THOUGHTS ON THE SHROUD ^{14}C DEBATE

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My first title for this paper was "The Shroud ^{14}C Fiasco." On reflection I decided not to use it, although the sampling, testing and interpretation done in 1988 were certainly very badly designed and executed. Rather, this testing could be viewed, taking a very long and kind perspective, as simply the first in a series of tests of increasing sophistication to determine the radiocarbon age of the cloth and the degree of likelihood that its radiocarbon age can be translated into calendar age.

The 1988 exercise was certainly a fiasco in the ordinary and very real sense that it was badly done, and that the results led to a well-nigh universal re-appraisal of the Shroud as a medieval artifact. The ^{14}C dates have been given a hugely disproportionate importance by the general public and intellectuals, whilst being given too little importance by some Shroud researchers.

For the public in general and the intellectual community in particular, the question of the Shroud's age was settled by these dates. No amount of counter-argument, evidence or experimental data has made or will make any significant dent in this wide public perception that the matter is closed. It is a totally moot point and debating the subject further seems completely futile to me. This is of course a completely separate matter from the actual age of the Shroud, or the real credibility of the ^{14}C results. It is merely a statement of the percep-

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tion present in the minds of most people, and the high (albeit unwarranted) confidence placed in the ^{14}C dates.

On the other hand, Shroud researchers and devotees have been too eager to dismiss the results in favour of a "preponderance of evidence" argument, coupled with whatever hypotheses were available to explain away the results. Some of these hypotheses have been credible; others wildly implausible. The undeniable fact is that the results pose a tremendous obstacle to an argument that the Shroud is genuine or even from antiquity. No such argument will be seriously entertained at large until there is new data and clarification on the ^{14}C dating, based on new testing of Shroud samples.

As an archaeologist who has used radiocarbon dating on a regular basis for the last 30 years, my own position is that nothing has been proved about the age of the Shroud. However, as someone who has debated the Shroud ^{14}C results with colleagues and scientists of various backgrounds (see for example my debate with a practicing radiocarbon physicist at http://www.shroud.com/c14debat.htm), it is clear that, at this stage, further debate on the subject is largely a waste of time and energy.

If that is so, what purpose could be served by reviewing the Shroud ^{14}C debate? In this article I will comment on selected aspects of the debate which strike me as particularly meaningful or problematic, and which merit attention in the future. I will not attempt to recount the entire debate in any detail, a task which would require much more space than has been alloted here.

The 1988 Fiasco

The first point I would like to make is that the ^{14}C dating of the Shroud, like the relic itself, is unique. This is a point that I harped on repeatedly in the years preceding the actual test, but alas, to no avail. As there is no provenance for this extraordinary relic, and there are no other similar or associated objects that can be dated, there is no way to seek confirmation or refutation of the ^{14}C dates in the normal archaeological

manner. Furthermore, if it is really 2000 years old, it has been handed down over most of that time and kept in extremely different environments. And it was partly burnt several centuries ago. I know of no other object ever dated by ^{14}C that has such a history.

Another aspect of the Shroud dating that was unique, highly irregular and unfortunate, was the fact that the person or group (STURP) studying the object were not the submitters and interpreters of the result. The British Museum stated flatly at the outset that it was not undertaking a study of the object, and its official role was that of coordinator and notary -- a role which, I might add, it carried out spectacularly poorly. It was thus an oddity and a fiasco to see Cardinal Ballestrero and the directors of the ^{14}C labs vying for the interpretive role when the results were announced. It was clear that this was going to happen, and in a 1987 letter to Luigi Gonella I wrote:

"The major problem with the entire Shroud ^{14}C issue seems to me to be that, unlike all other archaeological and museum ^{14}C dating, there is no person or body officially collecting and submitting the samples... The labs seem to have put themselves in charge of the entire operation."

I had made a similar warning in my submission to Cardinal Ballestrero in 1985. It may not have seemed so important to them at the time, but many of the errors that were made in 1988 were brought on by this highly irregular testing programme.

The ^{14}C results on the one sample that was taken from the Shroud and split into several pieces are not conclusive proof of anything, and this fact should have been stressed at the time the results were announced. "Rogue dates" are common in archaeology and geology (see http://www.shroud.com/meacham.htm for a discussion), and they are usually not subjected to any further detailed study. Instead, the normal practice would be to seek more and better samples, obtain new ^{14}C dates and review the overall clustering pattern indicated by the dates. Such has been my experience as an archaeologist who has excavated, submitted and interpreted more than

one hundred ^{14}Csamples from Neolithic, Bronze Age and Early Historical sites. Of these dates obtained, 78 were considered credible, 26 were rejected as unreliable and 11 were problematic. (This data is published on my website at the University of Hong Kong -- http://www.hku.hk/hkprehis). I mention this merely to inform the non-specialist that rogue dates are quite common in the general use of ^{14}C in archaeology. Willi Wolfli, director of the Swiss lab which dated a Shroud piece, co-authored (Johnson et al 1985) the following in a similar vein, after a set of interlaboratory comparisons on freshly excavated samples:

"The existence of significant indeterminant errors can never be excluded from any age determination. No method is immune from giving grossly incorrect datings when there are non-apparent problems with the samples originating in the field. The results illustrated (in this paper) show that this situation occurs frequently".

It is important for anyone wishing to understand the normal use of ^{14}C to know that a single date or even a series of dates on a single object or feature is seldom if ever cited to answer important questions about the age of a culture or a site. To put the radiocarbon method in the position of being the ultimate arbiter of the age of the Turin Shroud is a blatant departure from the way ^{14}C is normally used. Unfortunately, the blame for this fiasco lies mainly on the shoulders of the extremely over-confident, over-bearing and haughty attitudes on the part of most of the ^{14}C lab directors who were involved.

In this article I was going to resist the impulse to say "I told you so," but that was before I skimmed over the book by H. E. Gove (1996), which is a surprising and offensive melange of fact, fiction, rant and self-trumpeting, peppered with ad hominem attacks on those who disagreed with him. I believe it will be clear from what follows how wrong he and most of the other ^{14}C experts were in their approach to this milestone test for the Shroud.

The pre-1988 Debate

Unbeknownst to many people, there was a considerable and often heated debate about the ^{14}C dating of the Shroud before the test was carried out in 1988. The main topics in this debate were: 1) whether there was any need to carry out the test at all; 2) how the test should be conducted, and 3) what was the possibility of contamination or other factors that might have an adverse effect on the result. Much of the discussion in "pro-Shroud circles" centered on the first topic above, revolving around questions of how reliable radiocarbon dating was, whether it was necessary since the Shroud was already believed to have a high likelihood of authenticity, and whether it was worthwhile to sacrifice even a tiny portion of the relic for the destructive test. Whereas this last consideration had some validity before the days of small sample technology, it should have been permanently laid to rest when AMS was developed. Unfortunately, it has persisted even in recent years, in the form of a byzantine stance which cannot be justified on any grounds, either scientific or religious/devotional, and which has had the most unfortunate consequences for the Shroud. I will return to this issue below.

In the scientific community, the debate centered around how the testing would be done, and in particular on the reliability and statistical accuracy of the results. There was huge amount of acrimonious debate over the number of labs to be involved, whether the tests would be truly blind, etc. These were certainly valid concerns, but it was focused too much on the wood and not the trees! Unfortunately, despite my harping on the subject, no attention was devoted to the possibility of contamination which might escape normal pretreatment. Equally unfortunately, no significance was being accorded to the fact that the Shroud had been through a fire and a series of other events that could conceivably affect its radiocarbon content. When I first became involved in Shroud research in 1981, I was appalled by many of the things being written by STURP and by the radiocarbon specialists about the Shroud's eventual ^{14}C dating. These concerns were summarized in my

Current Anthropology article (Meacham 1983) and further elaborated on in an article in Shroud Spectrum (Meacham 1986).

During the mid-1980s, I was extremely critical of the two proposals then being formulated and discussed: that of STURP and that of Gove/Harbottle. Neither of these proposals took seriously the possibility of contamination and heat-induced isotope exchange. In a submission to Cardinal Ballestrero made jointly by me and two Italian archaeologists (Maurizio Tosi and Roberto Ciarla) of the Institute for Near and Far East (IsMeo), it was argued that "in this crucial test awaited by millions of people, it is necessary to proceed with great caution so that the eventual result is the best that modern science can produce. " Specific proposals were made to insure that extensive chemical screening would be carried out prior to testing. Sadly, this did not happen. But at least the debate was beginning to focus on some of the crucial issues, namely the number and location of the samples to be tested.

The Sampling Strategy

In the run-up to 1988, a major debate took place over the choice of samples to be dated. From the earliest discussions on the possibility of ^{14}C dating of the Shroud, it was generally assumed that any fragment of the cloth would suffice. The Gove/Harbottle proposal of 1979 (at first supported by STURP) called for the Raes piece to be used – probably the most ridiculous idea of the entire saga. McCrone and Sox attempted to obtain from Raes the sample "which was kept in what looked like an old scrapbook for postage stamps." Eventually Gove and Harbottle accepted that credibility and chain of evidence required a fresh sample to be taken from the Shroud, but they proposed, and STURP concurred (at first), to use charred material under one of the patches as the sole sample to be divided amongst the labs. My strenuous opposition to the use of charred cloth led Harbottle to write to more than 40 practicing radiocarbon physicists, seeking their opinions on the proposal. Although Harbottle misrepresented somewhat my

major concern (which was not with the possibility that carboxyl groups present in the linen could have exchanged carbon with CO_2 of the atmosphere but rather that they might have exchanged carbon with contaminants then on the cloth), the responses he obtained were interesting. He wrote that "no one had any data directly testing the Meacham hypothesis" nor did any of the respondents know of any case in which a sample had been charred long after its lifetime, but well before being dated. Comments were obtained such as " the use of the charred material would pose problems," "there is the possibility of isotopic exchange with volatile or gaseous combustion products," "why take the chance?", etc. After lengthy discussion at the Turin conference in 1986, it was agreed that the charred material would not be used. This was achieved with the strong support of Alan Adler and Bob Otlet, both of whom were well aware of the problems that might be involved in ^{14}C dating the Shroud.

One of the main points of debate was the number of samples. A major divergence of views occurred over the sampling strategy. Strangely, the ^{14}C specialists insisted on having splits of the same single sample. It appeared as if they wanted above all else to achieve harmonious results amongst themselves, as opposed to any results that might indicate a variation of the Shroud's radiocarbon content. It was said that the reason for this was "to maximize the credibility of the enterprise to the public." This led to the most unfortunate and unscientific sampling of only one location on the corner of the Shroud (and a terrible choice of site at that!). It seemed to me that, if isotope exchange had occurred during the 1532 fire, it would most likely not be uniform over the entire cloth, and three samples from different sites would provide the best evidence about this possibility, and also for inter-corroboration of the results obtained. At the 1986 Turin conference which was convened to draw up a protocol for ^{14}C dating of the Shroud, no amount of pleading and cajoling by me and Adler could persuade the assembled radiocarbon luminaries that a minimum of two sampling sites should be proposed. They were supported by the Church representatives who naturally wanted to limit the

disturbance to the relic to the barest minimum. Only Otlet and Hedges supported the proposal. In exasperation, I suggested using the charred material as a second sample, only to be generally heckled about a seeming reversal of position.

As a result of the sampling strategy which was adopted, no hard data is available on the radiocarbon content of the rest of the cloth. An extrapolation from the corner piece to the rest of the cloth is only that – i.e. it is little more than conjecture. Ironically, even though the labs did finally all obtain and date the same sample, and did produce reasonably harmonious results, it can be argued that a possible thermal gradient can be observed from their data. It is therefore quite possible that significant variations do exist across the entire cloth. The work of Kousnetsov and collaborators (1996), Moroni and van Haelst (1997; see also the paper by Moroni et al in this volume), Jackson and Propp (1997; see also the paper in this volume) all suggest that the fire of 1532 could have altered the ^{14}C content in the cloth by "carboxylation" or isotope exchange in a manner not yet fully understood or repeated under laboratory conditions.

Contamination Prospects

When the conference was convened in Turin in 1986, there was great hope that a thorough and rigorous plan would be adopted to insure that the ^{14}C date on the Shroud was indeed the best that science in the 1980s could offer. This hope was dashed very quickly. At the meeting itself, most of the ^{14}C laboratory directors were adamant, and rather arrogant, in their claim that a totally reliable date, to within one or two percent accuracy, could be obtained if they could just get their hands on a tiny piece of the cloth. Their attitude toward the question of possible contamination, which I brought up several times, was highly and haughtily dismissive. Gove and Harbottle were particularly dismissive of the possibility that any contamination might survive the standard pretreatment, even though I pointed out to them as forcefully as I could without shouting, that a simple SEM screening of the Shroud by

Marano (1978:202,381) had shown: "la superficie delle singole fibre presenta un aspetto 'sporco' con abbondante deposito di materiale estraneo inquinante ma intimamente conesso con le singole fibre del tessuto" (the surface of the fibers presented a "filthy" appearance with abundant deposits of pollutant material extraneous to but intimately connected with the individual fibers of the cloth). Later, Garza-Valdes looked for possible microbiological contamination, and found it with embarrassing ease: "Even the untrained viewer could see that the fibers of the thread were completely covered with a bio-plastic coating" (Garza-Valdes 1999:27).

During and immediately after the conference, STURP allowed themselves to be gradually pushed aside, unfortunately, in order for the dating to take place. Among the Church representatives there were various cliques and rivalries, and it was difficult to understand what their motives or reasoning was. The minutes of this conference will reveal that Alan Adler and I urged, pleaded, cajoled, and literally begged for extensive chemical screening of the samples before being dated, and for at least two sites on the Shroud to be sampled. One or two others at the conference were supportive, but these were voices crying in the wilderness.

In March 1987, I circulated to all who had attended the Turin conference a long paper on the problems and pitfalls that should be considered before the Shroud samples were dated. A major emphasis was on possible contamination due to "... mold, mildew and fungal growths which are encouraged on linen... organic materials [such as] bacterial or insect residues and fine particulates ... locked in the cellulose structure." To counter this possibility I suggested that "all samples be subjected to elaborate pretreatment, SEM screening and testing (microchemical, mass spectrometry, micro-Raman) for impurities and intrusive substances." The reaction of the ^{14}C specialists was precisely the same as it had been in Turin the year before – marked for the most part by arrogance and disdain. A year later, several of them did finally succeed in getting Shroud samples, which were run with only standard pretreatment, the results announced to all the world, and the rest

is history. The Shroud was relegated in the public mind to a medieval forgery, or at best a medieval oddity. This was a disaster that could have been averted. It was not until Dr. Leoncino Garza-Valdes began to publicize his dramatic findings in 1993 that the possible magnitude of this disaster began to unfold. I will return to this issue below.

Other Scenarios

When the results of the dating were announced in 1988, there was a rush of speculation amongst Shroud researchers about other possible causes for a ^{14}C date in the 14th century. These included:

1. Abnormal events related to the Resurrection – that radiation or coronal discharge or some other unusual phenomenon might have created excess ^{14}C in the cloth giving rise to a radiocarbon age much later than its true age. While anything is possible that hasn't been disproven, this scenario seems extremely unlikely to most people, even those who believe in Christ's bodily Resurrection. If the ^{14}C age was shifted 500 or 3000 years it might have slightly more credibility. But invoking such a bizarre scenario to explain a ^{14}C date which places the Shroud precisely in the period when it is first recorded in history simply does not hold much appeal.

2. Medieval restoration – that a skilled restorer could have re-woven the cloth adding linen threads of 14th century date. This hypothesis has more credence, and at one stage I also considered it to be a possible, if less likely, explanation. Identifiable "rogue fibers" of cotton were noticed by Hall on the Oxford sample, being of a different type and colour. If linen fiber was used it might not be "noticed." However, it is highly questionable that any medieval restorer would have had the skill and/or taken the time to do a re-weaving that would not be immediately obvious to a textile expert. This scenario could however have been eliminated very simply by taking samples from another site on the cloth.

3. Other types of contamination – that some intrusive material accounted for the late date. It was argued convincingly by John Tyrer that "because of the heat inside the casket (during the fire of 1532) natural moisture would turn into steam, in places at superheat ... any contaminants on, or embedded in, the fabric structure would be dissolved by this steam and would link chemically into the molecular structure (of the cellulose)." This model is certainly a plausible and attractive one, and may indeed account for some of the measured ^{14}C. The main problem with the model was thought to be that the amount of contaminant needed to shift the ^{14}C age from 2000 years to 600 would be so large (50% or more) as to be apparent to the naked eye. However, when linen samples with 60% microbial contaminant were prepared, it was not so obvious as previously believed. The Tyrer hypothesis thus still merits consideration and investigation.

4. Fraud in the sampling – that someone deliberately switched the samples. This conspiracy theory has been rejected by most people as an extremist notion with nothing to commend itself. It simply defies common sense that someone like Tite would switch genuine Shroud samples for known medieval ones. However, it must be stated that the amateurish sample handling procedure provided great fuel for this claim.

5. Statistical incompatibility of the results – that the results do not agree with each other. This line of reasoning by itself seems rather sterile. The samples did come from the same object, and the results are in fairly close agreement when compared against the usual dating on archaeological samples. Without a theoretical dimension, the argument goes nowhere.

The Need for New Tests

In the last few years, discussion has focused on two hypotheses that seem most likely to be able to account for a ^{14}C date skewed by 1300 years – namely, the bioplastic coating as described by Garza-Valdes, and isotope exchange stimulated

by the 1532 fire. I do not intend to go into a detailed consideration of these two models, both of which have some merit. What is of particular concern is why the Church has done nothing to assist the investigation. The treatment meted out to Garza-Valdes is especially baffling. Whatever may be the personalities, the rivalries, the improprieties, etc., it is nonetheless true that this man made a major discovery that has very important implications for the ^{14}C date. If there was any doubt about the Shroud fibers he obtained, or the manner in which he obtained them, why was he not given the opportunity to work on formally certified fibers from the Shroud? Two or three tiny 5mm fibers from different points on the cloth, similar to those removed previously for Frache, Filogamo, Zina and Baima, would have been sufficient. Instead, detractors of the Shroud were given the basis to claim that the fibers Garza examined may not even have come from the relic. One can only wonder, yet again, at why Church officials seem to make matters worse for the Shroud!

Could it be that some people in authority believe that the Shroud can somehow stand aloof from the ^{14}C dating? Three years ago, a prominent Shroud researcher, who has a background in science, wrote the following to me:

"... the ^{14}C test results are no longer a hot issue. No one, including Harry Gove, takes them seriously. We all know and accept that a whole number of things went wrong with that test, including the fact that the very method is faulty in the case of old linen. In addition to that, new and disturbing facts are emerging about cosmic radiation..."

I am afraid that this attitude is profoundly in error, and represents a tiny percentage of people. To the vast majority people who know what the Shroud of Turin is, the matter was settled by the ^{14}C dates – for them it is a curious forgery or oddity from the medieval period.

There is absolutely no point and no hope in attempting to change this broad perception by debate, or even by experiment. There is one and only one thing that will re-ignite wide interest in the Shroud and re-vitalize the possibility in the public mind that this really is or could be the burial cloth of

Christ, and that one thing is NEW ^{14}C TESTING! Why this should be a problem, and why 12 years have elapsed without this happening, is a great mystery to me. For it is in the best interest not only of the Church, but also of the intense public interest in the Shroud, and of science, that we focus the highest level of technology that can be mustered on the issue. What is required is merely a few grams of the cloth from three new locations. This would cause no damage or disfigurement to the relic and is a negligible sum to pay for the potential significance of what can be learned. Let us hope that the futile ^{14}C debate of the last twenty years can soon be left behind, and Shroud studies can begin the new millennium with new data and a fresh approach to the true age of this fascinating object.

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