

Foreword: I had requested that this paper not be published with the 2008 Ohio Conference papers because there were some questions about the nature and history of cotton I wanted to explore before doing so. However, in the interim, my attempts to investigate some issues did not produce results because I was unable to get in contact with the specialists who might have been able to provide the additional information I sought. Joe Marino recently requested permission to publish on-line my Ohio presentation and the appendices of materials I had gathered. I have granted him that permission late this year (December, 2014). The material is largely unchanged from my 2008 Ohio presentation. Bits of more recent information are set off from the body of the original text by my use of brackets [ ].

## **What Went Wrong With the Shroud's Radiocarbon Date? Setting it all in Context (1)**

By

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We are only two years away from a fresh exhibition of the Turin Shroud [occurring in 2010]--and with that will there be another round of testing? In this light it seems a valuable exercise to recap previous hypotheses regarding the C14 results offered in the years following the 1988 testing. (2). Professionally, I am an archaeologist--some of you might call me an "antique historian." This is a paper about history. What I shall attempt to do here is to gather together in one place observations and explanations that have been published elsewhere. There are many things about the Shroud we would all like to know but in this paper I shall deal largely with only one question: *What went wrong with the Shroud's radiocarbon date?* I will provide here a brief synopsis of proposed answers with focused examination of one of those proposals.

### **A Strange Story**

But first I want to share with you a "strange story". Many of you have already heard it. I first heard it many years ago as it was circulated by Bill Meacham. A single thread of the Shroud was sent surreptitiously to a West Coast Laboratory back in 1982. One end of that thread came up with a date of 200 A.D. while the other end resulted in a date of ca. 1000! How could this be? I thought about it long and hard and finally dismissed it as a complete fluke. Anyway, that was

quite a “yarn”! Bill Meacham preserves this story in his most recent book published a few years ago. (3)

### **Radiocarbon test results and reactions to it**

Here’s another story, also old, so much so, you are probably all tired of hearing it. Briefly, on April 21, 1988 a single sample was removed from the so-called “Raes’ Corner” on the Shroud by the late Giovanni Riggi di Numana. This was divided up between three labs, Oxford, Zurich, and Tucson, Arizona and the results analyzed by the British Museum. The analysis from that testing was released on Oct. 13, 1988: the cellulose taken from the Shroud was to be dated with 95% confidence to between 1260 to 1390 A.D. (4)

Most of us reacted first with a mixture of shock and consternation! How could this be? The late Fr. Albert R. Dreisbach liked to say that “the preponderance of evidence” argued for the antiquity as well as the authenticity of the cloth. After all, how could the Shroud have been rendered in artistry 60 some years *before* the first bracket of the 1260-1390 released radiocarbon date? As we all began to recover it was generally agreed that something was radically wrong. The question was “What?” There have been six major approaches to this question. Evaluative remarks and commentary have been confined to the endnotes due to time constraints.

I. When something this painful hits, often a case of cognitive dissonance sets in. Perhaps the first to react publicly was the late Fr. Werner Bulst. At a conference in the Spring of 1989 held in Bologna, Italy he voiced his opinion that somehow something fraudulent had happened: samples *must* have been dishonestly switched. (5). But it remained for Br. Bruno Bonnet-Eymard to study this possibility in detail and to set it in print repeatedly in the pages of the **Catholic Counter-Reformation in the XXI st Century**. (6) It was picked up by other Shroud publications and disseminated around the world. [To my knowledge, Bulst never changed his mind about this as it was indicated in his letters to me. P.C.M, Dec. 2014]

II. Marie-Claire Van Oosterwyck-Gastuche, a Belgian chemist now living in France, while agreeing with the suggestion of Fr. Werner Bulst and Br. Bruno Bonnet-Eymard, that the radiocarbon date was a fabrication, goes further to completely and unequivocally rule out the reliability of radiocarbon dating in this circumstance. She believes that no application of C14 testing to Shroud samples will ever produce an accurate or believable result. I wish to make clear here that she does not abrogate C14 dating *per se*; but she does insist that where water may be involved the results are not to be trusted.(7)

III. Meanwhile, even in France, not all accepted the Bulst/Bonnet-Eymard proposal. Members of the scientific committee of C.I.E.L.T., headquartered in Paris (<http://c.i.e.l.t.headquartered>) contacted the wealthy industrialist, Guy Berthault, who generously funded the now well-known work of Dimitri Kouznetsov and his associates in Moscow. Their proposal was that during the fire of 1532 the heat induced cellulose changes in a water vapor atmosphere which caused the isotopic exchange of modern radiocarbon or carboxylation which altered the date of the Shroud and make it appear considerably younger. Jackson and his colleagues gave this proposal a strong chance. (8 & 9)

IV. Another proposal was made by Leoncio A. Garza-Valdes who published his findings of a coating on the yarns from the “Raes Corner.” This film he called a “bio-genic” or “bio-plastic” coating and suggested that it had been created by microbial action just as desert patina is left on rocks and some ancient artifacts (10). In the case of the Shroud he identified this microbe as Lichenothelia. This would mean that modern extraneous carbon would have intruded into the cloth where the sample was removed. Radiocarbon specialists had, in fact, admitted to me that, if proven, microbial involvement could indeed alter the date. Follow-up studies--particularly with cloth taken from an ibis mummy--found some support from such experts as Harry Gove, the co-inventor of the accelerator mass spectrometer technology that was used to test the Shroud samples in 1988. (11).

V. Another interesting proposal began with a statement by Thomas Phillips in a letter to *Nature* in which he suggested that radiation could cause a skewing of the date. (12). This idea was developed in detail by the French scientist, J-B. Rinaudo in which a neutron flux would have occurred and was eventually connected with a resurrection event which caused the image. (13) This became clear in his paper entitled “The cause of the image on the Shroud and the results of the carbon date: A cohering hypothesis.” Soon the German scientist, Eberhard Lindner, also added his support to this thesis in his writings. (14) In all of this the argument has been that a neutron flux would not only make the date of the Shroud appear younger than it really was, it also would have contributed to making the image itself. And I can report that this thesis is still being probed with experimentation some of which may be shared at this conference. (15)

VI. At Orvieto, in Italy, in the late Summer of 2000, M. Sue Benford and Joseph Marino presented what, in my opinion, was perhaps the “cream” paper--among many other very fine papers--of that entire conference. (16). They proposed that there had been an invisible re-weave of the so-called “Raes Corner” and had three modern experts in the field of textile technology who verified it from photographs. Despite this, based on her personal inspection of the Shroud linen, it was completely ruled out by textile conservationist, Mechthild Flury-Lemberg of the Abegg-Stiftung in Bern, Switzerland, (17) who convinced even the late Alan Adler that he was wrong in his earlier support of the idea. (18). But this story doesn't end there.

More recently, STURP member and chemist Raymond N. Rogers of the Los Alamos National Laboratory, examined this theory. In addition to the 14 threads taken from the Raes sample in 1979, he also received an additional number of threads in December of 2003 (19), via the good offices of AM\*STAR (20) from Luigi Gonella extracted from the center of the remaining piece that had been retained in the collection held by Giovanni Riggi di Numana after the radiocarbon sampling removal. These he studied and produced the results the reader will see in the paper from THERMOCHEMICA ACTA. (21). What Rogers really set out to do was to examine and test the fresh threads and prove the Benford-Marino theory false. He told one of our colleagues, “I'll prove them wrong in five minutes.” Several hours later he called back and said, in some consternation, “They may be right after all!” Statements like that never came easy for Ray, but as a scientist, as a chemist, he was rigorous and honest to his profession. (22) (See Appendix I).

VII. A new approach has recently been offered by John Jackson. The Oxford lab will be involved and they will be testing the most recent proposal to explain “What went wrong?”--in this case the hypothesis of a two percent shift in radiocarbon by carbon monoxide contamination.

Bryan Walsh notes, that “Near the earth’s surface, C14 monoxide is naturally about 5X more abundant as a percentage of all CO than C14 dioxide is as a percentage of all CO2.” (Personal communication). The samples will probably come from Jackson’s Colorado research group and will not likely to have been obtained from the Shroud itself. (23).

## A Closer Look

Here’s an important question: Is the so-called “Raes’ Corner” homogeneous or heterogeneous? If Ray Rogers’ findings are upheld then that corner is heterogeneous and would go far in explaining what went wrong with the radiocarbon date. But not everyone agrees with the suggestion of heterogeneity. Marcel Alonso, in France, for example, is on record as holding to the homogeneous viewpoint. (24). Since this may go to the heart of the issue I believe a recap of the problem is in order.

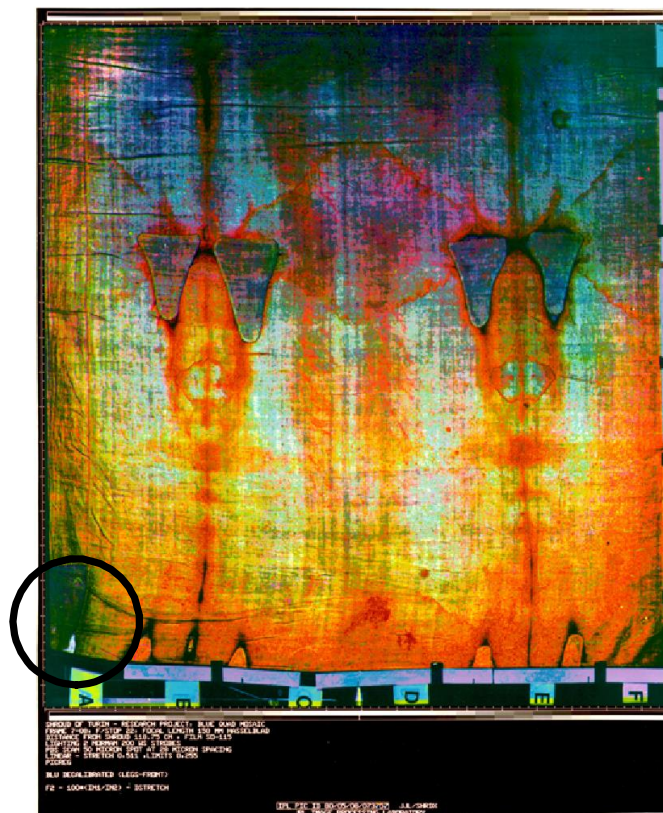
The first aspect of the issue is the presence or absence of cotton in the “Raes’ Corner”. It was the Belgian textile specialist at Ghent University, Gilbert Raes, who, in 1973-1974 studied a sample removed from the Shroud in 1973 and published his findings of cotton spun *inside* the linen yarns. (25). Later, however, the French textile expert, Gabriel Vial expressed his opinion, following his own examination of the Shroud, that the cotton was superficial on the cloth--not spun inside and therefore not really relevant to the issue. The reason for this lack of clarity is that, in fact, there is a lot of extraneous cotton on the surface of the Shroud. This is verified in my own studies of the Max Frei sticky tapes taken from the Shroud in 1978. (26)

I was in attendance at the New London, Connecticut conference held on Oct. 10-11 of 1981. I distinctly remember a remark made by Joan Janney (now Mrs. Ray Rogers) that STURP found cotton spun into the linen threads in the Raes Corner/radiocarbon sample area of the Shroud. I recently consulted with Barrie Schwartz (27) about this and he confirmed that STURP was finding a lot of cotton inside the linen yarns there as opposed to the main body of the Shroud where none was found. But this discovery was poorly understood then and so it was thought that the “Raes’ Corner” area would not make a good candidate for radiocarbon test samples which was then being designed by a committee headed up by the late Robert Dinegar. It clearly raised the question over whether *any* sample from that corner would be considered homogeneous and therefore suitable for testing. STURP’s preference seemed to be leaning toward taking samples from beneath the 1534 burn patches.

I met with Luigi Gonella on Saturday evening, Nov. 21, 1987 and discussed with him the possible sites on the Shroud from which a sample might be taken. Gonella placed great emphasis on the conservation of the Shroud. Although he would not openly admit that the “Raes’ Corner” would be the site from which a sample would be taken, all of the implications in his conversation with me were that, the Raes’ Corner would indeed be the “best” candidate site simply because it was thought the most appropriate place since a sample had been excised previously from this corner for Raes and since the royalty of Italy had stitched repairs on the patches, and they wanted to preserve that history, therefore no samples should be removed from anywhere else on the Shroud. By the end of that meeting my growing conviction was that Gonella was leaning very strongly toward taking a single sample only from a single site, the “Raes’ Corner”. He was perhaps following the wishes of Cardinal Ballestrero who had

appointed him science advisor for the project.

Following that dialogue I conducted a series of technical phone interviews around the world with leading specialists in the field of radiocarbon dating and from that series I developed a “white paper” which summarized the collection of data. On March 22, 1988, nearly a month before the April 21 sample removal session, I sent a copy of this paper to Pope John Paul II (actually to his then secretary, Cardinal Cassaroli) via his Papal Nuncio, Cardinal Pio Laghi in Washington, D. C. in a diplomatic pouch. I sent a second copy to His Eminence Anastasio Cardinal Ballestrero, and one to Gonella himself urging the need for convening a new Turin Workshop wherein specialists could analyze the fresh data. One of the most important points in this paper was made by Marian Scott of the International Radiocarbon Calibration Program, headquartered in Glasgow, Scotland: she asserted that a *minimum* of three samples must be taken from three *different* areas on the Shroud so that the results could be compared with all other results. Without this we would not know if the date obtained from the “Raes’ Corner” represented the date for the main body of the Shroud. I also suggested in that paper a method that could be used to circumvent Gonella’s argument that royalty had helped stitch the patches: single yarns could be teased out from under the many burn patches without interfering with any of the stitches known to have been placed there by members of the royal family. But the final decision, on April 21 of that year, was to take a single large sample only from the “Raes’ Corner.”



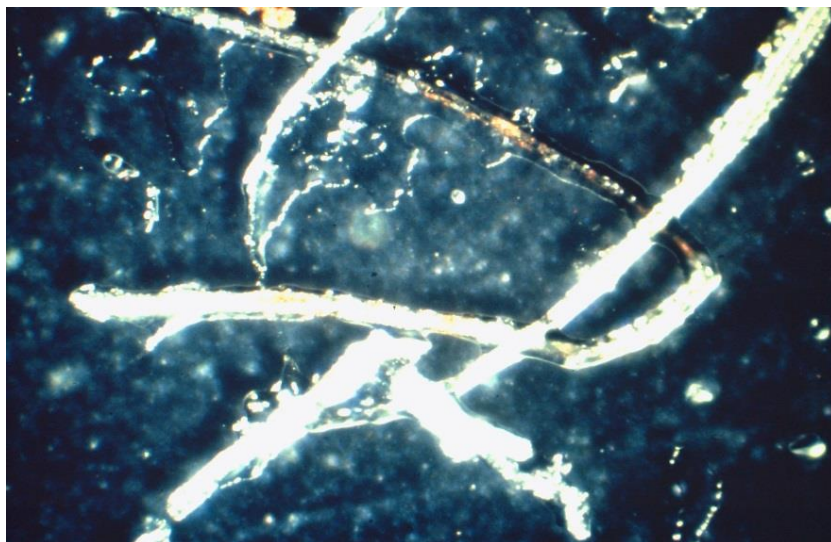
(1). “Blue quad mosaic” view of Raes’ Corner area from which the radiocarbon sample was removed. Notice the darker discoloration in the Raes’ Corner (circled area) as compared with the image area of the Shroud. (Courtesy of Barrie M.

Schwartz).

But a tool actually existed at the time of my meeting with Prof. Gonella, that, had I known about it, might have convinced Prof. Gonella to re-examine the question: the so-called “blue quad mosaic.” Some years before his death, I talked with the late Don Lynn about the use of remote sensing and what specialized photography of the Shroud could reveal. By using black and white film but with different filters, red, green, blue, Lynn told me that it could reveal surface chemistry through its reflectance--a kind of spectrum indicating that the surface was different from elsewhere on the Shroud cloth. When one looks at the “blue quad mosaic” one sees something very different at the corner from the deep orangeish red coloration of the image area. In the “Raes Corner” one sees a kind of “bluish-greenish” cast [see Fig. 1 above]. What causes this? Lynn told me that such special photography does not tell us what the chemical consistency is, it merely indicates that we must do chemical analysis to determine what that chemical signature is.

This is what Rogers’ paper in THERMOCHIMICA ACTA does: it confirms the presence of the mordant, aluminum, and reveals the presence of two other items--rose madder and gum arabic. The confirmation was a partial repeat of an earlier finding by the late Dr. Alan Adler who had observed salts of calcium, sodium, iron, aluminum and other metallic species (28) and starch (29) in the area of the “Raes’ Corner.”

(Fig. 2 A & B.)



**A. First photomicrograph of W. C. McCrone’s rose madder. STURP tape 3-AB from an area immediately adjacent to the blood flow across the back near the side strip but directly on a linen area. (Photomicrograph by W. C. McCrone. From the Paul C. Maloney collection of McCrone illustrative materials. No magnification listed by McCrone).**



**B. Second photomicrograph of W. C. McCrone's rose madder. STURP tape 3-CB very near to STURP tape 3-AB but taken on the blood flow across the back.**  
(Photomicrograph by W. C. McCrone. From the Paul C. Maloney collection of McCrone illustrative materials. No magnification listed by McCrone).

In March of 1981 the late Walter C. McCrone sent me several Kodak transparencies of shots he took looking at linen fibers on the Shroud. On those slides, still preserved in my collection, McCrone had written the following note: "madder rose, linen fiber, medium (blue) sample 3 CB" and sample 3-AB. McCrone was referring to photomicrographs made on STURP sticky tape samples 3-CB and 3-AB which came from the blood flow across the back nearest the side-strip side of the Shroud and directly adjacent to that flow on linen, itself. It was on that side where someone would have been working their repairs if the re-weave theory is held to be correct. McCrone, of course, due to his belief that the Shroud was painted by an artist, was trying to prove that the Shroud had been in an artist's studio. Hence, he sent me these photomicrographs as a piece of that evidence. But he was faithfully preserving the fact of the presence of madder rose on the cloth. There is now a new way of looking at the presence of that madder rose. Although this is some distance from the "Raes Corner" such trace amounts can now be conjectured to explain the dye that was used, along with the aluminum mordant and the gum arabic as a binder to create the wash to finish the re-weave. Thus, it may now be seen not as a contaminant from an artist's studio, but rather a contaminant from the weaver's workshop.

But if this was a re-weave, somewhere on the Shroud the re-weave comes to an end and the cloth of the Shroud begins. It is still difficult today to discern exactly where that change takes place. Does the piece of cloth today being called by the Shroud Science Group the "riserva" belong to the repair area or to the main part of the Shroud? Thus, the nature of the "repaired" area--if it exists--seems to be characterized by a combination of the following five components based on Rogers research:

1. linen-cotton spun yarns, spliced into the Shroud cloth and showing a coating of:
2. Starch (29),
3. aluminum mordant and other metallic salts,
4. gum arabic binder, and

5. madder rose dye.

If other samples taken from the Shroud differ from this, we need to stand back and re-think what we are looking at, perhaps engage in fresh research, before we can draw final conclusions. The Shroud, as it exists today, is beginning to look far more complex than was originally thought.



**(Fig. 3). Raes' comparison drawings between linen and cotton. Note the pentagonal shape of linen compared to the flattened ribbon- like shape of cotton.** (Illustration adapted by Paul C. Maloney taken from **Shroud Spectrum International**, Vol. IX, no. 38-39, p. 6).

Are there cotton fibers spun inside the linen yarns? Raes presented a very easy way to determine this: If one takes a cross section of a yarn one can determine the differences between linen and cotton by looking at the pentagonal shape of linen compared with the flattened look of a cotton fiber. (30) (See also Appendix II).

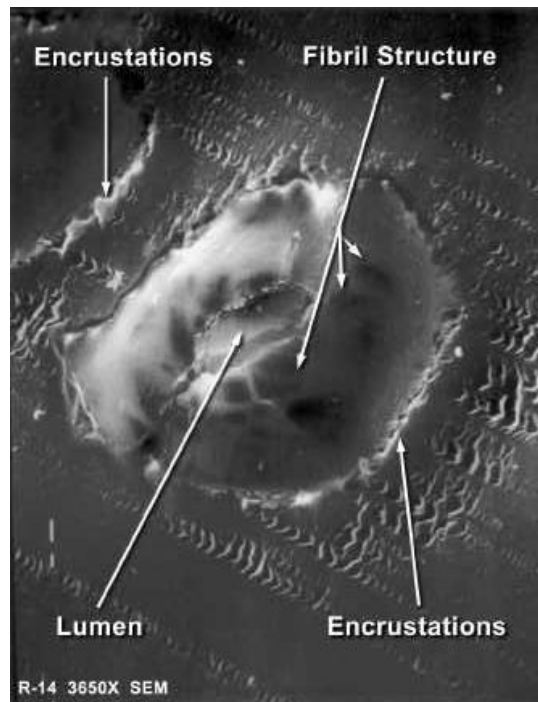


**(Fig. 4). Brown's discovery of a cotton fiber removed from *inside* the frayed end of Raes thread R14. Note the lack of any encrustation. It was protected by the surrounding linen fibers. Light micrograph taken by John Brown at 315 X magnification.** (Brown's Fig. 4).





**(Fig. 5).** Light micrograph of the cotton fiber that wrapped the *outside* surface of Raes yarn no. R14 showing the coating of encrustations. Light micrograph, taken by John Brown at 315X. (Brown's figure 5)



**(Fig. 6).** SEM photomicrograph taken by John Brown showing the encrustation surrounding the linen fiber that came from Raes weft yarn no. R 7. Note the pentagonal shape of the linen. SEM taken at 3300X. (Brown's figure 6).

If Rogers' theory is correct independent microscopical studies ought to be able to test it. Thus,

Rogers submitted several samples of the threads from the radiocarbon sampling area to microscopist John Brown, retired research scientist with the Georgia Tech Research Institute of the Georgia Institute of Technology, in Atlanta, Georgia. In a stunning study that reinforces Rogers' own study, Brown has shown that at precisely the intersections of the warp with the weft threads the yarn was tight enough and the weave tight enough to prevent the mixture of gum Arabic, madder root dye and mordant from penetrating. Further evidence that the viscous mixture did not penetrate is found with a cotton fiber removed from inside one of the threads. Brown unraveled one end of Raes' thread R14 and removed the fiber you see from inside that thread. But a cotton fiber that was unwound from the *outer periphery* of that same thread contains encrustation similar to the encrustation resulting from the viscous mixture deposited onto the surface of the cloth. (31a). (See Appendix II).

It is now clear that the presence of cotton *spun inside* linen yarns in the Raes' Corner is supported by the findings of five separate and independent investigators:

Gilbert Raes, (1973-1974)  
STURP's own early analyses reported by STURP spokeswoman, Joan Janney, (1981)  
Investigators at Precision Processes (Textile lab) Ltd in England (32), (1988)  
Ray Rogers' 2004 investigations, and  
John Brown at Georgia Tech (2004).  
Robert Villarreal & team, LANL (2008) (31b)

This issue of "homogeneity" of sample was brought into significant focus by Bryan Walsh at the 1999 Richmond, VA conference on the Turin Shroud. In his paper, "The 1988 Shroud of Turin Radiocarbon Tests Reconsidered." Walsh, after careful consideration of a statistical analysis of the samples given to the various labs and their results made the following summary statement: "The statistical analysis techniques employed in the 1988 radiocarbon dating of the Shroud of Turin appear to have underestimated the potential for a non-homogeneous distribution of radiocarbon in the Shroud linen...the statistical characteristics of the data from each radiocarbon lab appear to indicate that, in the case of the Oxford lab measurements, its observations were drawn from a statistically different population." (33) Walsh concluded: "Whatever the cause of this gradient, before anyone again attempts to date the Shroud of Turin using radiocarbon dating techniques, a thorough understanding of the nature and characteristics of any proposed radiocarbon enhancement mechanism in linen fiber must be developed through a series of rigorously-controlled experiments which evaluate the chemistry and isotopic behavior of the carbon atoms in linen over a wide range of physical parameters." Although Walsh was here alluding to a possible thermal event, I believe we must now expand our technical evaluations to include researches that also focus on the possible inclusion of medieval cotton fibers inside linen yarns. I shall return to this dual focus at the end of this paper.

## **The Date of the Shroud?**

Barring additional testing of samples from the main body of the Shroud, there is no easy current answer to the question of the date. What are some alternatives proposed?

Rogers suggested what I will call (for want of a better description) a “quasi-dating” method for the Shroud. In his January 2005 paper he made comparisons between lignin in the fibers from the main body of the Shroud cloth with that in samples from the Raes’ corner. There is a chemical component which occurs in the lignin called “vanillin.” Rogers discovered that all of the vanillin was gone from fibers in the main body of the Shroud but still present at the “Raes’ Corner”. Although Rogers published this finding in January of 2005 he knew that there was much more work to be done. In a series of e-mails between him and Bryan Walsh (34) (See Appendix III) it was revealed that Rogers’ revelation about the vanillin loss was a qualitative study, not a quantitative one. Moreover, the loss of vanillin may be affected by heat. This is a crucial issue considering that the Shroud was heavily damaged in the fire of Dec. 4, 1532. Thus, far more study needs to be done before anyone can rely on extrapolations from the lack of vanillin on the main body of the Shroud compared with its presence in the “Raes’ Corner” area.

Recently, Australian biologist Stephen E. Jones had suggested dating the pollen grains on the Turin Shroud as a way of establishing markers for separate historical events. (35) But as Dr. Lloyd A. Currie points out (36) at least 100 grains would be needed by current standards to achieve a viable result. The largest group of pollen of one plant type is that of *Gundelia tournefortii* with only 29 pollen grains. (37) If more of these could be hand-picked directly off the Shroud or discovered in the vacuumed dusts of the late Prof. Riggi di Numana’s collection (38) to make up the difference such a test might be feasible. Jones, as I understand him, does not assume that these pollen were placed there at the time the Shroud was woven. Rather, he believes that such C14 tests can be used as an independent means to evaluate the medieval dating results of 1988. Any C14 results older than the medieval date would call the medieval date into question.

As of this date in time we have no other clue to the antiquity of the Shroud except for the preponderance of evidence which suggests that the Shroud is ancient. As noted earlier, it must date prior to 1192-1195 A.D. because the earliest certain rendering of the Shroud in artistry is to be found in the Hungarian Pray Codex. This fact alone automatically rules out the possibility that the 1260-1390 date from the Raes’ Corner represents the date of the main body of the Shroud.

### **Where do we stand?**

When everything is properly understood, the entire picture of the Shroud should come together as a beautifully constructed puzzle. If something is out of place, the whole will not look right. We are currently still in that mode. Not everyone agrees with Ray Rogers findings. Especially in Europe there are those who believe his findings do not represent the real nature of the Shroud. Thus, this issue of “homogeneity” vs. “heterogeneity” needs to be resolved so that we can move forward. If a “re-weave” is not the explanation for the characteristics found at the Raes’ Corner then we badly need an explanation for why cotton is woven into that corner but is not demonstrated in threads in the main body of the cloth.

What does the opposite side of the ledger look like? Do the x-rays of the Shroud show any evidence of the re-weave? Bryan Walsh suggests they do not. (Personal communication). Walsh also notes that in discussions "...with textile conservators in the U. S., they said that while reweaving might be made difficult to perceive on one side of a cloth, it would be painfully obvious on the other side of the cloth because of the various threads and knots involved in stitching it."

I'm not arguing in this paper that the Benford-Marino-Rogers theory is THE sole answer to our question "What Went Wrong?" Nevertheless, the factors I've marshalled here suggest that it has an awful lot going for it. What the historical record suggests to me is that of all the evidences gathered thus far, the strongest clues appear to come from two approaches: the Russian work on carboxylation emanating from the heat event of 1532 and, what in my view, is the clear possibility of an invisible reweave. That implies that we should perhaps shift our focused application of Ockham's Razor for a singular cause, over to a multivariate approach in our continued research. Thus, we need to further explore the issues raised by Bryan Walsh and others as well as the points raised by the Benford-Marino-Rogers approach to resolve the matter.



**(Fig. 7) Photomicrograph of the Rogers' spliced thread.** (Photo courtesy of Barrie Schwartz). (See also Appendix I below: Ray Rogers' e-mail regarding the splice).

It is most unfortunate that the Shroud textile has, to this day, never been fully characterized. There are scattered observations and various studies on numerous aspects but none have been compiled into one reference. (39) For example, by implication, Rogers seemed to hold that the yarns were "hank bleached." Are they? Linda Eaton, curator for textiles at the Winterthur

Museum, in Winterthur, Delaware, has suggested to me an alternative--that the variegation of color in the individual yarns may have to do with incomplete retting of the batch of flax that composed it. She comments: "I have noticed more variation in color in linens in this country than I was used to when I worked in the UK – maybe because the stuff that is kept here is not always of as high quality in general, but perhaps also because the local stuff was field rather than pond or stream retted, which might also make a difference." (Personal communication: 8/7/2008). (40) If the Shroud was woven in the Near East where water was not always plentiful perhaps field retting was more common. In keeping with Bryan Walsh's concerns, we must see to it that more research is conducted and that the linen is fully characterized before there is ever another radiocarbon dating test done of the Shroud. Already, in some quarters, new examinations are under way on Shroud samples to shed fresh light on the nature of the cloth and the threads in it.

## Conclusion

I began this paper with a "strange story". The photo you see here is not the thread discussed in the story preserved by Bill Meacham, but it may be similar to it because it comes from the same Raes' Corner. As implied earlier, an answer to the puzzle it presented now suggests itself. If the Benford-Marino-Rogers' research on the reweave turns out to be correct, and that yarn was a spliced sample of old and new, then it is really quite possible to conceive of an 800 year spread producing a date on one end of 200 A. D. and 1000 A. D. on the other. It may turn out that that was quite a "yarn" after all. But we have a long way to go before we resolve all of the questions still hanging in mid-air.

## APPENDICES

**Appendix I:** Ray Rogers' e-mail regarding the splice. [Include in this are photographs of Raes Sample # 1 (the splice) and M. Sue Benford's color rendering of it hinting at the splice. [3 pages]

## Appendix I: Ray Rogers' e-mail regarding the splice

The following contains the earliest summarizing communique by the late Ray Rogers to the Shroud Science Group regarding the splice implying an invisible reweave: I have included it here because it should be on the historical record as a significant contribution to the debate related to the "invisible reweave". This e-mail was in response to doubts and views expressed by some members of the Shroud Science Group over whether or not the "invisible reweave" is the explanation for the radiocarbon dating results. This communique was dated March 05, 2004, 2:30 AM.

"In English we have a saying: "We will agree to disagree." With regard to the validity of the radiocarbon sample, we will have to do exactly that--for now.

"1) I agree that the uv-fluorescence photograph alone does not prove anything. A fluorescence spectrum may allow identification of some chemical species, but a lack of fluorescence does not. All the photograph indicates is that the entire area around the Raes sample and the radiocarbon sample shows an anomaly. It is probably the same anomaly all over the area. The important fact is that it is not the only piece of evidence.

"2) I agree that traces of cotton would not prove anything. However, I believe that important amounts of cotton exist in the sampling area. There is a big difference in importance between a few foreign cotton fibers found on the outside of a sample and cotton mixed throughout the sample. I have found copious amounts of cotton at the core of all of the yarn segments I have dissected.

"3) I agree that the sampling area certainly looked a lot like the rest of the cloth. However, on closer, careful inspection, it does not resemble the rest of the cloth in many ways. For example, it is coated with a unique gum/dye/mordant layer. That is only observed with proper microscopy technique, but it is very easy to see with a microscope.

"4) I agree that it is quite difficult to observe any difference in the sampling area by looking at it in reflected light. I believe it was manipulated to match the main part of the cloth, but that is no proof. However, a completely independent analysis by pyrolysis/mass spectrometry found anomalous amounts of furfural being produced from the sampling area. No other area of the Shroud gave the same results. There must be a reason. The most probable reason is the pyrolysis of a pentosan plant gum, exactly what Prof. Brown and I see with other tests.

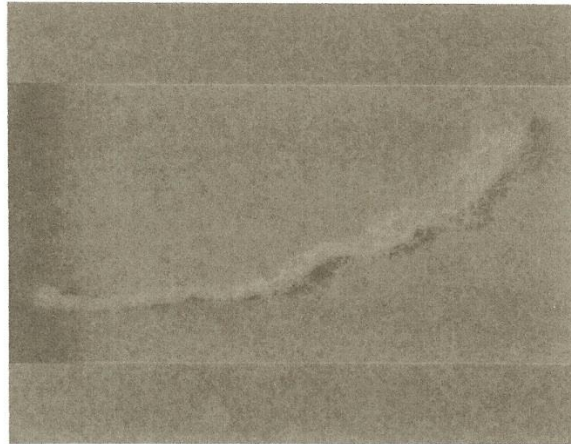
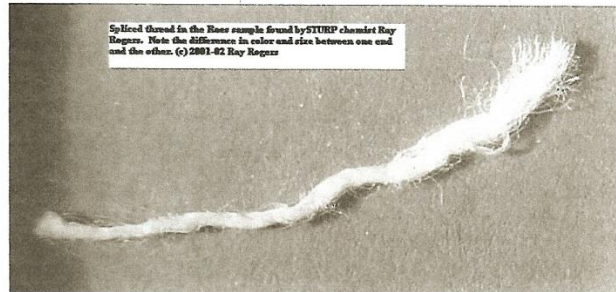
"5) I agree that I do not see any evidence for an "invisible" (or French) reweave by observations with reflected light. I do, however, see an end-to-end splice that was very obscure among the Raes threads. We have not seen any similar feature anywhere else in the Shroud. Is such a feature part of the French reweaving process? Incidentally, one end

Appendix I: Page 2

of the splice had a different chemical composition than the other.

“When I consider all of the evidence that the anomalously low-fluorescence area of the cloth also has anomalous features and chemical composition, I have to conclude that a terrible mistake was made in choosing that area for the radiocarbon sample. It is the combination of pieces of evidence that must be considered.

“Would you agree that a careful analysis of a documented sample from the middle of the radiocarbon sample could solve our problem and lead to complete agreement? Perhaps we could petition for such a sample.”



Appendix I: Page 3



Figures 1, 2 & 3 above are used here with the kind permission of M. Sue Benford. Each photograph shows the spliced thread originally studied by Ray Rogers. Fuller discussion may be found in M. Sue Benford and Joseph G. Marino, "Textile Evidence Supports Skewed Radiocarbon Date of Shroud of Turin," 2002. This document may be accessed at [www.shroud.com](http://www.shroud.com).

**Appendix II:** "Exploring Some Questions: Linen, Cotton, and Invisible Reweaving-- A Background Resource for the Turin Shroud." Included is a discussion of the nature of the French invisible reweaving technique. [At least 20 pages]

## **Disclaimer to Appendix II: Exploring Some Questions: Linen, Cotton, and Invisible Reweaving-- A Background Resource for the Turin Shroud**

Readers should be notified that Appendix II has not been peer reviewed. Nor is it ready for such a review inasmuch as it is, to this point, mostly an "open-ended" collection of observations and data pertaining to the historical background of ancient textiles. There is also some material, such as regarding starch, that needs yet to be added. This will happen at a later date but could not be included prior to the conference being held in Columbus, Ohio on Aug. 14-17.

Any comments readers might wish to make about this collection of material would be most welcomed. This includes requests for additional material, and corrections that need to be made.



## **Appendix II: Exploring Some Questions: Linen, Cotton, and Invisible Reweaving-- A Background Resource for the Turin Shroud**

Such specialists as Bryan Walsh and others have emphasized that before any future radiocarbon dating of the Shroud is ever attempted again, the Shroud sample must be thoroughly characterized. I have interpreted this in the very broadest way possible. What is the Shroud like? From raw material (flax/linen) to yarn (spinning) to woven cloth (herringbone twill), it is important to understand just how the finished product came through from its beginnings. But it must be understood that by “characterization” I also include questions that arise at the chemical and microscopically structural levels.

There has been much controversy and doubt over the suggestion that the Raes’ Corner has been repaired. In 1999 John P. Jackson and his colleagues published a study of the proposals which had been offered up until then. (1) They concluded from multiple evidences (taken from the 1978 x-rays of the Shroud cloth, and from Barrie Schwartz’ transmitted lighting photos done at the very end of the 1978 session) that the Raes’ Corner was not repaired--in essence, that it was an intact portion of the main body of the Shroud. However, as I have noted in endnote 21 of my paper, “What Went Wrong...”, this research was completed prior to the Sindone 2000 Congress held in Orvieto, Italy where M. Sue Benford and Joseph Marino first presented their proposal that an invisible reweave technique had been applied to repair the Raes’ Corner. It is quite true that if “knots” would have been used in the repairing of the Shroud cloth, these would surely have shown up in the x-rays and transmitted light photos.

But some aspects of “invisible reweaving” make use of a technique that is truly “invisible”. By this I mean that by splicing the new yarn into the old--not by using knots to connect--such a repair would not easily show up on either of the x-rays or the transmitted lighting unless one was specifically looking for it.

However, in studying this problem, the late Ray Rogers specified that he never saw the splice-type of approach used in the main body of the Shroud cloth. In Dan Porter’s well written internet article “The Biggest Radiocarbon Dating Mistake Ever” he notes as follows:

It was close examination of actual material from the shroud that caused Rogers to begin to change his mind. In 2002, Rogers, in collaboration with Anna Arnoldi of the University of Milan, wrote a paper arguing that the repair was a very real possibility. The material Rogers examined was from an area directly adjacent to the carbon 14 sample, an area known as the Raes corner. Rogers found a spliced thread. This was unexpected and inexplicable. During weaving of the shroud, when a new length of thread was introduced to the loom, *the weavers had simply laid it in next to the previous length rather than splicing.* [My italics] About this Raes’ Corner splice Rogers and Arnoldi wrote:

[The thread] shows distinct encrustation and color on one end, but the other end is nearly white...Fibers have popped out of the central part of the thread, and the

fibers from the two ends point in opposite directions. This section of yarn is obviously an end-to-end splice of two different batches of yarn. No splices of this type were observed in the main part of the Shroud. (<http://www.innoval.com/C14/>).

Porter told me during a phone conversation (Sunday, July 20, 2008) that some of this information came to him in an e-mail from Ray Rogers. I have personally, not yet been able to confirm either this splicing technique or what is truly meant by the statement I have emphasized in italics above from ancient or medieval sources currently at my disposal. I will explore, however, one possibility below when I discuss the wetting of linen during spinning. But it would appear to be a possible research approach using high resolution visible light photographs of the Shroud, the backlit light transmission photos, and the x-rays. To my knowledge no one has yet, to date, conducted this extensive type of study on the Shroud cloth. It is definitely something that must one day be done.

It seems appropriate to present a number of pieces of information here that would bear on such research.

For example, is there a role for starch in invisible reweaving? Marinelli & Petrosillo state: “During the pre-treatment, the presence of starch was noted that could have been used for the dressing of the cloth by a medieval restorer. It was commonly used for invisible mending.” (2)

## **Some comments about linen:**

### **Introduction**

Although originally designated under the Latin term *Linum bienne* Mill, today domesticated flax is usually termed as *Linum usitatissimum* L., subspecies *bienne*. Its habitat is in “damp fields and swamps” and therefore grows in Israel in Acco, Sharon, and Philistean Plains as well in Upper and Lower Galilee, Golan Heights and the Jordan Valley among other places. Its stems are “single or many, erect or ascending” (3) depending upon how closely the plants are cultivated together. If the plant is grown deliberately for use in fabric development, they tend to be grown closer together so that the longest possible fibers can be harvested from the stems for the spinning of yarn.



Fig. 1. *Linum usitatissimum* L. subvar. Bienne. (4)

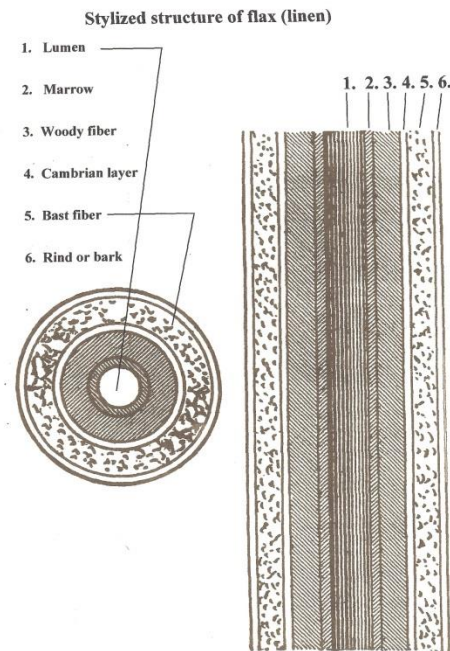


Fig. 2. Flax has a hollow lumen in the center of its fiber. But it is the bast fiber (no. 5 above) which is central to weaving linen cloth. It is this section of the stem that the process of retting, braking, scutching, and hackling, prepare the fiber for spinning into yarn for weaving. (5)

Linen has a central round lumen (no. 1 on above drawing) through which it can draw moisture up into the plant. During dowsing of the fire of 1532 this capillary action is known to have occurred:

At the time of the extinguishing of the 1532 fire, it is reasonable to conceive of this cellulose bound iron “chromatographing” to the water stain margins either as free iron or bound to the low molecular weight water soluble degraded cellulose present, where it could precipitate as  $\text{Fe}(\text{OH})_3$ . By a process similar to the “khaki” process we have described above [pp 87, 89], it could then produce in time the birefringent red particulate coated fibrils [sic] seen. Alternatively, the water used in the extinguishing of the fire could have been high in its iron content and would produce the same result by a similar migration process. We observed that Spanish “khaki” controls are microscopically and chemically identical to the birefringent red particulate coated fibrils [sic]. This includes the fact that some of these particles can be seen in the lumen or core of the fibril [sic] as seen on the Shroud. Since many of these internal particles on the Shroud are found between intact joints of the linen fibril [sic], it is difficult to conceive of any way they could have gotten there except by a precipitation process similar to the “khaki” process which we have postulated. (6) [John H. Heller and Alan D. Adler, “A Chemical Investigation of the Shroud of Turin”. **Canadian Society of Forensic Science Journal**, Vol. 14, no. 3 (1981), pp. 81-103, see discussion on p. 98.]

#### **Length and diameter and number of fibers in a yarn:**

Although I do not have access to precise measurements yet about the diameter of Shroud linen fibers, Ray Rogers did measure the diameters of the linen fibers that composed the yarn spun by Kate Edgerton to weave the large “shroud” for STURP. In his **Thermochimica Acta** paper (2005), he refers to tests done on “10-15- $\mu\text{m}$ -diameter fibers”. (7) It could be that the fibers of the Shroud linen may be comparable in diameter to this but measurements need to be taken to be certain. I do not know how many samples of such fibers on which Rogers based his statement but there were at least two.

It is interesting to note that Pliny the Elder discusses the netting of Cumae in Campania boasting of its ability to “cut the bristles of a boar and even turn the edge of a steel knife;...” but he adds: “Nor is this the most remarkable thing about it, but the fact that each string of these nettings consists of 150 threads...” (8). He appears, therefore, to be using the same term both for thread and fiber because in XIX. III. 18 he says “Then it is polished in the “thread“ a second time“ (“iterum deinde in filo politur,...”).

As for the Shroud itself, I am as yet unaware if anyone has done studies of how many flax fibers run on average through a single yarn. I believe such a study would be inaccurate without disassembling numerous yarns to count each fiber to obtain that average. But one might gain a “minimum” idea by using high definition photographs to count fibers running on the outside of the yarn.



Fig. 3. Reduced size photograph of the Shroud threads Gabriel Vial used for measuring the diameters of the Shroud yarns. (9)

As for the diameters of the yarns themselves we are also largely in the dark. The late Gabriel Vial used an enlarged photograph to come up with “relative” sizes of seven warp threads in his treatment published in the article “Le Linceul de Turin--Etude Technique”. (10) Yarn no. 1 = 20.5; yarn no. 2 = 31.0; yarn no. 3 = 12.2; yarn no. 4 = 22.3; yarn no. 5 = 19.0; yarn no. 6 = 23.3; yarn no. 7 = 18.8. Thus, in a relative fashion we can see that the diameters of the warp yarns go all the way from 12.2 up to 31.0. But these figures need to be converted into real-life metrics reflecting the true size of the yarns on the Shroud. Unfortunately, I was unable to determine exactly what spot on the Shroud the photograph used for the measurements it represented. Moreover, we are not informed as to whether Vial used the Enrie photographs, or had access to one of the STURP photographs, or possibly had access to some of the close-up photos taken by Arelio Ghio in October of 1978 or perhaps had taken close-ups of his own on April 21, 1988 which he later enlarged to make his analysis. But until we have a study that corresponds to many different areas on the Shroud we will not have a true handle on yarn diameters. I suspect that yarn diameters are a function both of the number of fibers inside the yarn as well as the tightness of twist. In modern times a semblance of consistency in yarn diameter was maintained by weighting out on a scale a certain amount of fiber to make a rove and the same weight used for each succeeding rove. (11) This was not likely done in ancient times.

One may also suspect that there may have been more than one person doing the spinning for such wide variations to occur. This would be supported by the observation made personally to me in September of 2001 by Mrs. Jean Brabender of the Scottish Woolen Centre in Stirling, Scotland who noted that at least two persons (probably an experienced person and a novice [young (female) apprentice]) were involved in the weaving of the Shroud. She based this observation on the banding she saw in the weft; the tight banding reflecting a stronger (adult) person and the lighter areas of banding, someone younger who was less experienced. E. W. Barber notes that in Egypt, in evidence from ancient wall paintings, each loom was monitored by

“two women, who crouched at either side and sped the weaving by passing the weft bobbin back and forth between them.” (12) By extension, then, we may likely suppose that another person might have also been involved in the spinning, too.

J. Merritt Matthews gives 960 mm (i.e. 37 and  $\frac{3}{4}$  inches) as the standard length for Egyptian flax fibers. (13) However, Barber says that flax can grow up to four feet in height making for a longer fiber to use for spinning yarn. (14).

### **Bleached or not bleached?**

Gabriel Vial, in his CIETA article, states his opinion that neither the warp nor the weft has been bleached. But he provides no reasons for it. (15)

On the other hand, other textile specialists have examined these features and have come to a different conclusion. For example, the late John Tyrer of Manchester, England notes: “Another interesting feature is the presence of numerous dark (pale in the photographic negative) warp threads than run for some distance through the Shroud and cross from image to non-image areas. A good example runs through one eye.” (16) In fact, it is that very prominently obvious thread that has convinced me that it played a role in the design of the Byzantine tremissis with the Christ-face on the obverse. (17) Tyrer goes on to suggest that “Their appearance would indicate that even though the cloth was piece bleached, the yarns must also have been at least part bleached before weaving, probably in hank form.” (18)

Hank/skein bleaching could explain the variegation of color in the various thread lots. Enid Anderson, in her *Spinner’s Encyclopedia*, (19) [A David & Charles Craft Book, 1987, p. 16 under “Bleaching--Grass”.] notes as follows:

The oldest, most widely used process and the most natural for bleaching linen. Causes no damage to the fibre structure and is permanent. The skeins of yarn are laid out onto grass for several weeks during which time they must be kept damp and turned occasionally to prevent staining. The colour is removed from the fibre by the oxygen given off by the plants and the atmosphere. Over a period of time the linen will gradually whiten. *Bleachfields* were common sight in the cotton districts before the discovery of chlorine in the late eighteenth century.

Because the natural color for flax is a “tow” (hence the adjectival reference to “tow headed” for someone who is a dark blond) we would need an overall explanation for why the linen, ivory color as it now is, is still lighter over all than would be expected if it is not bleached. One may suggest, as Tyrer does, that it was once piece-bleached, but this still will not explain the variegation in the thread lots.

But Linda Eaton, curator for textiles at the Winterthur Museum in Winterthur, Delaware has suggested that an alternative view, but one that needs research, is that the hanks were field retted rather than pond or stream retted. The latter type of retting should likely have carried away more of the deteriorated coloration than field retting. But this possibility needs to be fully explored. (20)

So far as my own research is concerned, I have yet to find clear historical evidence that “hank bleaching” actually occurred in ancient times. Ancient evidence--in this case from Pompeii (prior to 79 AD) shows fulling being done by men tramping in large vats. And what they hold in their hands appear to be woven pieces. This suggests piece cleaning, rather than a treatment of individual hanks since such trampling would surely have tangled the yarns. But one must be careful here. I have not yet determined a distinct historic separation between “fulling”--which, by the way, is usually applied technically to the treatment of wool--and “bleaching.” Did they happen in ancient times simultaneously?

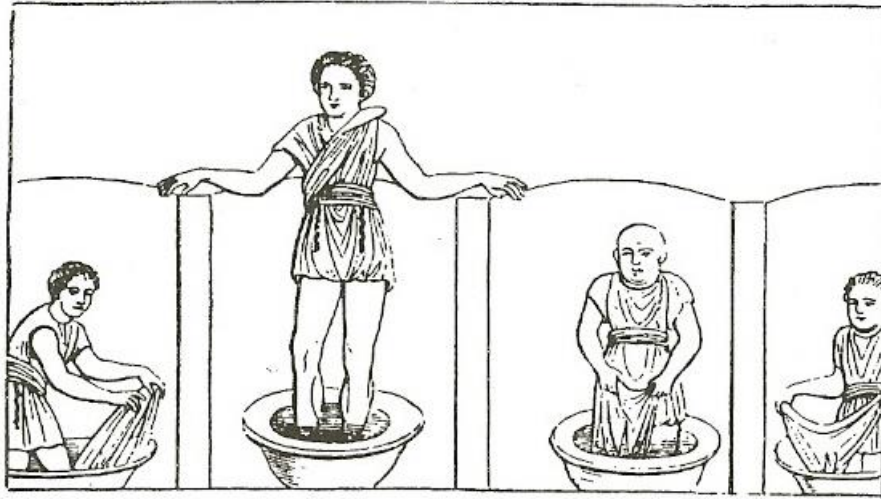


Fig. 4. Drawn from a mural preserved at Pompeii (pre-79 AD). Forbes designates these as “fullers.” (21)

STURP had tried to probe for *Saponaria officinalis* (soapwort) which is known to have been used in ancient times to bleach/clean cloth. They created a control from Spanish linen on which to determine that possibility. But the test was unable to find any traces of it. They reasoned that possibly over a long period of time it was “lost” through degradation. (22)

Soapwort was not the only thing used for bleaching. Pliny the Elder tells us that fullers (men who clean cloth) learned that men’s urine relieves gout (the fullers are said to have used it to take out ink stains) (Pliny, Natural History, XXVIII. xviii. 66). Camel urine is also referred to as a bleaching agent. (23) It is unknown whether or not urine of any sort was used to bleach the original cloth of the Shroud.

## Some comments about cotton:

### Introduction

Martha Goodway notes: “Cotton, a seed hair, has the form of a collapsed hollow tube. It collapses randomly, with a right-hand twist I one short section followed by a left-hand twist I another...” (24)

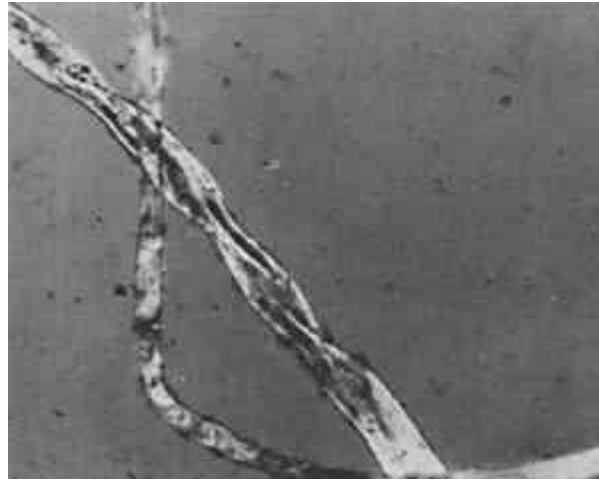


Fig. 5. Cotton fiber showing the “collapsed-tube” morphology typical of it. (25)

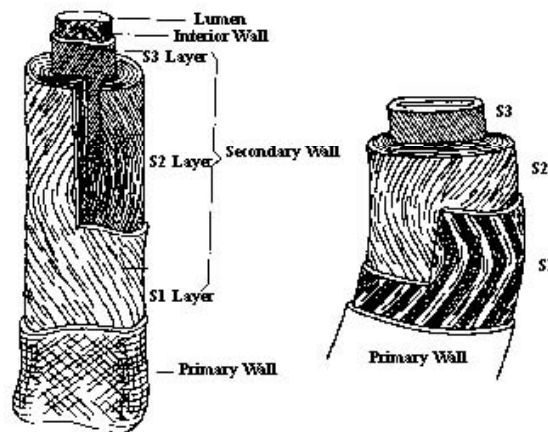


Fig. 6. Diagram of the structure of cotton. (26)

### “Pre-collapse” versus “Post-collapse” Cotton:

Before cotton collapses into its typical form analysts usually see for dried cotton under the microscope, its cross-section has a round shape. The lumen allows fluid to flow through it. This is sometimes referred to as “wet” cotton or “immature” cotton. Collapsing of the cotton fiber is caused by dehydration.

Is there a way to demonstrate photographically the presence of cotton *inside* the yarn? I have pondered this question. In some experiments--still on-going--I tried to capitalize on the idea that the lumen in the center of the fiber might act in a capillary manner to draw up liquid into the lumen, much as capillary tubes are used for blood samples, to show a discoloration outstanding



against the surrounding plain flax fibers. Thus far the water based inks and dyes I have used only draw up the coloration--not inside--but outside the fiber. Theoretically, this will eventually discolor the linen fibers in the yarn also. It is possible that some acetone, toluol, or xylene based solvents might work to accomplish this. Or possibly hot solutions. But I have as yet not found a solution that will work. Perhaps this is due to the “collapsed-tube morphology” (see above) that impedes the capillary action inside the lumen of the cotton fiber. Further research is required to explore the range of possibilities.

### Some comments about spinning:

Elizabeth Wayland Barber has drawn together a marvelous compendium of information on ancient weaving in her splendid **Prehistoric Textiles**. She explains that for many years European textile specialists had difficulty in understanding Egyptian technology simply because Western approaches to spinning were done so differently. Barber describes the Western method as follows:

...the carefully fluffed-up fibers are paid out a few at a time into the newly formed thread, so that their ends overlap everywhere, and cohesion is caused by the twisting together. This is draft-spinning...This ‘magic’ is possible only because the fibers are slipping past each other, so that the ends of the individual fibers all come at different places--there are no splice points. The method seems already to have been prevalent north of the Mediterranean in the Neolithic period; at any rate I have not been able to find any evidence that the early northern textile yarns were *not* drafted continuously. (27)

She points out that, by contrast, the Egyptians were not using the continuous draft-spinning technique. They were “splicing together the ends of pre-formed ‘strings’--fiber bundles two or three feet long, stripped from the flax stalks--so that the ends of the ultimate fibers overlapped in bunches and only at considerable intervals. In figures 7 and 8 below she illustrates with both photomacrograph and diagram what this would look like.



Fig. 7. “Magnified photo of splices in typical dynastic Egyptian linen.” (28)

Regarding the Egyptian process she quotes Louisa Bellinger

...whose experience comes from trying to clean and restore ancient textiles, explains the

process from the point of view of the product. A strong linen sheet, she says..., is typically made of thread in which three fine strands of flax have been twisted together. Each strand consisted of lengths of fiber whose ends were overlapped by a few centimeters, and which then were twisted at the splice just enough that it would hold temporarily... Then three of the long single strands were twisted together, in such a way that the rather evenly-spaced splice points in one strand would come at different places in the final thread than the splices of each of the other strands. Wherever ‘two straight pieces of flax were spun with a twisted splice in the third roving the resulting length of yarn looks as though it were plied, for the splice keeps to itself and is slightly separated from the other strands. Each length of yarn in which there is no splice looks, at a casual glance, like a single yarn spun from a single roving.’ (29)

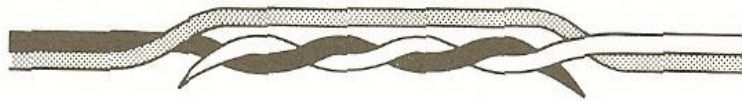


Fig. 8. “Schematic representation of a linen splice (black and white threads), with another strand running parallel (grey).” (30)

### **Wetting linen during spinning**

Linen can be difficult to handle if it is dry. In Elsie G. Davenport’s well written manual on spinning we find the following statement: “Flax can be made to adhere to itself and to other fibres if it is damp, and the right finger and thumb must therefore be kept slightly wet while spinning. The quite unhygienic method of wiping the ball of the thumb on the lower lip is the easiest and makes the best yarn, but most people prefer to have a small bowl of water or a saturated sponge in a dish for the purpose.” (31)

Flax has been present in the archaeological context since before about 5000 BC in Iraq--but we must keep in mind that flax was also grown for its seed. But in the Egyptian Faiyum evidence of domesticated flax (*Linum usitatissimum* L) is also present as archaeological evidence along with spindle whorls--hence clear sign of spinning for weaving. (32) In Egypt evidence suggests that the handling of flax for weaving went through a long evolution where there were independent developments in spinning and weaving techniques. Barber discusses this extensively. But, of interest to us here in this context is her treatment of wetting the linen during handling for spinning into yarn. The Egyptians apparently discovered that when linen was wet it could be spun into yarn much more easily than when dry. Thus, when bowls were discovered in Egypt and in the neighboring area of Palestine and also on the Island of Crete, and when it was noticed that there were loops in the bottom, many of which had fine lines showing where the linen fibers had been drawn up (through water) and put into twist to make the yarn, the deduction was clear: these were “fiber-wetting bowls”. Barber even gives evidence that the modern Japanese had also, independently, developed such fiber-wetting bowls (albeit for a different bast fiber). See the extensive discussion in Barber. (33)

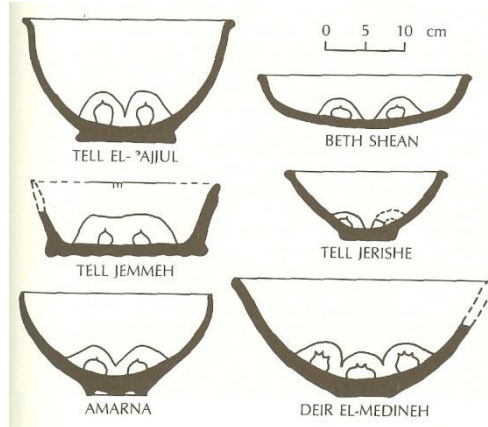


Fig. 9. “Clay bowls with internal loops, from Egypt and Palestine; Late Bronze Age. Note grooves worn inside the loops, from thread being pulled through.” (34)



Fig. 10. “Looped bowl, probably for wetting linen thread, from an Early Bronze II village at Myrtos, Crete; late 3<sup>rd</sup> millennium.” (35)

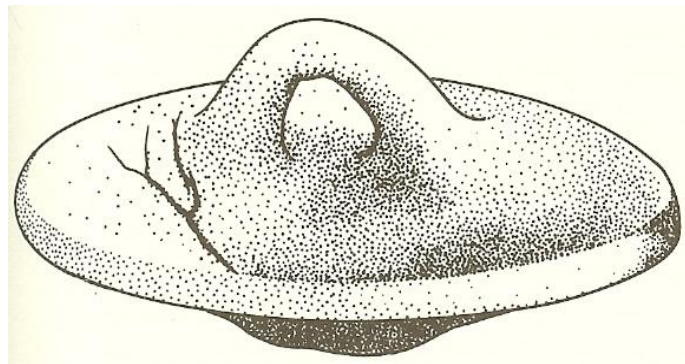


Fig. 11. “Looped bowl, from Drakones, Crete; Middle Bronze Age. Note wear under loop.”

(Original in Stephanos A. Xanthoudides, **Vaulted Tombs of the Messara**, (London), 1924, pl. 42 no. 5033) (36)

Elsewhere, Barber says,

Egyptian estate managers expected to store up enormous quantities of linens; we typically find hundreds of large sheets in a single plundered tomb. The job must have seemed endless, like filling a bottomless hole. The women probably induced the splices, which are merely twisted, not knotted, to stick together by wetting them with saliva, since saliva contains enzymes that decompose the cellulose of the flax slightly into a gluey substance. The Hebrews practiced the same method, learning it while living in Egypt; the special Hebrew word in Exodus for making thread out of flax, *shazar*, means both “to twist” and “to glue.” (37)

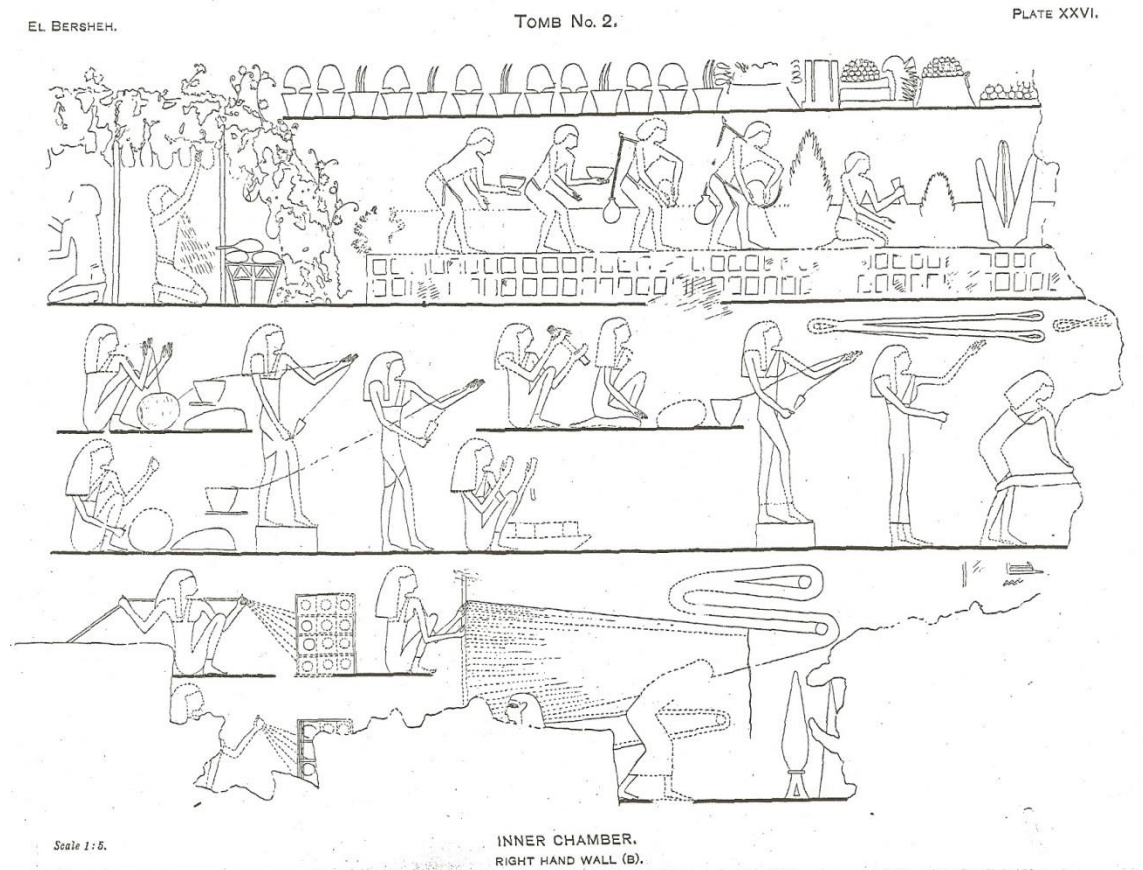


Fig. 12. In the above portrayal, taken from Egyptian tomb no. 2 at El Bersheh, one can see in the middle register the women working with the spinning process. It is apparent that the woman in the upper left of the middle register has one end of the string in her mouth, probably chewing on it to create a sticky substance from the cellulose. The fibers are coming from wetting bowls on the floor as the workers twist them into linen yarn. This is a copy drawn by archaeologists from the original mural on the tomb wall. Not all of the details were preserved because the mural was in a state of deterioration. But it is likely that the original showed lines going from the balls of fiber into the wetting bowl. One such line is preserved in the upper left of the middle register.

On the upper right of the middle register, a skein of completed yarn is wound onto pegs on the wall of the spinning room. That this is the correct interpretation is shown in small models that have been preserved in various tombs in Egypt. (38a [& 38b]).

Perhaps we are now able to ask a question about Rogers' statement with the above background as our setting. Can it be that what Rogers meant by his statement above, "*the weavers had simply laid it in next to the previous length rather than splicing*" was that the ancient weavers wetted the yarn, perhaps with saliva on the cellulose, to make the newly added yarn stick as they continued to develop the warp? Research into the actual development of the warp on the Shroud should be conducted to determine what exactly is the case here.

## **Invisible Reweaving:**

As noted in endnote 21 of my paper, "What Went Wrong..." I alluded to the fact that when John Jackson and his colleagues researched for evidence of a reweave in the so-called Raes' Corner of the Shroud, and found no such evidence, they could confidently dismiss a repair to that corner thus assuming that the nature of that corner was homogeneous with the main body of the Shroud cloth.

In this section I wish to explore the nature of invisible reweaving. Many years ago there was a company known as the "Fabricon Company" in Chicago, Illinois that specialized in the French system of invisible reweaving. They actually produced a manual--not on the history of the technique, but on how to do this invisible repair using the French method. Entitled **The Frenway System of French Reweaving** and subtitled "Detailed and Complete Instructions in the Art of French Invisible Reweaving" it constituted a 71 page manual with step-by-step explanations and drawing illustrating each stage in the process. It was copyrighted in 1951, 1954, 1962 and 1967 (39).

The introduction contains the following statement:

Although an intense search has been going on during the last several years for job opportunities, a very satisfying occupation, FRENCH INVISIBLE REWEAVING, has been overlooked...Probably the reason this art of reweaving has gone relatively unnoticed is the great secrecy which has heretofore kept all but a few people in the world in ignorance of the techniques involved. These secrets have been closely guarded and handed down from generation to generation to a select few. (40)

The manual shows with drawn illustrations and with clearly written text each step one must take to reproduce the technique known as "French Reweaving". Perhaps the single most significant feature of this method is that it *uses no knots* and therefore would not likely show up clearly on the x-rays unless one knew what they were looking for. (41)

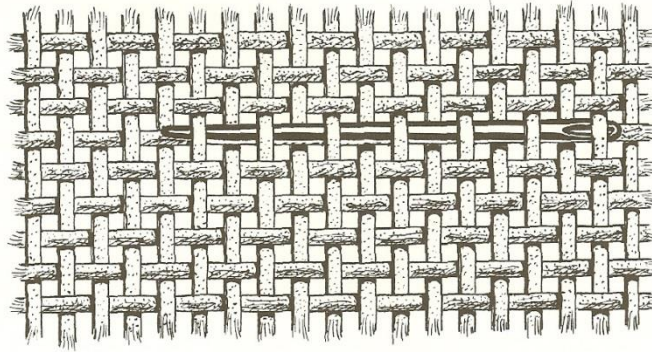


Fig. 13. Visual Gram no. 3 showing a needle tracing one woof (weft) thread on a simple one and one (tabby) weave. The needle goes under one warp thread and over the next warp thread and under the next, etc. The restorer pulls a yarn of the same ply as the original material through in this fashion to cover the area being repaired. This is done thread by thread until the entire repair is made. (42)

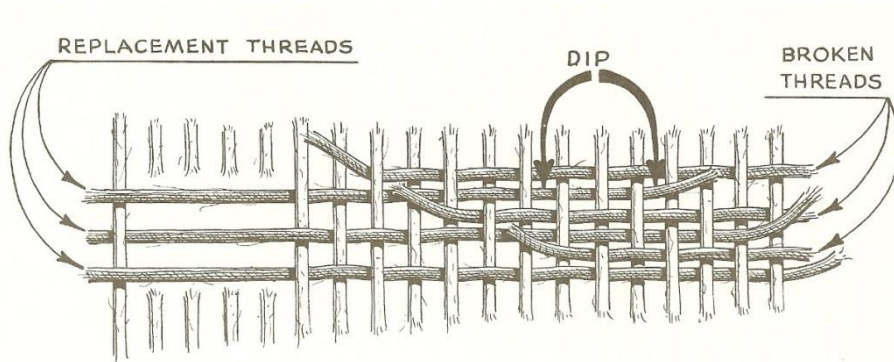


Fig. 14. “Frenway Visual Gram no. 8. Notice that the replacement threads run all the way across the warp. The broken threads will be teased out of the cloth. In this diagram the warp threads have yet to be replaced.” (43)

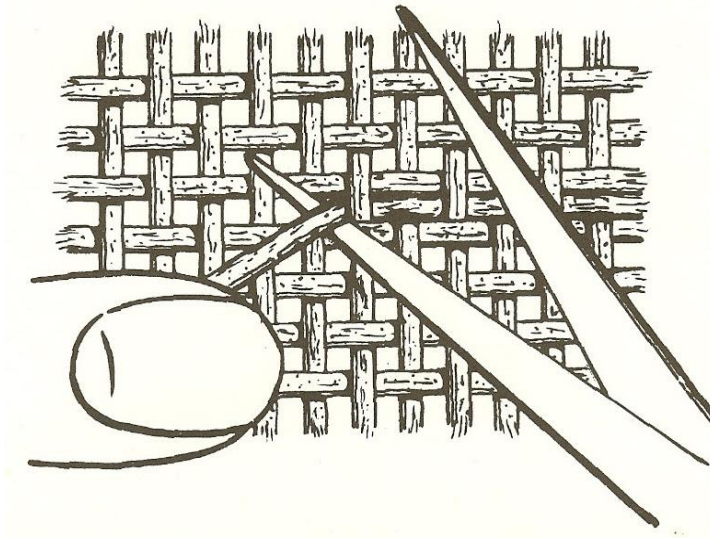


Fig. 15. Visual Gram no. 10. Notice that the damaged thread is being snipped off exactly at the margin of the warp thread. The authors of the Frenway system note that the thread should now pop back into the weave of the cloth. If it does not it should be pushed into the backside (presumably by using a tweezer to tease out the thread to be cut again, and the cloth to be pulled taut to withdraw the thread back into the weave of the textile. (44)

### **The finished product:**

If we think that the Shroud is a large piece of cloth, E. W. Barber tells us that “We have Egyptian linens as much as 9 feet wide and 75 feet long. At a mere hundred threads to the inch, that’s more than 153 miles of yarn to measure out--the distance from New York to Providence, or Seattle to Portland.” (45) But Pliny the Elder (Book XIX, VI.23 & 24) notes that large piece work was used in early times as awnings; Julius Caesar is said to have stretched awnings over the entirety of the Roman Forum.

## Endnotes To Appendix II

1. J. P. Jackson, K. E. Propp, & D. R. Fornoff, in their article, “On the Scientific Validity of the Shroud’s Radiocarbon Date” in **Proceedings of the 1999 Shroud of Turin International Research Conference, Richmond, Virginia**. Magisterium Press: 2000, pp. 283-301.
2. Orazio Petrosillo, Emanuela Marinelli. **The Enigma of the Shroud: The Challenge to Science**, Publishers Enterprise Group: 1996, p. 149. (See also their original Italian edition, **La Sindone: Un Enigma all prova della Science**, Rizzoli, 1990, p. 161).
3. Michael Zohary, **Flora Palaestina, Pt. 2, Text, Platanaceae to Umbilliferae**. Jerusalem, The Israel Academy of Sciences and Humanities: 1972, p. 260.
4. Michrel Zohary, **Flora Palaestina, Pt. 2, Plates, Platanaceae to Umbilliferae**. Jerusalem, The Israel Academy of Sciences and Humanities: 1972, plate 374.
5. Adapted from Merrimack Valley Textile Museum, **All Sorts of Good Sufficient Cloth: Linen Making in New England, 1640-1860**, Merrimack Valley Textile Museum, North Andover, MA: 1980, p. 35.
6. John H. Heller and Alan D. Adler, “A Chemical Investigation of the Shroud of Turin”. **Canadian Society of Forensic Science Journal**, Vol. 14, no. 3 (1981), pp. 81-103, see discussion on p. 98.
7. See p. 192, col. B. in work cited for this information.
8. Pliny the Elder, Book XIX, II, 11.] “centeno quinquageno filo constare”. But I am not entirely certain about what is meant by this. Does he actually mean “fiber” instead of “yarn”-- I.e. 150 fibers per string (thread/yarn)? I think he does because elsewhere, Pliny says that in the Temple of Minerva (goddess of weaving) at Lindus, on Rhodes, each thread consisted of 365 separate threads (“templo Minervae filis singula fila constare”).
9. See the original article in **Bulletin du CIETA**, no. 67 (1989), pp 11-24, esp. p. 13. This article was translated into English in large part and published as “The Shroud of Turin: A Technical Study” in **Shroud Spectrum International**, Vol. IX, No. 38/39 (March/June 1991), pp. 7-20, see esp. p. 10. I did not use the photograph published in the English translation since it was cropped there to fit the page. The total of all the diameters of the yarns (provided in the text below) is 147.1 mm. Vial has rounded this off to 147.0 total which has a width at the bottom of the photograph of 18 ½ inches. I am therefore making an assumption here: that the measurements are in metrics which, in turn, I have converted to the foot scale.
10. **Bulletin du CIETA**, no. 67 (1989), pp 11-24, esp. p. 13. This article was translated into English in large part and published as “The Shroud of Turin: A Technical Study” in **Shroud Spectrum International**, Vol. IX, No. 38/39 (March/June 1991), pp. 7-20, see esp. p. 10.



11. A “roving” is defined in **Fairchild’s Dictionary of Textiles**, Fairchild Publications, New York: 1959, 1967, p. 490 as “A loose assemblage of fibers drawn or rubbed into a single strand, with very little twist.”
12. E. W. Barber, **Women’s Work: The First 20,000 years. Women, Cloth, and Society in Early Times**. W. W. Norton & Company, New York & London: 1994, p. 197.
13. Matthews, **The Textile Fibres: Their Physical, Microscopical, and Chemical Properties**. John Wiley & Sons, New York & London: 1904, p. 180.
14. E. W. Barber, Op. cit., (endnote 12) p. 34.
15. G. Vial, Op. cit. (see above endnote 9), see warp in French on p. 13 (English, p. 8), weft in French on p. 15, (English, p. 11).
16. John Tyrer, “The Turin Shroud--Looking at the Turin Shroud as a textile.” in **Textiles Horizons**, Vol. 1, no. 4, Dec. 1981, pp. 20-23, see esp. p. 22, col. A.
17. For the details of this, one can consult my article, “The Shroud’s Image and Numismatics in Byzantine Research”, **The ASSIST Newsletter**, Vol. 1, no. 2, Dec. 1989, pp. 5-9, see esp. the special end-feature which focuses on the dark threads running through both right and left eyes.
18. Tyrer, *ibid*, p. 22, col. A.
19. A David & Charles Craft Book, 1987, p. 16.
20. See my reference to her discussion in the main body of the conference paper, p. 10 and endnote 40.
21. Presented in R. J. Forbes, **Studies in Ancient Technology**, Vol. 4, E. J. Brill: Leiden, The Netherlands, 1966. See fig. 4, p. 85 and discussion there.
22. J. H. Heller & A. D. Adler, “A Chemical Investigation of the Shroud of Turin” **Canadian Society of Forensic Science Journal**, Vol. 14, no. 3, (1981), pp. 81-103, esp. p. 95 for discussion.
23. See R. J. Forbes, **Studies in Ancient Technology**, Vol. 4, p. 84.
24. Martha Goodway, “Fiber Identification in Practice” **Journal of the American Institute for Conservation**, (JAIC Online edition), JAIC 1987, Vol. 26, no. 1, article 3 (pp. 27 to 44, p. 27).
25. This figure is taken from Martha Goodway, *ibid*, endnote 3, figure 1.
26. Structure of a cotton fiber taken from the “Pulp and Paper Dictionary” only at [www.paperonweb.com/dict11.htm](http://www.paperonweb.com/dict11.htm) under C, “cotton fiber.”
27. E. W. Barber, **Prehistoric Textiles**, Princeton University Press, Princeton, NJ: 1991, pp 46-

47. Italics hers.
28. Barber, op. cit, (see endnote 27), p. 47.
29. E. W. Barber, op. cit., (see endnote 27), pp. 47-48. She quotes Louisa Bellinger's "Craft habits, Part II: Spinning and Fibers in Warp Yarns" **The Textile Museum: Workshop Notes**, no. 20, p. 1.
30. Barber, op. cit., (see endnote 27), p. 47.
31. Davenport, Elsie G. **Your Handspinning**. Select Books, Tarzana, CA: 1953, 1964, p. 82.
32. Barber, op. cit (see endnote 27), p. 10.
33. Barber, op. cit. (see endnote 27), pp. 70-77.
34. From Trude K. Dothan, 1963, "Spinning Bowls", *Israel Exploration Journal*, 13:97-112. Quoted in Barber, op. cit (see endnote 27), p. 47.
35. Barber, op. cit. (see endnote 27), p. 75.
36. Barber, idem.
37. E. W. Barber, **Women's Work: The First 20,000 years. Women, Cloth, and Society in Early Times**. W. W. Norton & Company, New York & London: 1994, pp. 191-192.
- 38a. Reduced from plate 26 in Percy E. Newberry, Archaeological Survey of Egypt, **El Bersheh, Pt. 1, The Tomb of Tehuti-Hetep**, Egypt Exploration Fund. London: n.d. This plate is described and explained on pp. 35-36 of Newberry's introduction; the murals in the tomb encompass numerous occupations including spinning and weaving shown above.
- [38b. The author has a Bactrian bronze wetting "bowl" in his collection. Bactrians were nomads and, thus, such wetting bowls had to be compact and able to resist breaking during their travels. (P.C.M)].
39. My copy is the 1962 edition, "Privately printed and published by THE FABRICON COMPANY in a limited edition." My research suggests that the company now no longer exists but published another edition of this manual in 1967.
40. Ibid, p. 2.
41. See the comments of Linda Eaton in my paper, "What Went Wrong..." endnote 17.
42. **Frenway System**, p. 14.
43. Ibid, p. 26.

44. Ibid, p. 30.
45. Barber, op. cit. (see endnote 37), p. 196.

**Appendix III:** The post-*Thermochemica Acta* e-mail synopsis of the discussion between Ray Rogers and Bryan J. Walsh. (Courtesy of Bryan J. Walsh). [4 pages]

## Appendix III

*Dialogue between Ray Rogers and Bryan Walsh*  
February 2005

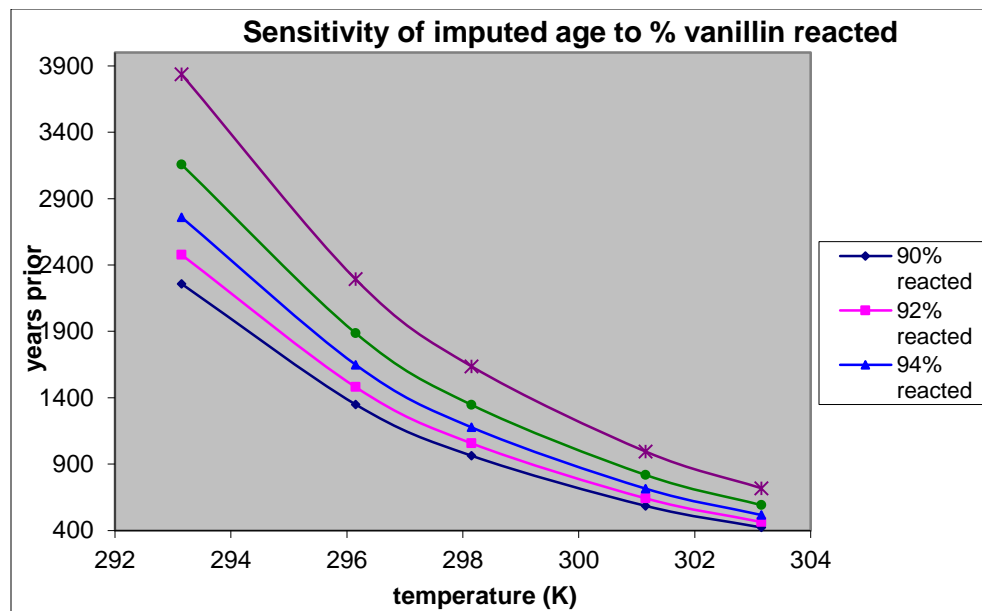
The following is a brief synopsis of a series of e-mail communications between Ray Rogers and I conducted during the first half of February 2005 – shortly after Ray’s paper was published in *Thermochemica Acta*. Ray and I originally exchanged views on the radiocarbon dating of the Shroud and the chemistry of areas adjacent to the radiocarbon sample site in May-June 2003. It was at that time that he mentioned his hypothesis regarding the artificial coloring of the Shroud sample.

The correspondence subsequent to the publication of his paper in January 2005 centered on my requests for clarification of a number of points made in the paper. I sent Ray a three page set of questions and accompanying graphs to which he responded. The following is a summary of that dialogue:

**Paper:** “*The lignin at growth nodes on the shroud’s flax fibers did not give the usual chemical spot test for lignin...*”

**Question:** *Is the phloroglucinol/HCl test for vanillin a quantitative test?*

**Reason for question:** The test appears to depend upon detection of a color change but it is not clear if the point of minimal color change corresponds to a >95% reaction of vanillin. The paper states that the test has good sensitivity, yet the sensitivity isn’t stated. This has a bearing on any imputed date since the percent of lignin reacted is a component of the reaction rate equation as shown on the chart below:



**Fig 1.** Sensitivity curves relating the percentage of vanillin reacted to the derived years of reaction. For example, at 25 °C (298.15 K) with 92% vanillin reacted, the number of years derived is ~1056 years while at 98% reacted at the same temperature the number of years is ~1636 years. A 6% change in % reacted produces a difference of ~673 years in imputed time.

**Rogers response:** You are absolutely correct, but I had to work within the limits of my laboratory. The phloroglucinol/HCl test has never been used quantitatively as far as I know. For one thing, lignin from different sources differs in composition somewhat...The detection limit for the test seemed to be relatively constant (keeping test conditions constant); however, I could not tell what the % decomposition was at the detection limit...Your calculations show exactly why I put such large error bars on my estimate. However, the fact that a consistent test with a presumably constant detection limit worked on medieval cloth and not on the Shroud or other very old cloths should be worth stating.

**Paper:** *However, some reasonable storage temperatures can be considered to give a range of predicted ages. If the shroud had been stored at a constant 25 °C, it would have taken about 1319 years to lose a conservative 95% of its vanillin.*

**Question:** *How accurate are the calculations shown?*

**Reason for question:** When the appropriate values are entered into the Arrhenius reaction rate equations noted in the paper, the results produced are somewhat different than those stated. Using a gas constant of  $8.314472 \text{ J mol}^{-1} \text{ K}^{-1}$  (NIST 2002 standard) produces the following results at 95% vanillin reacted:

<u>°C</u>	<u>T (K)</u>	<u>k</u>	<u>t (derived)</u>	<u>t (paper)</u>
20	293.15	3.2320E-11	2,937	3,095
23	296.15	5.4067E-11	1,756	1,845

25                    298.15                    7.5754E-11 1,253                    1,319

**Table 1.** Comparison of time calculated (in years) to time discussed in paper using NIST gas constant and values derived for flax by Rogers assuming 95% vanillin reacted.

The source of the difference noted is unknown. Reasonably close approximations to the time results published can be derived using 95.75% as the % vanillin reacted rather than 95.00%. In addition, further in the paper a comment is made that linen produced in A.D. 1260 would have retained about 37% of its vanillin in 1978. Using the equations noted above, at 20 °C ~48% of the vanillin would have remained *not* the 37% noted in the paper.

**Rogers response:** My point was that temperature was important. ...I have done all the math for degradation of explosives in nuclear weapons (when I knew temperature cycles), but I didn't have any temperature history for the Shroud.

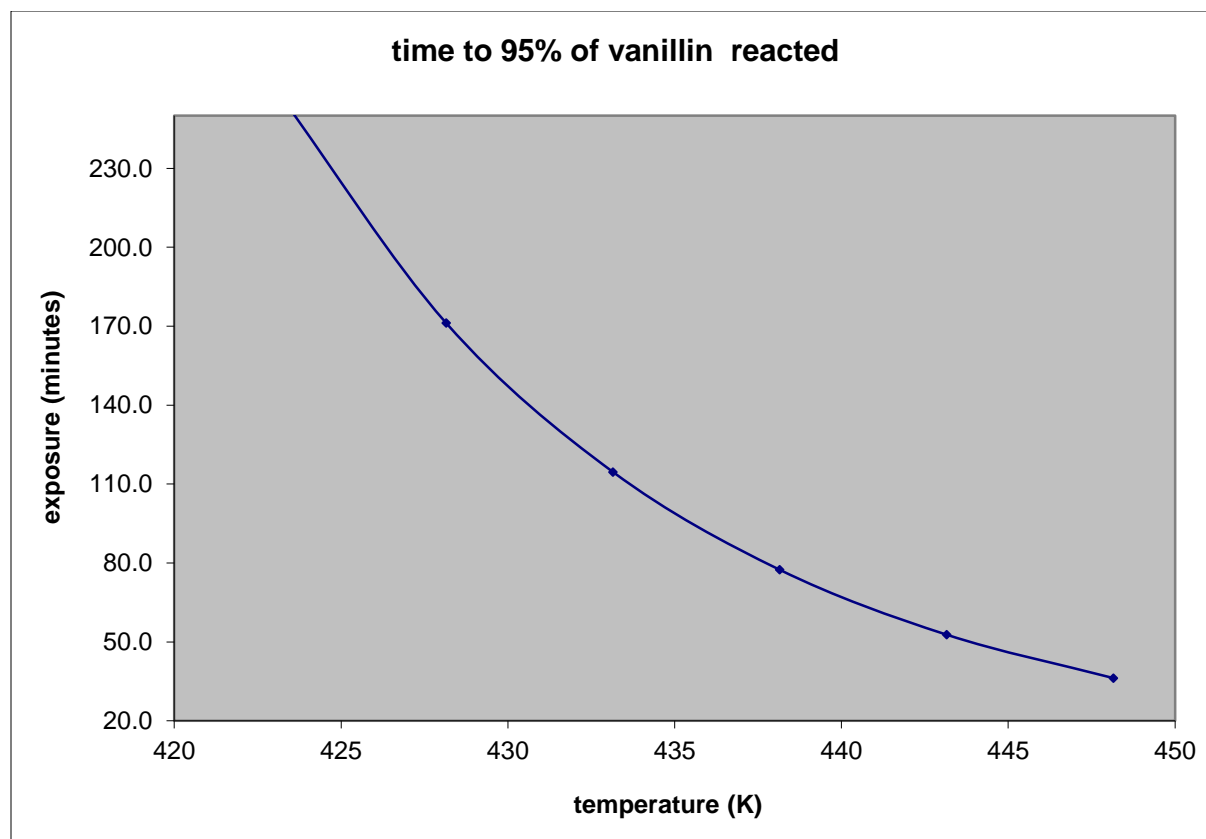
**Paper:** *The fire of 1532 could not have greatly affected the vanillin content of lignin in all parts of the shroud equally.*

**Question:** *Was the internal thermal environment of the Shroud reliquary box modeled?*

**Reason for question:** The 1532 fire was sufficiently hot that it melted a portion of the silver amalgam that made up the reliquary and burned sections of the cloth. As the paper notes, a very steep thermal gradient would have been established inside the box as the result of the placement of the reliquary within the stone wall of the Church that housed it and its surfaces exposed to the fire.

The temperature regime experienced by the Shroud flax was impacted by the length of time at elevated temperature, the rate of diffusion of the hot gases through the flax fabric, the radiative heat transfer between the flax fibers and the nature of the contact between the reliquary walls and the Shroud fibers. The net result is likely to be an elevated, but less-steep thermal environment observed across the Shroud flax fibers.

The precise nature of the thermal environment likely to have been experienced needs to be experimentally derived since the vanillin-dating hypothesis for the Shroud linen is directly impacted by the thermal environment that occurred during the 1532 fire. To illustrate the importance of the temperature environment associated with the fire, the effect of elevated temperature on reacted vanillin is shown on Figure 3.



**Fig. 3.** Effect of elevated thermal environment on the % of vanillin reacted. For example, at 448.15 K (175 °C), the flax would take ~ 36 minutes before 95% of the vanillin would have been reacted. The lack of vanillin-positive tests on the Shroud linen *could* be explained solely by the elevated temperature regime experienced in the 1532 fire.

**Rogers response:** I am retired. I could not get access to the computers or routines I used to make 3-D reactive-heat-flow calculations for weapons. This is a 3-D problem, and it has only been relatively recently that we could make such calculations. I made some 1-D estimates, but I didn't even publish them. A lot never made it into the paper...Remember there are blood spots all over the cloth, and sulfoproteins evolve H<sub>2</sub>S at low temperatures and they evolve hydroxyproline through roughly the same temperature ranges. Only blood spots near the scorches showed degradation. Your figure 3 shows a t/T curve. The same applies to the other compounds on the cloth. For example, pentose impurities on the non-image areas A (or even the hexose ketoses) would have shown degradation within 4 hrs at 100C.

**Paper:** *The Holland cloth and other medieval linens gave a clear test (for vanillin).*

**Question:** *Were the Raes yarn flax and Shroud fibers from the radiocarbon site also tested for the presence of vanillin?*

**Reason for question:** It is not clear from the paper whether or not these flax threads were included in the testing for the presence of vanillin. Further, it would be helpful to describe the location and vanillin test results for each of the Shroud samples tested since it is not clear precisely how many Shroud samples were tested.

**Rogers response:** The comparison among the Raes, Kate Edgerton's, and Shroud fibers was what first made me aware of the time-based degradation of lignin as a potential age-determining method. I thought it was clear that they came under the category "other medieval" samples. Both areas show the lignin test. You have to do the tests under a microscope, looking closely at the spots that look like lignin. The better the bleaching method the more difficult the test.

*Subsequent to this correspondence, Ray and I [Bryan Walsh] had another exchange where he clarified further some of the statements he made earlier:*

**Rogers (in response to a comment on the derivation of reaction rates from me):** As for historic samples of linen, the only ones that would be useful are ones with known storage conditions. Now...something that had been stored with desiccant in the caves at Cheddar for about 300, 1,000, 1,500, and 2,000 years could be great. The great dependence of rate on temperature makes maxima overwhelm years of normal storage.

**Rogers (in response to an observation of the 1532 fire conditions from me):** I did 1-D calculations before going to Turin. The center of mass of the cloth should have remained nearly constant during the time estimated. That could be checked by looking at different blood spots and cloth composition from different areas. For example, blood near a scorch fails to give the iodine-azide test for sulfoproteins. They lose H<sub>2</sub>S at quite low temperatures rather quickly. Blood in all other areas gives the test and the test for 4-hydroxyproline. When we consider reactions coupled with the heat flow, we have to get very complicated (see 1 - D.A. Frank-Kamenetskii, *Diffusion and Heat Transfer in Chemical Kinetics*, Plenum Press, New York, NY (1969), 2 - N.N. Semenov, *Chemical Kinetics and Chain Reactions*, Oxford University Press, London (1935).)

In an aside he stated that the kinetics constants he used were not derived from specific areas from the Shroud. The constants were derived from 'standard materials'.

## ENDNOTES TO MAIN PAPER

1. Credits: I wish to thank Bryan Walsh, Barrie M. Schwartz and Joe Marino for reading earlier versions of this paper. I also thank Mr. Schwartz for his kind and generous help with the illustrations. Additionally, thanks are due to Marie-Claire Van Oosterwyck-Gastuche for discussing with me many aspects of her own approach to the problem of the radiocarbon dating of the Shroud. Thanks are also due to Eleanor Bittle, weaver, for her discussion with me, and for the generous time given by Linda Eaton, curator for textiles at the Winterthur Museum, (Winterthur, Delaware), to personnel at the Peter Wentz Farmstead for their kind information on specialist contacts, and finally to Deborah Peterson who provided input on bleaching. Any errors that remain and, of course, my own point of view, are solely my responsibility and should not be attributed to those who reviewed and/or discussed my work.

In addition to pre-readings and discussions with the above named persons, I have conducted research at the following institutions and the librarians there should also be heartily thanked for their kindness and help: Bucks County Library, James A. Michener Branch; Lehigh University in Bethlehem, PA (Linderman and Fairchild-Martindale libraries); Philadelphia University (formerly Philadelphia College of Textiles and Science) with special thanks to Jordana Shane; University Museum Library of the University of Pennsylvania with special thanks to head librarian John M. Weeks; and finally Penn State University (Pattee & Paterno libraries in State College, PA).

2. For an excellent recap of the history of the progress of radiocarbon dating technology, including a review of the Turin Shroud testing, one may turn to Lloyd A. Currie's article "The Remarkable Metrological History of Radiocarbon Dating [II]" in **Journal of Research of the National Institute of Standards and Technology**, March-April 2004, Vol. 109, No. 2, pp. 185-217, esp. pp. 200-204. (I wish to thank Lloyd Currie for kindly making this available to me.) Also, an earlier assessment of the radiocarbon dating of the Shroud was released by John P. Jackson, Keith E. Propp, & David R. Fornoff "On the Scientific Validity of the Shroud's Radiocarbon Date" in Bryan J. Walsh, **Proceedings of the 1999 Shroud of Turin International Research Conference, Richmond, Virginia**. Magisterium Press: 2000, pp. 283-301.

3. Kenneth E. Stevenson & Gary R. Habermas, **The Shroud and the Controversy**. Nashville, Thomas Nelson Publishers: 1990, point 9 on p. 58 and fn. 21 for chapter 3. Thomas W. Case, **The Shroud of Turin and the C-14 Dating Fiasco: A Scientific Detective Story**. White Horse Press, Cincinnati, OH, 1996, pp. 33-34 & 75-77. William Meacham, **The Rape of the Turin Shroud: How Christianity's most precious relic was wrongly condemned, and violated**. Lulu.com, 2005, pp. 102-103. There is a puzzling discrepancy regarding the date obtained from one end of this thread. Case says that one end dated to 1200 AD--his stated source for this is Adler. But in the interview the dates quoted are 200 AD to 1000 AD (p. 75) where the source for this information was from a news release to which Stevenson and Habermas had referred (ibid, p. 58 and fn. 21).

4. Damon, P. E., Donahue, D. J., Gore, B. H., Hatheway, A. L., Jull, A. J. T., Linick, T. W., Sercel, P. J., Toolin, C. L., Bronk, C. Ramsey, Hall, E. T., Hedges, R. E. M., Housley, R., Law, I. A., Perry, C., Bonani, G., Ambers, J. C., Bowman, S. G. E., Leese, M. N., Tite, M. S., Trumbore, S., Woefli, W., "Radiocarbon dating of the Shroud of Turin." **Nature**, Vol. 337, 1989, pp. 611-615.

5. Bulst, W., S. J. "Some comments on the Turin Shroud after the carbon test." **Shroud News**, Aug. 1989, no. 54, pp. 4-9.

6. Bonnet-Eymard, B. "The Crime Committed against the Holy Shroud." **Shroud News**, June 1996, no. 95, pp. 10-27. --"The dating of the Holy Shroud: Summary of the Carbon 14 affair." **The Catholic Counter Reformation in the XXth Century**. June, 1989, no. 220, pp. 26-34. But see all of his publications from 1988--many of them appearing in either the French version or the English translation. At first sight it might have seemed to have fit an important scientific rule of thumb: the simplest answer is the best answer and this appeared, on the face of it, simple. But when looking under the surface it required the collusion of a very large array of persons all



the way from the top including Dr. Michael Tite of the British Museum, Cardinal Ballestrero's science advisor, Prof. Luigi Gonella and Prof. Giovanni Riggi di Numana who took the sample, all the way down to the individual members of the various laboratories who did the testing. Aside from impugning all of their various reputations, for no clear reasons other than that the original weights of the samples did not seem to tally, it really turned out NOT to be so simple! I have always taken the stance that unless we were given clear and unmitigated evidence of fraud, then fraud was really NOT the best answer. It *had* to be rejected because, in fact, it violated Ockham's Razor in that it required too complex a scenario to be believed.

7. Van Oosterwyck-Gastuche, Marie-Claire. **Le Radiocarbone face au Linceul de Turin: Journal d'une recherche.** Francois-Xavier de Guibert: Paris, 1999. One should also be apprised of her more recent paper, "**Attack of the Turin Shroud during the 1532 fire**". This paper, while it was not officially read at the Dallas 2005 conference, was allegedly attached to their proceedings. Unfortunately, those proceedings remain unpublished except in audio form in a CD set by Thomas Sullivan. Papers not verbally read at the conference are not included on the CD set. I checked my personal set of this CD and it appears that Van Oosterwyck-Gastuche's paper is most unfortunately not included.

8. Kouznetsov, D. A. "From Moscow's Dr. Dmitri Kouznetsov--an answer to the criticisms of Prof. Tite." **BSTS Newsletter**, Dec. 1993-1994, no. 36, pp. 3-5. "A progress report on research into the old Textile Radiocarbon Dating Results." **La Lettre Mensuelle du C. I. E. L. T.**, Vol. No. 54, June 1994, p. 6. A. A. Ivanov, P. R. Veletsky. "Effects of Fires on Biofractionation of Carbon Isotopes on Results of Radiocarbon Dating of Old Textiles: The Shroud of Turin." **Journal of Archaeological Science**, 1996, 23:1, pp. 109-121. Initially, there was considerably more support for this idea particularly as published by the late John Tyrer in Manchester, England. But Kouznetsov's proposal has now largely been ruled out by numerous reviewers and especially by A. J. T. Jull and his associates at the NSF Radiocarbon facility at the University of Arizona. (See next endnote).

9. Jull, A. J. T., D. J. Donahue, P. E. Damon, "Factors Affecting the Apparent Radiocarbon Age of Textiles: A Comment on "Effects of Fires..." by D. A. Kouznetsov, et al. **Journal of Archaeological Science**, January, 1996, Vol. 23:1, pp. 157-160. Bryan Walsh observes "Jull's test did NOT replicate the Kouznetsov conditions--there were several key changes to the conditions that rendered the comparison weak at best. Kouznetsov's hypothesis was supported by some theoretical work by John Jackson but has never been satisfactorily replicated in an experimental setting." (Personal communication). Van Oosterwyck-Gastuche also points out that water was never added to the Jull tests conducted to evaluate Kouznetsov's work. (Personal communication). See also J. P. Jackson, K. E. Propp & D. R. Fornoff "On the Scientific Validity of the Shroud's Radiocarbon Date" in **Proceedings of the 1999 Shroud of Turin International Research Conference, Richmond, Virginia.** Magisterium Press: 2000, pp. 295-299.

10. Garza-Valdes, L. A. "Biogenic varnish and the Shroud of Turin." in A. A. Upinsky (Ed.) **L'Identification Scientifique de l'Homme du Linceul Jesus de Nazareth. Acts du Symposium Scientifique International**, Rome, 1993: C. I. E. L. T.: 1993, Paris: C. I. E. L. T., 1995, pp. 279-282. --**The DNA of God.** NY: Doubleday, 1999.

11. Communication to me from the private conference convened in San Antonio, TX by Dr. L. A. Garza-Valdes where the “bioplastic” coating was measured by H. E. Gove. See further the related article by Gove, H. E., S. J. Mattingly, A. R. David, L. A. Garza-Valdes. “A Problematic Source of Organic Contamination of Linen.” **Nuclear Instruments and Methods in Physics Research. B.**, Vol. 123, 1997, pp. 504-507. But critics of this theory have noted that were this proposal to be genuinely the case the Shroud would have to weigh more it does today. (J. P. Jackson, private communication; see also Jackson, Propp & Fornoff “On the Scientific Validity of the Shroud’s Radiocarbon Date“ in Bryan J. Walsh (Ed.) **Proceedings of the 1999 Shroud of Turin International Research Conference, Richmond, Virginia.** Magisterium Press: 2000, p. 294). Technically speaking, however, this proposal has never actually been fully tested. (Personal communication, L. A. Garza-Valdes). For a technical evaluation of the work done by Garza-Valdes see: Alan D. Adler, “The nature of the body images on the Shroud of Turin.” in D. Crispino, **The Orphaned Manuscript: A Gathering of Publications on the Shroud of Turin. A Shroud Spectrum International Special Issue.** Effata’ Editrice: 2002, pp. 103-112, esp. pp. 108-110. This article was first published in Bryan Walsh, (ed.). **Proceedings of the 1999 Shroud of Turin International Research Conference, Richmond, Virginia.** Magisterium Press: 2000, pp. 19-29.

12. Phillips, Thomas J. “Shroud irradiated with neutrons?” **Nature**, Vol. 337, 16 Feb., 1989, p. 594. Also: “The letter that “Nature” did not print...” [Response to R. E. M. Hedges’ letter published in **Nature**, 337, p 594.], **British Society for the Turin Shroud Newsletter**, no. 22, May, 1989, pp. 8-11.

13. Rinaudo, Jean-Baptiste. “Une hypothese sur le carbone 14” **Il Est Vivant**, no. 73, Nov.-Dec., 1989, pp. 27-28. Also: “Nouvelle hypothese sur la formation de linage du Linceul de Turin. Invalidant son age radiocarbone.” **Montre-Nous Ton Visage**, no. 3, March, 1990, pp. 9-11. Also: “The cause of the image on the Shroud and the results of the carbon date: a cohering hypothesis.” [Unpublished manuscript]

14. Lindner, Eberhard. “Hypothese expliquant la formation de toutes les traces dans le Linceul de Turin.” in A. A. Upinsky (Ed.). **L’identification Scientifique de l’Homme du Linceul Jesus de Nazareth. Actes du Symposium Scientifique International, Rome 1993.** C. I. E. L. T., Paris. Francois-Xavier de Guibert, 1995, pp. 285-291. Also: “The Shroud and the Resurrection” in C. I. E. L. T. (Ed.). **AXEIPOPOIHTOS ACHEIROPOIETOS: “Non fait de main d’homme”.** Actes du IIIeme Symposium Scientifique International du CIELT--Nice, 1997. Paris, Editions du C.I.E.L.T., 1998, pp. 231-235. Bryan Walsh reminds me of some remarks Ray Rogers had made about this hypothesis: “...he thought there would be radiational damage to the cellulose which would be discernable under a microscope--which he found not evident.” (Personal communication; see further in note 15).

15. Ray Rogers obtained a source of radiation and tried to reduplicate what would happen under conditions of radiation such as a neutron flux. He discovered a signature of damage in the cellulose fibers--radiation tracks--which he believes is not present in Shroud image fibers. For a brief evaluation of the radiation approach see R. N. Rogers & Anna Arnoldi in “Scientific Method Applied to the Shroud of Turin: A Review” at <http://www.shroud.com/pdfs/rogers2.pdf> , points 11) and 12) on pp. 9-11 of their document.

For a more extensive evaluation see R. N. Rogers at <http://www.shroud.com/pdfs/rogers8.pdf>.

16. Marino, Joseph, M. Sue Benford. "Evidence for the Skewing of the 14C Dating of the Shroud of Turin due to Repairs." in Marinelli, E. & A. Russi (Eds), **Sindone 2000: Worldwide Congress: Proceedings**. Vol. I. San Severo, Italy: Gerni Editori, 2002, pp. 57-64. For online access to M. Sue Benford's and Joe Marino's research it is most convenient to log onto Barrie Schwartz' excellent website <http://www.shroud.com> and click on the PDF files of their important articles released in 2000 and 2002 "Textile Evidence Supports Skewed Radiocarbon Date of Shroud of Turin" in his new posting of January 21, 2005.

17. To this information Linda Eaton, curator for textiles at the Winterthur Museum adds: "...high quality re-weaving would not have knots on the back, but it is immensely difficult, if not impossible, for it to be done so that it remains invisible to close and careful examination, with or without a microscope. This is not something that you can do from photographs, either, as sometimes it is so good that you have to feel it too." (Personal communication, Aug. 9, 2008). This suggests that the Shroud should be independently re-examined close-up with invisible re-weaving in mind to settle, once and for all, any evidence there for such an invisible reweave.

18. A. D. Adler, private phone communication to me not long before his lamented death.

19. See the article by National Geographic writer, Bijal P. Trivedi (April 9, 2004), at <http://news.nationalgeographic.com/news/pf/77128977.html>. This article provides the date plus other insights and information.

20. AM\*STAR is the American Shroud of Turin Association for Research. Some of the members of this new group were formerly with the older Shroud of Turin Research Project (STURP), including its president, Thomas D'Muhala.

21. Rogers, R. N. "Studies on the Radiocarbon Sample from the Shroud of Turin." **Thermochemica Acta**, 20 January 2005, pp. 189-194.

22. J. P. Jackson, K. E. Propp & D. R. Fornoff, in their article, "On the Scientific Validity of the Shroud's Radiocarbon Date" in **Proceedings of the 1999 Shroud of Turin International Research Conference, Richmond, Virginia**. Magisterium Press: 2000, pp. 283-301 have produced an assessment of the "repair hypothesis" that rules it out. However, it needs to be observed that their research into the proposed repair was made prior to the publication of M. Sue Benford's and Joseph Marino's first paper on the "invisible reweave" concept which was given in at the Sindone 2000 Congress held in August, 2000 in Orviedo, Italy. And they have (in 2002) released a second (historically oriented) paper on this research. Although Jackson, et al do not seem to discuss what they are looking for in deciding that the "repair hypothesis" is not valid, I am presuming here that they are looking for "knots" on the backside of the Shroud. They show x-radiographs of the Raes Corner that exhibit no evidence for a repair. I believe that Mechthild Flury-Lemberg also may have critically ruled out an "invisible reweave" on the same basis. However, neither assessment takes into account a true "French invisible reweave" approach where the joining of threads is strictly based upon thread splicing instead of knots. To my knowledge, no one has ever undertaken a thorough study of the continuity of the threads of the main body of the Shroud (I.e. creating connectivity in the spinning process) by which we could

develop criteria to compare and contrast a “French invisible reweave”).

23. See John Jackson’s brief explanation in the scientific papers posted on Barrie Schwartz’ website, [www.shroud.com](http://www.shroud.com). The brief article from which the evaluative quote below was taken, simply entitled “Shroud of Turin,” was posted on the Oxford Radiocarbon Accelerator Unit web site in March 2008: see <http://c14.arch.ox.ac.uk/embed.php?File=shroud.html>

They have given three reasons they do not expect this test to have any effect on the results of 1988:

“Carbon monoxide is only present in very low concentrations in the atmosphere.

Carbon monoxide is not very reactive and would not be expected in normal circumstances to react with a long chain polymer like cellulose in linen.

No contamination like this has been detected before, even on very old samples (up to the 50,000 year limit of radiocarbon) which would be much more severely affected.”

Ramsey continues: “...initial tests show no significant reaction--even though the sensitivity of the measurements is sufficient to detect contamination that would offset the age by a single year.” It is not at all likely that this new round of testing will have any bearing on the much sought after samples from the Turin Shroud itself.

24. Post # 12475 to the Shroud Science Group on the internet dated to ca. April 21, 2008 and, again, to me personally in a post dated to May 3, assures us that the Rae’s Corner is homogeneous. (I thank Joe Marino for helping me pin these messages down.)

25. Raes, Gilbert. “Appendix B -- Rapport d’Analyse: Pl. II-III.” in **La S. Sindone: Recherche e studi della Commissione di Esperti nominata dall’Arcivescovo di Torino, Card. Michelle Pellegrino, nel 1969**. Supplemento **Rivista Diocesana Torinese**, gennaio 1976, pp. 79-83.

26. Maloney, Paul C. “An Index to the Max Frei Sticky Tape Collection.” [Unpublished Manuscript]. One has to carefully discern the difference between the fibers from the cotton gloves Max Frei was wearing from the other extraneous cotton found on the tapes. Usually this can be determined by the coloration and the length of fibers. Whereas the cotton from the Frei gloves [incidentally, the white gloves he wore were borrowed from STURP (Personal communication, R. H. Dinigar)], other cotton will be an off-white or even a beige to “brownish” cast. And sometimes even dyed or pigmented cotton fibers will be seen. The fibers are rarely more than a few millimeters in length on the tapes. All these are clearly *surface* phenomena). This issue was never obfuscated on the STURP tapes. Although those who took STURP’s samples such as, for example, Tom D’Muhala who is seen in Barrie Schwartz’s photographs as wearing gloves, contamination from those gloves to the tapes is not at all apparent. That may be one reason why cotton became an issue with STURP when they discovered significant evidence of cotton from the Raes’ Corner.

27. I am very deeply indebted to Barrie Schwartz for his constant feedback when I called him. Barrie was not only a member of STURP and on the site when much of the original sample taking was done in 1978 (including both Frei's and STURP's), but more importantly since this paper is about the homogeneity/heterogeneity issue of the radiocarbon sample corner and thus about Ray Rogers' chemical investigations into the nature of the Raes' Corner, Barrie was an absolute MUST source for this paper. Barrie knew Ray much better than most of us right down to the end of Ray's life. Barrie will vouch for the absolute rigor Ray expected of himself and others when studying the Shroud. Barrie's website, [www.shroud.com](http://www.shroud.com) is one of the very best and most authoritative websites on the internet about the Turin Shroud. Both its depth and its breadth cannot be surpassed.

28. See Adler, A. D., R. Selzer, DeBlase, F. "Further Spectroscopic Investigations of Samples of the Shroud of Turin" in D. Crispino (Ed.) Alan D. Adler, **The Orphaned Manuscript: A Gathering of Publications on the Shroud of Turin**. Torino, Italia: Effata' Editrice, 2002, pp. 93-102. One must consult p. 94 along with the chart on p. 97 which shows relatively "high" proportions of aluminum on the two warp threads with less on the weft. The authors have chosen "water stain" area samples to compare to these current threads being investigated (see p. 94, 2<sup>nd</sup> full paragraph) but, at least in the case of aluminum, the comparison is very poor. There is no aluminum in the water stain margins. They also do not investigate the coating they found. They merely describe it as "...unevenly colored from dark yellow to splotchy brown, roughly surfaced (even showing patchy encrustations in spots) and showed a very strong and variably multicolored birefringence pattern. Considerable micro debris was also evident." (p. 94). It remained for Ray Rogers to investigate and identify the details. [This article was also published in Adler, A. D., Dame I. Piczek, M. Minor (Eds.) **The Shroud of Turin: Unraveling the Mystery**. Alexander Books: 2002, pp. 166-181.] Bryan Walsh and Rogers often discussed the limitations of Rogers' own home laboratory. Rogers was painfully aware of this and had made arrangements for further studies to be done independently. Hence, we shall have to await the results on the Rogers' threads done post-humously by other researchers.

29. On Saturday, March 28, 1987, ASSIST made the first of a total of four examinations of the True Copy preserved at the Monastery of Our Lady of the Rosary in Summit, NJ. We were very kindly and generously hosted by the late Mother Mary Albert and the Dominican Nuns. The ASSIST team was composed of some 16 specialists in all fields including our president, Frederick T. Zugibe and chemist, Alan D. Adler. While examining the cloth with UV light, Adler observed that he was seeing evidence of starch there. He then remarked to me that there was also some evidence of starch in the so-called Raes' Corner area on the Shroud. Adler may have been referring to the thread Heller had sent to a West Coast laboratory for dating. One end had starch on it. For the details of this one may turn to Thomas Case, **The Shroud of Turin and the C-14 Dating Fiasco: A Scientific Detective Story**. Whitehorse Press, Cincinnati, OH: 1996, pp. 76-77. [See also Meacham, ref. In endnote 3, p. 102; also O. Petrosillo & E. Marinelli, **The Enigma of the Shroud: The Challenge to Science**, Publishers Enterprise Group: 1996, p. 149 make the following statement: "During the pre-treatment, the presence of starch was noted that could have been used for the dressing of the cloth by a medieval restorer. It was commonly used for invisible mending." Unfortunately, the authors do not document this statement. But the observation of starch is otherwise both surprising and puzzling given that both McCrone [see McCrone & Skirius, "Light Microscopical Study of the Turin 'Shroud' I", **The Microscope**,

Vol. 28, Third/Fourth Quarter, 1980, p. 115 & 119; also *ibid*, **Judgement Day for the Turin Shroud**, p. 85] and STURP [Heller, J. H. & A. D. Adler, “A Chemical Investigation of the Shroud of Turin,” **Canadian Society for Forensic Science Journal** Vol. 14, no. 3, (1981), p. 94f (see table and discussion; also Schwalbe, L. A. & R. N. Rogers, “Physics and Chemistry of the Shroud of Turin“ **Analytica Chimica Acta**, Vol. 135, no. 1, (Feb. 1, 1982), p. 24 & 31] found no evidence of it in the main body of the Shroud. To my knowledge neither Heller nor Adler ever published this observation anywhere.

Why didn't starch show up generally and commonly in the main body of the Shroud? Rogers and Arnoldi explain: “Many of the pyrolysis fragments observed by pyrolysis-mass-spectrometry would be the same products of thermal degradation whether they came from cellulose, hexose sugars, or starches; i.e. a starch impurity would not have been detected. UV and visible spectrometry would not see any differences among the carbohydrates. The -OH vibrational states of all the carbohydrates and water are very broad and intense, and IR spectrometry could not distinguish among them. Laser-microprobe Raman is similar to IR. We were not looking for trace carbohydrate impurities, we were looking for painting-type impurities on the cloth.” (See R. N. Rogers and Anna Arnoldi, “Scientific Method Applied to the Shroud of Turin: a Review” 2002, at [www.shroud.com/pdfs/rogers2.pdf](http://www.shroud.com/pdfs/rogers2.pdf), p. 6f.) However, Rogers and Arnoldi further add: “Microchemical spot tests with aqueous iodine indicated the presence of some starch fractions on Shroud fibers.” (Ibid, p. 7; see further below in a reference to Ray Rogers' post-humously published book). Thus, the finding of starch on the main body of the Shroud and at the Raes' Corner is not an unequivocal observation pertaining solely to the Raes' Corner--but the finding of a mordant, rose madder dye, and gum arabic do spell out major differences between the Raes' Corner and the main body of the Shroud. “These deposits are unique to the Raes sample...” (Ibid, p. 7).

Ray Rogers discusses the use of starch in ancient weaving technique. But much of STURP's studies on starch did not, unfortunately, make it into the early scientific literature. Rogers, in his post-humously published book, **A Chemist's Perspective On the Shroud of Turin** (privately published, Raymond N. Rogers, Joan Rogers, Barrie M. Schwartz, 2008), refers to starch on some 16 pages of his book, and alludes again to the microchemical spot test with aqueous iodine on p. 38. He comments about its presence on the Raes' Corner samples: “Some starch could be detected on HCl - cleaned Raes fibers with an aqueous iodine reagent.” (p. 72).

30. Raes, Gilbert. “The Textile Study of 1973-1974”, **Shroud Spectrum International**, Mar./June, 1991, Vol. IX, no. 38-39, pp. 2-6, esp. p. 6.

31a. Brown, John L. “Microscopical Investigation of Selected Raes Threads from the Shroud of Turin. Can be accessed on the internet at: <http://www.shroud.com/pdfs/brown1.pdf>

31b. Robert Villarreal is the team leader of seven scientists at Los Alamos National Laboratory (LANL) who studied some the Raes' Corner samples in 2008. Their report, presented at the Ohio Conference held in Columbus at the Blackwell Hotel on Saturday, August 16, 2008 revealed their initial studies to point to the presence of cotton in these samples. But it now seems, from their preliminary evidence, that the cotton found there contains both a post-dehydrated cotton (I.e. cotton that has already collapsed into the classic form--a ribbon that

twists with fair regularity--and a pre-collapse (sometimes called “wet” or “immature” cotton) form. Please see my further exploration of this information in Appendix II.

32. Anonymous. “Rogue fibers found in the Shroud.” **Textile Horizons**, Vol. 8, no. 12, Dec. 1988, p. 13.

33. Walsh, Bryan J. “The 1988 Shroud of Turin Radiocarbon Tests Reconsidered” in Walsh, Bryan J. (Ed.) **Proceedings of the 1999 Shroud of Turin International Research Conference, Richmond, Virginia**. Magisterium Press: 2000, pp. 326-342, esp. pp. 340f. Rogers would have agreed with this but he said it differently: “No chemical or microscopic investigations were made to characterize the sample. I believe that was a major disaster in the history of Shroud studies.” (See R. N. Rogers and Anna Arnoldi, “Scientific Method Applied to the Shroud of Turin: A Review” (2002) at <http://www.shroud.com/pdfs/rogers2.pdf>, p. 13).

34. I am deeply indebted to Bryan Walsh for kindly sharing a post-**Thermochimica Acta** publication synopsis of the Rogers-Walsh dialogue. I am appending that synopsis along with charts at the end of this paper. See Appendix III. It represents a number of e-mails that were sent between Bryan Walsh and Ray Rogers and gives a more up-to-date view of the problems and nature of work that still needed to be done on the questions surrounding the issue of homogeneity/heterogeneity of the Shroud and, particularly, how the Raes’ Corner relates to the main body of the cloth.

35. Jones, Stephen E. . “A proposal to radiocarbon-date the *pollen* of the Shroud of Turin.” **British Society for the Turin Shroud Newsletter**, no. 66, 15 January 2008. The full article of this proposal may also be accessed on the web at <http://www.shroud.com/pdfs/n66part6.pdf>

36. Currie, Lloyd A. , “The Remarkable Metrological History of Radiocarbon Dating [II]” in **Journal of Research of the National Institute of Standards and Technology**, March-April 2004, Vol. 109, No. 2, pp. 185-217. See p. 204, col. b for discussion of the radiocarbon dating of pollen grains.

37. Danin, Avinoam, A. D. Whanger, Uri Baruch, M. Whanger. **Flora of the Shroud of Turin**. Missouri Botanical Press, St. Louis: 1999. See *Gundelia tournefortii* in U. Baruch’s chart on p. 14.

38. These dusts have been preserved but they are fraught with certain complications. Riggi told me personally (Nov. 21, 1987) that 50 % of his vacuumed pollen were mineral coated. These would have to be separated from the whole and treated differently since the coating would have to be cleaned off using a dilute hydrochloric acid. They should be catalogued separately from the non-coated grains since it is clear that the mineral coated grains occurred ONLY on the non-image side of the Shroud (the side, presumably, that laid directly on the burial bench if we are to assume this cloth to be a shroud; any archaeologist would perceive this as a kind of “stratum”), so that a professional palynologist could discern patterns in the pollen traces. This observation of 50% and ONLY on the non-image side of the cloth was based by Riggi upon his examination of my photo-inventory of 163 pollen grain photomicrographs which I had made of the findings on lead end of Frei’s sticky tape 4 B/d. It was made in response to my question to him “Are any

of these grains, in your opinion, ‘mineral coated?’“ His response was, “None of them--with maybe one exception.“ The “50 % finding” is therefore an astonishing discovery because this statistic is far too high to be explained away as a random figure.

But currently, the question to address here now is: “Are these mineral coated grains representative of Israeli plants?” The most organized way to treat these dusts would be for a professional palynologist to separate and study *all* of the individual grains for classification so that the study could be made a part of an integrated whole. During this study the *G. tournefortii* grains could be set aside for special consideration such as for Stephen E. Jones’ proposed radiocarbon dating project. If enough *G. tournefortii* are discovered among the mineral coated grains it would actually be preferable to date those because, due to that coating, they can already be perceived as a “single block”. But it should not be supposed that the results would establish a C14 date for the Shroud itself. It is merely to establish a date for an event--in this case, the deposit of a certain set of pollen grains on the burial bench of the tomb (conjecturing that the mineral coated grains came from that environ).

39. See Appendix II for the beginnings of such a compilation. Because no one has as yet ever brought together all of the observations made in various publications of the Shroud, and because other observations also need to be made against a historical background, this Appendix should be considered “open-ended” and incomplete. It is intended to cover such matters as, for example, the nature of the fiber (including average diameter) the nature of the yarns (including the average number of fibers in each [if that can be determined], the retting and/or possibility of hank (skein) bleaching), the weave (3:1 twill, faults, etc.), the finished product (including the possibility of piece bleaching).

40. She and I both agree that this suggestion needs further research and testing. It should therefore address the question of how well processed the fibers were in the retting stage. As a comparison one may consult the types of studies conducted by M. L. Ryder and Thea Gabra-Sanders “A Microscopic Study of Remains of Textiles made from Plant Fibres.” **Oxford Journal of Archaeology**, 6 (1), 1987, pp. 91-108. My thanks to Linda Eaton, at the Winterthur Museum, for her discussion of this and for bringing Ryder’s and Gabra-Sanders’ work to my attention.

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