With the intention of skirting this relativism, I offer a brief example of what I am asking. To use one understanding of Marx, we might assume that ideology masks actual economic and political relationships and that at least that part of archaeology the public is exposed to contains ideological statements. Then we can ask two questions: First, if Colonial Williamsburg, Virginia (a site I am interested in which can serve as an example here), is a faithful representation of pre-revolutionary America, then that America was one without classes, poverty, or exploitation—one without conflict and not in need of revolution. To what extent, and in what ways, does modern ideology operate through archaeology to say, in this instance, that America is what America was, and that what it is is what Americans see in Williamsburg? A second question spots a broader issue. What is our notion of time, object, and person? Time is both continuum and segment for us. One implication of this conception is that concentrating on one point of time diminishes the importance of others, halts change, and prevents comparison. It is important to know how this works because it specifies how we form hypotheses and come to alternative conclusions. I would like to ask Klejn if, from his vantage point, systematic answers to these questions are possible and worthwhile and whether the reasoning supporting these questions seems sound or familiar.

On Criticisms of “Some Paleolithic Tools from Northeast North America”: Rejoinder

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Responses by Raemsh, Vernon, and Carter (CA 19:157–60) to our criticisms of “Some Paleolithic Tools in Northeast North America” (CA 18:541–46) continue the saga of claims for great antiquity of human occupation at the Timlin site in New York. Several new issues are raised and old ones are reassessed, but most of our critique seems to have been ignored. The “early man” issue has implications for world archaeology far beyond the New York area and should be treated seriously and scientifically. As Carter notes, evidence rather than emotionalism is needed to resolve this situation and others like it. Although constraints of space limit us to a joint comment, several points need to be made. Raemsh and Vernon’s original report remains unconfirmed, and these recent responses illustrate some of the reasons.

None of us has said that Raemsh has not found any artifacts at the Timlin site—although we believe that he has misinterpreted what he has found, both artifactual and nonartifactual—so we do not wish to defend a straw man. But are his “eoliths” artifactual? Are they properly dated?

Carter’s assertion that “naturefacts” cannot be confused with artifacts is belied by the European “eolith” controversy and the controversy surrounding alleged early sites in the New World (cf. Barnes 1939; Cole 1972; Haward 1914; Haynes 1973, 1976; Moir 1919; Obermaier 1905; Rose 1968; 1

1 P. Jay Fleisher’s advice and comment are gratefully acknowledged, although views expressed here are the authors’ responsibility, of course.
from glacial deposits near Oneonta, New York. Carter's observations do not prove the artifactual nature of clasts from the Timlin area. Attrition, and some include examples of pseudo-retouch. (Carter's emphasis on work by Kuenen is simply irrelevant, as natural when he included them for comparative purposes)

Because Kuenen discussed water, not glacial, transport.) Many flints are eroded from the Onondaga outcrop by glacial action. Some clasts in the area of the site retain a tabular or angular form; some with sharp edges appear to have been splintered or fractured rocks exhibit the same range of flaking angles as human industries. Among these one may mention the groups described and illustrated the development of clasts from sharp-angled forms near their source to rounded shapes at greater distances (Flint 1971: pl. 7-12). Crushing produces pressure-flaking (cf. Warren 1905a, 1920, 1923) which can appear surprisingly systematic (cf. Wymer 1968:12)

By the 1920s, in comments on the debate which had occupied his attention for 30 years, Warren was using rather unequivocal language: "The 'Eolithoid standard is beginning to disprove itself," allowing scholars to pursue more substantial issues (Warren 1928:8). "For many years I have consistently maintained that certain groups of flakings are not prehistoric human industries. Among these one may mention the groups found below the Crags and the Forest Bed of East Anglia and within the Red Crag at Foxhall" (p. 6). Warren equates fractures in these deposits with "Eolithoid flaking" in the Bullhead flint bed "which underlies the oldest Tertiaries" (p. 8), and he sarcastically describes an "'Acheulean' (or, perhaps 'Mousterian') form of ovate' biface from the Permian deposits of Devonshire (p. 8)!

There can be no question that Warren assumed his readers would automatically recognize his view of Sub-Crag materials as natural when he included them for comparative purposes in his table of flaking angles in 1951. The British Crag deposits occur as Lower Pleistocene-Upper Pliocene strata (cf. Flint 1971: 638-39), so flints from beneath the Crags are Pliocene in age (or earlier). The point of concern is not the possibility, now recognized, that Crag and Sub-Crag deposits could contain artifacts, but rather Raemsch's misreading of Warren as a selective (mis)using of evidence. "Naturefacts" are confused with artifacts in print as well as in person. It seems, and unwary readers are given a very misleading impression of Warren's position. Warren worked and wrote on the eolith problem for 60 years with elegance, wit, and scientific rigor, and to misuse his work in support of this latest eolith claim is unfortunate in its implications of scholarship rather ignorant of prior work. The reference is misleading. Warren's (1951) Sub-Crag "materials." He notes that Warren considered naturefacts (from a different locality altogether). Warren uses "Sub-Crag" materials merely to contrast natural flaking with the crude but human flaking of Ctonian artifacts. Elsewhere he clearly argues that Sub-Crag and other "eolithoid" flints are products of natural agencies (Warren 1905a, b, 1913, 1920, 1922, 1923, 1924, 1928; Wymer 1968:12 and pl. 1).

Carter writes that abundant broken flint can only be found in close proximity to flint outcrops. As we noted before (CA 1968:41-46), the Timlin site does lie near the chert-rich Onondaga limestone belt. The flints at the Timlin site and downstream from it were carried those few miles by ice after being eroded from the Onondaga outcrop by glacial action. (Carter's emphasis on work by Kuenen is simply irrelevant, because Kuenen discussed water, not glacial, transport.) Many clasts in the area of the site retain a tabular or angular form; some with sharp edges appear to have been splintered or crushed. Most display the effects of some tumbling or marginal attrition, and some include examples of pseudo-retouch. Carried 30-40 miles farther (to the Oneonta area where Carter says he looked in vain for unrounded clasts, for example).2

Carter's comment illustrates our argument but contrasts oddly with earlier claims by Raemsch (1968, 1969, 1970) of true artifacts from glacial deposits near Oneonta, New York. Carter's observations do not prove the artifactual nature of clasts from the Timlin site; rather, they demonstrate the principles of glacial attrition documented and explained by Wentworth (1936), Holmes (1960), Flint (1971), and Sugden and John (1976). 3

Raemsch seems to have allowed Carter access to more evidence than others have been privileged to see. On the very day in November 1976 that Carter examined materials in the laboratory and museum, Raemsch refused access to us before and after public requests at a conference which Carter attended. Requests in prior years were also refused. We thus make do with published accounts as best we can.

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Raemsch compares his materials, and others). One of the serious criticisms of claims for some New World “early” sites is their lack of lithological diversity. It is odd that only New World “Lower Paleolithic” sites seem to present this aberrant pattern. Proof that the Timlin site conforms to a dubious pattern does not strengthen its case.

Carter writes (p. 159) that “Lower Paleolithic” is simply a term for “a stage in human cultural development” which is “timeless and free of connotations of diffusion” and for which “any evidence for age and connection must be separately developed.” We may accept Carter’s usage, but does Raemsch? Raemsch (1977a:15) replied to a question about Timlin-site dating by saying that the site lacked Holocene artifacts (i.e., that aside from a “questionable” Archaic component, the site had only typologically ancient artifacts). Raemsch’s typological dating cites Old World Paleolithic similarities (p. 157) and contradicts Carter’s dictum. Raemsch implies that he has “index types” and makes explicit claims about a few pieces—an adze, Levallois- and Mousterian-like points, and a “handaxe.”

Adzes are typical of New York cultures at least from the Archaic Frost Island Phase (c. 1250 B.C.) onward, contrary to the claim (p. 158) that they are unknown in the local Holocene (cf. Ritchie 1969, 1977b). Bifaces are nondiagnostic of any particular time or culture, and they are known in Paleolithic, Archaic, and later New York sites in abundance (Ritchie 1969: pl. 25; 1977b:117-18). The same nondiagnosticty is true of the two flakes Raemsch illustrates (figs. 1, 2) as “points.” Figure 1 shows a presumed flake with edge-battering on the left margin reminiscent of natural wear rather than retouch. The right margin may have scalar or shatter flaking. The piece may be a side scraper, simple debitage or natural. (One “artifact,” or a short series, can never be evaluated definitively.) The Combe-Grenal point shows faceting produced by flake removal initiated beyond the margins of the piece. In contrast, faceting on Raemsch’s flake seems to follow a natural surface. The French piece has systematic marginal retouch; Raemsch’s does not. The “base” of the latter seems too thick to be a point base. Resemblance to the Bordes line drawing is thus superficial, limited to plan shape. Figure 2 shows damage along the dorsal right margin, at its tip, and on the dorsal ridge in the form of nicks. Raemsch notes a “bulbar scar” on its reverse, but perhaps he means “eraillage scar,” since bulbar scars by definition appear on cores, not flakes. The bulb of percussion may show eraillage scarring, but not intentional bulb removal. A crude initial flaking blow seems to have created a broken bulbar area. Like the other “point,” this one lacks Levallois-technique dorsal faceting. Marginal damage seems fresher (sharper) than the worn dorsal ridge and tip scars, which would be equally fresh or worn were this a “Levallois point.” It is a flake and thus a “uniface”; it may be artificial or natural, but it is not a projectile point which can be equated with Bordes’s drawing. There is no demonstrable retouch. Dorsal ridge nicks cannot be intentional unless the intent were to shatter the flake, but they would be expected if natural battering and crushing were at work. This would also explain the irregular minor edge damage of differing degrees of freshness, a criterion of “nature-facts” emphasized by Warren (1924:309).

Raemsch also claims geologic proof of the Timlin site’s antiquity based upon weathering, but the extent of weathering and decalcification of his artifacts is duplicated in Holocene materials. Ritchie (1977a:14; 1977b:118) reports excavating identically leached and weathered Archaic artifacts in New York. Vernon (1977a:27) has conceded that weathering is not a dependable dating standard, but Raemsch simply reasserts his weathering argument without qualification.

Carter defers to Vernon’s argument for the antiquity of the Timlin site, but the latter strikes us as perplexing, indeed. Vernon (1977b and p. 158) refers to the work of Dreimanis and Goldthwait (1973) for the age of “Olean” deposits in Central New York, but Dreimanis and Goldthwait offer an Early Wisconsin age very tentatively, questioning their own attribution (p. 85). Had the work of Cadwell (1972) or the more recent work of Krall (1977) been available to them, Dreimanis and Goldthwait would certainly not have considered “Olean” deposits in the Upper Susquehanna or Schoharie drainages to be of Early Wisconsin age. Cadwell and Krall have demonstrated that deglaciation of the Upper Susquehanna and Schoharie Valleys was as recent as 15,000 years B.P.

Vernon’s argument for great antiquity of surficial deposits at the Timlin site presupposes that postglacial erosion obliterated along West Creek all of the deposits associated with the most recent glacial advance into the region. Preservation of recent glacial landforms precludes such an interpretation. To assume large-scale stripping of what had to be hundreds of feet of valley fill (cf. Coates 1974) is simply inconsistent with erosional evidence in this entire region. Regional valleys much larger than West Creek have escaped significant modification since deglaciation and still reflect their original topographic expression. Preserved in the landscape of both the Upper Susquehanna and the Schoharie drainage are moraines, pitted outwash, kames, kame terraces, hanging deltas, etc., all of which show only slight alteration by postglacial erosion. Even the end moraine at the Timlin site is well preserved. We suggest that interested readers consult the works of LaFleur (1969), Coates (1974; CA 18:588), Fleisher (1977a, b), Kirkland (CA 18:544-46), and Krall (1977). Godfrey and LaFleur (n.d.) are preparing a report on the geology of the Timlin site area.

Carter notes the plethora of dating techniques now available, so we ask why they are not used at the Timlin site. In fact, Raemsch (1977b:11) reports radiocarbon dates ranging from A.D. 1000 to A.D. 1800 but rejects them as inapplicable.

Carter notes that “leading archaeologists” now “admit . . . a 20,000-30,000 year probable antiquity for man in America,” but this fails to prove the claim for the Timlin site—as he notes. He neglects to observe that we all “admitted” this possibility at a conference he attended in 1976. Ritchie (1977b:122) said: “Most professionals would agree with you that we are going to be surprised by some early find someday that we can accept,” and Funk (1977b:34) asserted, “I am personally inclined to the belief that cultural remains dating back to an interstadial period about 30,000 years ago will eventually be found. But such sites will almost certainly be located outside glaciated parts of North America.” Cole (1977:131) said, “I am not at all biased against the possibility of the 12,000-year cutoff date being wrong.” Godfrey (1977:137) said, “Of course, it could have happened, but that is not the essential question [which is] ‘Have we good evidence for early man at the Timlin site?’”

Carter refers to “finds from within and under the glacial drift,” but in fact provenience information is very sketchy and contradictory. Raemsch (pp. 157-58) repeatedly mentions artifacts “on a till surface,” and he has claimed to have “Early Archaic” remains under till deposits (1977b:9-10). An adze is reported to be associated with till when it is actually a surface find in a stream bank, and Raemsch speaks of trying to find living floors in a till deposit—an impossibility in a secondary deposit, of course. A geologic diagram (Raemsch 1977b:2) seems to indicate that the modern floodplain dates to 53,000 b.p. At the site Raemsch indicated that he had no
photographs or diagrams specifying the provenience of the "handaxe" allegedly imbedded in a bovid radius (again, this presumably in a secondary deposit).

Raemsch (p. 157) seems to admit to considerable disturbance at the site suggesting that Holocene contamination is quite possible as an explanation for the presence of artifacts on or in tills. The till surface, at least, seems to be contaminated with Holocene and even historic materials. How, then, are recent or historic materials distinguished from "ancient" artifacts except by undefined selective views of evidence?

Raemsch (p. 158) claims to be excavating an area "removed from" the "Holocene floodplain," but fails to mention well-defined floodplain silts overlying gravel at his excavation which we brought to Vernon's attention during our site visit in 1977. Records of proveniences and associations simply have not been produced, and claims seem to fluctuate. Association with till, as opposed to reworked floodplain gravels, has not been satisfactorily established (leaving aside the question of whether the actual tills and artifacts have been identified properly). Carter thus starts with an assumption of geological context which is probably erroneous and certainly unestablished.

As Carter notes, reference to newspaper reports for archaeological information is unsatisfactory, but that has unfortunately been the primary medium for Raemsch's "early man" claims. The supporting data, if any, remain essentially unpublished. Raemsch (1976) published a letter commending the accuracy of one such story he now claims to repudiate (Willcox 1976), saying the author "did a superb job." He told Cole he went over McDonald's (1975) National Observer story line by line, and he allowed it to be reprinted at least twice by his college without modification. What in his many press releases would he now modify or rescind? Where is alternate information available in more detailed scholarly format? Summary statements (CA 18:97–99, Stagg, Raemsch, and Vernon n.d., Timlin and Raemsch 1971) add little insight. Vernon's (1977b) symposium paper is unclear and seems to contradict Raemsch (1977b) on crucial points (cf. their contradictory geological columns for the Timlin site). Raemsch's latest "detailed" report (1977b) is little more than a reworking of press claims.

Carter asks why critics concentrate arbitrarily upon the Northeast in this discussion. We do so because that is where the Timlin site is located. Putative Wisconsin-age sites in California are irrelevant to Raemsch's claims.

The question of "a high emotional charge" in our previous replies (Carter, p. 160; also p. 159) should be examined in a perspective which includes Raemsch's most recent reply and earlier responses to disagreement or question. Were we thin-skinned we might consider rather emotional his opening comment that our criticism is "specious and totally lacking in basic knowledge of the material treated" (p. 157). That Funk "has resisted accepting even the possibility of the great antiquity of man in the New World for years" (p. 158) has already been shown to be nonsense. The image (p. 157) of Cole and Godfrey unable to read Raemsch's Adequentaga site report (1970) with its discussion of Holocene evidence is slightly humorous. It may be rather murky, but despite Raemsch's denial now that report clearly discusses "tools in glacial deposits of Wisconsin age" and features a section headed "The Glaciated Tools" to which Cole and Godfrey referred when they alluded to Timlin-Adequentaga comparisons.

At a conference in 1976 we tried to explore Raemsch's claims calmly. To use Carter's phrase, "more heat than light" was evident in responses to questions. Vernon noted (1977a: 130), "You can say Where is your proof that this is pre-Wisconsin?" I would say, 'Where is your proof that it is not?'" Raemsch's colleague J. Timlin (1977: 136–37) said, "For six years I have been working on this site. We have dug there. Unless you dig on a site and unless you dig down to our levels and find this material, you won't believe it. You have to do it. You have to be there, so I say, 'Vas you dere, Charlie?'" and "The stratigraphic records are right in front of you out there." And so it went each time we tried to obtain documentation of these claims or asked to see artifacts. Carter, Raemsch, and Vernon do not take into account the proceedings of that conference (Cole and Godfrey 1977) in their replies, although they maintain the same level of heat and light.

Elsewhere Carter (1976) has deplored appeals to "experts" rather than evidence, and we thoroughly agree with him. Yet he falls into the trap himself when he asks us to accept Raemsch's work because he has a "Ph.D. in anthropology" (p. 159). That is irrelevant—but also untrue (it is in American civilization).

Carter chides critics for commenting without visiting the site or handling the artifacts. In fact we have all visited the site, but this is irrelevant because as scientists we have to respond to the published evidence; we are not required to visit the site or dig in it, because site investigators are responsible for documenting and communicating their work to colleagues. We have not visited Abri Pataud, for example, but rely upon evidence from Movius and others.

We care about this odd debate because we care about the nature of anthropology and would like to see it conducted scientifically. Issues such as the earliest lemurs in the New World are as exciting to some of us as the anthropocentric hominid controversy. The shopworn "frontier" metaphor should not obscure the problems of a weak argument. As Carter and Raemsch stress, workers "on the frontier" (p. 158) may be on to something most scholars reject, so we should try to keep open minds to even the most poorly argued claims. But our reactions to Raemsch and Vernon's claims have to be based on the evidence available, not their remote "possibility" or the possibility of "early man" evidence elsewhere in the New World. "Frontiersmen" are not justified in dispensing with scientific method, logic, and theory, the only criteria by which their claims can be evaluated.

Or is anthropology still a mere forum for eccentric speculation?

[Debate on this topic in the columns of CURRENT ANTHROPOLOGY is now closed.—Editor.]

References Cited


