testing procedures. If the number of cross reactants is reasonably constant across all populations and if the number of specific reactants varies widely with the population, then the *percentage* of confirmable positives compared to nonconfirmable positives will vary widely.

In our study we found 19 ad-type serums and 13 ay-type serums, very similar to our previous study where the distribution was 47 ad and 34 ay. Vyas *et al.* state that this "reflects a distribution of ad and ay subtypes normally not found in unselected donor population." We believe our findings are correct, and suspect that they are normal.

The cost and value of testing for HBAg have been subjected to much discussion. Hopefully, the value scale does not depend on whether one is a recipient of a blood transfusion, a blood donor, or a blood bank scientist. The primary purpose in hepatitis B screening of blood is to protect the recipient. It is our opinion, based on these and other data, that at the present time RIA does this better than CEP. Measures must be taken to protect the donor also, but not at a risk to the recipient. Until all commercial hepatitis tests are free of false positives, confirmatory testing, such as neutralization, will be necessary and good judgment is essential.

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25 October 1973

Artifacts of Early Man in the New World

Vance Haynes, in his article "The Calico site: Artifacts or geofacts?" (*1*), makes the important point that the maximum age of the site is still very much in doubt, although the minimum age was established by carbon-14 tests at the University of California at Los Angeles as "greater than 50,000 years" (*2*, p. 15). He is also quite right in insisting that the products of the natural processes to which the chert fragments were subjected require more study than that presented so far by Oakley (*3*) and Jelinek *et al.* (*4*). However, I believe he is neglecting the factor of logically patterned flaking also pointed out by Oakley (*5*) in distinguishing artifacts from geofacts.

For example, what would be the probability of natural processes producing a series of 8 to 15 flakes detached alternately from opposite sides of one edge of a piece of chert, each flake of very nearly the same size—that is, produced by the same amount of force applied alternately in opposite directions? Flakes of varying sizes and random alternation would be expected, but the Calico collections have many specimens of cherts with a long series of edge flake scars of very similar size and precise alternation. I personally witnessed the removal of one such piece from section J-13, on the 306- to 309-inch level below datum (1 inch = 2.54 cm), last winter.

Natural processes in the alluvial fan might well produce a few cherts shaped like primitive tools, but the number of tool-shaped cherts of set and regular Paleolithic patterns found in the really very small area excavated so far would appear to be overwhelmingly beyond the probability of accidental fabrication. Over 600 "tools," plus another 1500 technically diagnostic flakes, were selected by L. S. B. Leakey before his death.

One would expect that natural processes producing such pieces would form them indiscriminately from any quality of chert. The "artifacts" selected by Leakey as undeniably man-made have all been of excellent quality stone, although both good and poorer quality material is present in abundance. Such selectivity is characteristic of man—is it characteristic of nature?

In another aspect of patterning, it appears that Haynes is in error. The artifacts are found only in the lower Yermo formation, as he says, but they are not randomly distributed, either vertically or horizontally. There are definite concentrations at 2 and at 3 m below the juncture with the sands of the upper Yermo formation. They are also concentrated toward the northwest corner of master pit 1 and the southeast corner of master pit 2 (*2*, pp. 39, 40, and 42). The center of the site may well lie between the two pits.

When Haynes says, "No specimen from Calico is as obvious an artifact as, for example, a typical Chellean hand ax, a Levallois flake, or a Mousterian point" (*1*, p. 307), he is unfortunately expressing as fact what is only an opinion. Leakey disagreed with him here, as did François Bordes, the French expert in Paleolithic tool types and technologies, who was particularly interested in the diagnostic flakes. Agreeing with Leakey and Bordes are Pierre Biberson of the Institut de Paleontologie Humain, Paris, and Yves Coppens of the Musée de l'Homme, also in Paris. Because of differences in their training and viewpoint, it seems to be easier for those experienced in the prehistory of the Old World to accept the Calico chipped flints as man-made; however, the flints are now accepted as man-made by at least 75 percent of the professionals who have visited the site and inspected the collections (*6*).

Haynes is to be commended for bringing this controversial site to national attention. Now that the possibility of very early man in the New World has been broached, a number of people are actively searching for more evidence, believing they will find it. Let us hope that some of the new sites will be easier to date geologically.

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5. "As a general rule, naturally chipped flints are easily distinguished from the works of man, for they lack logical design, flake scars occur in uneconomical profusion, the edges have a bruised appearance, and the flake surfaces are usually scratched" (*3*, p. 18).
6. R. D. Simpson, personal communication.

23 August 1973

Wade's comment is an excellent example of the problems encountered in interpreting the Calico collections. Much depends upon the opinions of the observer, and different observers have different opinions, even in regard to what is considered fact.

In the stated case of "many specimens of cherts with a long series of edge flake scars of very similar size and precise alternation," the facts are (i) they are made of chert and (ii) they have a series of edge flake scars. The

opinions are (i) scars of very similar size and (ii) precise alternation. It is in these opinions that scholars disagree, especially if the qualitative statements regarding size and precise alternation are not backed up by quantitative data.

Regarding the more than 600 "tools" selected by the late L. S. B. Leakey, it should be pointed out that Leakey's selection is made from a larger selection made by the excavators and that he did not personally inspect each of the thousands of pieces that the excavators rejected as not being demonstrably artifacts. It would be instructive to know (i) the total number of pieces of chert excavated and (ii) the number selected from this total population by the excavators for presentation to Leakey for his selection. It is apparent that both the number of "tools" and the individual specimens selected will vary significantly with the authority making the selection.

I disagree with Wade's opinion that natural processes producing flaked cherts "would form them indiscriminately from any quality of chert." The physical properties that make a piece of chert knapable are the same regardless of whether man or nature is doing the knapping; therefore, those pieces that are more homogeneous, with fewer flaws, and more glasslike in regard to pressure and percussion fracturing are going to be the ones showing the most flake scars in a natural mixture.

Regarding the concentrations of artifacts in the lower Yermo formation, there should be published quantitative data on patterns of distribution and relative frequencies of artifacts, chert clasts, and nonchert clasts in various size ranges per unit volume of the deposit. Unfortunately, the only published data in this regard are mostly generalized statements such as "a tremendous concentration of material occurs at 6 and 9 feet below the fan surface" (*1*, p. 39). In the only published stratigraphic section I know of, I see no obvious concentration at 2 and at 3 m below the ancient fan surface (*2*, p. 72). The distribution appears to be random over a vertical interval of at least 8 m.

Wade is absolutely correct that my statement regarding some of the more diagnostic Paleolithic artifacts is an opinion. It was not meant to be anything else and is based upon examination of *typical* specimens in various parts of Europe and Africa. But opinions, no matter how experienced the authority may be, do not constitute proof, as the Calico site controversy so ably demonstrates. As for the opinion poll of "professionals" offered by Wade, it is indeed fortunate that we do not settle scientific questions by the votes of experts. No doubt others could come up with a three-quarters majority who would *not* accept the flints as man-made, and the names of the authorities would be just as impressive as those mentioned by Wade.

The point of my article is that the evidence for human manufacture of flints at Calico Hills is not compelling and is not likely to be at a site where more than 10 percent of the matrix of the controversial specimens is composed of the same chert, and where all were deposited in a sedimentary environment in which few, if any, angular pieces of chert are apt to have survived unchipped (*3*).

In conclusion, I would add that, while the possibility of very early man in the New World is real, it can only be established as fact where incontrovertible evidence is found. Were this not true, then the case for very early man in America would have been won in the last century (*4*), and maybe in the opinion of some it was.

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Low-Temperature Thermal Decomposition of Water

It is eminently worthwhile to consider chemical processes whereby hydrogen could be generated directly from a primary heat source, so that hydrogen could be efficiently used as an energy carrier and as a versatile, clean fuel (*1*). However, the three-reaction process that Abraham and Schreiner (*2*) have proposed is not achievable.

The authors point out that the critical reaction is

$$LiNO_2 + I_2 + H_2O \xrightarrow{300^\circ K} LiNO_3 + 2HI \quad (1)$$

and appear to be discussing this reaction in aqueous solution. The solution will be acidic and nitrite will almost completely be in the form of the weak acid HNO_2 (dissociation constant = 4×10^{-4}). The appropriate electrode potential for the oxidation

$$HNO_2(aq) + H_2O(l) \rightarrow NO_3^-(aq) + 3H^+(aq) + e^- \quad (2)$$

is -0.94 volt (aq, aqueous; l, liquid). For the overall reaction

$$HNO_2(aq) + I_2(c) + H_2O(l) \xrightarrow{298^\circ K} NO_3^-(aq) + 2HI(aq) + H^+(aq) \quad (3)$$

the standard Gibbs free energy ΔG^0 (298°K) = + 18.8 kcal/mole (c, crystalline). Reaction 1 will not go in acid solution.

The "oxidation-reduction potential of the nitrite-nitrate couple" to which Abraham and Schreiner refer is that observed in basic solution, where hydriodic acid would be neutralized to iodide (which in essence provides the driving force for the reaction). Thus the reaction proposed will not proceed in acidic solution and will not produce the desired HI in basic solution. In actuality, nitrous acid oxidizes iodide to iodine in acid solution.

There are review references to oxidation of nitrous acid by iodine (*3*). But the literature (*2*, *4*) describes measurements made in neutral or basic solution.

If reaction 1 were to be attempted heterogeneously, with $LiNO_2(c)$ and $LiNO_3(c)$, then enthalpy and Gibbs free energy changes, ΔH and ΔG, for reaction 1 would be 62.3 and 45.0 kcal/mole, respectively. (Entropies for lithium compounds must be estimated, but it is clear that this reaction is impossible.)

A possible modification to the proposed reaction might be to make the solution basic by adding ammonia. If $NH_4I(c)$ could be isolated from the solution, it might be decomposable under appropriate high-temperature conditions as follows

$$NH_4I(c) \rightarrow NH_3(g) + H_2(g) + I_2(g) \quad (4)$$