’s sense of fair play.

Insights into Lathrap’s views on culture history and evolution

When, reflecting on Lathrap’s work, most colleagues would probably agree that his most influential contribution is his model of population movements out of

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the Central Amazon and their correlations with specific proto-linguistic groups and archaeological (ceramic) traditions—which Carneiro (n.d.: 13) aptly called the "Cardiac Model." They would, perhaps, also point out that Lathrap stimulated a renewed interest in generating historical-developmental synthesis models based on multiple, independent lines of evidence such as historical linguistics, ethnohistory, archaeology, and subsistence-ecology. I believe that Lathrap's most important contribution, lies in his insistence on searching for explanations to elucidate the complex historical, ecological and evolutionary processes that lead to population expansion and outmigration—the process upon which his model rests. Here, I have chosen to highlight his use of Neo-Darwinian Synthesis theory as applied to problems of culture history and evolution.

Migration hypotheses inferred from the archaeological and linguistic record have been previously invoked by many scholars working in the lowlands of South America and the Caribbean. But it was Lathrap's (1976c, 1962a, 1970a) work which had first explicitly examined migration as a demographic and evolutionary process explained in terms of the Darwinian concept of adaptive radiation. Migration explained cultural distributions, but it cannot explain itself: what conditions triggered population movements? Are there any principles to account for the inferred patterns of migration? I suspect that while most are familiar with his contributions within the strict framework of the culture historic-classificatory paradigm (cf. Willey and Sabloff 1974), perhaps few are consciously aware of the influence that Neo-Darwinian Synthesis had in Lathrap's views on Amazonian cultural evolution. Even fewer are probably aware that he had worked out the basics as early as 1956. (There is a tendency to rely too heavily on the Upper Amazon book as the source of all of Lathrap's contributions.) In 1984, he wrote;

As part of my Harvard graduate education, I was forced to read fairly extensively in the evolutionary writings of such scholars as George Gaylord Simpson and Ernst Mayr... I took a seminar on cultural evolution from Cora DuBois in which I was forced to examine the... evolutionary writings of Leslie White. The whole seminar, and in particular White's corpus of writing, brought home to me... the impoverished and self-limiting nature of what passed for evolutionary theory in anthropology as opposed to the richness and complexity of Charles Darwin... It became clear to me that this defeatingly narrow view of the meaning of evolution in the anthropological literature was due to the field's reliance on Herbert Spencer as a source of all inspiration... All of these considerations left me with a firm conviction that explicitly

11 William Whewell devised the word consilience, meaning "jumping together," to designate the confidence gained when many independent sources [lines of evidence] 'conspire' to indicate a particular historical pattern." The evidence for repeated historical pattern is "so abundant and so diverse that no other possible coordinating interpretation could stand, even though any item, taken separately, would not provide conclusive proof." The strategy of coordinating disparate results from multiple, independent lines of evidence is called consilience of induction (Gould 1989: 282).

12 Much of what Lathrap internalized and applied in his work from the Neo-Darwinian authors can be sampled in the two volumes edited by Sol Tax (1960a, 1960b) in commemoration of the centenary of Darwin's Origin of Species.
evolutionary writing in anthropology would remain largely sterile until there was a return to the complex position of Darwin and the authors of the Neo-Darwinian Synthesis and an explicit rejection of the limitations imposed by Spencer's thought. It is amazing how little convergence exists between these two schools of thought other than Spencer's coining the glibly... and largely inappropriate phrase "survival of the fittest" (Lathrap Ms., 1984b: 2-3). 13

Lathrap's views have no room for the Neo-Spencerian evolutionism as proclaimed by Leslie White or for the kind of historical-materialistic view of what he called "the uniquely narrow and static view" of human ecology such as that advocated by Marvin Harris. Lathrap rejected White's "culturology," since it dismissed historical processes based on the principle of contingency as irrelevant to an understanding of cultural evolution. White's brand of evolution is built on the assumption that this process is pre-eminently determined by technological variables found within and explained by culture alone; he explicitly denied environmental, psychological and historical contingent processes as relevant to cultural evolution (White 1959: 107; Harris 1968: 123-37; Vogel 1975: 170-174; Gould 1989: 282-289). For Lathrap (cultural) evolutionary history must be both directed and undirected; predictable by some general principles or rules, but largely unpredictable (though non-random) and contingent upon prior developmental states in history.

Lathrap (pers. comm.) strenuously rejected Harris' (1968) reading of Darwinian strategy, which the latter translated into the sociocultural domain as the principles of techno-environmental and technoeconomic determinism. This is not to say that technoeconomic and technoenvironmental revolutions are irrelevant, but Lathrap was hard-pressed to accept cultural evolution as largely determined by these two factors. It makes it easier and more comfortable for the scientist to work with a tight, closed, well-controlled package of a few variables (as in closed systems ecology), but it does not necessarily follow that it is an accurate representation of how the empirical world functions and is structured. He had too much respect for human and cultural creativity to feel comfortable with "tall-order laws" dictating their potential and the intricate interaction with nature. For Lathrap, cultural evolution is a much more complex process, involving multiple interacting systems in which causality cannot be reduced to simple linear equations of cause-effect. For him:

The point of view derived from Darwin and the Neo-Evolutionists remains basic to all my published work... Concepts such as niche, niche saturation, competition for specific niches, differential fitness, the long term effects of even the slowest rates of of population growth, the conviction that an "explanation" of, or modeling of, cultural evolution was inevitably to be found in the long term systemic interaction of a number of factors rather than in a single "cause," and finally a respect for opportunism and the unique all became so basic to my thinking that I never felt it necessary to explain or belabor these base ideas. All of these are legitimate parts of Darwinism (Lathrap Ms., 1984b: 3).

13 At the time of writing this article I cited from a manuscript which Lathrap had given me for comments (Ms., 1984b). Later William M. Denevan informed me that it had been published in the Journal of Economic Botany 60(4) 339-344 (cf., Lathrap 1984c). The two versions are quite similar.
For Lathrap, peoples and their culture have a far more creative and active role in cultural evolution than is assumed in more deterministic "Law and Order" approaches. Such human-cultural creative process is as "determining" or "limiting" in evolutionary history as, for example, is any kind of ecological determinism or limitation. His views on this matter are crucial in understanding his approach to cultural evolution. Moreover, all of the assumptions he makes in the formal classification of archaeological artifacts are rooted on his views of human intentionality and selectivity. These have only been explicitly considered in his review of David Rindos' (1984) *Origin of Agriculture* book, and are worth quoting at length:

I remain to be convinced that a purely selectionist model will ever be totally successful for cultural evolution... Let me consider one instance of potential disagreement... I grant to human behavior somewhat more intentionality than does Rindos. But it must be noted that I use intentionality in a far more restricted sense... which precludes the idea of the "invention" of agriculture. Rindos and I have argued over his use of "intent." His usage appears to encompass everything up to what theologians have called "free will" and, in terms of inventiveness, the possibility of an Australian Aborigine inventing the atom bomb out of the conceptual raw material of his native cosmology. I too disbelieve in an intentionality of that scope. I would prefer to use the term in the far more restricted sense developed by Rollo May in his provocative book, *Psychology and Human Dilemma*. May contrasts the totally deterministic position of B. F. Skinner with the position of Carl Rogers, which May suggests is hardly distinguishable from the "free will" of the theologian. He shows in a scathing way the operational inadequacies of both positions and suggests there is only a narrow band of possibilities in which man actually operates. A trite example of intentionality in May's sense would be the decision of the farmer to plant his field this year in terms of his belief in the existence of next year. [Another] example would be that of the Amazonian Indian who had the two food categories of *fainha* [flour] and beer, and each year carefully selected among the cuttings for the next year's crop in terms of an approach to ideals or conceptual categories: the perfect manioc for flour and the perfect manioc for beer. The ideals are always beyond the actual genotypes and phenotypes of the two existing cultivars; but it is in the ideals [culturally regulated] that behavior is rooted and this leads to intentional behavior. May and I would agree that it is the possession of this very mode of intentionality that distinguishes Man from all other known organisms (Lathrap Ms., 1984b: 4-5, 8).14

Thus, intentionality is not only the conscious selection among a culturally approved range of choices (kinds of manioc) but also of choices to be made against an ideal, future, model or expectation (also culturally demarcated). In effect, intentionality puts the cause and effect in the future and, thus, linear cause-effect equations are rather inappropriate. It is this mode of intentionality and selection

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14 Consult Lathrap (1962a: 214-246) for a full discussion of intentionality and selection with regard to assumptions in artifact classification and (modal) analysis culture complex.
that led him to reject any notion that the emergence of agriculture is the result of an incident or discovery (cf. 1977a: 714). Rather, he argued that:

Agriculture appears because of the working out of systemic relationships that are set in motion as soon as the first traces of domestication, [the] coevolutionary relationships between plants and Man, appear. The state of the [agricultural or subsistence] system at any point in time can only be understood in terms of the state of the system at prior points in time, and even to raise the question of external causality is fatuous. The search for a cause for the appearance of agriculture is a futile, bankrupt enterprise (Ms., 1984b: 8)

This is, for example, where Lathrap fundamentally differs from Roosevelt's (1980) views on Amazonian-Orinocan cultural evolution. He noted that Roosevelt "argues that all [e.g., rise of complex societies, population explosion] can be explained in terms of the effects of the introduction of corn [and beans] as an event" occurring sometime during the Corozal II Phase in the Middle Orinoco. "Before this event everything was totally stagnant, only... after corn, could things get going. She suggests that corn, when it appears, is such an obvious improvement over manioc that its adoption will be instantaneous and its effects stupendous" (1986a: 15). This objection echoes another observation made by William Denevan (pers. class notes, 1979) while lecturing in a seminar on Amazonian cultural ecology in Illinois-- that "agricultural potential" is not something inherent in nature and independent of culture; that it varies with perceived need (i.e., Lathrap-May’s "intentionality"), technology and economic considerations in applying technology. In other words, the limiting conditions of agricultural potential are not just a matter of soils, calories and proteins.

Lathrap fully accepted Roosevelt's (1980) demonstration that in Parmana a highly specialized agrosystem, dependent on storable parched maize led to a dramatic population increase, particularly in the Camoruco phases. However, he was more concerned with her views regarding the agrosystem it replaced (i.e., Ronquian-Saladoid; Corozal-I), which in his mind, remained "an as yet unspecified agrosystem" (Ms.,1986a: 16). He noted, "If there is one weakness in Roosevelt’s more general argument on the effects of corn on cultural evolution within the Amazon Basin is her failure to distinguish between the presence of corn as one member of a large arsenal of high calorie crops... and the total dependence on corn as the storable staple" (Lathrap et al. 1985c: 56). Instead, Lathrap (1987c: 359) notes that there is evidence suggesting that maize had a long evolutionary history in Amazonia before it became the highly productive, genetic "monster" and the staple crop (as opposed to one of a range of crops) that characterized the Corozal II-Camoruco levels. He specifically cited the presence of a distinctive first Brazilian string cob maize (ca. 3,000 B.C.) which shares similarities with the earliest corn in Coxcatlán (5,000 B.C.) and with the Central-Southern Peruvian-Ayacucho (3,000 B.C.) maize, Confite Morocho. Given this distribution, he proposes the alternative hypothesis that "by 3,000 B.C. maize had saturated the tropical lowland alluvial network and was penetrating beyond its limits. The presence of maize as a crop,
though not as a staple, in the middle and lower Orinoco by 3,000 B.C. was thus, more likely than not." In summary, Lathrap felt that Roosevelt had neglected the possibility that behind the "event" she observed in Parmana there was a long evolutionary trajectory for maize in tropical South America.\(^{15}\)

He also disagreed with Kent Flannery's views for similar reasons. He (Ms., 1977a: 714-16) praised the latter's insistence that the origin of agriculture must be viewed as a process, but was criticized for failing to follow through when faced with the sudden appearance of well developed Formative Mesoamerican villages in the highland basin. Lathrap paraphrased and exaggerated --in his own inimitable style-- Flannery's parable:

Scene: on an escarpment at the edge of the Tehuacán Basin; Time: 4:45 P.M. Date: December 31, 1501 B.C. The headman of the macroband is giving his daily harangue to his followers:

"Hey guys, I figured it out. Calculating with my gambling sticks, it turns out that the corn in the little patch over in the barranca [cliff] will give us now more calories per hectare than the old fashioned, obsolete, mesquite down on the floodplain. Only one progressive thing to do. Tomorrow, we all got to get down there and grub out all of that nasty, old, wild mesquite, and put the whole floodplain in corn. Hey gals, you got a big day tomorrow too, got to invent that thin, hard, technologically sophisticated pottery that goes with the Real Formative. We all got to put our minds [on] how to [build] a permanent house, otherwise we won't make the grade. Remember, this is it, the Formative Revolution! Get [on] with it: tomorrow we're going to make history!"

Can one detect an aura of an event in the above? (Ms., 7-8).

Lathrap's insistence that the evolution of agricultural systems --indeed, of any subsistence pattern-- can only be understood in terms of a long series of prior and subsequent states (see quote below) betrays his preferential focus on historical explanations based on the principle of contingency. It is this approach which interested him most about cultural evolution, but with the acknowledgement that general principles and rules of evolution are in the background and coordinate with the principle of history: contingency. No one has said it more eloquently than Harvard paleontologist Stephen Jay Gould:

Historical explanations take the form of a narrative: E, the phenomenon to be explained, arose because D came before, proceeded by C, B, and A. If any of these earlier stages had not occurred or had transpired in a different way, then E would not exist (or would exist in a subs: antially altered form E', requiring a different explanation). Thus, E makes sense and can be explained rigorously as the outcome of A through D. But no law of nature enjoined E; any variant E' arising from an altered set of antecedents, would have been equally explicable, though massively different in form and effect... I am not speaking of randomness (for E had to

\(^{15}\) I am fully aware that my focus on Lathrap's perspective is unfair to Roosevelt and Flannery. A full evaluation and weighting of all sides of the issue would be prohibitive in terms of space. My intent here is to provide an insight to Lathrap's approach and its underlying intellectual influences rather than to evaluate the merits of each model. As all complex models, each has its strengths and weaknesses.
arise as a consequence of A through D), but of the central principle of all history--contingency. A historical explanation does not rest on direct deductions from laws of nature, but on an unpredictable sequence of antedated states, where any major change in any step of the sequence would have altered the final result. The final result is, therefore, contingent upon everything that came before--the unerasable and determining signature of history (Gould 1989: 283).

I strongly suspect that Lathrap would wholeheartedly identify with Gould’s rendition of the explanatory power of “just history.” Having known Lathrap for over ten years, I know that what most satisfied his intellectual curiosity, is in agreement with that advocated by Gould and exemplified in his excellent treatise, Wonderful Life: The Burgess Shale and the Nature of History (1989). In it he presents an analysis of the implications of the Edicara, Tommotian and Burgess faunal complexes (dating to shortly before and after the Cambrian). The Burgess fauna is characterized by a large array of distinct, bizarre anatomical designs, but after the Burgess decimation only a few major phyla survived, from which all modern life forms evolved, including hominids. The classic Darwinian model of gradual, progressive, selectionist evolution alone cannot accommodate the Burgess facts. Gould (1989: 288) convincingly demonstrated that “the modern order of life and anatomical forms that survived massive extinction” were “not guaranteed by basic laws (natural selection, mechanical superiority in design), or even lower level generalities of ecology or evolutionary theory;” it was largely the product of contingency. But, does this mean that either Lathrap or Gould would regard evolutionary history as totally unpredictable and contingent upon past states? It is instructive to quote in extenso Gould’s cautionary remarks (and think also of its relevance to cultural evolution and history):

A final point about predictability [invariant laws] versus contingency: Am I really arguing that nothing about life’s history could be predicted, or might follow directly from general [directed] laws of nature? Of course not, the question is one of scale, or level of focus. Life exhibits a structure obedient to physical principles. We do not live amidst chaos of historical circumstance unaffected by anything accessible to the “scientific [law- and-order] method” as traditionally conceived. I suspect that [for example]... the laws of surfaces and volumes, first recognized by Galileo, require that large organisms evolved different shapes

16 Interestingly, the world’s first known chordate, Pikaia, from which all Chordata (including humans) developed had no obvious advantages in anatomical design or population density, in comparison to other members of the Burgess assemblage that would suggest its selection for survival. No evolutionary laws or principles of ecology would guarantee its survival. The only explanation: historical contingency. Should the tape of history be rewound and replayed, Pikaia would probably not survive, and humans would not have evolved (cf. Gould 1989: 317-322).

17 Evidence such as the Burgess Shale fauna, gave rise to Gould and Niles Eldredge’s punctuated equilibria evolutionary scheme (cf. Gould 1980: 182-183).
from smaller relatives in order to maintain the same relative surface area. Similarly, bilateral symmetry can be expected in mobile organisms built by cellular division (Gould 1989: 289).

But these phenomena, rich and extensive as they are, lie too far from the details that interest us about life's history. Invariant laws of nature impact the general forms and functions of organisms; they set [determine] the channels in which organic design must evolve. But the channels are so broad relative to the details that fascinate us! The physical channels do not specify arthropods, annelids, mollusks, and vertebrates, but, at most, bilaterally symmetrical organisms based on repeated parts. The boundaries of the channels retreat even further when we ask the essential questions about our own origin: Why did mammals evolve among vertebrates? Why did primates take to the trees? Why did the tiny twig that produced Homo sapiens arise and and survive in Africa? When we set the focus upon the level of detail that regulate most common questions about the history of life, contingency dominates and the predictability of form recedes to an irrelevant background... (Gould 1989: 289-90)

Charles Darwin recognized this central distinction between laws in the background and contingency in the details... ultimately, the question of questions boils down to the placement of the boundary between predictability under invariant law and the multifarious possibilities of historical contingency... I envision a boundary sitting so high that almost every interesting event of life's history falls into the realm of contingency. I regard the new interpretation of the Burgess Shale as nature's finest argument for placing the boundary this high (Gould 1989: 290).

Lathrap's probable reasons for his disenchantment with the "Law-and-Order" variety of New Archaeology are, among other things, because they place the boundary of law and order too low (deterministic) to accommodate obvious discrepancies with the empirical archaeological record. He preferred a less axiomatic and deterministic approach. His boundary between "law and order" and contingency approximates that advocated by Gould. Perhaps more important, to raise the question whether a given approach is evolutionary or historical is, in his view, a fallacy. For him:

...the so-called historical approach; Kulturkreise, "Age-Area," migrations studies, etc., can be [articulated with] the so called evolutionary approach. Perhaps no pseudo-issue has muddled the pool of anthropological thinking more than the distinction between history on the one hand and "pure" Cultural Evolution on the other (1976c: 528).

Lathrap, however, has specific views as to what kinds principles and rules ("channels") are to broadly guide cultural evolution, although these always articulate with historical contingency. His views on this matter are most explicit in his publications on cultural radiation (1976c) and the subject of the evolution of subsistence systems (1962a, 1970a, and especially 1977a, Ms., 1984b, 1987c).

The evolutionary process of radiation: a biological perspective

It is useful to briefly outline some basic Neo-Darwinian concepts pertinent to the process of radiation, since they have a direct bearing on Lathrap's modeling of
the cultural evolution of subsistence systems and provide the rationale for population movements. In doing so, I have purposefully selected only two Neo-Darwinian writers who had most influenced Lathrap's views. In addition I have included a more recent reference, S.J. Gould, since his views parallel those of Lathrap.

Biologists and natural historians agree that most major evolutionary advances depend on a shift by a population of organisms into a new environmental zone and that the feasibility of this shift depends on their available pre-adaptive characteristics (Mayr 1960: 370). In biology, radiation refers to the evolution of a group of organisms (population) that is characterized by spreading or expanding into different environments and by a divergence of structure-function as a result of new selective pressures. Darwin, in 1844, was the first to identify the process of adaptive radiation. He noted the tendency of organisms descended from the same ancestral stock to diverge in character and, thus, become modified (evolve). The explanation for this change, in Darwin's own words, "is that the modified offspring of all dominant and increasing forms tend to become adapted to many and highly diversified places in the economy of nature" (cited in Simpson 1960: 161). Simpson further notes that every marked expansion of a group, whether at the level of species or at any higher taxonomical level, is an adaptive radiation.

Each [radiation] starts with a group of a certain adaptive status in a particular range of environments. (Such a group is "generalized" only in the sense that it is not irrevocably committed to a special, narrow range.) Each radiates by a combination of two processes: a parceling-out of a broader ecological range among more specifically adapted separate lines of descent and the invasion of new ecological niches by modifications of the ancestral adaptation" (Simpson 1960: 162).

Two broad kinds of population expansions are recognized: primary expansion and relay expansion. But the patterns of both are normally those of adaptive radiation. Primary expansion involves the occupation of an empty, uncontested ecological niche. Depending on what level of resolution and what scale one is operating (species, phyla, kingdom etc.) extinction is generally irrelevant relative to primary expansion, although at the lower level one may find a species expanding at the expense of other closely related species (Simpson 1960: 162).

A relay expansion is a reoccupation of the niche, "frequently by a group that is not closely related to the one replaced" (Simpson 1960: 162). Extinction by replacement, he writes, is an essential feature of relay expansion, since it cannot occur unless the local, resident group is either reduced in scope or completely eliminated. Frequently "the contraction of one group and expansion of another are concomitant and are almost certainly causally related; that is, "the spread of the relaying is itself the cause or one of the causes of extinction" (ibid). This is known as competitive replacement. In other instances, the cause of extinction is reversed: "a new radiation occurs because of prior extinction, instead of extinction resulting from radiation" and it implies that the ecological niche was already open for a new
occupation. Simpson (1960: 163) notes other causes besides competitive replacement, such as the case of Mesozoic reptiles relayed by Cenozoic mammals (with no overlapping temporal distribution). The latter instance also suggests that "a replacing group may be able to exploit an ecological situation more intensely and to sub-divide it more finely [niche specialization] than the older group" (ibid).  

Ernst Mayr (1960: 370-ff.), in his discussion of the emergence of evolutionary novelties (origin of new structures), provides an additional series of useful insights relating to adaptive radiation. Some basic "rules" relating to the evolutionary aspects of a shift into a new ecological zone can be summarized as follows: (1) such a shift can occur only if the (biological) organism, its population, is pre-adapted to it; (2) once the shift has been achieved, a whole new set of selective pressures will have a tendency to modify all of those structures that are more specifically concerned with life in the new environment; (3) the more drastically different is the new ecological zone, the more rapid will be the evolutionary change and, generally, the more profound will be the organism's structural reorganization; and (4) the evolutionary changes that result from adaptive shifts are often initiated by a change in behavior and followed by a change in the structure-function of the organism.

Finally, Mayr (1960: 377-78) reasons that what controls the emergence of "evolutionary novelties" is normally due to, among other things: (a) the acquisition of a new function by a pre-existing structure, thus, the new emerging structure is merely the modification of a preceding one, (b) the selection favoring the structural modification is greatly increased by a shift into a new ecological niche, by the acquisition of a new habit, or both; and (c) a shift in function will expose the fully formed "pre-adapted" structure to the new selection pressure, "which explains how an incipient structure could be favored by natural selection before reaching a size and elaboration where it could be advantageous in a new role."

Adaptive radiation relates to the process of speciation (e.g., the emergence of novelties). Evolutionary changes proceed in two major modes, either by phyletic transformations, where an entire population changes from one state to another, or by speciation, the branching-off of new species from a persisting parental stock. The former exhibits discontinuous variation, whereas the latter shows continuous variability. Since extinction by extirpation (not by evolutionary change to some other form) is a common occurrence, only through speciation the earth is replenished; the latter allows for increased diversity whereas the former, as a result of extinction does not—it is "only a transformation from one thing to another" (Gould 1980: 182).

As Gould (ibid: 183) noted, all theories of speciation agree that divergence takes place rapidly and in very small populations. One theory of speciation, allopatric (geographic) speciation, suggests that "a new species can arise when a small segment of the ancestral population is isolated at the periphery of the ancestral range. Large stable central populations exert a strong homogenizing
influence. New and favorable mutations are diluted by the sheer bulk of the population through which they must spread... but small, peripherally isolated groups are cut off from their parental stock [and] live as tiny populations in geographic corners of the ancestral range [where] selective pressures are intense because peripheries mark the edge of ecological tolerance for ancestral forms" (Gould 1980: 183-4). Hence, not only there is the founder's effect (the population moving toward the periphery would bring only part of the traits of the parental stock) but also will develop significant structural-functional changes as a response to new adaptive pressures. Peripheries (and also ecotones) are cauldrons of potential, rapid change and divergence that can be further accelerated if there is a concomitant expansion into a new environment. The periphery (e.g., ecotone zones) exerts new selective pressures for the development of new adaptive features that would result in pre-adaptive advantages for the population to penetrate adjoining but different environments.

The emergence of novelties has a direct relevance for those of us interested in the origin and development of a wide diversity of cultural phenomena. Indeed, I would venture to suggest that at least Mayr's rules (a) and (b) above are applicable in the case of the evolution toward agricultural intensification as described by Lathrap (1977a) --involving the shift from house garden to conuco agricultural fields. One might conceive this shift as a sort-of cultural speciation of gardening practices. At a lower level of resolution (Lathrap 1974a), an argument has been put forth for the emergence of early Valdivia vessels with dimpled bottoms from a pre-existing structure (rule 'a'): the preceramic decorated bottle gourds (such as those of Huaca Prieta, Perú).

Adaptive radiations, subsistence patterns and the emergence of agriculture

The remainder of this essay will give a "guided tour" of Lathrap's views on the emergence of agricultural systems. The focus is on a paper he presented, as a graduate student, in Cora DuBois' seminar on evolutionary theory in anthropology. This paper, written in 1956, lays out the foundation upon which Lathrap would build his model of Amazonian culture history and evolution. Twenty years later, Lathrap (1976c) published it with a few added comments set-off in brackets. As he stated "Almost all of my publications have, in one sense or another, been elaborations of the paper's themes or further wrestlings with problems left unresolved here" (1976c: 494). Despite being at the core of all of Lathrap's thinking, it has received little attention (to my knowledge first cited by Willey 1960: 129). It was not until the 1970's that the basics of Lathrap's approach --divergent evolution and the radiation process-- began to take roots in archaeology.

Lathrap's paper had a substantial impact among some Mesoamerican archaeologists; specifically it inspired Flannery's and associates' essays on the divergent evolution of the Zapotes and Mixtecs (Flannery and Marcus 1983). In the introductory chapter, Flannery noted that much of the anthropological work of the so-called Neo-Evolutionists such as Julian Steward, Leslie White, Elman Service and Morton Fried were exclusively concerned with general evolution focusing on
the emergence of higher levels of integration ("stages") at the expense of other "forgotten" kinds of evolutionary processes; mainly, divergent (specific) evolution. Flannery and Marcus' The Cloud People was aimed at remedying this state of affairs. In this book, Flannery commented: "fortunately, even at the height of general evolutionism (between 1955 and 1975), a handful of anthropologists thought to focus their... research on this otherwise forgotten process: the specific evolutionary divergence from a common ancestor (Flannery and Marcus 1983:2). Among the first to focus on the process of divergence were ethnologists Goodenough and Sahlin's in 1955 (Malayo-Polynesian cultures), but independently in 1956 Lathrap (1976c) came up with the same approach for archaeology in his "Radiation" paper, followed in the late 1970's by others including Olga Linares and Anthony Ranere (Flannery and Marcus 1983: 2-3). Flannery also credits the University of Chicago's Man in Nature Program (1956-61), lead by linguist McQuown, which was concerned with Mayan linguistic divergence. It must be noted, however, that the foundations of historical linguists, from early on, were rooted on the principles of specific divergent evolution: the study and reconstruction of common ancestral languages and speech communities and the history of the radiation and speciation of languages. It is no accident that historical linguistics and its concern with linguistic divergence experienced a significant surge in the period between 1955 and 1965, particularly with the seminal work of Morris Swadesh (a student of Sapir) and his introduction of glottochronology. Historical linguistics was ahead of the times and Lathrap knew it. His constant references top Edward Sapir and his students are at least as equally important as his acknowledgement of the Darwinian Synthesis authors and, undoubtedly, both were instrumental in crystallizing Lathrap's views on cultural evolution.

In contrast to his Upper Amazon book and the "Neolithic Revolution" paper (1970a, 1977a), where the ecological-evolutionary principles are somewhat masked by the details of historical contingency, this paper (1976c) explicitly lays down the "rules," expressed as a series of derivative propositions that are involved in the evolution of subsistence patterns. It is here where Lathrap has brought the "laws" to the forefront and relegated the details of contingency to the background.

In his "Radiation" model the society, not the individual, is explicitly regarded as the "adapting organism," and that what it adapts to in a very large measure is the environment. In order to simplify the model, he restricts the range of cultural phenomena to be considered only to those cultural patterns "which structure the food quest and the distribution of food within a society," with the full knowledge that the entire range of cultural patterns ultimately ought to be treated not as constants but variables (1976c: 497).

Postulate 1: "Within a given environment a particular subsistence pattern will only support a certain density of population. Any change in either the environment
or the subsistence pattern [will probably] influence the population density one way [+\ldots] or the other [-\ldots]. This can be measured as the relative efficiency of a particular subsistence system "in terms of the population density which it can support in a given environment" (1976c: 498).^{19}

Postulate 2: "Given a locality where two [or more] societies [peoples] with different basic subsistence patterns are brought into contact, the more efficient of the two societies [peoples], in the sense defined above, would eliminate the less efficient" (1976c: 502). Later he refined it as follows: "People cannot preempt the land of their neighbors unless there is some kind of competitive superiority either in sheer numbers, more integrated organization or in other factors giving a military edge" (Lathrap et al. 1985c: 43). Thus, "efficiency" in terms of a successful niche competition involves other variables besides an efficient subsistence and a higher population density.

The first postulate defines efficiency simply in terms of people (organized into one or more societies) and their culture (e.g., subsistence patterns) adapting and reacting to that environment and does not consider niche competition between two different peoples.\(^{20}\) The second postulate, however, adds the latter as a variable. Based on these two postulates, the following propositions were suggested:

Proposition 1: "A subsistence pattern and the society [people] maintaining it can be said to be adapting to a particular environment, when the total of changes becoming stabilized in the total cultural pattern tend to increase the efficiency of the subsistence pattern" (1976c: 498).

Proposition 2: If they behave in an analogous manner to biological organisms then "societies with a particular subsistence pattern should continually expand their geographical range up to the limits beyond which their particular subsistence patterns could not be practiced" or "beyond which the efficiency of the pattern becomes zero" (1976c: 498).

Proposition 3: If the population growth rate rapidly increases while the subsistence pattern remains constant, maximum efficiency would be reached

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\(^{19}\) In another part of Lathrap's (1976c: 506) paper, he reformulates efficiency in terms of the number of man-hours which are necessary to provide a person with an adequate diet under given conditions of subsistence patterns and environment. This includes concepts such as food surplus, number of individuals relieved from subsistence activities, food storage, etc.

\(^{20}\) Throughout this paper, Lathrap did not make a clear distinction between peoples-cultures and societies-subcultures, a distinction that many anthropologists would demand. Rouse (1986), for example, maintains that a society does not have a culture, but rather a subculture and only a people—which is organized into societies—may be characterized by a culture (or a cultural tradition). For him, peoples and cultures evolve; societies and communities adapt. Hence, where appropriate, I have bracketed the concept of people(s) as a clarification.
quickly. In such case, as long as there is an adjoining, unoccupied ecological niche the excess population will tend to expand outward "rather than starve" (1976c: 498). Lathrap (1977a: 734) later rephrased it as follows: "It was [Gregory] Bateson's belief... that as long as there is open territory to [expand to], groups will fission rather than suffer an increase in the expense and complexity of their institutions" (1976c: 499). 21

**Proposition 4:** Given the limitations of the basic kind of subsistence pattern originally established in the area, and given a people, who have been adapting to a particular environment for a long time, the subsistence aspects of their culture will tend to approach maximum efficiency for the immediate environment. The maximum efficiency of a given subsistence pattern would involve "progressively greater emphasis on the most efficient food sources" available and "progressively more efficient mechanisms for procuring and processing these food resources." In other words, there would be an adaptive tendency toward greater intensification and specialization of subsistence patterns and the attendant adaptive changes in other aspects of culture (1976c: 499-500).

**Proposition 5:** Peoples that have reached nearly maximum specialization in a geographically limited environment will tend not to expand beyond the environmental range to which their specialized cultural patterns are adapted to unless (a) there are significant changes in the environment, or (b) a new subsistence pattern is introduced and successfully adopted.

**Proposition 6:** However, "a specialized society [people] which has a tendency not to spread beyond the environment to which it has limited itself and which has a pronounced tendency to concentrate on a single food source may, itself, radically alter the environment by destroying its own basis for existence. Such society [people] also would tend to loose the ability for radically new developments since specialization is largely in terms of of emphasizing certain alternatives which were present in the original culture and discarding others" (1976c: 501). That is, specialized peoples are initially in a position which is advantageous but which is a precarious one from the perspective of long term survival.

Adaptive radiations: from generalized to specialized in subsistence patterns

Lathrap's model begins with a hypothetical scenario that assumes an uninhabited (by humans) continent, as was the case of the New World some 50,000 yrs. ago, with all of its environmental zones open for occupation and exploitation

21 It follows that only when population pressure builds-up and when there is no possibility of expanding into adjacent areas that a society will "turn-on" Neo-Malthusian checks, one of which includes an energy investment and expenditure in increased institutional complexity (socio-political, religious, etc.)—one of Carneiro's (1970, n.d.) requirements for the emergence of complex societies (social circumscription).
by a single hypothetical founding society (population). He further assumes that they are characterized by a generalized subsistence economy; that is, the people are not irrevocably committed to the exploitation of a narrow ecological niche and, therefore, it is pre-adapted to exploit a broad range of environments facilitating its spread over the continent. Such a founding population (people) would undergo the following adaptive radiation (evolutionary change):

(1) They first would spread and occupy all those ecological niches to which their original generalized subsistence pattern is pre-adapted to; that is, where the relative efficiency of the subsistence system would be arbitrarily defined as higher than (+) zero. One could predict then that from the original people a number of relatively similar societies with similar cultures and subsistence patterns would spread over a fairly wide territory (1976c: 498-9).

(2) As groups fissioned from the parent people and expanded (without experiencing niche competition with others) along the preferred ecological niche(s), they would become progressively differentiated as they become more isolated (peripheral) from their parental stock. Some of this differentiation "should be strictly adaptive to the range of environments into which the parent society spread" and some is the result of "random loss of certain cultural traits due to the smallness of the spreading groups and the development [invention] of new traits which would not spread rapidly because of the lack of extensive contacts" which he views as the "cultural analogue of genetic drift" (1976c: 499).

(3) Over a long period of time, "many of the originally uninhabitable environments of the hypothetical continent would become inhabited." This would occur in the following manner: daughter peoples which occupy environmental zones peripheral to the people's original subsistence pattern, would tend to undergo adaptive changes in their subsistence patterns toward a greater efficiency in terms of their immediate niche (in analogy to the biological allopatric speciation). Therefore, "changes in the subsistence pattern would allow the penetration of adjoining areas which previously could not be utilized" (1976c: 499).

(4) If no other variables are factored in, the long term results of the continuous repetition of this expanding process would be (a) the near occupation of all ecological niches, with each group well adapted to its immediate environment, (b) the sharing a number of cultural elements as a result of common ancestry, but (c) increasingly more differentiated as a result of "cultural drift." Likewise, "the subsistence economies, though originally all of one pattern, should show progressively greater divergence as each became more completely adapted to its own limited environment" (1976c: 499).

Clearly, the very beginnings of the hypothetical radiation outlined above, correspond to what archaeologists have identified as the Paleo-Indian generalized subsistence pattern; to what in those days Willey and Phillips called the "Early Lithic Stage" (Lathrap 1976c: 516). The existence of a primary adaptive radiation stage based on a generalized subsistence economy has for a long time been hotly contested among two groups of archaeologists. Proponents of the "Pre-Clovis" position, with varying degrees of confidence, interpret the archaeological evidence
as supporting the existence of Pre-Clovis (pre-12,500 B.C.) Paleo-Indian traditions with a generalized subsistence pattern (e.g., Bryan 1986). Proponents of the Clovis position (e.g., Lynch 1990) reject such evidence as equivocal and argue that all the earliest, demonstrable evidence belongs to the Clovis tradition, from which all subsequent Paleo-Indian traditions evolved. The Clovis (and related traditions) hunters were characterized by a highly specialized subsistence economy: the Big Game hunting pattern. In their view, the initial occupation (primary expansion) of the New World was already characterized by a people with a highly specialized subsistence pattern, with an environmental range largely determined by the habitat of the megafauna (but still a large territorial range).

Following the Martin-Haynes Hypothesis, specialized Big Game hunting bands developed progressively more "efficient" (toward larger, better organized communal) hunting techniques, resulting in an "explosive radiation." They rapidly spread throughout the grassland range of the megafauna. But eventually these "efficient" techniques became mal-adaptive and inefficient (in the sense defined by Lathrap). The megafauna kills exceeded the capacity of the fauna to reproduce in sufficient numbers to replenish their stock which in turn resulted in the extinction of an entire subsistence pattern with its attendant repercussions in other aspects of culture. The effects of the Big Game hunting pattern are predicted by Lathrap's proposition No.6 (above) and, as he noted, are in essential agreement with the model later developed by Paul S. Martin in 1973 (Lathrap 1976c: 517). For Lathrap (1976c: 517-18), however, the Big Game hunting pattern resulted from a later radiation (population movement) which added to the earlier generalized Paleoindian economy.

This debate is a good example of the distinction between "rules in the background" and "contingency in details" discussed earlier. The principles of evolutionary change are not so much in question as are the details of the sequence of "states," which are governed by the principle of historical contingency: Was the primary radiation ("peopling" in Rouse's [1986] terminology) of the New World characterized by a people with a generalized subsistence strategy or by a specialized Big Game hunting pattern? Assuming that the generalized pattern was introduced first, none of the principles and propositions above suggested would predict the same outcome if one were to re-play the tape history, but it would still have to proceed according to the broad "channels" set by the adaptive radiation process with regard to the specific subsistence pattern (generalized or specialized) which happened to be introduced first. At an even more basic level, however, we have yet to reach the kind of consilience that Gould (1989) considered as necessary to completely rule out either hypothesis.

Let us assume, asks Lathrap (1976c: 499-500), that a society with an initially generalized subsistence pattern —hunting and gathering — has been adapting to a particular niche over long periods of time. On the basis of propositions 4 and 5, the

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22 Also consult Oliver and Alexander (1990) for a more detailed discussion with reference to the Paleo-Indian complexes from El Jobo Basin (Pedregal Valley) Paleoindian archaeology.
subsistence aspects of their culture will tend to approach maximum efficiency for
the immediate environment, gravitating toward more effective mechanisms to
procure and process food and to progressively rely on a few major food sources.
Such a society would be able to flourish and support a high population density if
they make efficient use of certain localized and abundant food resources, such as
fish (1976c: 500). He describes such a hypothetical society of "maximum" efficiency
and well adapted to its immediate environment (but based on the cultures of the
Pacific Northwest Coast in North America).

The[r] cultural patterns were highly adapted to the environment, not only with respect
to the catching and preservation [surplus] of salmon, but also to other features of the
environment less directly concerned with food production but important in that they lead to a
high efficiency in all aspects of survival. Notably there was the pattern of woodworking
technology... such technology could only be practiced within the range of a couple of species
of trees, namely the red cedar and the coast redwood. In their specific adaptation the
societies... attained a high degree of efficiency but as a consequence their possible geographic
range was restricted. Their subsistence patterns and the surrounding patterns in other aspects
of culture could be practiced only at the mouth of rivers with heavy salmon runs and within
the fog belt forest immediately adjacent to the coast, which contained either cedar or redwood
(Lathrap 1976c: 500).\(^{23}\)

Given the above, it is reasonable to expect that their subsistence and cultural
pattern would seldom expand beyond the range to which such a specific adaptation
is possible. Such people would continue to flourish as long as there are no major
environmental changes or as long as no now radically different subsistence patterns
are introduced (cf., propositions 5, and 6 above).

Lathrap noted that a continued adaptation toward maximum efficiency of the
environment (assuming no other disturbing factors) should theoretically result in a
continental map in which each natural life zone would coincide with a fully
adapted, efficient subsistence pattern with only slight, graduated cultural
differences from one area to the next. "If no disturbing factors are introduced, the
system of the hypothetical continent would drift toward a cultural and
demographic equilibrium in which subsistence economics so closely approached
their maximum efficiency and population densities so closely approached their
limits under the particular pattern that variations would tend to be random and
minor" (1976c: 502).

Of course, he noted, such arbitrarily imposed limitations are unrealistic. Any
number of factors, disease, climate, extinction of food sources, warfare, etc. would
produce a disequilibrium in the system and change the direction of evolution long
before the theorized continental cultural-demographic "equilibrium" is reached. He
suggests, however, that perhaps more significant than any catastrophic factors
would be the introduction of a new subsistence pattern characterized by a much

\(^{23}\) One can cite the Warao of the Orinoco Delta, Venezuela, as another example of successful specialization
of a hunting and gathering economy largely determined by the range of a species of palm (moriche) in a
wetland environment.
greater efficiency, that could support a much denser population for the same relative area of exploitation. One such radically different subsistence pattern would involve the domestication of plants (and/or animals).

The Emergence of Agricultural Patterns

In his "Radiation" paper, Lathrap assumed that agriculture is at some point "introduced" in a hypothetical continent which has already "run the full course of the spread of societies with hunting and gathering patterns" (1976c: 502). At the beginning the minor addition of plants or potential crops would be marginal to the groups' basic diet and would "not pose a challenge to the efficiency of hunting and gathering economies" in more favorable environments. It is only after a long period of experimentation that "cultivated plants would change sufficiently from their wild ancestors to be markedly more efficient food sources than uncultivated plants" and progressively become the major component of the groups' diet. It is at this juncture that agriculture would produce "a violent upward swing in the amount of food produced" and, consequently, an increase in population density: more individuals could be supported per area than it was possible with the previous subsistence pattern (1976c: 503).

Increased population density would build-up pressures on the agrosystem's carrying capacity and on the cultural institutions regulating the system and the environment. Such pressures would be temporarily relieved by population movement (again following propositions 2 and 3). He further proposes that by the time such pressures are mounted, the people would be somewhat specialized to its original environment and may have "a number of cultural traits which [may be] neutral or detrimental with regard to efficiency in adjoining environments" (ibid). The migrating agricultural groups will then preferentially move into areas in which they are already pre-adapted and secondarily to other areas where they might be moderately or even poorly pre-adapted in terms of the agricultural pattern. Three "type" scenarios are suggested (1976c: 504), although one could envision a whole gradation of possibilities in between:

a) Areas such as deserts lacking major rivers, or areas with cold climate and short growing seasons, will support hunting and gathering societies with varying degrees of efficiency but where agriculture cannot be practiced even at a low level of efficiency. Here it would be expected that hunting and gathering societies would continue to develop, but agricultural peoples will not penetrate.

b) Areas where environments can support hunting and gathering societies at a low level of efficiency and agricultural peoples at a much higher level of efficiency. Lathrap (ibid) expects that in such areas the principle (postulate No.2) or rule of "the fittest subsistence system," agriculture, will prevail. Hunting and gathering societies will have a low density population and, thus, be swamped by the higher density population of agricultural groups.

c) Finally, there will be areas which can support with a moderate to high degree of efficiency of both hunting-gathering and agricultural peoples.
The latter circumstance has the most interesting and diverse possibilities for students of culture history and evolution.²⁴ Lathrap (ibid; cf. also Ms., 1984b, 1987c) suggests that the intrusive agricultural societies will not be initially pre-adapted to these environmental zones and that their success would be dependent on the co-evolutionary (adaptive) processes of plant and man and the latter’s deliberate rescheduled behavior to increment the productivity (fitness) of the agrosystem in its new setting. That is, agricultural systems must be gradually readjusted to new niches. For example, this would be the case for the introduction of maize from Mesoamerica to a temperate zone in North America (which given the photoperiodic requirements of maize would require genetic adjustments to the new conditions).

A different kind of circumstance from the three outlined above attested by the ethnohistoric record in Venezuela is what I would call a symbiotic subsistence pattern. Two different highly specialized subsistence patterns are mutually interdependent for a balanced diet that results in high efficiency as defined by Lathrap. In the Northern Llanos of Venezuela the Guaiquerí people are transhumance, specialized fishermen and gatherers fully adapted to the riverine niche. Their neighbors, the Caquetío people are highly specialized agricultural people (maize, beans, squash, manioc) settled in the interfluves a distance away from the rivers and on higher lands protected from seasonal floods. The Caquetío agricultural produce is exchanged for fish (proteins) in rotating regional Guaiquerí fish markets (Oliver 1989: 297). There may be other aspects of culture diffusing between the two peoples, but each largely maintains the integrity of its own cultural pattern (evolving parallel traditions). The result is an increased subsistence efficiency that depends on a symbiotic fishing/gathering and agricultural pattern, which in turn can support higher population densities. I suspect that a symbiotic subsistence pattern as described above characterized the Manicuaroid maritime fishermen and gatherers and the agricultural Eastern Venezuelan coastal Cedrosan-Saladoids around the time of Christ.

Toward the end of Lathrap’s second hypothetical (agricultural) radiation, a far more complex scenario emerges. He proposes that:

In the hearth area of the agricultural pattern the process of adaptation, which was already begun before the outward spread from this region, is continuing probably at an accelerated rate. In regions where agricultural societies easily replaced [relayed] the earlier societies, there will be cultural patterns which were [initially] similar to those of the hearth area... as these societies become established in their new home the process of regional specialization produces progressively greater divergence. In intermediate zones... further adaptation may be more in response to the problem of [competition] the co-existence of two hostile subsistence patterns than to the actual geographical environment... the fusion which may be the end product in these regions may be quite different from either of the parent cultural traditions. In areas where agricultural societies could not penetrate, hunting and

²⁴ The range of possibilities of what might occur in cultural contact situations has been discussed from an archaeological perspective in Lathrap ed. (1956) and by Rouse (1986).
gathering societies will continue to perfect their specialization in exploiting available resources. These marginal societies may still preserve some shared cultural features which point back to their common origin at a time before their continuous distribution was broken by the spread of agricultural societies" (Lathrap 1976c: 505-506).

Later, in the "Man the Hunter" conference, Lathrap (1966f: 25) added yet another possibility which is analogous to the process of introgression in botany: as a result of a variety of pressures, agricultural peoples could revert back to a hunting and gathering subsistence pattern (and its attendant cultural consequences), which could forge the illusion of a direct development from an ancestral "archaic" group. Likewise, peoples with less efficient agricultural patterns (e.g., in interfluvial or riverine headwaters; tierra firme swidden agriculture) could also be the result of an "introgression" as a result of being defeated or forced out of a more productive niche (e.g., floodplain agriculture). Again, the details of contingency are of crucial importance to issues that are highly relevant --perhaps even more so than the general principles-- in reconstructing any accurate model of Amazonian cultural evolution.

His 1977 "Neolithic Revolution" model is built upon the propositions above synthesized and discussed. In it, Lathrap (1977a) pays closer attention to the paleobotanical evidence in South America and presents a more precise mechanism that could account for the shift from non-agriculture. His model is largely inspired by Sauer.

Sauer's [1952: 23-ff] basic point is still sound: the massive genetic remodeling of wild plants into effective cultigens could only be understood in terms of slow, ongoing processes and that such slow, ongoing process can best be modeled in the context of the residential stability and environmental modification offered by sedentary fishing villages along the major river systems of the moist tropics (Ms., 1984b: 1-2).

The mechanism for the gradual shift into agricultural dependence is the house garden or the experimental plot adjacent to the house. But for the house garden to develop into an effective agricultural plot in Amazonia would require a certain degree of residential stability --the group could not be constantly or frequently shifting residence following highly mobile animal (protein) food resources. Hence, one has to identify an environmental zone that could promote or encourage residential stability by a hunting and gathering group. Since the tropical rain forest of Amazonia is notoriously poor in terms of the availability and abundance of terrestrial game and equally poor in terms of soil fertility, Lathrap (1970a) identified the floodplains and the immediate waterfront lands as the only ecological zone that
could fulfill such requirements: (a) abundant, year-round aquatic resources (the river itself would be the storage facility) that would stimulate and allow hunters and gatherers to remain settled for a significantly longer period of time than it would otherwise be possible; (b) the presence of fertile soils would permit the house garden to evolve into high yield canuco plots. Moreover, relatively sedentary and stable fishing communities along the banks and mouths of major rivers (along with their canoeing technology) would allow preferential access to a vast communications network that could access a vast array of potentially useful plants to be brought into the house garden from geography and environmentally diverse localities. Access to a vast spatial range is crucial since potentially useful wild plants (as well as animals) in the moist tropics are characterized by being widely scattered and by having few individuals represented per species.

Lathrap (1977a) also notes that wild plants, potential cultigens, ought not necessarily to be initially perceived as food components, but probably as contributions to other aspects of culture: e.g., medicinal, sacred plants crucial to the shaman and the physical survival, safety and psychological well-being of his people (i.e., tobacco for rituals, hallucinogenic plants, poisons for "magic darts," rattle-gourds) or, as he astutely pointed out, plants which serve to intensify their fishing and gathering efficiency: cotton for nets, leguminous fish poisons and bottle gourds for net-floats.

Given the radiation model, following the first six propositions or principles outlined above, such a fishermen population would radiate and spread with canoes along the major waterways of the tropical lowlands, and would carry --more or less-- the same basic, ancient (Tropical Forest) cultural pattern throughout the lowlands --the unitary basis for the development of agriculture. Such early sedentary fishing communities "formed a network through which evolving crops circulated as early as 7,000 B. C." (Lathrap 1987c: 356). In his last published paper, Lathrap (ibid) cited Stothert and Ranere's data as further supporting evidence for his model:

...a generalized, early tropical forest tradition, that developed in the Late Pleistocene among immigrants to South America who became adjusted to the forest as a result of their occupation of Central America. This has been called [by Ranere] the Tropical Forest Archaic... These people fished, hunted and gathered a variety of animals and plants in addition of having a tradition of house gardens in which they cultivated bottle gourd, cotton and other tropical plants... data from Las Vegas [Archaic Ecuadorian] sites support the hypothesis that

2 Lathrap's characterization of aquatic protein in Amazonia is a source of debate: Roosevelt (1980) argues that such bountifulness is not supported by her data and Lathrap begs to differ pointing out evidence to the contrary (both sometimes citing the same references!). Carneiro (n.d.) has also jumped into the fray, and provides extensive ethnobiological evidence to suggest that year-round aquatic fauna is available in such riverine systems and that, hence, the rise of chiefdoms does not require maize. One could raise the issue of just how much aquatic resource variability exists between the Amazonian and Orinocan systems both at a very large and at a very localized scale. For now these remain as competing hypotheses that require testing at a local level.
the cultivation of domesticated plants was a very ancient feature of human adaptation to the tropical forest environment... It is likely that they participated in a network of exchange and communication that was the forerunner of the long-distance trade that linked Mesoamerica, northern South America and Peru as early as the third millennium B.C. (Stothert, in Lathrap 1987c: 356).

I do not know where his "house garden" inspiration came from, but the agricultural potential of the house garden was a subject of discussion among botanists since about 1950. Edgar Anderson (1960: 72-3) noted that in modern Asia, Africa and Latin America "one of the most common patterns is a plot immediately adjacent to a house in which are grown a variety of plants used for a variety of purposes... it will contain trees, shrubs, vines, and annuals. It will be simultaneously an orchard, a vegetable garden, a dump-heap compost-heap, and a medicinal garden," etc. and hints at its potential in understanding the origins of domestication. The shift from the house garden, incipient agriculture, to agriculture is explained in detail by Lathrap in his "Neolithic Revolution" paper (1977a). Another excellent synthesis, however, comes from lectures he gave ca. 1979 at the University of Illinois in his seminar on the evolution of agricultural systems and from a presentation at Southern Illinois University in Carbondale (Lathrap 1986a). The following is a paraphrased synthesis based on the notes I took from these lectures, and only somewhat revised for style and continuity with my comments or clarifications added in brackets (compare with Lathrap 1977a: 730- ff.).

Sedentism [of specialized fishermen] led to an organization of space which is still ubiquitous in native Amazonia. Around the house was a clearly delineated rectangle adjacent to the river front or river bank and which was considered as a fully "cultural" space. This garden was maintained in maximum order through intentional and strenuous human labor. The safety of this space was assured by the diligence with which it was swept and weeded, and contrasted with the perceived dangerous, disordered and --in a word-- non-cultural surrounding forest and probably protected by four huge cosmic harpy eagles perched at the four corners of cultural safe-space and devoured all evil and wild forces attempting to enter it. This is a common scenario for Amazonian Indians and is beautifully described by Goldman and Reichel-Dolmatoff for the Desana [Tukanoan-speaking groups of N. W. Amazon]. No plant, bush or tree existed in this space except by conscious human decision. One example stressing the importance of space-symbolism in native Amazonian cosmology comes from the creation myths of an Eastern Brazilian society [I neglected to record its name] described by Roberto DaMatta: On the first day God divided the wild space, the jungle, from the tame space, the house and house garden; on the second day God created the jaguar to keep women and children within the safe space of the house and house garden --within cultural space.

The sexual politics involved with this view of cosmic space are complex: Women are basically considered to belong to the unordered, sexual, wild side of things and are potentially regarded as a disruptive factor within safe space. Men are
regarded as the gentle, orderly, reasonable element and entrusted with the discipline and vigilance (of women). Real jaguars, snakes, poisonous ants, etc. are dangerous enough, but just in case, these Indians populated the wild forest space with a multitude of supernatural beings most of which are evil and dangerous... In invisible form these creatures seduced women with fatal results and they would also overpower men and drill a hole on their skulls and suck them dry.

Men focused their economic activities (scheduling of time) on the river or lake. Hunting tapir and peccary (etc.) in the deep forest was undertaken only after considerable spiritual preparation. All sorts of taboos, purification and propitiation rites were observed and performed. The time and opportunity for a systematic exploitation of the tropical forest was limited. However, a number of plants were necessary as an adjunct to their fishing economy, thus a steady supply of these had to be secured. It should be remembered that the tropical forest is of enormous ecological complexity and that any square kilometer of forest would contain a large variety of species, yet each one will be represented but by a few, widely separated individuals. In this ecology the overexploitation of any useful species within a convenient range of a community is an ever present danger. As a logical result useful plants were transplanted, curated and concentrated within the confines of safe space—the house garden.

Each plant was brought to the safety of the house garden by a conscious decision and reproduced by human decision and selection on the basis of its immediate need [as medicinal plants or cotton for nets] or in terms of its promise for the future, its potential utility and desirability. Any mutation or change which would distance the —cultural—plant in the safe [controlled, ordered] space of the house garden from its semantic pair (the wild and disorderly plant of the natural [jungle] space) would be selected [Note: here Lathrap makes a long digression on Levi Strauss’ discussions on the raw and the cooked: the wild-natural- forest and the tame-cultural-safe space]. Thus mutations, whose utility would not be immediately evident, would be preserved for later use. The combined effects of intentional artificial selection and mutations with unexpected beneficial results effectively re-enforced the boundary between the natural (forest plants) and the cultural (house garden plants). It is in this context that "wild" manioc and teosinte became ennobled.

But at some point in time what one calls conventionally agriculture is achieved. Can we define such point? We must return to the safe space of the house garden of the Amazonian fishing community... there is a third kind of cultural space that is defined: the conuca [or chacra, agricultural field]. It is a patch of cleared forest so that one can plant a large number of one or several high yield crops. No one would have the temerity to cut down a (dangerous) forest zone to plant teosinte or "wild" manioc, because at this juncture these are not sufficiently productive. They are at a relatively low level of genetic modification in terms of productivity and stand at a high risk for survival in terms of Darwinian selection. Only after several millennia of artificial selection by humans would result in a productive crop such that extensive planting of high yield crops would be economically
advantageous. In this context the third kind of space is generated, the conuco. It is a space that has a more ambiguous symbolism; it partakes of some of the wild sexuality and dangers of the forest and some of the ordered, controlled, safeness of the house garden. It is a fecund space both in terms of productivity and in terms of human sexuality. At this point it could be assumed that the key crops would contribute at least half of the total caloric intake of the community. It is doubtful that one can find this flip point directly (since it is a gradual process not an event).

The stability of such subsistence economy should induce an increased rate of population growth and an outward expansion of the more successful agriculturalists at the expense of their less efficient neighbors... and only when the alluvial floodplain was saturated did such societies migrate and readjusted to other ecological zones.

It is evident from the above that the Neo-Darwinian concept of adaptive radiations, as applied to cultural evolution phenomena, provides the operational architecture and framework upon which Lathrap developed and refined his model of the emergence and evolution of agricultural systems in the tropical lowlands of South America and even Africa (1977a: 723-724). It provides a rationale for invoking population movement as a major "player" in the history and evolution of tropical South America and, on this, the evidence of historical linguistics could not be more persuasive. The set of principles elaborated in 1956 in his seminal "Radiation" paper, with subsequent refinements, have given remarkable consistency and continuity to all of his publications. Such continuity could not occur if the "laws in the background" were non-operational or if Lathrap had viewed cultural evolution as completely under the spell of contingency. In his subsequent writings, (e.g., 1970a, 1977a, 1985c, 1987c) the details of historical contingency become more prominent, as they should, for only with such details could consilience be reached. That we are still far from reaching the level of consilience enjoyed by our colleagues in natural history is evident in the general lack of consensus among scholars working in Amazonia and the Orinoco. I would submit that consilience can only be reached by a return to induction --the strategy followed by Darwin, Gould and others. That is, far more basic archaeological field work data is now needed for all those geographical areas (e.g., Central Amazon) that are crucial in order to test Lathrap's model and its hypotheses.

Concluding remarks

In the first part of this essay I presented a personal view of Lathrap's achievements as a professor and hopefully offered a sense of his persona --an essential ingredient in understanding the man behind the ideas. In the latter part my objective was to highlight an aspect of his intellectual basis --the influence of Neo-Darwinian Synthesis-- that I regard as important in understanding his work as a whole. Lathrap's fascination with the details of contingency in the development of great art styles and the symbolism of the Olmec, Izapa, Chavín and San Agustín iconography or the Huecan-Saladoid (Vieques Island) lapidary art, with the endless
minutia of ceramic styles, decorative designs, forms, etc., or his use of population movements to explain the dated distributions of specific cultural traditions, can only make sense if one returns to his model of cultural adaptive radiations and the principles embodied in it. His approach, as a whole, reminds me of a central topic that guides José M. Cruxent's own research: the theme of "unity and variety" (unidad y variedad) in New World culture history and evolution.

Don Lathrap will be missed as a friend, a professor and as an active, prolific contributor to a better understanding of the New World. There is still much fuel and ammunition in his models and theories to keep us all busy for quite some time to come. I know that he would prefer that such time would pass quickly, for if his model remains unmodified, unchallenged, it almost certainly would mean that very little progress has been accomplished since his departure. This is why, I think, Don respected most those who stood-up and challenged him despite of his outward critical style. It is gratifying to see that Lathrap's ideas, coupled with those of Meggers and Evans, have stimulated and presented new challenges in the more recent lowland/floodplain archaeological projects, such as that of Alberta Zucchi and associates in the Upper Orinoco, Guaviare-Atabapo Rivers or Anna Roosevelt and her Brazilian colleagues in the Lower Amazon and Tapajós Rivers. Needless to say, many of Lathrap's students working in the tropical lowlands will continue to test, modify and/or improve on his models on South American culture history.

Abstract

On May 13, 1990, New World archaeology lost one of its most salient, vocal figures and great generalists in the Boasian tradition. This essay, in honor of Donald W. Lathrap, examines my personal experience of his contributions in the field as a professor, advisor, and friend. It profiles Lathrap's professional and academic achievements and provides a discussion for understanding his intellectual output, with emphasis on his views on culture history, evolution and the emergence of agricultural systems in the New World.

Resumen

El 13 de mayo de 1990 la arqueología del Nuevo Mundo perdió una de sus más destacadas figuras, y uno de sus grandes generalistas de la tradición "Boasiana." Este ensayo en honor a Donald W. Lathrap examina, a través de mi experiencia personal, su impacto en nuestra especialidad como profesor, consejero, y amigo. Este trabajo destaca sus logros profesionales y académicos, proveyendo así un marco de referencia para la mejor comprensión de su producción intelectual. Particularmente se eximirán sus ideas respecto a la historia cultural, evolución cultural y el origen de los sistemas agrícolas del Nuevo Mundo.
Bibliography

Anderson, E.

Arvelo, L.

Bischof, H. and J. Viteri Gamboa

Bryan, A. L.

Carneiro, R. L.

Collier, D. W.

Cruxent, J. M. and I. Rouse

Damp, J. E.

Evans, C.

Flannery, K.V. and J. Marcus (eds.)

* Consult the appended Bibliography of D. W. Lathrap for all references of Lathrap and associates and master/doctoral dissertations chaired by Lathrap.


Rouse, I. and J. M. Cruxent

Sauer, C.O.

Simpson, G. G.

Tarble, K.

Tarble, K. and A. Zucchi

Tax, S.

Vidal, S.

Voguet, F. W.

White, L. A.

Willey, G. R.

Willey, G. R. and J. A. Sabioff

Zucchi, A.

1988  Lingüística, etnografía, arqueología y cambios climáticos: La dispersión del los Arawaco en el noreste amazónico. Paper read at the 46th International Congress of Americanist. Amsterdam.

1989  Prehispanic Connections Between the Orinoco, the Amazon and the Caribbean Area. Paper read at the 13th International Congress of Caribbean Archaeology. Curaçao, Netherland Antilles.

Zucchi A. And K. Tarble


Zucchi A. And K. Tarble and J. E. Vaz


Appendix:

Bibliography of Donald W. Lathrap

Compiled by José R. Oliver

Donald W. Lathrap's published works, articles and book reviews, unpublished manuscripts (some widely distributed by means of xerox or mimeograph) and selected papers read in professional meetings, spanning nearly four decades, are gathered herein. Also, a compilation of all the doctoral dissertations chaired by Lathrap, along with a few selected Master thesis titles, are included since these can be considered as part of his contribution as an educator as well as a researcher. There might be a number of unpublished manuscripts left out, but all of the important published work is contained herein. Clarifications by the compiler are added in brackets.


1967b  Report on the Continuing Program of Research on the Cultural History of the Upper Amazon Basin. Unpublished Ms. on file at the


1970a  The Upper Amazon. London: Thames and Hudson.


1975d  (with R.L. Hoover) Excavations at Shilimaqshutush (Sba-205) Occasional Papers of the San Luis Obispo County Archaeological Society, No.10. San Luis Obispo. [although dated to 1975, was not published until 1979].


1980c (with J. P. Brochado) Amazonia. Unpublished Ms, Department of Anthropology, University of Illinois, Urbana. [A revised version dated to 1982 was widely circulated among specialists in xeroxed form].


1985b (with J. R. Oliver) The Exotic Lapidary Art of la Hueca, Vieques: Possible Trade Network Connections in the Circum-Caribbean. Unpublished Ms. on file at the Ancient Technologies and Archaeological Materials Laboratory, University of Illinois. Urbana. [also cited as "Vieques and Long Distance Trade Networks in the Caribbean," 1986].


Ph.D. Dissertations and Selected M. A. Theses in anthropology
Chaired by Donald W. Lathrap

All M.A. and Ph.D. theses are on file at The Oscar Lewis Library, 607 S. Mathews St., Davenport Hall, Department of Anthropology, University of Illinois at Urbana-Champaign. Urbana, Illinois 61801 U.S.A. All Ph.D. theses are also obtainable from University Microfilms International Ltd., P.O. Box 1764, Ann Arbor, Michigan 48106 U.S.A. This list excludes theses in which Lathrap served as a committee member, and includes only those in which he officially served as Thesis Director, or as Chair of the Defense Committee.

Allen, William L.

Arnold, Dean

Bolian, Charles E.
Braun, Robert A.  

Brochoato, José P. J.  

Chmurny, William  

Erickson, Clark  

Isbell, William  

Isaacson, John  

Lundberg, Emily  

Magalis, Joanne E.  

Marcos, Jorge C.  

Mester, Ann M.  

Myers, Thomas P.  

O'Brien, Patricia J.  

Oliver, José R.  

Pearsall, Deborah


Raymond, Scott J.


Richardson III, James B.


Roe, Peter G.


Spath, Carl


Sullivan, Shaun D.


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