

## W F LIBBY AND THE ARCHAEOLOGISTS, 1946-1948

GREG MARLOWE

Department of History, University of California, Santa Barbara

**ABSTRACT.** Archaeologists began to participate in cross-disciplinary endeavors in the 1930's, albeit on a very limited basis. The passage of time found members of that discipline unprepared for collaboration with physical scientists when W F Libby announced the development of the radiocarbon dating method. Libby proposed to apply to

archeologic and geologic samples techniques based on ideas that were completely foreign to archeology. . . . The initial reactions of archeologists were sometimes amusing but more often significant, for they led to the foundation and emergence of the radiocarbon chronology that has so profoundly affected our understanding of prehistory (Johnson, 1967, p 165).

To date, our historical knowledge about the nature, function, and impact of the early (1946-1948) relations between Libby and American archaeologists has come to us in the form of published anecdotes, many of which contain inaccurate information. The author's access to W F Libby's private  $^{14}\text{C}$  correspondence, combined with data obtained from interviews with some of the principal participants throughout this period, offers many new or different insights into the nascent years of radiocarbon dating. When, and under what unexpected circumstances, did Libby first encounter representatives of the archaeological community? What strategies were employed to facilitate diffusion of knowledge about  $^{14}\text{C}$  dating across disciplinary boundaries? How did archaeologists respond to the introduction or "intrusion" into their field of Libby's radioactive age-measurement tool?

When W F Libby initiated his radiocarbon dating project at the University of Chicago in the fall of 1945, he decided to pursue it in secrecy. That policy remained in effect until May, 1947 (Anderson and others, 1947). Yet, Libby broached the subject of the ultimate goal of his research during a Christmas party in 1946. Also in attendance at that gathering was James Arnold. From February through June of that year, Arnold had worked under Libby's direction isolating the first millicurie of reactor-produced  $^{14}\text{C}$ . Not until that December evening, however, was Arnold cognizant of Libby's plan to date archaeological history with an isotope of carbon.

A few days later, Arnold departed from Chicago, travelling to his parents home to spend the holidays. While there, he conveyed this exciting information about Libby's research to his father, A S Arnold. Although a lawyer by profession, he also possessed a working knowledge of Egyptian archaeology.

Upon returning to the University of Chicago in early 1947, Arnold found a package and a letter waiting for him. Both items had been sent by Ambrose Lansing, the Director of the Metropolitan Museum in New York. Lansing's letter explained about the contents of the parcel.

As you know, your father called on me some time ago with the suggestion that you might be able to determine the approximate age of organic materials through techniques which you have developed in your atomic energy studies. The whole business is Greek to me (or rather, Chinese, since I do know some Greek), but I am very glad to furnish you with the samples and have sent you in a separate package eleven different items, the boxes which contain them as per the enclosed list. My own copy of the list bears the dates according to our chronology and I shall be very much interested to see what your findings turn out to be when compared with our own records.

I realize, of course, that exactitude is impossible in the circumstances, but even if we could get scientific evidence for even an approximate date in the period before 2000 bc it would be of considerable use to us. As you may know, there is still some argument as to the dating of the Old Kingdom for which period we have no astronomical data.<sup>1</sup>

James Arnold was shocked by what had transpired, especially since his father's communication with Lansing had taken place "without my knowledge." Furthermore, Arnold realized full well that "this was all ludicrously premature." For example, <sup>14</sup>C had not yet been discovered "in nature." Hence, Arnold "offered to send the samples back and spare Libby the embarrassment of explaining. I felt that this was all my fault. Libby's response, however, was a silent one. He took the package and simply placed it on the shelf of his desk." (Taped interview with James Arnold, May 24, 1976)

Thus occurred in January 1947, the first in a series of diverse encounters between the "physicists" and the "humanists."<sup>2</sup> During the next two years, as efforts were made to establish the efficacy of the radiocarbon dating system, Libby and his two assistants, Ernest Anderson and James Arnold, began to collaborate with archaeologists in an attempt to provide that discipline with more precise chronologic specification. What strategies were employed to diffuse knowledge about this new dating tool across disciplinary borders? How did archaeologists respond to the introduction or intrusion into their field of Libby's radioactive age-measurement technique?<sup>3</sup>

Within weeks following the "Lansing incident" Libby lowered the veil of secrecy surrounding his work. A paper delivered at a private seminar at the University of Chicago's Institute For Nuclear Studies revealed the possible dating applications of Libby's <sup>14</sup>C research. Among the few individuals present was Harold Urey. A Nobel Laureate in Chemistry for his discovery of heavy water, Urey would shortly assume an instrumental role in Libby's next unsolicited contact with another representative of the archaeological community, Paul Fejos.

Hungarian born and holding a degree in medicine, this "self-taught ethnologist and archaeologist" was the Director of Research for the Viking Fund for Anthropological Research, a New York based foundation devoted to the support of "field and systematic research in anthropology and allied sciences" (The Viking Fund, Inc, 1952, p 7). Fejos envisioned

<sup>1</sup>W F Libby <sup>14</sup>C correspondence, Ambrose Lansing to James Arnold, January 16, 1947.

<sup>2</sup>The use of the word "humanist" to classify the intellectual orientation of most American archaeologists during the 1940's is borrowed from Frederick Johnson (1967, p 165). It is an appropriate term to dramatize the myriad problems involved in communicating information between "two cultures;" that is, relating to archaeologists knowledge about the physics and chemistry peculiar to <sup>14</sup>C dating. If one accepts the contemporary observation (MacNeish, 1978) that archaeology has yet to achieve the status of a social science, then further justification can seemingly be offered for applying the concept of "humanist" to this specific historical context.

<sup>3</sup>Archaeologists were not the first scholars to seriously question the efficacy of radioactive age-measurement techniques. For two useful analyses of the reactions of biologists and geologists to the Rutherford-Boltwood uranium-lead dating process see, Badash (1968) and Burchfield (1975).

the Fund's role to be that "of a pioneer in new approaches — the risk bearing areas of research — involving cooperative and cross-disciplinary research" (The Viking Fund, Inc, 1952, VII). In fact, suggests his biographer, John Dodds, Fejos "liked 'risk' projects, the subsidization of ventures that no other foundation would touch" (Dodds, 1973, p 92). Commensurate with Fejos' conviction about the value of inter-disciplinary collaboration was his belief that "ignorance and indeed contempt for sophisticated instrumentation was a defect of those engaging in field work" (Dodds, 1973). As one result, the Viking Fund began to encourage the utilization of magnetometers to explore archaeological sites.<sup>4</sup>

Fejos' perception that all learning "is a seamless garment" proved invaluable in the summer of 1947. He received word that the Dutch paleo-anthropologist Ralph von Koenigswald, presumed dead, was actually alive and well. Famous for numerous discoveries, including 'Java Man' and 'Solo Man', Koenigswald soon arrived at the Cold Springs Harbor Laboratory in New York. During lunch hour one day, a stranger initiated a discussion with him. When Koenigswald mentioned that the 'Solo Man' skull was approximately one-half million years old, the man replied, "Too bad it is so old. If it were younger, I could have told you its exact age." Later, Koenigswald informed Fejos about that rather bizarre conversation. His curiosity aroused, Fejos ultimately determined that Koenigswald's previously unidentified noontime guest was none other than Harold Urey. The two anthropologists then decided to travel to Chicago where they met Urey. In turn, he introduced Fejos to Libby. Apprised of the Viking Fund's interest in his research, Libby agreed to accept Fejos' offer of financial support if sufficient progress on the <sup>14</sup>C dating work was forthcoming in the next several months.<sup>5</sup>

By that autumn, Libby could offer the observation that "although we are not entirely certain that we will succeed, we (now) see no great likelihood of our failure."<sup>6</sup> Thus, on October 31, Samuel Allison, Director of the Institute For Nuclear Studies, wrote to the Viking Fund on Libby's behalf, formally requesting financial assistance as well as outlining, in general terms, Libby's plans for the ensuing months.

The tasks immediately ahead are the development of improved methods of detection of radiocarbon, the construction of, or arrangement for borrowing the use of, a thermal diffusion plant for the isotopic enrichment of the samples, the completion of tests on the method with dated samples, and the training of men to carry the work on in archeology and other fields. We estimate that the first year's work could be carried on successfully with a grant of \$13,000. . . .<sup>7</sup>

That Allison's letter elicited a rapid response is indicated by the fact that on November 5 the Fund's Board of Directors mailed to him a check for \$13,000. Interestingly enough, that grant was awarded in

<sup>4</sup> Fejos often brought new ideas to the attention of anthropologists on the occasion of a Viking Fund Supper Conference. For example, on October 17, 1947 Hans Lundberg discussed "New Possible Applications of Geophysical Methods in Archaeology" (Viking Fund, 1952, p 49).

<sup>5</sup> The full version of Fejos' colorful chronicle is contained in Dodds (1973, p 88-89).

<sup>6</sup> W F Libby correspondence, Libby to Paul Fejos, November 11, 1947.

<sup>7</sup> W F Libby correspondence, Samuel Allison to Paul Fejos, October 31, 1947.

the names of both Libby and Urey, even though the latter had no direct involvement in the  $^{14}\text{C}$  research.<sup>8</sup> More attention shall be directed to that point.

Two other decisions were made during November, with each one specifically designed to facilitate future collaborative efforts. On November 11 Libby notified Fejos that James Arnold, then a National Research Council Fellow in Chemistry at Harvard University, would become the senior research man on our project. Dr Arnold has a real interest in Egyptian archaeology. I consider that we are very fortunate in being able to gain the services of a man of this calibre. It is just a coincidence that a physical chemist of his ability happened to have this interest in archaeology.<sup>9</sup>

Arnold's value to the carbon dating work was further evidenced by Libby's ready and frequent admission that he "personally had no competence in the field of archaeology."<sup>10</sup>

In the same week that arrangements were finalized for Arnold's participation in the  $^{14}\text{C}$  dating program, Fejos initiated preparations for Libby's appearance at a Viking Fund Supper Conference. Those functions, Fejos has noted, were generally held

every two weeks, on Fridays, which meant that we invited all of the anthropologists on the Eastern Seaboard to come over here. At these affairs they got cocktails and dinner, and there was discussion until 11 o'clock, and then they all went home. We paid their costs (Dodds, 1973, p 101).

The selection of the Supper Conference as a forum for conveying, for the first time, Libby's novel idea to a large group of archaeologists stemmed, in part, from Fejos' recognition of the problems arising when transmission of information between specialties occurs. Hence, an informal mode of communication was deemed imperative because "unfamiliar jargon or concepts could be elaborated in more familiar terms."<sup>11</sup>

On November 17 Fejos formally requested that Libby come to New York. In his letter of invitation he warned that "it would be advisable if the section of your talk dealing with physical chemistry could be on a popular level as most of the anthropologists have little or no training in natural sciences."<sup>12</sup> In a further attempt to ensure that Libby might speak to an attentive audience, Fejos asked whether Urey "would be able to be present."<sup>13</sup> That inquiry was most assuredly based upon the knowledge that

new ideas are more readily accepted from sources that have already demonstrated their reliability. The more unusual the information, the more essential it is that its importance be emphasized by someone whose professional credentials are above suspicion (Meadows, 1976, p 260).

This is not to suggest that Libby lacked standing in the scientific community. He had experienced a very productive career prior to becoming the youngest full professor in the Department of Chemistry at the University of Chicago after the end of World War II. Nevertheless, unlike

<sup>8</sup> W F Libby correspondence, Paul Fejos to Samuel Allison, November 5, 1947.

<sup>9</sup> W F Libby correspondence, Libby to Paul Fejos, November 11, 1947.

<sup>10</sup> W F Libby correspondence, see, eg, Libby to Thorne Deuel, April 12, 1949.

<sup>11</sup> For a useful historical analysis of this problem, see Meadows (1976).

<sup>12</sup> W F Libby correspondence, Paul Fejos to Libby, November 17, 1947.

<sup>13</sup> W F Libby correspondence, Paul Fejos to Libby, November 17, 1947.

Urey, Libby did not yet possess either a national or an international reputation outside of his own scientific specialty, physical chemistry. Urey, on the other hand, aside from already having received a Nobel Prize, was well known and respected in several other disciplines through his research on a number of important scientific problems, including the separation of isotopes, the chemistry of the solar system, and the development of the paleo-temperature scale.<sup>14</sup> This, then, was a prime reason why Fejos thought it important that Urey be knowingly associated with the <sup>14</sup>C dating project, an undertaking Libby has appropriately characterized as “both highly unusual and unbelievable to even some of the most enlightened individuals.” (Taped interview with W F Libby, September 29, 1977; see also, Libby, 1975)

Libby delivered an hour-long lecture at the Viking Fund nearly two months later, on the evening of January 9, 1948. His audience, numbering more than one hundred, was an unusually large one.<sup>15</sup> Since Fejos considered Libby’s research “of the utmost importance,” the occasion of the Supper Conference had been scheduled simultaneously “as a General Meeting which would include scholars from the various fields of anthropology.”<sup>16</sup> Among those individuals present were: A S and James Arnold, Junius Bird, Gordon Ekholm, Richard Foster Flint, Frederick Johnson, Ralph Linton, Froelich Rainey, H L Shapiro, W Duncan Strong, and Aristid von Grosse, to name just a few.

The only previously published exposition of the events of that evening’s activities comes to us from Fejos. After dinner, Libby “gave a high talk on a rather low level (which) I think anybody could have understood. . . .” Yet, when his presentation was completed “nobody rose for discussion.” Despite prodding from Fejos, silence prevailed. Only after the Yale University geologist, Richard Foster Flint, suggested that his Pleistocene research might benefit from the application of Libby’s radioactive dating technique did the anthropologists respond. Suddenly, “everybody wanted some things of their (sic) dated” (Dodds, 1973, p 101).

A good portion of Fejos’ account, as related by Dodds, is, to use James Arnold’s words, “as inaccurate as it is colorful.”<sup>17</sup> Therefore, several points require amplification. First, examination of the text of Libby’s lecture does seem to indicate that he remained mindful of the need to convey his ideas in a clear and comprehensible fashion. Yet,

<sup>14</sup> For two analyses of the wide-ranging prestige and influence enjoyed by Urey, see, Kohler (1977) and Meadows (1976).

<sup>15</sup> The author has not yet determined exactly how many individuals were present that night. The guest lists contained in Libby’s files indicate a number exceeding one hundred. However, as Johnson notes, those affairs “were open, and everyone was welcome.” Some anthropologists who attended the meeting were not included on the guest roster, while others listed “never came to any meeting.” Personal correspondence, Frederick Johnson to Greg Marlowe, July 28, 1979.

<sup>16</sup> W F Libby correspondence, Paul Fejos to Libby, November 17, 1947.

<sup>17</sup> Personal correspondence, James Arnold to Greg Marlowe, September 11, 1978. It is possible that one of the reasons a number of inaccuracies are included in Fejos’ published reminiscences stems from the likelihood that the taped conversations, the source of this information, were not edited.

it is misleading to suggest that “anybody” could have understood them. As Frederick Johnson comments,

aside from Jim Arnold and (Aristid von) Grosse, there was no one there who understood the physics and the chemistry. Fejos had briefed (Richard Foster) Flint and me separately so we were neither shocked nor surprised. Still, I only barely understood the gist of what Libby had to say. I suspect that Flint was not much better informed (Parentheses mine).<sup>18</sup>

Another factor which influenced the response to Libby’s speech involved the “background of status, politics, and human frailties” in American archaeology. From its inception, the Viking Fund was engulfed by controversy. In 1941, Axel Wenner-Gren established the Fund by offering an endowment of \$2½ million. That gift represented an amount of capital that could not be withdrawn from the United States since the Internal Revenue Service had a suit pending against the Swedish industrialist. A short while later, Wenner-Gren was “blacklisted” by the U S State Department for alleged collusion with representatives of Nazi Germany. Those charges were never substantiated. Yet, the Viking Fund, and by implication Fejos, was considered by some people to be the source of “dirty money.” Although Dodds (1973, p 3-4) acknowledges that such doubts about the legitimacy of the Fund’s activities adversely affected its image during the first few years of that organization’s existence, his analysis fails to fully assess the residual impact resulting from that clouded past. Following the termination of World War II, various anthropologists continued to solicit financial support from that foundation “even while making the nastiest of accusations concerning the donor.” Correlatively, many anthropologists within commuting distance of the Fund’s New York facilities came to the Supper Conferences “mainly to get a good meal and some social contacts while contributing little or nothing to the discussions,” Frederick Johnson recalls.<sup>19</sup>

At least one other aspect of the Fejos-Dodds chronicle merits comments.<sup>20</sup> Immediately after Libby finished his analysis of the current status of the <sup>14</sup>C research, its future potential, and of the need for testing archaeologic samples, most of those anthropologists in attendance did remain silent. However, the assertion that within a few moments “everybody wanted some things of their (sic) dated” is not only incorrect,<sup>21</sup> but it also falls short of describing the muddled atmosphere that prevailed. After a brief interlude following Libby’s presentation, Johnson remembers vividly, “a lively series of comments and discussion, some of it either irrelevant or exhibiting ignorance, quickly broke out.”<sup>21</sup> In part, that condition arose because the archaeologists, presumed by Libby to have the desired organic materials “under some chronological control” misunderstood the hypothetical nature of the request for samples. Libby and Arnold needed authenticated artifacts in order to test the efficacy of the method, but many of the archaeologists were not able to draw

<sup>18</sup> Personal correspondence, Frederick Johnson to Greg Marlowe, May 1, 1979.

<sup>19</sup> Personal correspondence, Frederick Johnson to Greg Marlowe, May 1, 1979.

<sup>20</sup> There are other important factual errors or instances of omission. See, eg, footnote<sup>30</sup>.

<sup>21</sup> Johnson, May 1, 1979.

the distinction between the basic hypothesis indicating probability of application and their own initial assumption that the dating technique was indeed valid. Predictably, aside from those individuals who expressed some form of interest in Libby's work, there were those, suggests Johnson, "who questioned whether or not this was a sound proposal rather than some fly-by-night idea from a wild and woolly physicist driven mad by the exigencies of the Chicago stadium operation."<sup>22</sup> Ultimately, though, the confused discussion was led "step by painful step" to the notion that some consistent mechanism for selection of datable samples was necessary.

The experience derived from his January sojourn to New York left no doubt in Libby's mind but that the rank and file of the archaeological community would require a substantial amount of education if radiocarbon dating was to become a useful tool. Nevertheless, in a number of other respects Libby's presence at the Supper Conference can be evaluated in more positive terms. First, as he later informed Fejos, "the discussion helped me to understand the archaeological aspects of the problem."<sup>23</sup> Second, by establishing a personal base of contact with several respected archaeologists, Libby made possible the fulfillment of a major and immediate goal, that of securing a supply of datable materials. But most important of all, the impetus had now been provided for the creation in February, 1947, of the Committee on Radioactive Dating, a body organized under the sponsorship of the American Anthropological Association. Appointed to serve on the Committee were three distinguished archaeologists: Frederick Johnson of the Peabody Museum in Andover, Massachusetts, Froelich Rainey, representing the University of Pennsylvania Museum, and Donald Collier from the Chicago Natural History Museum. The designation of Johnson as chairman of the <sup>14</sup>C Committee proved especially insightful. By his own admission, he was "deeply interested" in the possibility of establishing a reliable chronology of world-wide application "so essential to archaeology." Of even greater significance was Johnson's long-standing advocacy of collaborative endeavors. For a number of years he had "been struggling with attempts to develop interdisciplinary research."<sup>24</sup> By temperament and training, therefore, Johnson was ideally suited to work closely with Libby, who himself had collaborated with other specialists for more than a decade.<sup>25</sup>

Although offering assistance to Libby, Anderson, and Arnold early on, the "Johnson" Committee did not begin to function on a formal and active basis until the first months of 1949. By the previous fall, however, sufficient progress had been achieved on various scientific and technical levels to permit initial radiocarbon dating of "known" archaeological

<sup>22</sup> Personal correspondence, Frederick Johnson to Greg Marlowe, December 19, 1978.

<sup>23</sup> W F Libby correspondence, Libby to Paul Fejos, January 16, 1948.

<sup>24</sup> Personal correspondence, Frederick Johnson to Greg Marlowe, December 19, 1978. For further evidence of Johnson's prior collaboration with biologists, geologists, and paleo-botanists, see Johnson (1942) and Johnson and Raup (1964).

<sup>25</sup> To cite one example, Libby's research on low-level radioactivities at Berkeley in the 1930's encouraged forms of collaboration with biologists and oncologists.

specimens (Libby, Anderson, and Arnold 1949, p 227). Improvements in the dating technology, though, far outpaced efforts<sup>26</sup> to enhance the archaeologists comprehension of the chemistry and physics inherent in <sup>14</sup>C dating. One (among many) particularly troublesome consequence emanating from problems relevant to cross-disciplinary communication of information centered upon the existence of overly simplistic perceptions about the mechanics of Libby's radioactive age-measurement system. As Arnold recollects, "the attitude was pretty widespread that 'you guys have a machine, you put something in it, turn the dial, and it points to a date' — a kind of magic black box."<sup>27</sup> The reality of the situation was otherwise. For example, the radioactive counters, temperamental and in constant need of adjustment, were frequently inoperative.<sup>28</sup> Thus, during some months, there were but two or three days when acceptable counting rates were recorded. It was the persistence of that "black-box syndrome," combined with the fact that Libby and the Committee on Radioactive Dating had not yet formalized a program of collaboration, which eventuated in the first crisis to confront Libby and Arnold.

The initial artifact measured by Libby was taken from the package sent by Lansing almost two years earlier. <sup>14</sup>C analysis of the wood sample from the tomb of Zoser at Sakkara indicated an age of  $4650 \pm 75$  years (Libby, Anderson, and Arnold, 1949, p 227). The Chicago physical chemists then decided to date an object with an age about one-half that of the Zoser piece; something from the period circa 300 bc. Arnold, responsible for sample procurement, contacted John Wilson, a senior professor of archaeology at the University of Chicago's Oriental Institute. The remaining details of this incident have never been published,<sup>29</sup> but Arnold describes them in his own words.

(Regarding my inquiry, Wilson) said, as I expected, that it was an easy task, for there were just oceans of available material from the Ptolemaic period. He then furnished us with a sample. (Parentheses mine)

My Christmas was ruined that year, because the first measurement we made suggested an age of zero years. The results of the second run were identical, as were those of the third. Keep in mind, the undertaking represented a full month's labor in a very good month.

Naturally, we were rather upset for we were not yet totally confident that our method would work. Anderson's thesis (a world-wide assay of radiocarbon) had been brilliantly successful, but still, we had dated only one sample correctly. Something now appeared to be amiss.

<sup>26</sup> A few anthropologists attempted to enlighten their fellow brethren on such matters. See, eg, Merrill (1948).

<sup>27</sup> Arnold interview, May 24, 1976.

<sup>28</sup> Even a casual examination of Libby's laboratory notebooks reveals the frequent frustrations encountered, eg, notation by Anderson dated 11/21/47 — "NG (No Good) — counter is completely haywire." (Parentheses mine)

<sup>29</sup> A number of garbled versions of this incident were published. See, eg, Dodds (1973, p 102). In this particular instance Fejos described the details of an episode that "never took place," Arnold maintains. Except for the set of archaeological samples sent by Lansing, Libby and Arnold never analyzed "blind" artifacts during the period in question. Personal correspondence, James Arnold to Greg Marlowe, September 11, 1978.

Libby finally said to me one day in December, "Go tell our friend about the results and see how he reacts." I then went to Wilson's office and showed him our laboratory notebook. He looked at me, smiled, and remarked that our figures must be correct. I was furiously angry. (Parentheses mine)

What had occurred became obvious later. When the museum had originally opened, its curators had ample money to finance archaeological expeditions, but they lacked a sufficient supply of artifacts with which to begin operation. Hence, they arranged exchanges with other museums while also purchasing a few items from a 'reputable' dealer in Cairo. Of course, that was very wrong. These people should have given us a sample with an assured pedigree. But one of the instructive aspects about recalling this episode is to emphasize the point that these archaeologists were so confident about our method's reliability that it probably never occurred to them that they were actually testing us. In other words, their attitude was: our technique, and thus our analysis, was obviously valid, hence, we would either find that the sample provided was of this origin or that.

Well, I returned to our laboratory and apprised Libby of my conversation with Wilson. He was also angry, but expressed that emotion in a different manner. He asked me what I thought might be the most precious archaeological specimen in the museum's collection. After I gave him my opinion, he contacted Wilson on the telephone and demanded a portion of that item; a rather visible one.<sup>30</sup>

The aftermath of this episode proved significant in at least two respects. On the one hand, a valuable lesson had been learned. Libby and Arnold realized that they should not have been "so casual" in their approach to sample procurement.<sup>31</sup> More important still, the <sup>14</sup>C Committee would shortly assume a more active and coordinated role in the future development of radiocarbon dating.

Thereafter, concerning such matters as the proper selection of materials for dating, arbitration of disputes arising within the archaeological community, and the task of "selling" the dating method, Libby and Arnold relied heavily on the Committee. An examination of that topic must, of course, be reserved for another occasion. Suffice it to say, the history of the nascent years of radiocarbon dating indicates the success of that endeavor depended "to a large degree upon the character of collaboration" (Johnson, 1951) involving individuals from many disciplines. The events of 1946-1948 paved the way for that eventuality.

<sup>30</sup> Arnold interview, May 24, 1976. Evidence that Libby and Arnold received other archaeological samples of dubious origin is contained in Natural <sup>14</sup>C Group Sample Record #1 (laboratory records). An entry by Arnold refers to two other specimens sent to them on November 12, 1948 by "Watson Boyes (thru Wilson):" — "This was a piece of wood marked 962 'from a mummiform coffin from the Ramsseum in Thebes, Egypt.' Excavated by Pertie in 189?, its history (?) from then on doubtful, to say the least." A second notation appears as follows: "Wood from an Egyptian coffin dated in the Roman period (30 BC-284 AD). Piece marked 374, 22 g. What the letter does *not* say is that it was bought from a dealer."

<sup>31</sup> The next artifact measured by Libby and Arnold was a portion of a deck beam from a funerary ship obtained from the Field Museum in Chicago. Donald Collier, a member of the Committee on Radioactive Dating, arranged that transfer. "It could not possibly have been a fake," Arnold recalls (Arnold interview, May 24, 1976), "for the wood came from a thirty foot long boat, the excavation of which had been photographed from the beginning." Radiocarbon analysis of the "Sesostris" sample indicated an age proximate of the known one.

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