

The Bulletin

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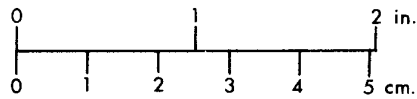
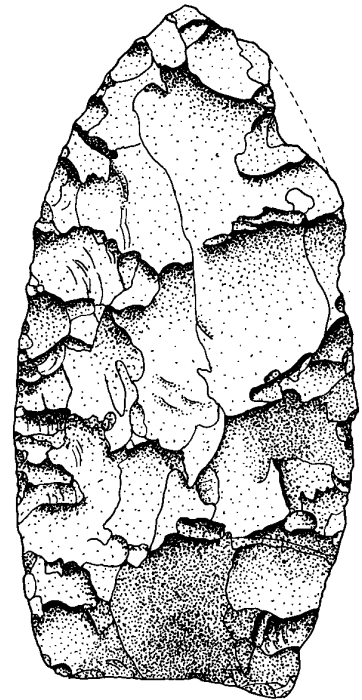
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THE BULLETIN

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KINGS ROAD

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KINGS ROAD SITE: Further Evidence from this Paleo-Indian Component

Paul L. Weinman, NYSAAF
Thomas P. Weinman, NYSAAF

Morgan Chapter

The Kings Road Site (Cox 25), as reported in *The Bulletin*, No. 45 (Funk, Weinman, and Weinman, 1969), lies on a slight uplift of land in Greene Co., N. Y., the exact location of which must remain undisclosed. Since 1968, the field had not been plowed until 1974, and will not be plowed again in the foreseeable future. The initial publication of the Paleo-Indian material was based on intensive surface collecting and archeologically fruitless test trenching. This second report is an addendum to the trait list of the first. It describes material gleaned from the surface after the 1974 plowing. More complete descriptions, conclusions and comparisons with other Paleo-Indian sites can be found in the initial report and in Robert E. Funk's analysis of his work at the nearby West Athens Hill Site (Ritchie and Funk, 1973). Unless noted, all data and descriptions relate to the recent discovery of 183 artifacts. This brings the site total to 630 worked pieces. All are of nearby obtainable Normanskill flint unless otherwise noted. Quotations, except for all numbers, are from the first report of 1969. The up-to-date totals are printed as (site total - . . .). Guided by Funk's categorization (ibid 1969, 1973) and his personal help in the present paper, we arrived at the following tallies:

Clovis Projectile Point

We recovered a single, nearly complete Clovis point but no point preforms (compared to 10 in 1968). This point (cover and Pl. 1, Fig. 2) is a strikingly beautiful, mottled yellow jasper with interlacing veins of quartz crystal. It is large, measuring 95.6 mm long, 48.2 mm in width and 11.7 mm in thickness. Enterline fluting is shown on the surface illustrated on cover left, 47.7 mm long and 29.4 wide. The single flute on the reverse side is 21 mm long and 17.6 mm wide. Light grinding was done along the base, extended 21.5 mm up the left edge and 16.2 mm up the right edge of the cover, left illustration. The dotted line shows a slight fracture in this otherwise striking piece.

Bifaces

Stage A

"In this group are 38 [new site total - 74] whole or fragmentary items. These crude, rather thick objects appear to comprise the first steps in reduction from a core or spall. Most of them are, or were, of roughly ovate shape. Percussion chipping has resulted in broad, deep, unevenly placed flake scars, and these are usually unmodified facets of the original flint core. These objects might be referred to as 'quarry blanks,' roughed out at the quarry and carried to the site for final shaping." (Funk, Weinman and Weinman, 1969).

<u>Stage A Bifaces</u>	<u>Number</u>	<u>Mean</u>	<u>Range</u>
Length	9	90.6 mm	66-127 mm
Width	22	51.	44- 64
Thickness	37	19.3	10- 36

14 of these specimens show intentional retouching, or use-fracture along short spans of at least one side. 8 have some wear, either from scraping or cutting.

Stage B

The 26 [new site total - 40] "Bifaces in this category have been further reduced toward the final product from the cruder Stage A. Flaking is still by percussion, but applied more evenly, with less force. A more definite and symmetrical form is emerging. Thickness is a critical factor in comparisons, because four Stage B bifaces are whole." (Funk, Weinman and Weinman, 1969).

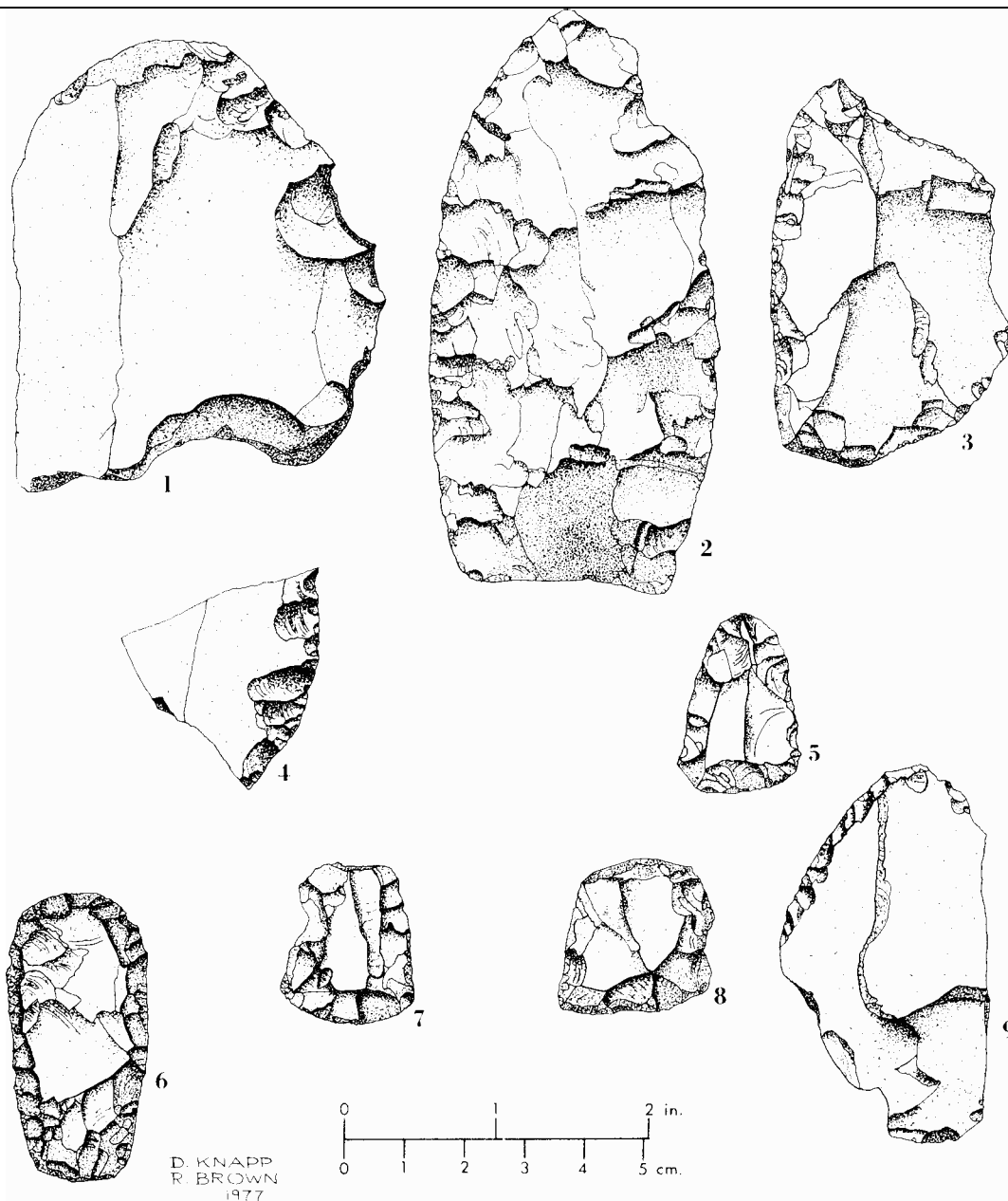


PLATE 1: 1. massive side scraper with spokeshave, of green Normanskill flint, 2. Clovis point, of mottled yellow jasper with interlacing veins of white quartz, 3. sidescraper, of brown jasper, 4. retouched flake knife, of green Normanskill flint, 5. endscraper, of yellow jasper, 6. retouched combination side/endscraper of red jasper, 7. keeled endscraper, of maroon jasper, 8. spurred endscraper, of red jasper, 9. flake sidescraper, of brown jasper.

Stage B Bifaces	Number	Mean	Range
Length	4	67.7 mm	47-106 mm
Width	20	45.5	28- 65
Thickness	23	11	7- 20

7 of these Stage B bifaces have slight to moderate wear evidence, probably having been used as knives. One is fashioned from purple Granville slate, an unusual material for Paleo artifacts.

Stage C Bifaces

"There are only 9 [new sites total - 19] of these 'advanced' bifaces . . . None is whole. These items are rather symmetrical, relatively thin, and have attained a definite shape. " (Funk, Weinman and Weinman, 1969).

Stage C Bifaces	Number	Mean	Range
Length	-	-	-
Width	4	37.7 mm	30- 45 mm
Thickness	6	10	7- 12

Unifaces

End Scrapers

44 [new site total - 101] end scrapers are in the inventory, usually consisting of medium-sized expanding ovate flakes struck from cores and invariably retouched on the dorsal face to a steep working edge at the broad end, opposite the striking platform.

"Of the total number, 34 are of classic triangular or trapezoidal shape. Two are oblong, 2 are oval and 2 are 'squarish' in outline. On 27 examples, the broad end and both sides are retouched; on 3 pieces, the end and one side are retouched; only the end had been modified on 13 scrapers. " (Funk, Weinman and Weinman, 1969).

2 examples, one oval, the other "squarish", seem to have been battered at the base more than would be necessary for manufacture of a scraper. We suggest these had an additional use as strike- a-lights. 10 specimens had graving spurs at one or both end corners.

As noted in the 1969 Kings Road report, the occurrence of exotic Pennsylvania jasper was very high in percentage as related to local Normanskill flint. Together, 29 of the 91 end scrapers are of jasper – close to 30%

End Scrapers	Number	Mean	Range
Length	34	36 mm	20-78 mm
Width	31	27.7	20-45
Thickness	36	9	3-26

Side Scrapers

"This category embraces 54 [new site total - 120] objects, highly variable in form, which possess the unifying attributes of (1) generally large size, and (2) one or more long scraping edges, parallel or oblique to the long axes of the flakes on which they are based. These are produced by steep retouch. Most are formed from secondary or tertiary flakes, but 4 [new site total - 6] are based on retouched cores and one [new site total - 7] on natural vein plates of flint. Of the scrapers based on flakes, 14 [new site total - 36] retain the striking platform. The majority of trimmed flake sidescrapers conform in a general way to Byers' 'ear-shaped' category for the Bull Brook site in Massachusetts (Byers, '54).

"Only 4 [new site total - 5] examples can be termed convergent scrapers, with 2 beveled edges joined at the tip. Of the remaining scrapers, 43 [new site total - 89] were retouched for varying distances along one edge only; 5 [new site total - 20] possess 2 retouched edges; and 1 [new site total - 4] had been beveled on three edges." A single fragment has chipping on two sides - from opposing planes. All of the sidescrapers display wear on one or more edges. Wear is from slight to heavy.

4 [new site total - 12] specimens were fashioned from exotic material - 3 of Pennsylvania jasper and 1 of Ohio chalcedony.

<u>Side Scrapers</u>	<u>Number</u>	<u>Mean</u>	<u>Range</u>
Length	38	57.7 mm	13-117 mm
Width	56	38.9	13- 70
Thickness	53	18	5- 38

Flake Knives

"The 18 [new site total - 59] worked uniface items classified as knives differ from side scrapers in their generally smaller size, and in the relative shallowness and thinness of the retouched working edge. No absolute criteria exist for separating the two artifact types, which really constitute metrical intergrades on the spectrum of flakes retouched along edges parallel or oblique to the long axis. Obviously, a tool primarily used as a sidescraper would occasionally have served as a knife, and vice versa.

"A majority (13) of knives are retouched along one edge only. Three pieces are retouched on 2 edges, and one on 3 edges. Wear is universally present on one or more edges." (Funk, Weinman and Weinman, 1969). Two flake knives are of Pennsylvania jasper.

<u>Flake Knives</u>	<u>Number</u>	<u>Mean</u>	<u>Range</u>
Length	18	55.8 mm	17-88 mm
Width	18	38.9	20-67
Thickness	17	12.8	5-27

Additional tools were: 4 spokeshave scrapers [new site total - 7] fashioned from flint flakes; a denticulate engraver with two spurs worked from a plane of a 34 mm wide flake; a Normanskill grit piece with a single pecked pit on one side, double-pecked pits on the other. (This is not known by the authors to be present at other Paleo-Indian sites and may be intrusive from later people who are well-represented in the area); 4 quartzite chipping tools showing heavily battered edges. These are 84 mm, 96, 85, and 136 wide; 65 mm, 67, 64, and 135 long; and 25 mm, 30, 35, 37 thick. A milky white quartz flake (35 mm long, 22 wide, and 10 thick) appears to have been used as a scraping tool.

Lithic Debris

The 1974 collecting at Kings Road produced 273 blocks, cores, and their fragments [new site total - 431], 3 of Eastern Onondaga flint, one of quartzite, 10, 955 chips and spalls [new site total - 16,486] were salvaged and were grossly divided into 4 categories based on arbitrarily judged sizes. These ranged from very large flakes (306-3 of Eastern Onondaga flint), large (452-3 of Eastern Onondaga flint), medium (1377-4 of Eastern Onondaga flint, 3 of quartzite, 2 of Pennsylvania jasper), and small pressure or fine percussion flakes (3324-72 of Eastern Onondaga, one of quartzite, 21 of Pennsylvania jasper). Undoubtedly, thousands of fine pressure flakes were not seen. Spall and chip size and number was certainly altered by modern cultivation and past fires (approximately one-third of all stone was fire-spalled). Of the 5 hammerstones recovered, 1 was of quartzite the other 4 of Normanskill flint, an unusual occurrence. At present, we have made no concentrated effort to analyze flaking techniques and welcome any scientific inquiry by others,

As stated at the outset, this report is an addition to the previous article (Funk, Weinman and Weinman, 1969) in which analyses, conclusions and comparisons with other sites are set forth. That work, this addendum, and Funk's analysis of the related West Athens Hill site can expand our knowledge of the Paleo-Indian in the Northeast.

Acknowledgements

The illustrations, cover and text were made possible through the generous co-operative efforts and skill of Dale Knapp and Ralph Brown, Morgan Chapter, N. Y. S. A. A., Rochester, N. Y.

The artifacts were illustrated using the photo/ink line/bleach technique. All were photographed individually on black-and-white film and enlargements were made, 2 times the size of

the objects, on smooth photographic paper. Outlines and most features were drawn on the prints using a technical fountain pen and waterproof ink. The prints were then bleached leaving a line drawing. Additional detailing, shading and stippling was done after bleaching. The line drawings were assembled as a photomechanical paste-up to the author's layout. The paste up was then reduced to publication size using normal photomechanical procedures. (Sterud and Bohlin, 1976).

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A GARLAND OF VIEWS AND REVIEWS

When I reported in the November issue of *The Bulletin* that the manuscript cupboard was bare what I said was no less than the truth; the inventory of contributions had been exhausted, in putting that number together. Nevertheless the statement was not quite accurate. Piled high in that space on the shelf where I keep *Bulletin* material were the several publications that have dropped in from the mails over, roughly, the past 12 months, five of immediate importance to New York archaeology in this vintage year for state publications, and four from out-of-state sources. It was their very bulk, I suppose, about six inches in thickness and comprised of, probably, over 500,000 words of reading matter, that finally delivered the message. These books had been submitted for review, not for the lengthening of my library and, because they were all unequivocally devoted to New York archaeology and prehistory, they had to be reviewed. It was my duty and obligation to see that they were reviewed. Logistically it was impossible to get them out to selected reviewers in time for the March issue; further, I have had mostly bad luck with getting reviews done, an experience I will not dwell on here. Shortly before Christmas it came to me in the middle of a sleepless night what I had to do: I had to sit down for a month or six weeks of reading and thinking and writing and that would be the March issue. What is about to follow took, actually, all the time I could steal from other commitments for seven weeks. Even so I was able to do only the New York reviews. The rest will have to wait awhile.

Quite a while, I would judge. The editorial warning in the November issue that we had run out of manuscripts had its hoped-for effect. Some contributions have been received, enough for two issues; many more have been promised. The future seems assured, if all the promises are kept. This crisis seems to have passed. There is literate life in NYSAA.

Some of you may idly wonder why the word "garland" in the title of this review series. Among late Victorian literary men it was a custom to issue books of essays which, by reason of theme or some special manner of treating material, were intertwined, thus a garland. Those who read their way through what follows will see what is meant.

L. A. B.

Recent Contributions to Hudson Valley Prehistory. Robert E. Funk. New York State Museum Memoir 22. Albany. No price given.

By almost every standard of assessment *Recent Contributions* is a major work. It is certainly the longest and weightiest volume ever published on the archaeology of this state. Running to 325 pages, in a format that uses up copy at the rate of about 1000 words per page, it offers a season's reading matter of at least 300,000 words, not counting the plate pages but including the tables which are true summaries and compressions of data that would run to a plethora of text were the data presented by the regrettably growing practice of devoting a paragraph of description and metrics to each artifact recovered by excavation. No, there is no padding and no pretentiousness, in the eye of this practicing editor, in Funk's massive correlation of the archaeological investigation that took place (and was somewhere published or revealed) in the Hudson Valley up to-to guess at a cut-off date-1970.

Twenty two sites, in the excavation of which Funk was the director or guiding hand are compactly detailed; 21 Bear Mt. area sites (most of which were investigated by James Burggraf) heretofore unpublished, are added to the literature; and ten sites from the Lower Hudson are rather more lengthily precisised. Incorporated, of course, in the text, since they are at the roots of Funk's synthesis, are the 7 sites of Ritchie's "*An Introduction to Hudson Valley Prehistory*" wherein there began to emerge that the Hudson Valley was a physiographically determined culture province. And, finally, Funk seems to have examined every collection of any importance still in existence; he has, for instance typed and tabulated over 8000 projectile points. As a survey *Recent Contributions* covers the ground.

And it is along conventional structural lines that the book has been organized:

An introduction where Funk lists his objectives, defines his terms and recapitulates previous work, with some omissions.

A short chapter on the environmental setting-much too short, it seems to me, in that no mention is made of the information in print even in 1970 on the geology and late Pleistocene and Holocene history of the Lower Hudson.

A 182 page section of individual site reports that will constitute for many years to come the standard reference on them.

A concise section on type distribution and subsistence patterns that interprets site data; it introduces no new ideas but adheres to already established doctrine and regional relationships. A relation of stage sequence by local complexes, from Paleo-hunter (about which we are beginning to know something) to Late Woodland (about which, by reason of land development, we are never likely to learn very much) where the Valley's (the middle and upper reaches, at least) prehistory, as Funk sees it, is firmly welded into a cultural framework.

A short chapter of summary and conclusions, in effect an interpretive crisis, ending with the usual closing remark, that much more work needs to be done. But the direction that work should take is not suggested.

What I feel sure Funk had in mind here was to do for the Hudson Valley what his mentor and predecessor in the office of State Archaeologist, Dr. William A. Ritchie, had done for New York State. And I think he has succeeded in doing what Ritchie would have done with the material had he had it to work with 20 years ago. Having compiled exhaustively, described conscientiously, having derived a credible chronology and having fitted cultural and eco-environmental data into a cogent model Funk has delivered himself of an achievement which, he makes clear, is not the final picture of Hudson Valley prehistory but which will be proved to be fundamental and valid when the final picture has taken shape. The achievement is worthy of the ambition.

It is important, I think, to quote Funk on his objectives:

"(1) to expand and, if need be, to modify the tentative framework in Ritchie's preliminary report, filling in the gaps in culture sequence and content, with primary emphasis in the field on stratified and closed sites;

(2) to develop an absolute chronology for the areal sequence, based on radiocarbon dates;

(3) to reconstruct prehistoric Indian cultures of the Hudson Valley in their environmental settings within the limits afforded by available data;

(4) to compare these cultures with others outside the area, establishing differences and similarities and determining possible cross-ties, sources or directions of influence."

The amount of work and the scope Funk has put into the realizing of these objectives is prodigious. (And while I am at it I may as well pay tribute here to the work he has turned out over the past four years -he makes his contemporaries look like Andy Capp scrimpshanks and layabouts -that includes, beside the magnum opus *Recent Contributions*, the editing, with Charles Hayes III, of the Ritchie festschrift *Current Perspectives in Northeastern Archaeology*, to which he contributed, the collaboration with Ritchie on *Aboriginal Settlement Patterns in the Northeast*, a piece with Bruce Rippeteau in *Amerinds and Their Paleo environments in Northeastern North America*, several pieces, with Rippeteau, on the Upper Susquehanna, to which he has transferred his attention, in the *Pennsylvania Archaeologist* and *Man in the Northeast*, a piece in *Archaeology and Geochronology in the Susquehanna and Schoharie Regions*, and I don't know what all else). Yet, for all the labor, with a truly awesome mass of detail, it seems to me that Funk has realized his objectives only in the narrowest and most denotative sense. A great deal of information has been amassed which can be construed as falling within these objectives but the result is encyclopedic, not cumulative. What should have come of it, or at least what I think should have come of it, is a vision of the grand design of Northeastern prehistory, of the broad current of movement of peoples northward as the north opened up just as our white pioneers went westering as the west opened up, in a pattern of responses to new lands opportunity that was guided and modified by social imperatives, by what we may call kinship anthropology, by trade and by near-neighbor relationships. The new generation of young archaeologists being turned out by Charles McNett and William Gardener in the Washington area (American and Catholic Universities) have been exploring the function of trading in the movement and cultural interchanges within the region. While I disagree with their view of trade as more or less commercial exchange of goods, since our eastern aborigines seem to have had very little sense of market economics, I agree that what is a culture region is defined by the network of group relationships, sympathetic and antipathetic, that certainly included exchanges of goods. Those who are focusing their studies on trade are very probably following a warm trail, and they are starting in the right place; the exporting centers of the traits by which we discern cultures appear to be, in the main, south of New York. This was so during the Archaic, probably through the Paleo-hunter horizon and very visibly during the so-called Transitional and Early and Middle Woodland.

What happened to Funk's perception of the flow of prehistory in New York is, I believe, his angle of vision. That angle is clearly stated in his first objective "to expand and, if need be, modify the tentative framework in Ritchie's preliminary report-".

Here Funk is positing as an objective what is really his premise, a logical no-no. He remarks somewhere in the book that Ritchie adopted for his survey of Hudson Valley prehistory *An Introduction to Hudson Valley Prehistory* the culture sequence and chronological model he had previously developed for upstate New York because it was the only applicable model in existence at the time. What Funk's first task, as an investigator, was then to test the model, since it was tentative, not to expand it as an article of faith. And there was good reason to test it. In the Ritchie survey of sites the southernmost was the Bannerman site, approximately at the Dutchess-Putnam County line, about 80 miles upriver from the Narrows. That Ritchie had not examined a site within that 80 miles, or referenced any site report from there left 40 per cent at least of the length of the Valley without any archaeology. But this is the reach of the river within which it changes character from freshwater stream to estuary, an estuary which divides into two great bodies of water, Haverstraw Bay and the Tappan Zee at the junction of which is the mouth of the Croton River, the Hudson's largest tributary from the east, and at the end of which is a mouth that opens wide enough to give access upriver to traffic from the coast and from the hinterland to the south as far as Maryland and to the west to the Delaware. Ritchie had no data for this most important length of the river and the Valley. Funk had, and he dutifully cited them, but only to select from them such saliences as would enable him to annex the Lower Hudson to a Finger Lakes sequence and chronology, this region that is as close to Delaware as it is to Lamoka Lake or Frontenac, and has more in common with Delaware environmentally.

It is understandable that Ritchie titled his *An Introduction to Hudson Valley Prehistory* at that time, though it might better have been titled *An Introduction to Upper and Middle Hudson Valley Prehistory*. But Funk should not have, in 1970 if I have the cut-off date right, continued to look at the Valley wrong-end-to. In Ritchie's time there was still a strong feeling that many

Archaic cultural traditions had originated in Asia and had migrated into upper New York from the north; thus they were earlier there than in the southern part of the Valley and were parent to their southern aspects. By 1970 such a view was no longer persuasive. The work had been done which demonstrated a northward spread of the Archaic from the Carolinian biotic province with the advance to the north of that province in the wake of retreating glacial and pro-glacial conditions. There can be no doubt that cultural movement into the upper Northeast came from the west along the Great Lakes, so the very least that can be said is that there is somewhere in the length of the Hudson an interface of western and southern aspect cultures, with a detectable mixing zone. The Bannerman site probably lies within this mixing zone, if not at the interface, and so Ritchie's use of his upper New York sequence and chronology was appropriate. But the farther south you come from that interface the less comfortable the fit.

The fact is that there is an in-built unfairness to an author to review a book that was written, certainly in parts, a decade before the reviewing process. But there you are. There is the book to be reviewed, and there is the reviewer who cannot ignore the developments of a decade. Once innocence is lost it is lost for all time. But then I doubt that were Funk to write the same book today that he would change his views by 180 degrees.

As to Objective No. 2, I have always been uncomfortable with the use of the phrase "absolute dating", even though I am fully aware that it is used to distinguish C-14 and other calculated dates from relative ages obtained by stratification or seriation. But absolute means, when you look it up, complete or perfect or exact without exception. To question Funk's objective of obtaining a chronology of "absolute dates" would be a mere quibble were that my sole reason for raising the issue. But what I think he has in mind is a column of dates that has the factuality of, say, those lists of the presidents we used to have to memorize. The fact is that C-14 does not give absolute dates; it gives order of age, which is relative age within stated calendar time limits. What with the standard deviation, the statistical probability of the true age falling within the deviation, the use of the Libby or the Bureau of Standards half-life for C-14 and the dendrochronological calibration of laboratory results by either the MASCA or UA tables there is enough tolerance to lose whole complexes. Now that archaeology has become engrossed with culture change, its rate, direction, causes etc. absolute dating, down to a matter of half centuries has become imperative; by definition change must be measured on a time scale, the finer the better. On the gross scale of the C-14 method, using the rather cumbersome kinds of evidence available to detect change we would be hard put to identify a culture that had not changed a whit in 200 years or one that had changed, by reason of a sensitivity to change and a sufficient cause for change, unrecognizably within a generation; we cannot identify, really, the transients from the locals, or a partial intrusion from a full displacement.

It is most unlikely that we in the East will ever have at our disposal an absolute chronology that will provide us with the timing for culture change and enable us to distinguish it from culture shifts of locale. But I am very impressed with the kind of chronological net Funk has woven for the Hudson Valley and adjacent areas. Here is something that will not unravel, that will stay solidly in place and underpin all work for the next 20 years. The dates are mainly corroborative but in some cases they serve to clarify and refine and they do support the chronological succession Funk has adopted a priori.

The third objective has been stated with some care: "to reconstruct prehistoric Indian cultures of the Hudson Valley in their environmental settings, within the limits of available data". But here is implied again that Funk is speaking for the whole length of the valley as one environment. By his figures the Valley is 200 miles long. For at least 50 miles of that length the river is saline, the habitat of marine shellfish, the spawning ground of anadromous fish, quite palpable environmental factors. Which is to say that one-fourth of the Hudson's length is so different from the other three-fourths as to render the habits of its residents specifically different, or our firmly held rules about environmental adaptation are of no consequence.

The reconstruction of the prehistoric Indian cultures of the Hudson Valley that Funk describes is that for the Valley above the salt line, and I do not find that Funk has considered the possibility that they could or would be different from those at, say, Croton Point. I find it rather telling that Funk has not used the term Lower Hudson, certainly not in the sense that environmentally it is a region all its own, but reports sites from the Lower Hudson as from the "Bear Mt. region" and from the "Croton-Ossining region". These, of course, are not regions; they are vicinities within an environmental region which is very sharply defined.

There is little, if anything, that is new in what is said about the lifeways and culture successions of the Middle and Upper Valley. Which is in no way a criticism. Funk knows that region far better than I do; and the plain fact is that the sites he has had to deal with yield sparse data and the conclusions that can be drawn from them are general. Within the restrictions he has placed on himself of using "available data" he has provided a firm and credible, if conservative picture. There could be no quarrel with it were it not for the fact that he has, by virtue of his title and his intention, included the Lower Hudson in it. To repeat: from circa Poughkeepsie northward the Hudson is an inland stream; from at least Bear Mt. in is an arm of the sea. Funk has made use of the data from the Lower Hudson as though they had no specific relation to the estuary and might just as well have been located in Greene County.

But can it be shown, not simply rhetorically declared, that there is a real and significant difference between, the anthropology-archaeology of, for instance, Croton Point and Ulster or Dutchess or Columbia Counties? Because if there can't be then the whole basis of the criticism is no more than forensic, a debater's point, an argumentative ploy.

Let us begin with the fact that the banks of the Lower Hudson are lined with middens of shell, from the latitude of Yonkers northward composed 99 per cent of oyster. I have reported excavation of these middens from Tarrytown to Scarborough, to Crawbuckie Beach and Piping Rock in Ossining, and to Croton Point, to Parham Ridge, to Oscawana Island to Georges Island to Dogan Point to the Wollcott site in the Town of Cortlandt and at Twombly Landing in New Jersey. I could add a dozen other loci where shell middens still can be seen and could be investigated. It can be alleged that a proved four millennia of the exploitation of oyster beds, and a probable six millennia of subsistence use of oysters, shows nothing more than that oysters were a locally available food resource, easily obtained, and so were merely another component of the diet where they could be had. But I have several times described a model of lifeway in which the oyster was a prominent factor in the pattern of movement and seasonal round. It is that the inhabitants of the area, *and possibly inhabitants from outside the area* of oyster bed occurrence had resort to the riverbank in the late winter-early spring because, in this time of slim pickings, oysters were a reliable harvestable item while campers awaited the spring runs of anadromous fish.

Because they literally dissolve, under weathering, into the soil like sugar in coffee, shells make a difficult archaeological context. But almost three decades of work with them have given me a feeling for them you can't get from even a season's excavation. The Lower Hudson middens are composed of heaps each of which represents a camping period; they are not horizontally stratified like soil, but will rise from the level of deposit to two or three feet above that level, very often, in fact almost always, the top half or more has been weathered away and the remainder is filled with soil, forming a new surface. This surface becomes, in time, ground level on which later camps are settled. Thus the occupation evidence found on or in middens does not belong in time to the period of camping represented by the heap. Campers did not camp on their own heaps.

Despite all the confusion of context major midden sites present a surprisingly uniform pattern of occupation evidence from Early Archaic to Middle Woodland as determined by easily recognized diagnostic artifact differences. The shell heaps are there, but the shell is not in such quantity as to indicate sole dependence on oysters. In the midden are scattered numerous scraps of bone. It has long been the custom to interpret these scraps as fragments from the hacking apart of bone for the marrow. But that explanation does not fit the bone found in our Lower Hudson middens. In the first place a moment's reflection will show that bones do not have to be cut down into small pieces to extract marrow; the removal of one end will suffice for that operation. The bone fragments we have found and are now having analyzed are *cut* fragments, from about one to three inches long and as wide as, perhaps, one third to one-fourth the diameter of the original bone. They have been cut, the cut marks being clearly visible, from long bones, which are rich in collagen; bones where corpuscles are manufactured are not rich in collagen. The Lower Hudson riverbank midden sites seldom produce bones from any other part of the skeleton, and the long bones which were reduced to fragments are the lower leg bones, as the joints found with them attest. These fragments, then, are not the offal from butchering; they are food bones, bones from which food is obtained. That food is collagen. An analysis done by a General Foods Corp. laboratory which specializes in collagen extraction

revealed that the collagen in the archaeological bone was only 20 per cent of that in fresh bone (fresh bone is 25 per cent collagen, the archaeological bone was 5 per cent) regardless of the age of the archaeological bone.

So what we have is a recipe for stew, if you like, consisting of protein rich oyster meat and protein rich collagen. Roberta Wingerson, of our Museum and Laboratory for Archaeology has devised a menu consisting of this "stock" with carbohydrates derived from various roots and rhizomes such as cattail and skunk cabbage, available at riverbank loci that is as salubrious as any nutritionist could ask for. The manos and pounding stones for the pulping of roots and rhizomes are plentiful among the artifact recoveries from shell midden sites.

It seems probable that this "spring stew" was considerably enriched when the shad and other anadromous and resident fish began to activate with the warming of the river to 40 deg. F, which occurs rather quickly, once the ice begins to melt, as tides flush out the colder water of the winter. That the riverbank sites were occupied for this late winter- early spring interim seems further corroborated by the absence of fishing gear. It would be inconceivable that the abundance of fish during spring runs would be ignored by hunter-gatherers. But what did those who lived along the Lower Hudson use to take what they needed of this abundance? To show for almost three decades of riverbank-midden site excavation we have no more than a dozen net sinkers, found not in clusters but one at a time, as though they had been weights for throw-lines. And that is all; no harpoons, no leisters, no fishhooks; only the hunting style projectile points of stone.

Perhaps there are other, ingenious, explanations for the absence of specialized fishing gear, but the Occam's Razor principle tells us that specialized fishing gear is absent from where we would expect to find specialized fishing gear because it wasn't needed. All the fish that could be eaten could be taken by herding the teeming runs into simple tidal weirs (the tide off Croton Point rises about 3.8 ft.) or by spearing with hunting style points or by clubbing. If this is the true explanation, then the absence of specialized fishing gear implies that fishing was done only when the fish hordes were running, i. e. during the early spring. (Shad will enter the estuary in the spring as soon as water temperature reaches 40 deg. F.)

Evidence related to subsistence activities falls clearly into a pattern of resort to riverbank midden sites for the purpose of tiding the campers over a lean period during which there was very little food to be had elsewhere. Not mentioned above is the presence of bolas stones and, almost certainly sling missile stones, for the taking of birds during their northern migration. But signs of what might be considered normal activity at a campsite are rare. Heavy industrial tools usually occur as discarded fragments, often converted to such uses as whetstones. And the stone debitage is negligible. In one unusual instance of a chipping station at the Piping Rock site at the mouth of the Croton River we collected five pounds of chippage; it was probably no more than a morning's work. But at the South Hillside Midden locus we picked up only about seven pounds, for a habitation area that had been camped on during a period of over 2500 years. By actual count we had about 2900 chips and other pieces of debitage for the C-14 dated span of 2400 B. P. to 5000 B.P., or about 1 chip per year. This is not to say that the locus was camped at every year by any means, but the ratio is suggestive. Moreover, as I have shown in a piece in the Bulletin on the occurrence of utilized flakes, about 30 per cent of the chippage can be shown to have had edge use, with perhaps another 5 to 10 per cent having been used without showing signs of attrition. Much of this chippage may well have been carried to the site as a supply of pick-up tools, like old razor blades. Heavy work was not done during these stop-overs by the river. The pick-up tool chips had probably been saved from other loci of chipping and only a little touch-up work was done, or a needed tool was roughed out. The bone for the preparation of boiler fragments had undoubtedly been saved from the winter's hunting, dried and then hacked up-the sharpness of the cuts show that the bone was dry before cutting-and brought to the site. It was a time of low vitality.

The richness of the subsistence resources of the Lower Hudson during a period when the rest of the land had little to offer suggests that it was not only the residents of the immediate vicinity who flocked there in March-April. There are several streams that enter the Hudson from the east (only one from the west along the length of the Palisades), Annsville Creek, the Croton River, the Pocantico, the Nepperan or Saw Mill and others. Of these the Croton River is by far the largest, with the most extensive drainage area, into which drain the Muscoot, Cross and Byram Rivers. As a highway the Croton could bring scattered bands to the Hudson from as

far away as Connecticut, perhaps a day or a day and a half journey. One would think that it would be worth it to travel that long to get to the oyster beds and anadromous fish runs of the estuary. But if it was no more of an effort for the bands of the Croton watershed than that, then it was no more of an effort for the bands upriver by a matter of 50 or 75 miles.

Projectile points, the diagnostic evidence usually used to identify complexes, do not come, unfortunately, with tags by which their point of origin can be determined. So it is impossible to say which of the groups were from the neighborhood and which were sojourners in a kind of riverine transhumance. Of course such a practice might be dismissed out of hand as not being within "available data". But that only raises the question "what are available data?"

The phrase has all the connotations of intellectual rigor, of clear-cut distinction between fact and fancy. Actually data are, first of all, what we consider data and, second, what we consider data to mean. As archaeological investigators we collect data to be used as evidence. Why else? But the value and effectiveness of evidence is in the inferences that are derived from it. Sherlock Holmes is the exemplar for all archaeologists, the collector of "facts" or information ("we must have more facts, -Watson") who turned them into evidence by divining their significance in a chain of reasoning toward an exact conclusion. Holmes could do that because he was following a trail that led to a single conclusion. But archaeologists cannot stop with single conclusions because they are seeking the truth about not individual but group behavior; one conclusion reached is only the premise for the next stage of investigation. Holmes could reject this fact or that because it was irrelevant, that is, not evidence. To the archaeologist no fact is irrelevant, not evidentiary; if he ignores a fact or assemblage of facts, if he merely collects data, then he admits he does not know what to do with them.

It is a fact that the Lower Hudson, by reason of being a salt-water estuary, supports a unique (in this region) ecology. It is a fact that this ecology includes some very valuable subsistence resources which, moreover, are available at a time of year when food is scarce elsewhere. It is a fact that the water of the Upper Hudson flows unimpeded into the Lower Hudson, making it an easy travelway to the seasonal abundance of the Lower Hudson. It is inference that upriver bands descended the river during times of scarcity (which would occur in other seasons than late winter) to partake of the Lower Hudson's bounty. But the alternatives to this inference is that these mobile hunter-gatherers were willing to move longitudinally but not latitudinally, were unaware of the resources of the Lower Hudson, could not stomach oysters and shad or were unwilling to associate with the Lower Hudson people. The inference is plausible, though it needs to be tested as a proposition, against which it would be difficult to present contradictory evidence.

This foregoing homily has been interjected to bring into perspective the applicability of the qualification or parameter "available data". The facts about the Lower Hudson stated above were known in 1970. Their relevance to the archaeology, prehistory and prehistoric anthropology of the Lower Hudson could have been discounted only on the assumption that the Lower Hudson had little or no relevance to the prehistory of the entire Valley, within which it is an environment *sui generis* and of which it constitutes a quarter of the length.

All the objections stated above, which amount to one objection, the exclusion of the Lower Hudson from the prehistory of the Valley (except for the recognition that there are sites along the Lower Hudson) apply to the pursuit of Objective 4. But there are further ramifications. The Upper Hudson cultures, as Funk sees them, have been compared "with others outside the area, establishing differences and similarities" with the purpose of suggesting (I would say, rather than Funk's "determining") "possible cross-ties, sources or directions in influence". But this has been done as though the Lower Hudson did not exist, certainly not as the inescapable conduit of cultural influences and culture migration or spread. That the mouth of the Hudson opens like a funnel for the intake of everything moving north from both the coastal and inland south has been ignored. Yet how else could Upper Hudson complexes identified as having diagnostic similarities to complexes from outside the area, mainly to the south, have got there without passing through the Lower Hudson with an environmental ambiance so strong that it could hardly not have influenced them?

Let me cite an example of the strange avoidance of the Lower Hudson in any consideration of the Valley's prehistory:

On page 257 Funk says "The hypothesis that the River phase is intrusive in eastern New York requires a source outside that area. On present evidence no related complexes existed to the south, east or north".

They didn't? Funk turns to the Farrell Farm site in the Genesee Valley of western New York for something that "may have a bearing on the problem. At the Farrell site "The lithic assemblage comprises Sylvan Stemmed points (including a few Lamokas) Normanskill points, small, relatively crude, broad side-notched points (Sylvan Side-Notched?) drills, four beveled adzes used as hammerstones, choppers, anvilstones and a series of bone implements identical with Lamoka forms".

With the exception of the bone tools and the Sylvan nomenclature for points, this hodge-podge of material can be found on Lower Hudson sites where the narrow-bladed, stemmed point tradition that we call Taconic is overwhelmingly strong. But you do not have to believe, as I do, that Normanskill points are a speciation from the Taconic tradition, to wonder why the Lower Hudson was overlooked; in fact not even mentioned as having relevance. Did nobody live along the Lower Hudson during River phase times? That would be interesting. Or if, instead, somebody else lived here that would be equally interesting.

That I am so acutely aware of Funk's cavalier treatment of the Lower Hudson (though, as I will later confess, I am probably partly responsible for it) will surprise no reader of the *Bulletin* for the past quarter century, or decade, or pentad, since I have written a great deal about the region for its pages. But perhaps not enough, or not in the right terms, for I have directed, and scraped my finger nails in, as many sites here as Funk as reported for the Upper Hudson and, though such comparisons are undoubtedly odious, have recovered as much material by excavation and obtained as many C-14 dates as have come from the upriver region. But of the many reports I have made in the *Bulletin*, the *Pennsylvania Archaeologist*, *Archaeology of Eastern North America* and elsewhere only one on Twombly Landing has been devoted to material culture, tabulating and listing what was found. That makes it very difficult for any author on the Hudson Valley to assess what is in the Lower Hudson.

The fact of the matter is that we of the Briarcliff College group, who have been the only investigators of sites south of Bear Mt. , have been, from the beginning, problem oriented. We had to be. The most salient feature of Lower Hudson riverbank sites (which are innumerable) was the shell, 99 per cent oyster, that marked them boldly to the eye. Since no oysters, or other marine shellfish species now live in the Lower Hudson and, from all we could discover, had not been common during historic times, it was obvious that the great fields of shell had been laid down during a period of flourishing that had come and gone in prehistoric times. But we could get no information at all about Lower Hudson oysters that made any sense. One archaeologist told us that pollution had driven oysters from the Hudson; we believed him until we discovered that oysters are growing now in the equally polluted East River. When we submitted valves from a Croton Point Midden that we estimated as prehistoric to the American Museum of Natural History we were told that they were modern, not over two or three centuries old. We were also told that though we might think we were taking Archaic material from our middens we were just doing bad archaeology; shellfish were not eaten in the Northeast until the ceramic period, as though they weren't palatable until cooked in a clay pot.

It was one canard after another but we could not make a beginning in Lower Hudson archaeology until we had exposed them and got to the geological-environmental facts about oysters which held the key clue to why the Lower Hudson was a region apart. A C-14 date of 5850 years ago on a horizon of very large shell (called the GO horizon) disposed of the American Museum's assessment and the no-oyster-eating-before-ceramic-pots-to-cook-them-in, and it opened up a whole new bag of whys and whens and hows. As the title of a recent paper of mine ran "The Midden is the Message". So what is the message? We have spent 15 years decoding it, with the help of Dr. Walter Newman of the Queens College Geology Department.

We know that the Lower Hudson region, the southern tip of New York and adjacent New Jersey were covered by a system of very large lakes until about 13,000 years ago.

We know that this lake system emptied before 12,500 C-14 years ago, shortly after which the Lower Hudson became estuarial. We know, by C-14 dating, that oysters were growing in the Lower Hudson north of Croton Point at 7000 years ago; by reasonable inference this age can be extended backward for at least a millennium.

We know that from the moment the Lower Hudson became estuarial and would admit anadromous fish, and was saline enough to become oyster habitat, that it was a unique environment for human occupation and was the first segment of the Valley so occupied, by the Archaic

population advancing northward with the advance of the meridional biotic province of deciduous trees, Virginia deer etc.

We know that oysters grew in the Lower Hudson through the span of the Archaic, from about 9000 C-14 years ago to 3000 C-14 years ago, while the sea was rising during the post-glacial melt period. As long as the sea continued to rise the Lower Hudson was deep enough to admit more seawater than it does now, thus maintaining a salinity level sufficient for oyster existence.

We know that when sea level was stationary for, perhaps, a century or so, silting built up, the estuary became too shallow to admit enough sea water to maintain oyster habitat salinity. We also know that the silting factor was complicated by post-glacial rebound and by the retirement upriver of a suspected fore-bulge in the sub-crust.

We know that the continuing rise in sea level pushed peoples living along the estuary inward and up the Valley, decade by decade, injecting a coastally habituated culture farther and farther inland. This culture is characterized by the narrow-bladed stem point and its variations that we call the Taconic tradition but which others have broken down into several localized types.

We know that sea level attained approximately its present height between 3000 and 2500 years ago. Thereafter oysters established themselves only intermittently and for short periods, never again becoming the dietary factor they had been during the Archaic. The average salinity was probably sufficient for oyster growth but periodic spates of freshwater, from heavy rains or rains and melting snow, reduced the salinity to near zero for several changes of tide.

We suspect that the hole in the diet left by the absence of oysters was filled by an increased emphasis on the taking of migratory birds. The bolas stones we have recovered seem to belong to the end of the Archaic as, perhaps, do the missile stones for slings that we believe are present, though it will probably be forever impossible to prove these are missile stones. We know that not only did oysters, migratory birds and anadromous fish make the Lower Hudson a rich environment early on; the advancing meridional biotic province fingered up the Valley sooner than it spread over the uplands, because the Valley shores are at sea level.

We know enough, in short, about the environmental history of the Lower Hudson to realize that it afforded just that mixture of resources to draw both coastal and inland peoples into this region.

It would seem, from the foregoing, that we may have, while stressing our environmental pursuits, neglected the material culture of the Lower Hudson. This would be a misapprehension. The reason for our pursuit of the facts of environmental was to understand the material culture. We have recovered all the kinds of projectile points, considered as cultural diagnostics, to be found in the Upper Hudson, though in different ratios and circumstances, and a great deal more; we could now describe perhaps a dozen projectile point types not mentioned in the literature of the Northeast at present. Similarly the ceramics; our Lower Hudson ceramics follow the familiar sequence, but there are differences of some importance. Vinette I is scarce, but there occurs, beyond the shadow of a doubt, after analysis, an untempered ware (we call it mud ware) which does not show typical fiber-tempered structure internally, is certainly not shell-tempered (strangely, there is not a single sherd of shell-tempered pottery in our entire collection) and is quite porous or "cell-tempered" as Ted Guthe once jocularly called it. The more we dig the more strongly individual becomes the character of Lower Hudson archaeology.

Which is why I have never tried to write a prehistory of the Lower Hudson. We are years away from it, still in the data-gathering, proposition-making stage.

Back in 1950, fresh from Ohio, ignorant of the finer points of New York archaeology as it then existed in the literature, I tried the Ritchie central New York sequences and culture patterns on the materials from two sites in the Croton area, the Van Cortlandt and the Winterich site, both very mixed. I well remember showing some stemmed points from these two sites to Ritchie the first time I met him, at a dinner where he spoke on Lamoka which had just been dated. I was sure my points were Lamokas. Ritchie examined them, queried me about the other materials from the site and pronounced his judgment.

"But you don't have Lamoka down there. Read my Lamoka reports".

I read them. No, we did not have Lamoka down here. Nor did we have Laurentian. And why should we have had? I tried to interpret one of our Crawbuckie sites as a Frontenac Island acculturation situation, only to have to conclude that it was accumulation, not acculturation. Then, as Holmes used to say, when all explanations that are impossible have been eliminated,

what remains, however improbable, must be the answer. I turned from looking to the north for origins to the south. On the work done up to that time in New Jersey and down the line, the south did not look promising. But in 1963 Joffre Coe's *"Formative Cultures of the Carolina Piedmont"* came off the press and the south took on a brighter look. Data on the late Pleistocene and early Holocene climate and sea level movement became available, and the stimulus for south to north spread of Archaic peoples was no longer a mystery. The notched blade makers followed the northward trend of their forest-river environment; the makers of the very different stemmed points ranged farther up the river valleys as the sea advanced. These were the two great unconscious migrations and the generations involved in them could not have got into the Upper Hudson without having passed through the Lower Hudson. And so it was with the Delaware and the Susquehanna.

When it was established that this is how it was, projectile points of Carolina piedmont styles, and then of St. Albans, West Virginia, styles began to be found in this region. Now, unless we prefer to believe that the Early, Middle and Late Archaic immigrants into this region all died out in turn, we must believe that they left descendants who became the resident population whose artifact inventories must have evolved from ancestral traditions. Thus the cultures of the Lower Hudson are transplanted southern cultures which rooted themselves here very quickly.

But the story is not quite as simple as that. In the first place there must have been admixtures of both the pioneers and of their descendants. And in the second, the influxes from the south never stopped. They continued up through Middle Woodland, with the Fox Creek-Cony-Selby Bay aspect. But here the stimulus is not obvious. The appearance here of the Broadspear complexes, of the fishtails and of Fox Creek-Cony-Selby Bay is certainly not merely a matter of cultural diffusion; they represent population incursions. But there is no reason to believe that they displaced the resident cultures, or entered a region emptied of population. Not until the Late Woodland does the population of this region appear to have reached a state of relative stability. And that may be only an illusion.

Almost three decades of excavation in the Lower Hudson have produced the evidence for the scenario of movement, shift and change as outlined above; it is a scenario that, by its very nature and the nature of the participants, could not have been played out in the Lower Hudson, without involvement of the upper reaches of the Valley. The impression left by *"Recent Contributions"* is that the prehistory of the Valley is a simple stack of cultures, one on top of the other in a stratified sequence. Yes, the intra- and inter-regional breadth of these cultures has been explicated by Funk. But this is not the same thing as setting up models of where the cultural saliencies originated and how and why they spread. History, and prehistory, does not unfold according to a preordained design (as far as we can discern) but looked at retrospectively it has design, a development along lines of progression that can be traced out, when the evidence can be found, like a stream system.

In the first sentence of this review and disquisition I said that *"Recent Contributions"* is a major work in almost all respects. Where it falls short is in the failure to suggest a grand design for what took place over 10,000 years of Valley prehistory; there had to be, in retrospect, such a design because history (what happened to the people who lived in past time) makes a design of beginnings and endings of starts and directional change, toward success (survival) or failure (extinction).

We have taken our share of the blame for not having supplied the data we had been (and still are) recovering from the Lower Hudson, though we do not think our tardiness culpable; every time we think we know what we have, something new turns up to change our minds. But even a decade ago the data was falling into a design of supra-regional change. The author of a work as major in bulk and as thorough-going in analysis, as comprehensive in data presentation and ambitious in temporal and geographic scope, even though he puts it forth under the modest title of *"Recent Contributions"*, has incurred an obligation to reveal the design he perceives in it. Data are evidence, or they are nothing; in the understanding of a site they constitute the vocabulary and the syntax. Sites then become paragraphs in the body of a text of larger meaning, and they inevitably build into an argument which, in turn, converts into a statement of design or, to use the current term, into a model. Surmising more than you can prove is much less a fault than obliviousness of what you have. It seems to me that Funk has sold his material short.

This has been so long a review, and disquisition, because both *"Recent Contributions"* and its author deserve it. Funk is a first class archaeologist in the classic mold, a primary data-gatherer, an investigator at the scene of the action, a holder of the tenet that the human eye, backed up by the human mind, is still the best scientific instrument in the practice of archaeology who distrusts (I feel sure) the servility of computers and, as a writer, a sober and honest and unambiguous stylist. He had thoroughly prepared himself to do *"Recent Contributions"* and he did it thoroughly.

As has been frankly stated throughout this review I differ with Funk diametrically in his interpretation of Hudson Valley prehistory, and I have given my reasons; this disagreement extends to his pet construct, the Sylvan Lake complex, which will not be discussed here since the reasons for my suspicion of it should be clear from the general trend of my disagreement. But this does not mean that I do not hold to a sincere admiration for the work. It is in a fine, even noble, tradition of archaeological effort and it is profitable reading for anybody who approaches it with the alternative I hope I have provided here. What the reader accepts, then, is up to him. One thing is certain; *"Recent Contribution "* will not have a rival in the literature for a long time, if ever.

My feeling about the book is that it is a young man's work. Funk was getting his feet on the ground, gaining command of the terrain and the data, setting up his premises. There is even a more than slight, suggestion of youthful cocksureness: the way he sees it is the only way it could have been. But there is the promise that this will be neither his last big job, or his best one.

Adaptation, Continuity and Change in the Upper Susquehanna Valley, Robert E. Funk and Bruce Rippeteau. Occasional Publications in Northeastern Archaeology. Georges Mills, New Hampshire. \$9.

Adaptation, Continuity and Change is no more than one-fifth of the bulk of *"Recent Contributions"* but it is at least twice the book. One would hardly think that the same author had been at work in the two. If Rippeteau had a strong influence on the senior author, either in the contribution of new approaches and insights or as sparring partner in the development of Funk's capacity to deal with archaeology as investigative anthropology, then this is a significant collaboration. The kind of problems Funk and Rippeteau set for themselves are a quantum leap in sophistication beyond the "objectives" of *Recent Contributions*. It is as though Funk and Rippeteau have taken advantage of the intellectual liberation of the "new" archaeology to unleash their own powers without, at the same time, falling into the "new" archaeology's weakness, for cant and self-delusion and the habit of mistaking a computer solution to a problem for an anthropological solution. *Adaptation, Continuity and Change* is a textbook on how to do archaeology in the Northeast in this day and age.

The authors have written perhaps the best summary and assessment of their work in the second paragraph of "Conclusions". It is here quoted:

"In our upper Susquehanna program, implementation of the expressed theoretical assumptions in a consistent research design is still, we feel, imperfect and in its early stages. The model of regional prehistoric culture change and ecology which has been presented must be considered very preliminary, general, tentative and heuristic. Yet some parts of it represent a real advance in our knowledge, certainly in terms of a culture sequence and radiocarbon chronology, but also in terms of accumulated data on stratigraphically isolated components in which the internal patterning can be discerned and on certain aspects of the Holocene environments. In many respects this knowledge supplements and complements the abundant data already obtained for other parts of New York and the Northeast. "

In the "Conclusions" chapter we find the authors very sharp indeed not merely in their evaluation of the subject work but on archaeology in the Northeast generally. I find the following paragraph most arresting:

"A fundamental strategic problem concerns the archaeologist's control over relative and absolute time. A major difficulty flowing from the vagaries of deposition and disturbance on sites is that of defining the component, or single occupation; by limiting the time factor through

stratigraphic analysis and radiometric dating one tries to hold culture change to a minimum, thereby "getting at" the patterned remains of a real, extinct community. Stratified flood plain sites are well suited for this type of analysis, hence we have chosen to focus attention on such sites in our upper Susquehanna project. Some archaeologists of both "New" and "Old" persuasion have not devoted sufficient thought to this very important factor. As one example, there is a recent and welcome in the information potential of surface sites, including those which are disturbed by cultivation. In the past such sites have usually been "written off". Unfortunately, such sites often produce few, if any artifacts which are diagnostically useful, and it is usually assumed by many colleagues that they are single component stations. This is an unfortunate assumption which has led to a good deal of confusion. We never cease to be surprised at how frequently even the small, isolated surface site turns out to be multicomponent".

The above has the solid ring of reality observed, of much dirt passed through the fingers and of the eye having been trained by the observation and the experience, but the authors are at their best in Chapter II, "Theoretical Orientation" where they define their goals as "frankly culture-historical" and present the arguments for the choice of such goals. They are arguments in the modern mode, developed out of the attitudes, methods and aspirations of the "New" archaeology. Their value to the New York-Northeastern archaeologist is the selectivity with which the usefulness of the "New" archaeology for the Northeast is evaluated. Most serious archaeologists were "problem-oriented", were seeking anthropological interpretations of excavated data well before the gospel of the "New" archaeology began to be shouted from the pulpits of academe. Funk and Rippeteau point this out while also accepting, and submitting themselves, to what the "New" archaeology did bring to the practice of archaeology, formal research design and logical procedure in the testing and "explanation" of propositions. As one who believes that archaeology is not and cannot be a science in the sense that experimental natural sciences are science I do not see this belief as rendering scientific procedures inapplicable to archaeological investigation. On the contrary: data are evidence, as asserted in the previous review, only when argued from, toward an acceptable conclusion or statement of meaning. But the "New" archaeologists ambitiously strive for the formulation of covering laws of culture change and continuity whereas I believe the best, and highest, objective, is a perception of reality, an understanding of how and why culture changed, or did not change, or changed superficially but not fundamentally. It has always intrigued me, for instance, as to why the Lower Hudson riverbank campers never developed a permanent fishing village economy.

Those who have been put off by the "New" archaeology, by its apparent irrelevance to the circumstances of the Northeastern environment, by the silliness of some of the propositions it tests and the incongruity of the meager and ambiguous data it feeds into computers to extract large and positive answers will now understand, I think, the right use of the "New" archaeology in our regional studies. Funk and Rippeteau have provided the first lessons in what to use and how to use it out of the "New" archaeology rubric. What they have accomplished can best be exemplified by the Fig. I chart "Correlation of Environmental, Chronological and Cultural Data in Upper Susquehanna Prehistory", which will rank, I believe, with Ritchie's famous Synthesis of New York cultures. This kind of visualization brings home at once the direction and the impact of this basic and in-depth study of a riverine sub-region. Stream valleys, it seems to me, are the natural geographic units of archaeological investigation and Funk and Rippeteau are very much at ease in the Upper Susquehanna; more at ease than Funk is in the Hudson Valley, where the upper New York culture-chronological sequence fits better and there is no Lower Hudson estuarial ecology to deal with.

Having expressed my admiration for "Adaptation, Continuity and Change" and my confidence that it sets a new standard for Northeastern research I feel that some questions may fairly be raised about the Funk-Rippeteau interpretations of their data; these do not seem to me to follow necessarily, the one from the other. I am highly suspicious of the strict, mechanistic correlation of significant cultural variation with climatic-biotic phases, or with translation of the implications of these into demographics. Perhaps I had best state my objections on relation to a quote:

"The described trends in the sequence of phases and their populations are hypothesized to be directly linked to environmental changes associated with the C-2 subzone. Lamoka and the other narrow point phases were successful adaptations to the initial warmer, drier con-

ditions which resulted in increased quantities of deer, other animals and most foods. By Vestal times an optimum had been reached with respect to the equilibrium of human populations, their subsistence technology and food resources. After that time it is hypothesized that a decline, perhaps relatively slight, occurred in the carrying capacity of the environment, a result of the return of hemlock and concomitant decreases in the abundance of mast-producing trees. Resultant stresses on human populations and cultures were deviation-amplifying leading to several centuries of rapid, in situ culture change and experimentation in an effort to maintain equilibrium without substantial loss of population. It appears that these efforts failed in the long run, since population had dropped almost to pre-Lamoka levels by Meadowood times, at or near the close of the period of the C-2 subzone".

On its own face this is a one-to-one, single factor, non-causal correlation. But this is not, I think, what Funk and RippetEAU intended it to be. They mean that Lamoka "was a successful adaptation" to the warm dry conditions of the contemporary and if "adaptation" means what they have defined it to mean, alteration and adjustment to specific local conditions they have begged the question of whether it was an adaptation by not showing how it was a successful adaptation to contemporary conditions and by failing to consider an alternative which is probably the true interpretation of the Lamoka phase of a widespread Middle Atlantic and Northeastern small, narrow-bladed stemmed point tradition.

Almost 30 years ago, having found in the Croton River mouth area a series of Lamoka-like points (those mentioned in the previous review as having been shown to Ritchie) I hypothesized that the small, narrow-bladed stemmed point tradition that I was later to call the Taconic tradition had arrived here from the south. It was well over a decade later that enough information on this tradition had accumulated to show that the tradition had originated along the coast, probably centering on the estuaries of the Hudson, Delaware, Susquehanna, Connecticut and other such streams. As sea level rose and estuaries penetrated farther and farther inland, the Taconic tradition (call it what you will) perforce stayed with the estuarial environment to which it was accustomed. Our Twombly Landing site, opposite Yonkers, New York, gave us a chronological fix on approximately when it first entered the Tappan Zee and Haverstraw Bay. A hearth which had burned *after* the deposit of a small shell heap in the bottom of which were two small, stemmed points, was dated at 4750 plus or minus 200 C-14 years, thus providing a stop-date-forward for the points. It makes little difference whether the elapsed time between the heap deposit was two days or 200 years. The date falls within what I have called the GO II horizon of large shell and large shell, in our reading of the relation of salinity to oyster occurrence, means a period of sea level rise when the rise exceeded the rate of deposit of silt. The simple stemmed point people (the points are not always small and the blades not always narrow) verged year by year farther upriver as the sea encroached on the river mouths.

But the stemmed point people did not stay in the estuarial environment. Their archaeology, at least in the Lower Hudson, shows them to have been a vigorous and numerous people who needed to extend their territory, by reason of population growth, into the upper, freshwater reaches of estuarial streams. It is obvious that the cultural equipment and attitudes they took with them were flexible enough to take full advantage of what the environment had to offer. A generalized and versatile hunting-gathering culture, the Taconic (or simple stemmed) tradition was pre-adapted to the upper Susquehanna during the C2 era. After all C2 conditions may have presented no substantially different living problems, and may have presented the same aspect, from those from which they had come to the south. That is, they were actually staying with their environment regardless of where it lay latitudinally; the Lamoka (Taconic tradition) people flourished because they were living where they had long been accustomed to live. It is true that the Lamoka culture has its individual, characteristic features, the beveled adz and the painted tally sticks or wands or whatever, but we took two beveled adzes, one thick and one thin, from the Twombly midden, and Ritchie reported shell, freshwater mussel of course, from the midden at Lamoka Lake.

The simple stemmed point tradition, to which Lamoka plainly belongs, has not been particularly well defined. The abundant use of pick-up tools has never been mentioned, though this is hardly a distinctive trait. But its economy of tool forms is distinctive. The simple stem med projectile point form was used for almost all cutting tool purposes, for perforating, reaming, boring or short-drilling, edge cutting and scraping. One tool, of projectile form, occurs that

was apparently never hafted in any way, since the base of the stem is an end scraper. Items of bone are scarce to absent, suggesting the use of wood instead of that material, and non-industrial artifacts seem to consist of fetishes or amulets of the simple kind, in the Croton River area "clay babies", oddly shaped clay concretions that occur in the clay deposits of, among other places, Croton Point. Given the long duration of the simple stemmed point tradition, its wide distribution (it is to be found over most of the Eastern Woodlands) and the apparently high (relatively) population of its users, the conservative tool assemblage suggests high efficiency in the use of it. The most significant trait of the people of this stemmed point tradition-it does not appear in the material culture-is their functional adaptability, all the tricks, to put it that way, that they could do with what they had. There is no place in North America where they could not have established themselves without changing their material culture because the "adaptation" was in themselves.

The material culture of the contemporary Laurentians living under the same conditions provides a contrast to the functional adaptability of the Taconic tradition. One example will suffice, I hope. In the Poverty Point culture of the Mississippi Valley plummets are interpreted as bolas stones, and their numbers and other characteristics are compatible with such a conjecture. But the plummet of the Northeast is found as a solitary and comparatively rare item, not in bolas-like groupings and numbers. The best explanation I have heard for Northeast plummets is from a veteran fisherman who thinks they were fishing line anchors for fishing lines in swift water streams with rocky bottoms; the weight and shape of the plummet gives it the property of being able to find a secure lodgment between stones for holding a line in the current. It would work equally well, in my opinion, (I haven't tried it) in sandy or mud bottom, where the shape would enable it to bury itself as an anchor. Whether this anchor hypothesis is true or not the plummet is a specific tool, for a specific kind of work in specific conditions. Some cultures "invent" or devise adaptive material culture traits; some adapt their behavior, rather than their inventories.

Funk and Rippeteau have taken a very strong stand against what they call migration, without defining it in any special way that would adjust it to the obvious mobility of cultures environmentally adjusted to post-glacially mobile environments. Perhaps they would call the incursions of Early Archaic bands into northern latitudes "spreading" instead of migration, but they do not make any such distinction; so that when they say "As archaeological knowledge accumulates, migration theories become less and less tenable as models of northeastern prehistory", the only alternative to that unqualified statement is to assume that north-eastern cultures, and the people who lived them, grew up Topsy-like, out of the ground. It is the burden of meaning of finding Adena traits in the Northeast, and Kirk-like points and Susquehanna points, and Perkiomen points and bifurcate points and the Fox Creek-Cony-Selby Bay paradigm that people associated with these culture traits came from elsewhere. Otherwise we must think that each new recognizable culture pattern sprang up spontaneously all over a broad region simultaneously, in response to some event of climate or ecology that probably did not affect the whole region equally or in the same way.

The denial of inter-territorial mobility to a clearly mobile population is even a denial of the principle of culture adjustment to environment espoused by Funk and Rippeteau, since environment itself is mobile. But it is not necessary to deny migratory or diffusion mobility to make the point that certain culture phases manifest a "settled in" or indigenous adjustment. Migration or inter-territorial mobility or spread or diffusion or whatever you choose to call it is relatively short term. After the migrants have found a niche in which to "settle in" cultural conservatism will stabilize that culture in place for centuries. In the sometimes frantic search for new "complexes" for which they can claim the credit for discovering, archaeologists overlook that the hunting-gathering cultures, which include all Northeastern cultures up to agricultural times, are basically alike, distinguished mostly by their preference for certain projectile point shapes. It will take a very subtle line of reasoning to prove that any shape is directly related to the climatic phase or cycle even though the size-weight of a point, being ballistically determined, has to relate to the weight of its shaft, which may be solid wood, or a hollow cane. Was a Lamoka point more efficacious in killing game during its span of use than a Meadowood would have been under the same circumstances? Until it can be shown that a climatic shift caused a whole new faunal assemblage to appear in an area, which required a

specific form of projectile point to exploit successfully, it is much more reasonable to see variations in projectile point shapes as the blurring or drift away from mental templates influenced, perhaps, by the workability of the lithic material available.

Calling themselves "adherents of an evolutionary philosophy" Funk and Rippeteau define evolution as change, any kind of change, forward, backward or sidewise (the direction of change in projectile point styles), which is fair enough. Anybody past his sixth birthday is an evolutionist or ought to be, if he is conscious. In their Upper Susquehanna project, however, Funk and Rippeteau find little evidence of the transitions between sharply contrasting phases, which is to say they find the evidence of continuity but not of evolution. In the Hudson Valley, at least in the Lower Hudson, the evolutionary line in projectile points follows rather smoothly from the first entrance of the simple stemmed point makers, through several treatments of the stem shape including the "pinched stem" Normanskill-like points to the fishtail. (I am not one of those who believes in the direct derivation of the fishtail form from the Susquehanna broadspear; throughout the Taconic tradition there occurs a type with one angular and one rounded shoulder, that is, a half-fishtail, impresses me as being a more likely fishtail predecessor.) Unless we are prepared to believe that all the Lamoka or stemmed point makers became extinct like the mammoth we must believe their descendants, however few, must have continued to survive but are no longer recognizable by the type of projectile point they made. So what kind did they make? Very probably kinds that have been assigned by Funk and Rippeteau to one or another of the catalogue types. One starts out as a typologist with a very clear notion of the holotype and then, as actual specimens turn up, one must allow more and more deviations from it. Without fine chronological controls there is no way of knowing what the deviations mean. But this is the way changes in form occur, by drift away from a holotype that was the ideal for one period of time. Then one becomes the dupe and victim of what he has created.

We could not operate at all in archaeology or any other intellectual pursuit without typologies but they are heuristic devices, not something immanent in culture. Types correspond to reality, but that reality is systemic, in the process of change, always in transition, which is evolution.

One also wonders about the demographic-climatic phase correlation of the Upper Susquehanna project study. Did population really decline during Meadowood times because of a "decline of oak in some profiles; increasing moisture" and was there really a marked decrease in deer and small mammals at the same time? Once a principle or hypothesis or operating assumption has been taken to the bosom the temptation to interpret everything as in accord with that assumption can hardly be resisted. It is not long term adversity that kills people; to that they can adjust and can contrive expediences that may become permanent improvements. What decimates populations is the sudden and unexpected crisis, the single winter that egregiously disrupts the sources of subsistence, the uncontrollable fire, a plague, either among men or animals or a blight of vegetation. An anomalously late frost killing off the acorn and other nut crops followed by a severe winter would have played havoc with the subsistence of the Northeast, or of any subarea. Such a conjunction occurred in the middle 60's. While animals usually replace their populations by an upsurge in fertility after such a setback, human beings do not recover so quickly. Two or three conjunctions of lean harvests followed by starving winters within a generation (and we know very well that there are such short term cycles in weather) would have reduced population to very low levels but not for long, in historical perspective; perhaps 50 years.

I much prefer this catastrophic explanation for actual, as opposed to locational, population diminution. Deterioration of climate to the extent that the subsistence base of an area or region is severely restricted would have taken place over a period that would have forced or at least given a chance to, populations to move out, that is, migrate. Migration, dispersal, shifting about under stress, these are as inherent in the idea of hunter-gatherer adaptation to the environment as is stability and continuity.

Local fluctuation in population density of aboriginal peoples is hardly to be doubted as an historical occurrence. The research problem is in fixing on the highs and lows. Were Meadowood times one of the lows in the Upper Susquehanna valley? We have the judgment of Funk and Rippeteau that it was, using their excavated data. But was Meadowood the only "complex" resident in the area? As was pointed out in the first review, C-14 dates are not all that precise

and if the Meadowood people were in scant numbers, one would suspect that there were other people in the area contemporaneously. In the Lower Hudson the Meadowood occurrence (except for one classic mortuary blade and one point, the Meadowood points are much smaller, similar to those reported by Kraft for the Upper Delaware) is as sparse as Funk and Rippeteau found it in their survey. But, in the Croton River mouth area, in the final Archaic through Early Woodland, into which Meadowood falls, there is a very strong manifestation we call the "dwindle" (contracting) stem point complex, into which are subsumed Poplar Island and Rossville-like forms. It is thinkable that, if Meadowood is of northern origin, their manifestation, somewhat altered, in the Lower Hudson signifies refugee groups from the harsh environment of upper New York. But under no circumstances would I regard the manifestation as the sole culture present here during a period or era.

The foregoing commentary aside, I still regard "*Adaptation, Continuity and Change*" as highly as it was rated in the beginning of this review. Funk and Rippeteau set for themselves as research design the investigation of an area; without the results of such a design we can hardly be said to have commenced a prehistory. They stated cogently their philosophical premises and positions. They have tested propositions that are proper to their design and philosophy. They have done the work and reported their conclusions in a paper that holds tautly together. The work is a landmark.

My comments can be summed up simply as a questioning of whether the rigid application of a limited principle; the interpretation of culture change by environment, is sufficient to explain culture change so precisely, since the data on neither culture nor environment are any thing like precise. The kind of time by which people live is not the kind of time in which climatic cycles take place. People living on the land have to make year-to-year, season-to-season, day-to-day decisions, not century to century adjustments. Pollen counts are excellent for describing climatic-floral trends, but they are far too gross, in many instances, to tell us where the pollen came from; that is, when hemlock returned were the stands of oak seriously depleted, or did the hemlock establish itself in plantations on the fringes of the main oak woods?

An argument is not complete until the alternatives and the objections to the favored hypothesis or explanation have been given due consideration. This is the defect in "*Adaptation, Continuity and Change*"; Funk and Rippeteau have not only not been very shrewd in seeing alter natives and objectives, they seem to think non? could exist.

A biologist whose name I have forgotten, once said of human beings "Their properties are complex and not susceptible to precise analysis". If we could remember this we would not take as gospel the results of statistical probability formulae that do very well in predicting the behavior of atoms and the fall of (unloaded) dice; we would be skeptical of "laws" that explain human behavior or predict it infallibly; and we would accept hypothesis only, as another scientist once said, until we can find a better one.

Current Perspective in Northeastern Archeology: Essays in Honor of William A. Ritchie. Edited by Robert E. Funk and Charles F. Hayes III. Researches and Transactions of the New York State Archeological Association, Vol. 17, No. 1. New York State Archeological Association. Rochester.

Since the Ritchie festschrift has been distributed to the membership of NYSAA, which membership also will receive this issue of the *Bulletin*, a lengthy review seems hardly necessary. The book will have been read by now and readers will have decided on their most favorite and least favorite pieces, depending on their loci of interest. With the regrettable exception of Owasco the list of contributions includes every era of New York prehistory in which Ritchie has been seminally concerned. I should think that Ritchie is more than satisfied, perhaps even flattered, by the level of research, the insights, the scholarship and the writing competence of those who contributed these essays in his honor. Though my favorite piece is Dolores Elliott's account of Otsiningo, I would imagine that Ritchie might well have been most interested in Neal Trubowitz's intensive analysis of Frontenac Island; though the statistical methods used by Trubowitz are not the approach for which Ritchie has expressed high enthusiasm, the Trubowitz

objective, to ascertain the social mode of Frontenac Island occupants is well within the humanistic anthropological spirit that has always seemed to me to be the burden of Ritchie's teaching: aborigines were human beings.

As it happens, however, *Current Perspectives* is a major NYSAA publication and is entitled to more than a short-shift review. Perhaps a paragraph on each entry will provide a sampling of what seems to me to be most significant in a collection of stimulating ideas. I could not hope to do justice to each of the 17 contributions to this large format, 200 page book.

The introductory material, William Cornwell's "Preface", a personal recollection, Charles F. Hayes' "Rochester 1924-1949", an account of Ritchie's career at the Rochester Museum where he began it, and Robert Funk's "An Archeologist for All Seasons: A Biographical Sketch of William A. Ritchie", which will probably become Ritchie's official biography unless somebody undertakes to write a full-scale one, are all thoughtfully and gracefully handled. But I do wish Funk, whose appreciation of the Ritchie persona is as marked as one would expect from his principal disciple and chosen successor, had been able to re-create more vividly the salt-and-pepper in the Ritchie constitution. Bill Ritchie is not a man who will be forgotten by those who knew him, quite apart from his primary role in Northeastern archaeology.

Herbert Kraft's "The Paleo-Indian Sites at Port Mobil, Staten Island" will stand as the main reference on this manifestation of fluted points and related material until a major discovery, not anticipated, is made in the area. The piece is of itself a monograph, a thorough-going study of every item that the locale has yielded.

Robert Funk's "Early to Middle Archaic Occupations in Upstate New York" is, as far as my knowledge goes, the first synthesis as such of data on the Early and Middle Archaic in New York. It provides a solid foundation for, and shows the need of, additional exploration of these critical-to later culture development- times. A quote that ought not to be overlooked: "Other workers have proposed that relevant artifact styles have been present in the small minority of untyped items in surface collections, or forced into the Procustean bed of named types. This suggestion has proved to have some validity". To rephrase: we don't yet recognize all the Early and Middle Archaic materials that are (I will say must be) present in the region.

James Tuck's "A Look at Laurentian", is the canniest analysis of what is and is not "Laurentian" we are likely to see for sometime. It is a "must read" for those who are in the habit of using the term loosely, especially in the assignment of projectile points. The notched blade projectile point does not by itself indicate "Laurentian". Tuck says, and I am in accord, that there is a culture aspect that was detected by Ritchie and named Laurentian, but it only vitiates this concept to use the term uncritically, by extending it wherever there turns up something that looks like a Vosburg or Otter Creek or Brewerton side-notched point. As Tuck says "Much of the confusion surrounding the use of 'Laurentian' arises from overextension of the term in every way imaginable: geographically, 'typologically' and, to a certain extent, temporally". If there has to be a best piece in the book, this is it.

Don Drago's "Prehistoric Iroquoian Occupation of the Upper Ohio Valley" is a piece that I, as an editor, happen to know he has wanted to write for some time. Unhappily it is not as comprehensive a study as I understand he intended to do, especially as to Owasco roots of Iroquois in the Upper Ohio Valley. But what it does is to raise, with cogency, the matter of a simplistic in situ origin for Iroquois. That problem, he indicates, has by no means been solved for all time.

Peter Pratt's absorbing "A Perspective on Oneida Archeology" is very high quality scholarship, with a polished literary gloss. It does so many things, from disposing of Nichols Pond as the site of Champlain's humiliation of the Iroquois to a recounting of Gordon De Angelo's computation of the amount of wood required by an Iroquois village, that it is impossible to classify. It belongs, with a little editing, in an anthology. I didn't know that the Iroquois probably did not know tobacco until contact times; I thought they obtained it from the Tobacco Nation. Enjoy. Enjoy.

Donald Lenig's "Of Dutchmen, Beaver Hats and Iroquois" is a patiently and solidly documented presentation of the argument that "These early wars (by the Mohawks against the Laurentian Iroquois and the Mahicans) were fought to gain and maintain access to sources of European trade goods while the wars that would follow were fought to obtain fresh sources of beaver and other peltries but the underlying cause in both instances was economic". This is not

a new evaluation of the cause for Iroquois, particularly Mohawk, aggressiveness beginning with contact times but it has been whittled away at by several specialists. In this piece Lenig shores it up and repels attackers convincingly.

The late Marian White's "The Shelby Site Reexamined" is a very specialized revision of the chronological position of the Shelby Site in the Niagara Frontier village sequence. White now regards it as contact rather than pre-contact, as her earlier seriation of the pottery indicated. It is for Iroquois experts.

Dolores Elliott's "Otsiningo, an Example of Eighteenth Century Settlement Pattern" is, as mentioned above, my favorite because it is the best organized piece of expository writing in the book and because it deals with the sprawling, polyglot "community" or district of Otsiningo, at the "southern door" of the Iroquois Longhouse, on the Great War Trail. This post-Contact period pattern of cluster settlements is entirely new to me.

Dean R. Snow's "Archeology and Ethnohistory in Eastern New York" deals with the Iroquoian-Algonquian linguistic boundary in Upper New York and elsewhere, attempting to coordinate the archaeology with the distribution of language groups. While I don't believe that the archaeology is well enough understood (see the reviews of the two Funk books above) nor that the coordination recognizes what we do know tolerably well I am of the opinion that the Snow line of investigation, which few are now pursuing, is in a productive direction and should be encouraged. Not too many archaeologists have Snow's expertise in linguistics and are prepared to contribute to cultural-linguistics coordination.

James B. Richardson III's "The Impact of European Contact on North-eastern Iroquois and Algonkian Art Styles" will probably be skimmed through by most readers. The subject is simply not that significant in culture studies at the moment, partly because the data are so few. Very probably Richardson has said, or summarized, nearly all there is to say about it.

Neal Trubowitz's "A Statistical Examination of the Social Structure of Frontenac Island" is one of those compendiums of the results of statistical maneuvering for which I hold more admiration than enthusiasm. The amount of detail work, the sheer counting and measuring and calculation awes me, in my ignorance and sloth. But I never find the bottom line of such studies very impressive. The results are usually ambiguous or inconclusive or forced. And I think the premises could be successfully attacked on several scores if the conclusions ever seemed to be significant.

Marilyn C. Stewart's "Pits in the Northeast: A Typological Analysis" is concerned with "identifying primary purpose as reflected by form" of 335 pit profiles from the Engelbert site where pits furnished almost all the archaeological data. It is most informative survey of the features we call pits in excavation.

Bert Salwen and Sarah T. Bridges' "Cultural Differences and the Interpretation of Archeological Evidence: Problems with Dates" is a very salutary, cautionary essay on the uses and abuses of certain mean dating formulae, such as the Harrington-Binford pipe bore equation and ceramic nomograms. The discussion of the difference between the perceptions of prehistoric and historic time in the interpretation of site data is particularly useful.

Funk and Hayes have assembled a most creditable collection of contributions to Northeastern prehistory in "Current Perspectives". The quality is very high, as is the readability. With the many competent workers in the field we are not a far cry from the days when Ritchie, to whom the volume is dedicated, was almost the sole authoritative voice in New York. Ritchie has fathered a whole generation of archaeologists worthy of their heritage.

Archaeology and Geochronology of the Susquehanna and Schoharie Regions. Proceedings of the Yager Conference at Hartwick College. Edited by John R. Cole and Laurie R. Godfrey. 1977. Hartwick College, Oneonta.

Pay no attention to the title of this little volume. What the conference was really about was the claim by Dr. Bruce E. Raemisch, made not only here but in numerous press releases and journals over the past seven or eight years that certain artifactual looking stones recovered from Wisconsin (and earlier) deposits at the Timlin site near Oneonta are man-made Paleolithic tools

at least 70,000 years old. Several papers not on this matter were delivered by various workers but they drew little comment, even less that was not on the provenience and identity of the Timlin site items. The confrontation between the believers who consisted of Dr. Raemsch, Mr. Timlin, a minister and discoverer of the site, and Dr. George Carter, a long time advocate of aborigines 50,000 years old and older, and the doubters, led by Dr. William A. Ritchie, former New York State archaeologist, and Dr. Robert Funk, present State archaeologist, must be at least the 150th such confrontation that has taken place in conferences or in print since the glory days of Ales Hrdlicka. One side sees artifacts where the other sees naturifacts; one side sees the hand of man, the other the debris of geology.

But this time there was a difference, very heartening to this reviewer, author of "No Stone Unturned" (1959) and "American Dawn" (1970); while Ritchie and Funk and, I suspect, a goodly majority of those attending the conference found the Raemsch material either dubious or from disputable contexts, the probability of at least a 30,000 to 40,000 year habitation of America is no longer hooted at. One quote, from Funk, should suffice to insure that a much more liberal attitude about the age of Early Americans is now de rigeur. During an exchange with Carter, Funk said "I think someday we are going to have dates up to 30 or 40,000 years ago".

In the case of the Yager conference, however, it is clear that the doubters had the best of it. Archaeologists and geologists alike would accept only a very few pieces as the products of a lithic industry and these were not well documented to the claimed antiquity. Nor did the Raemsch-Timlin group do well forensically. They had to rely on testimonials, of which some seemed grudging and equivocal at best. Carter, who did a good job on defending the reality of a mid to early Wisconsin horizon of human occupation of America, seems to be unfamiliar, as Ritchie pointed out, with the contents of glacial till, his experience with rock clastics having been in the unglaciated West. The Raemsch Paleolithic remains where it was before the conference, not very convincing.

Two of the papers in this volume have appeared elsewhere, Franklin Hesse's "The Egli and Lord Sites: the Historic component-'Unadilla'-1753-1778" in the NYSAA *Bulletin* and Dolores Elliott's "An Ethnohistorical Approach to Settlement Pattern: Otsiningo, a Case Study" in the *Current Perspectives* festschrift reviewed above. William W. Vernon's "Geology and Geochronology of the Timlin Site" and F. Jay Fleisher's "Deglacial Chronology of the Oneonta, New York, Area" are concerned directly with the controversy over the Raemsch material, are informational on a subject where most of us must accept the word of such topical investigators and belong in the notebooks of every archaeologist doing business in the state. Robert Funk's "The Earliest Aboriginal Occupations of New York State" provides the prehistoric background as it is now orthodoxly viewed, for the questioning of the Timlin-Raemsch claims. It is a typical Funk piece, smooth, solidly substantial, succinct, up-to-date and plainly revealing its scholarly bias.

This bias does not affect the account of early occupations in the Northeast, where Funk is as authoritative as anybody in the field. But it does breathe off a strong aroma in his brief, all to brief, recounting of the evidence of Amerinds of classic Wisconsin (Wisconsin III or Cary) age. He conveys, for instance, the impression that Richard MacNeish downgrades his 20,000 year old Pikimachay material from Ayacucho Valley in Peru, but this is hardly the impression MacNeish himself conveyed when he wrote "Early Man in the Andes" for *Scientific American*, April, 1971, where he is positive in his identification of fragmented stones found in the basic, 20,000 year old, level with bones of an extinct sloth as artifactual in use and argues from this evidence that man may have been in America for the last 100,000 years.

By listing Early Man sites that have "generated controversy", Funk casts the shadow of suspicion on them, while the list itself gives no hint that some sites are more suspect than others and that some, like Valsequillo in Puebla, Mexico, are not suspect at all, merely enigmatic and aberrant from current views on the development of lithic technologies. Nor does he review all the evidence, (Chivateros, Guaterrero Cave, Crow Creek) as he should have done were he really addressing himself to the unlikelihood of the 70,000 years old age of the Raemsch material by reason of the unanimity of evidence to the contrary. But this section of the synthesis is gratuitous, in that the sites deprecated are not in the Northeast, and irrelevant because Funk accepts the presence of man at 20,000 years ago at Meadowcroft Rockshelter, Avella, Pennsylvania. Thus, ironically, he implies his rejection of all sites outside the Northeast,

which are not pertinent to the Timlin site, and accepts the Meadowcroft Rockshelter, which is pertinent, all the while suggesting a scientific skepticism about an Early Man horizon. Of course, habitation at a site within 50 miles of the ice front 20,000 years ago inescapably postulates Amerind inhabitants in the New World from 30,000 to 40,000 years ago and once that time depth has been acknowledged the door to a much more remote past stands wide open.

The foregoing is logical analysis, not a statement of view. I placed no credence in the Raemsch claims before I read the conference report, and I have had no change of heart since the reading. But the stances taken in controversies over these very Early Man manifestations owe more to the religious instinct than to scientific equability. The believers accept far too much uncritically; the agnostics reject far too much peremptorily. The agnostics, over the years, have had to give ground steadily and stubbornly, and do not seem to be aware of the trend. But the believers use the trend as a sanction for accepting everything, like the Raemsch material or the much more plausible Carmen Baggerly material from Michigan, as an article of faith. But it happens in every field, because there is inherent in all of us a disposition to believe and, where the evidence is not sufficient for certainty, a necessity to believe.

In addition to the above the Yager conference report includes two papers outside the limits of the Raemsch controversy of more than passing interest and significance: Gene Sterud's "The Application of Small Site Methodology to the New York Archaic" and William A. Starna's "Late Archaic Lifeways and Archaeological Variation". The two complement each other effectively and are the beginning, one hopes, of some new departures in the examination of the culture patterns of the Late Archaic which has for too long been understood only according to perceptions now several decades old. Sterud, arguing for more attention and better research design for the small, shallow campsites so characteristic of the Archaic, proposes that "The organization of research in any region should be tailored to the local peculiarities of the remaining archaeological record." Starna is distinctly more challenging. His questions are directed toward such broad culture classifications as Laurentian and Lamoka, so broad in fact that significant local variations are masked by the breadth of the class concept, but it is in these small, local or drift variations that the pace of culture change and adaptation is to be found. Those who have read the preceding pages will remember that I suggested that the culture "transitions" Funk could find no trace of in his Upper Susquehanna project material were actually right there before him, in what he had dug, the clues of transition obscured by the typological assignment. This is what I think Starna is talking about, and his piece, with Sterud's to a lesser degree, bespeak the same message as Tuck's review of the Laurentian: cultures are more complex than our present classification of them.

Incidentally, if you enjoy Ritchie at his best in extemporaneous comment, read his response to Starna's paper, even though it is defensive and rather misses Starna's point.

Sterud accepts the Ritchie-Funk differentiation, as defined in their *Aboriginal Settlement Patterns in the Northeast* of a qualitative difference between large sites and small ones. Small sites are the very limited, shallow, open sites that we call campsites or stop-over sites. Large sites are deep sites that have been rather intensively occupied by large groups for a long enough time for the full range of cultural remains to have been deposited there. Ritchie says that he knows of only three such Archaic sites in New York: Lamoka Lake, Brewerton and Frontenac Island; without the good fortune of having been able to dig these sites he would not have been able to arrive at his definition of Lamoka and the Brewerton phase of the Laurentian.

It certainly cannot be gainsaid that the three sites Ritchie calls unique are all of that, in central and upper New York, for their like has not turned up three decades of survey. But whether they are qualitatively different, as settlement patterns, or merely accumulations of smaller camp set occupations may be subjected to inquiry.

The inquiry is inspired by our experience in the Lower Hudson. We have excavated here two sites that may be called large, in that considerable quantities of material were taken from them and the dates fall within the Late Archaic, on the Lamoka-Brewerton-Frontenac Island level. One site, Twombly Landing in Palisades Park, was about an acre in extent; we dug a quarter of this (another quarter had been potholed by a collector) and recovered about 625 projectile points, about the same number as produced, if I recall, by Lamoka Lake. Almost half of these were in the Taconic tradition, as we then defined it. Much bone, preserved by the shell (it is a shell midden site) and over 200 lbs. of lithic material was taken by us. Three

dates, two on hearth charcoal and one on shell, ranged from 4000 to 4750 C-14 years. The site was, in places, three feet deep, though this was not a stratigraphically useful depth because it was decayed shell heap.

The Piping Rock site is hard to dimension. It is an approximately three-acre area within a bowl of slopes on the top of which are sites. We have recovered cultural materials from about 30 acres in the vicinity. But this area is only a segment of the almost continuous area of aboriginal occupation on the southern bank of the mouth of the Croton, extending for nearly a mile from opposite the Van Cortlandt site to the Crawbuckie Beach sites. But Croton Point, on the northern shore of the Croton's mouth, was even more extensively occupied. We have excavated about an acre of the Piping Rock site, have taken about 800 projectile points from it and quantities of other material, including bone and pottery, and we have about a dozen C-14 dates from it between the ages of 3 500 and 5300 years B. P.

A further large site is at the Dogan Point locus on Montrose Point where we have five dates, from 4950 to 6750 C-14 years ago. We have not dug at this site as extensively as at Twombly or Piping Rock but it was, in its time, as large as Twombly.

What reveals the settlement pattern of these large sites is the structure of the shell middens; they are clusters of shell heaps, the heaps being of a size that would have been deposited by a small band during a short seasonal stop-over. These stop-overs are hypothesized to have occurred during the early spring runs of anadromous fish. Where, as "large" sites, they differ from Ritchie's classic three is in the absence of burials and of shelter evidence. The absence of shelter evidence can be explained by the soil conditions, but the absence of burials is, after almost 30 years of excavation, no longer ascribable to adventitiousness.

What our large sites are, it is almost certain, are simply most favored camping loci. Despite the presence of burials on the Ritchie "large" sites, it is possible to interpret them the same way. Since there are only three of them in all of New York State north of the Lower Hudson this interpretation is as likely as that they were central base camps or semi-permanent, proto-village- like settlements.

Although he may or may not have originated it, in his *Danger Cave* study of the Desert Culture Jesse Jennings presented a lifeway model for that culture in which small, subsistence bands, which had to scatter and forage widely throughout the year, gathered in during the spring and fall in macro-band assemblies for social and ritual purposes. These were of brief duration, at favored places where, it is assumed, there was a temporary supply of harvestable food. This model fits our Lower Hudson riverbank sites comfortably and the Ritchie large sites as well, with this qualification; because the favored loci at Lamoka Lake, Brewerton and Frontenac Island were very much better favored with food resources, the assemblies were longer and the material culture deposits richer and more comprehensively representative of a culture as a whole.

It is all too obvious that if the paucity of large sites in middle and upper New York is real, and it seems to be real up to, I believe, Owasco times, we are dealing with a major cultural phenomenon, and a dismaying archaeological future. Sterud's concern with small site research methods, then, is most timely.

How the Sterud and Starna papers fit together can best be shown by a quote from Starna: "Late Archaic lifeways in New York State are characterized by substantial diversity and variability among a number of human groups which were practicing fundamentally the same subsistence pattern". From which I derive the meaning that small sites may very often tell us more about lifeways, as distinguished from the material and trait inventory of a classified culture like Laurentian, than large sites. Large sites tend to disguise variables and diversity in the small band units; certainly they will in the case of the model described above. Starna says "What is being emphasized here is that reified classifications tend to conceal or obscure variations in the total assemblage. This variation, I contend, is crucial for the understanding of prehistoric change and evolution. "

The direction of strike of Starna's piece is, however, quite different from Sterud's. Starna attacks the "rigidity" of culture classification so long in vogue in the Northeast and the classification he chooses to attack is, like Tuck, the Laurentian. Again I quote (Starna is very quotable)

"Another kind of problem can occur if only the diagnostic traits and not the variation within the assemblage is taken into account. For example, preliminary data from the Middle Mohawk

Valley indicate that the diagnostic projectile point forms of the Laurentian tradition are well represented and occur throughout this river drainage system. Yet other diagnostics are virtually unknown, e.g. plummets, gouges, bayonets etc. How do these assemblages fit known taxonomic units? Are they Laurentian because of the presence of the diagnostic projectile points? Are they not Laurentian because, although the projectile points appear, other diagnostics do not? Should a new name be given these assemblages? Of course, the difficulty in this case is that the convenient taxonomic 'pigeon-holes' are not applicable. Variation from the given taxonomic units presents a classification problem because these 'aberrant' assemblages simply do not fit into the established scheme of things".

This repeats, in essence, my objection to the application by Funk of a central and upper New York classification and chronology to the Lower Hudson, and it is clearly valid by any application of the principle of environmental adaptation and evolution, defined broadly as change. It would seem that the battle has been joined over the defense of the "old" and the "new" taxonomists.

The last paper delivered at the Yager conference was Helen Gutierrez's "Salvage Archeology - A Status Report". It is not going to raise anybody's pulse rate by reason of innovative or controversial ideas but every student of archaeology should read it once and those who are engaged in the salvation (not salvage) of cultural resources should re-read it periodically. It is the kind of entry you might find in an encyclopedia under the heading "Cultural Resource Preservation-History of".

Hartwick College can be very proud of this publication, of its competent editing by John R. Cole and Laurie R. Godfrey and the promptness with which it was issued: in mid 1977, with the conference having been held on Nov. 6, 1976. The discussions, which usually tend toward a certain incoherence and ambiguity when first uttered, were so ably edited that they added considerably to the conference's intellectual values.

Readings in Long Island Archaeology and Prehistory Vol. 1 Early Papers in Long Island Archaeology. Series Editor Gaynell S. Levine. Suffolk County Archaeological Association. Stony Brook. 1977. No price given.

The series of which this is Volume I is a most commendable project of the Suffolk County Archaeological Association in what may be called the conservation of archaeological literature. The reprints in Vol. I are all from out-of-print publications from the first decades of the century. Vol. II will be a hefty one, containing about 300 pages of articles from our NYSAA *Bulletin*, compiled by the editor of *Bulletin* and series editor Gaynell Levine. There is very little to be said or done about Vol. I by way of review except to list its contents, some titles of which students may have been trying to come by for years, as I have. The list is:

Ancient Shell Heaps Near New York City. Mark R. Harrington. American Museum of Natural History Anthropological Papers Vol. 3, 1909.

A Montauk Cemetery at Easthampton, Long Island. Foster H. Saville. Museum of the American Indian: Heye Foundation. Indian Notes and Monographs Vol. 2, No. 3, 1920.

An Ancient Village Site of the Shinnecock. American Museum of Natural History Anthropological Papers, Vol. 22, pt. 5. 1924.

A Cache of Blades from Long Island. Foster H. Saville. Museum of the American Indian: Heye Foundation Indian Notes, Vol. 3, No. 1. 1926

A Matinecoc Site on Long Island. Frederick P. Orchard, Museum of the American Indian: Heye Foundation Indian Notes, Vol. 5, No. 3, 1928.

Archaeological Exploration of Fisher's Island. Henry L. Ferguson. Museum of the American Indian: Heye Foundation Indian Notes and Monographs, Vol. 11, No. 1. 1935. "Readings" is a book that belongs in every library of New York archaeology. Write Gaynell Levine. P.O. Drawer AR, Stony Brook, N. Y. 11790.

AUTHOR AUTHOR AUTHOR

For some reason that cannot reflect very well on their attentiveness contributors in all too many cases have not been paying any heed to the standing material "Notes to Authors" on the inside of the back cover. Too many single-spaced manuscripts, too many incorrectly styled references, too many Xeroxed manuscripts, too many reproduced illustrations are being received from authors who apparently don't believe they fall into that category but are entitled to do as they please. Unless you, as an author, are sure you have followed all instructions to the letter, you had better include with your manuscript a self-addressed, stamped envelope for the return of that manuscript. Otherwise, if it flouts too many rules it may languish in the files for a year or so. No manuscripts have been returned so far in my 18 years as editor for that reason, and none are now languishing in the files, but something has to be done about the increase in carelessness.

Read those "Notes to Authors" and mind what they say.

Read, in particular, No. 7, which lays down what you have to do if you want extra copies of *The Bulletin*, in which your contribution appears. We have lately had some unfortunate results because authors want to buy more copies than we can spare, from our printing of 1200. If you will refer to Carolyn Weatherwax's recapitulation of memberships for the years 1972 in the minutes of the 1977 meeting printed in this issue you will observe that those memberships, which means those entitled to receive copies of *The Bulletin*, did not, during that lustrum, ever reach 900. A print order of 1200 should, then, be sufficient for the membership, with at least 250 copies for later sale. At three issues a year that would be 750 copies, and over a few years such a stock makes for inventory and storage problems when some, usually most, issues sell slowly. It has been in an attempt to control inventory that we have kept our print order to 1200 per issue. To increase the print order, with our present memberships still at only about 75 per cent of that figure, would only over-load the stacks of those issues that do not move out quickly.

We do not want to increase the standard print order, but it can be increased for a particular issue if we know in advance how many to increase it by. Hence instruction No. 7 in "Notes to Authors". The way it was supposed to work is that an author who intended to buy extra copies, say 50, of the issue in which his piece appeared, would order that number in advance of publication and he would receive them at the production cost, now \$1.10, as a perquisite of authorship. Copies purchased after publication were to be charged at the back-number price of \$2, which may soon have to be raised.

Perhaps instruction No. 7 is not as explicit as it might be. It is here spelled out, as it will be in future issues in "Instructions to Authors".

An author who intends to purchase additional copies of the issue in which his article appears must order them *in writing*.

The order will accompany his manuscript, submitted to the editor, but should not include a check for the order. The editor will add the order to the standard print order and will transmit to the repository at the Rochester Science Center the information that the author has properly placed his order.

Since manuscripts are, whenever possible, published in the order in which they are received, the author will have to wait for the issue in which his piece appears. When it does he may then send in his check to Rochester, which will ship his order to him.

Hereafter Rochester will not honor orders for more than five copies of issues after publication date. This will obviously not prevent a purchaser from ordering five copies several times, but it will allow Rochester to stop a "run" on its inventory when that inventory nears depletion.

I am aware that this will be read by only the present readership of *The Bulletin*, most of whom will promptly forget it. The present language of Instruction No. 7 has been changed to convey the same message, but as I well know, that will not be heeded. Nevertheless, authors who suddenly conceive a desire to buy up a quantity of their issues after publication will find the number they can purchase restricted, and will have to pay the full back-number price.

NEW YORK STATE ARCHAEOLOGICAL ASSOCIATION

Minutes of the 61st Annual Meeting

April 22-24, 1977

Long Island Chapter Museum, Southold, New York

Executive Committee Meeting

President Henry Wemple called the meeting to order at 8:15 P.M. on April 22nd, 1977. All Chapters, with the exception of the Frederick M. Houghton Chapter, were represented and the following members and alternates were in attendance.

State Officers

President: Henry Wemple, Chenango Chapter
 Vice Pres.: Charles E. Gillette, Van Epps-Hartley Chapter
 Secretary: James P. Walsh, Auringer-Seelye Chapter
 Treasurer: Richard F. LaBrake, Auringer-Seelye Chapter

AURINGER-SEELYE CHAPTER

Virginia Stiles*
 Gloria Miller*

VAN EPPS-HARTLEY CHAPTER

Gwen Gillette*
 Kingston Larner, MD

CHENANGO CHAPTER

Earla Burton
 Rosalind Kupris

WILLIAM M. BEAUCHAMP CHAPTER

Barbara Harris
 James Bradley*

FREDERICK M. HOUGHTON CHAPTEREDITOR & ESAF REPRESENTATIVE

Louis A. Brennan

INC. LONG ISLAND CHAPTER

Robert Hawkins
 David Detrich

COMMITTEE CHAIRMENMembership

Carol Weatherwax

Finance

Dolores Lalock

Program

Elizabeth Dumont

Publications

Roberta Wingerson

Awards

Charles Wray*

Education

Carolyn Schrier

Budget

Edwin Phillips

Nominating

Kingston Larner

Public Relations

Vicky B. Jayne

Constitution

Paul Huey

Legislative

Franklin B. Hesse

Program 1977Historic Arch.

Dolores Elliott

Public Arch.

Charles E. Gillette

NYSAA/NYAC

Vicky B. Jayne

GrantsMETROPOLITAN CHAPTER

Ann Browning
 Roberta Wingerson*

MID HUDSON CHAPTER

Alvin Wanzer

TRIPLE CITIES CHAPTER

Dolores Elliott

UPPER SUSQUEHANNA CHAPTER, INC.

Helen Gutierrez

*Alternates

- 1, The meeting was called to order at 8:25 P.M.
2. Roll call was taken.
3. Motion was made to accept the minutes of the last meeting as printed. Motion was made by Elizabeth Dumont, seconded by Henry Wemple. Motion carried,
4. Reports of the Officers:
 - a, Report of the President: First I wish to thank all these Committee people for their dedicated support of the NYSSAA both present and past. Speaking of the past I wish to recount a bit of history and suggest

at least one future obligation of our society. We have been in existence now for sixty-one years and a recognized Chartered Institution for fifty years. We have grown from three chapters to twelve, with the thirteenth chapter now requesting membership. This time period has seen many changes, foremost of which is the improvement in competence of our members plus an adequate program, our Awards Program, for recognition of this competence. We have extended our scope in a renewed interest in Historic Archeology, developed a much needed department of Public Archeology to demonstrate to the public the many facets of our discipline, cooperated with and supported sister organizations such as NYAC, developed educational slide programs, and continued to publish three excellent Bulletins each year as well as excellent monographs whenever they are available. We are now confronted by another exacting demand on our experience, the use of our members in assisting our professional people in the preparation of environmental impact statements and in specialized fieldwork. This again brings up the question of Certification of the Para-professional. I hope to see some action on this in the near future as I feel that our organization has both the depth and the prestige to develop a respected program for the scientific recognition of our members abilities and training.

b. Report of the Vice President: The office of Vice President has been active in several different spheres in the year 1976-77. In mid-July I met with the President in Vernon Center and discussed possible actions that he could take in several matters. I attended the only meeting regularly called, e.g. the Mid-Year Executive Committee Meeting held in Binghamton on Oct. 23, 1976, As Chairman of the Committee on Certification, I have accomplished little. I am still convinced that if NYSAA wants such a program which is equal to the State of Arkansas it must be done in conjunction with some professionally sponsored program. As Co-Chairman of the Committee on Constitutions I have reviewed the proposed Constitution and Bylaws of the proposed Sebonac Chapter, and the revised Constitutions and Bylaws of the Inc. Long Island and the Metropolitan Chapters, I have utilized the office of Vice President to respond to inquiries regarding NYSAA which routinely come to the Office of the State Archeologist.

c. Report of the Secretary: My first order of business upon taking office in April of 1976 was the printing and distribution of the Informational Handbook, Vol. 9, Copies were distributed to the Officers, Committee Chairmen and Chapters of the Association. Nos. 3, 4 & 5 of Volume 7, and No. 1 of Volume 8 of the NYSAA NEWS were printed and distributed, 1,000 ESAF Publication Flyers and 1,000 ESAF Notices have been distributed. The minutes of the 1976 Annual Meeting and the 1976 Mid-Year Executive Committee Meeting were printed and distributed. During the year I have received 95 letters and sent 98. During the past year I have received \$182.00 for petty cash. \$180.00 from the Treasurer and \$2.00 from the sale of Handbooks (this does not include bills paid directly by the Treasurer). Disbursements are as follows: Postage, \$71.50; supplies, \$10.38; phone, \$36.39; printing, \$12.50; and conference expenses of \$30.00. This leaves a balance of 521.23 in petty cash. At the Mid-Year Executive Committee Meeting I made a motion that "Instead of Volume 10 of the Informational Handbook being issued in 1977, a supplement to Volume 9 be issued to save funds," Since then I have found a printing source that is 50% cheaper than last year, and, as there is a great need for a revised and updated handbook, I propose to issue Volume 10 of the Informational Handbook as soon as is possible.

d. Report of the Treasurer: As of January 1, 1977

Savings Accts: 47-930	\$3085.91
47-753	1000.00*
<u>Checking Acct: 61-02-810-9</u>	<u>3623.42</u>
Total	\$7709.03

*\$1000.00 set aside for Life Memberships.

Receipts & Disbursements for the period ending December 31, 1976.

Total Receipts	\$1114.00
Total Disbursements	\$1095.18

5. Reports of the Committees:

a. Membership Committee - Carol Weatherwax, Chairman. The Chapters & Membership Committee has been trying very hard to keep a correct as well as a current tally on membership of NYSAA. To help me with this sometimes enormous task would the chapter secretaries and/or membership chair men please use the forms supplied to them by the State Secretary in the correct way? That is, please use the TOTAL COLUMNS at the top and bottom of each page and number the pages consecutively for the entire year. Please remember that reports are due to the Secretary each month, especially the first of March, July and November for the Bulletin. On the first of September a numerical count is needed for ESAF. New Membership Leaflets will be distributed in April. The cover design is by James P. Walsh, NYSAA Secretary. The total cost for 2,000 leaflets was \$130.55. I am happy to announce that the Sebonac Chapter of the East Membership of NYSAA over Hampton, Long Island, has applied for membership in NYSAA. WELCOME! Membership of the NYSAA over the past 5 years is as follows:

<u>MEMBERSHIPS</u>	<u>MEMBERS</u>
Dec. 1972 782	978
Dec. 1973 881	1106
Dec. 1974 813	1024
Dec. 1975 879	1093
Dec. 1976 880	1089

Total membership as of April, 1977 is 620 with 759 individuals. The Membership Award this year will go to the Lewis Henry Morgan Chapter of Rochester, NY.

b. Finance Committee - Dolores Lalock, Chairman. (No report received)

c. Program Committee - Elizabeth Dumont, Chairman. Liz reported on the progress of her committee during the past year.

d. Publications Committee - Roberta Wingerson, Chairman. Roberta reported that three issues of the "Bulletin" had been printed during the year, and that the "Festschrifts" were in Rochester and would be distributed with the next issue of the "Bulletin".

e. Awards Committee - Charles F. Hayes, III, Chairman. Charles Wray reported that the committee had met, and that the awards would be announced at the banquet.

f. Education Committee - Carolyn Schrier, Chairman. As directed by the Executive Committee at the October meeting, the reaffirmed NYSAA Resolution on Historic Site Preservation was reprinted. Copies are on their way to all County Historians, Boy Scout Councils, and school districts of the state. Many letters have been sent this year, making the point that archeology is important to New York State. Two sample news releases on this theme have been prepared. NYSAA can be very influential in acquainting the public with the reasons for archeology. All of us must speak up. We can no longer afford to be silent.

g. Budget Committee - Edwin Phillips, Chairman. A three page report was submitted by Mr. Phillips and has been distributed. Estimated receipts were compiled from information received from the chapters and total \$5,929.00. Estimated expenditures of \$4,345.00 were compiled from Officers and Committee Chairmen information. The estimated excess is \$1,584.00.

h. Nominating Committee - Kingston Larner, MD, Chairman. This Chairman has had little to do these past twelve months. However, I cannot be sure that the ensuing twelve months will be as carefree. For the past decade our Association has had difficulty in finding enough candidates to provide a contest for the various elected positions. I would like just once before I retire from this chairmanship to be able to present a slate with two candidates for every position. The Nominating Committee is composed of a chairman and the secretaries of each chapter. Without the secretaries' arm-twisting, brow-beating, and otherwise inveigling suitable chapter members to run for office, the slate of candidates cannot be presented.

i. Public Relations Committee - Vicky B. Jayne, Chairman. Two years ago a set of guidelines was distributed to members of the committee and to secretaries of those Chapters without a designated member of the Committee. I am now compiling a more detailed handbook to be used with these guidelines, and it should be ready for distribution in September.

j. Constitution Committee - William F. Ehlers, Chairman. The 1976-77 Committee began with the appointment of Kenneth Robinson (Mid Hudson) as Chairman. Charles Gillette (Van Epps-Hartley) completed the Committee. Due to illness in the family, Mr. Robinson was forced to resign, and President Wemple appointed a new Chairman. In process or accepted, prior to October were the following Chapter Constitutions: Accepted - Auringer-Seelye, Chenango, Mid Hudson, Triple Cities, Van Epps-Hartley and Wm. M. Beauchamp, In process - Metropolitan, In January, letters were sent to the delinquent Chapters with the following results: Submitted - Inc. Long Island and the new Sebonac Chapter of East Hampton. Letters were received from Morgan and Inc. Orange County and both these Chapters should complete their revisions shortly. No correspondence was received from the Houghton or Upper Susquehanna Chapter, Inc. At the 1977 Annual Meeting the Constitution revisions for the Metropolitan and Inc. Long Island Chapters were accepted. Also the Constitution of the Sebonac Chapter was accepted upon receipt of the suggested changes.

k. Legislative Committee - Paul Huey, Chairman. Mr. Huey presented a three-page report on two bills currently in the New York State Legislature which, if passed, would have an effect on archeology in New York State. They are Assembly 2914 and Senate 1650.

l. Program 1977 Committee - Franklin B. Hesse, Chairman. Buzz gave a brief report on the fine program prepared for the Southold meeting.

m. Historic Archeology Committee - Bert Salwen, Chairman. (No report received).

n. Public Archeology Committee - Dolores Elliott, Chairman. At the Mid-Year Executive Committee Meeting I urged each Chapter to develop a committee on Public Archeology. I alerted the Association that we no longer have the luxury of being armchair archeologists; that we must act to preserve sites. I distributed to each Chapter a folder containing information on Public Archeology and suggestions of activities that might be undertaken. I requested that each Chapter appoint a member to the State Public Archeology Committee.

o. Grants Committee - Vicky B. Jayne, Chairman. The first eight pages of information regarding sources of funding which may be available to Chapters and/or individual members is being distributed to Chapter Secretaries at the 1977 Annual Meeting. A short bibliography is included, as these references contain valuable tips on writing proposals, figuring budgets, progress reports, etc. The list of agencies will be added to from time to time.

Note: Many of the preceding reports have been condensed to save printing space. All reports that were received are on file with the NYSAA Minutes 4/22/77 of the Executive Committee Meeting.

Discussion during Committee Reports:

Program Committee - Louis Brennan stated that the Fellows of the Society should be made responsible for part of the Program. He felt that the Fellows should be more active as a group. Robert Hawkins said

that there should be a division of responsibility between the State Association and the host Chapter. David Detrich felt that the State Association should be in charge of the whole Program, not the host Chapter. President Wemple stated that Association members like to gain regional knowledge when they visit other Chapters. Time should be left in the Program for local papers. He said that he would like a Committee to work on the problem. Elisabeth Dumont felt that there should be two or three symposiums in the program, one on local archeology. President Wemple asked Elisabeth Dumont to work on the problem and to pick a few Committee members by August 1st.

Legislative Committee - Elisabeth Dumont moved that: "The President be empowered to compose a letter stating the New York State Archeological Association's support for Historic Preservation, and the letter be directed to the appropriate state officials." Seconded by James Bradley. Motion carried, President Wemple instructed Secretary Walsh to write a letter to Mary Krawczyk and the Rochester Museum, on behalf of the Officers, Executive Committee and the Membership of NYSAA, thanking them for their fine work on Members -at-Large and the distribution of the "Bulletin".

6. Old Business

a. Oneida Indian Situation - Dolores Elliott referred to remarks made at the Mid-Year Executive Committee Meeting of the previous October. She felt that perhaps inaccurate information was brought out and people had gotten the wrong impression of the situation. President Wemple offered his apologies for not having the correct information.

b. Membership Drive - Carol Weatherwax reported that Elisabeth Dumont had made up publicity reports.

c. Certification - Charles Gillett stated that the professionals have not been able to establish guidelines for certification. Elisabeth Dumont reported on SOPA, Dolores Elliott reported that NYAC had just adopted SOPA standards for NYAC membership. President Wemple felt that certification should not be confined to professionals. Lay members should be able to achieve certification. He stated that we are waiting for standards. Vice President Gillette asked that persons interested in working on the project get in touch with him or President Wemple, Dolores Elliott noted that the number of persons whom we consider to be professional archeologists in NYSAA is very low, President Wemple re-appointed Charles Gillette to chair the Certification Committee, The position was accepted by Mr. Gillette.

d. Marion White Memorial Volume - Roberta Wingerson reported that progress is coming along very slowly.

e. Site Recording - Charles Gillette noted that the reporting of sites was slow, most additions to the list were coming from Long Island and Binghamton. He stated that the file is confidential and that no information shall be released from the State files unless the sites are threatened by construction. The information is kept confidential by all parties. If people want sites preserved they should place them on the Site Register. Forms may be obtained from Charles Gillette at the State Museum.

f. Marion White Scholarship Fund - It was reported that the money is in the treasury, but no papers were received.

7. New Business

a. Sebonac Chapter - Vicky B. Jayne made a motion that: "The Executive Committee accept the new Chapter and place it up to a vote by the Membership," Motion seconded by David Detrich. Motion carried.

b. 1978 Meeting Place - President Wemple asked for suggestions for the next year's meeting place. No Chapter invited the Association at this time.

The passing of Selwyn Gibbs, a founder of the Inc. Orange County Chapter, was noted with regret.

c. A motion for adjournment was made and carried at 10:25 P.M.

Respectfully submitted:
James P. Walsh
Secretary, NYSAA

Minutes of the General Business Meeting Saturday, April 23rd, 1977

1, President Wemple called the meeting to order at 9:15 A.M.

2, A motion to accept the minutes of the last meeting, as printed, was made and carried.

3, The readings of the President's, Vice-President's and Secretary's reports *were* waived.

4. Treasurer LaBrake reported on the finances of the Association, (See the Treasurer's Report - Executive Committee Meeting).

5, Chapter Reports - Louis Brennan moved that the Chapter Reports be dispensed with. Motion seconded and carried.

6. Old Business - President *Wemple* gave a report on Fred Rath. The statement was read by Elisabeth Dumont, A NYAC letter by Tom King was also read.

7. President Wemple reported on the highlights of the Executive Committee Meeting. He appointed Elisabeth Dumont Program Chairman for 1978.

8. New Business - The Sebonac Chapter has submitted its constitution and has applied for membership in NYSAA. A motion to accept the Chapter was made by Kingston Lerner, MD. The motion was seconded and passed unanimously.

Alfred Dart qualified his position on the formation of the new Chapter. We welcomed the Sebonac Chapter into the Association.

b. Publications - Roberta Wingerson reported that the "Festschrift" should be in Rochester this week. Copies will be distributed to the Chapter Secretaries with the next "Bulletin" which is also in Rochester.

c. Membership Drive - Carol Weatherwax reported on the new Membership Folders and the NYSAA T-shirts that Auringer-Seelye Chapter has for sale.

d. Annual Meeting 1978 - President Wemple noted that no Chapter had volunteered to host the 1978 Annual Meeting. He mentioned the possibility of extending a few papers to Friday afternoon. Ann Browning, Metropolitan Chapter, mentioned that hotel prices in the metropolitan area were prohibitive to hosting a meeting. If they could find reasonable accommodations, Metropolitan Chapter would be happy to host the Annual Meeting. President Wemple mentioned the Mid Hudson Chapter, and felt that there could perhaps be a combined effort to host the 1978 Annual Meeting.

A motion was made to adjourn the meeting at 10:05 A.M. The motion was seconded and carried.

Report of the Awards Committee, Annual Awards Banquet

<u>The Achievement Award</u>	-	Robert E. Funk
<u>Fellowship</u>	-	Peter Pratt
<u>Certificate of Merit</u>	-	George R. Hamell
<u>Meritorious Service</u>	-	George Cottral
		Virginia Stiles
		Barney Chernoff
		William Sternitzke
		Lewis A. Dumont
		Pamela Augustine
		Marion V. Lloyd
		Helen Gutierrez

Respectfully submitted:
James P. Walsh
Secretary, NYSAA