

# **Private Internet Debate Challenges Ray Rogers' Thermochimica Acta Paper**

An exchange of views between Mark Antonacci and Dr. Thibault Heimburger that was originally posted on the Internet in 2005 on the members-only Yahoo ShroudScience Discussion Group

*(Editor's Note: This first paper is the one I received directly from Mark Antonacci via e-mail attachment on April 21, 2005 and which I refused to publish. It was ultimately posted on the Yahoo ShroudScience group by Giulio Fanti on May 11, 2005).*

## **An Evidentiary Analysis of the Proofs Claimed by Ray Rogers By Mark Antonacci**

In an article recently published in *Thermochimica Acta*, Ray Rogers claims that certain results and observations from procedures that he performed on Shroud samples “prove that the radiocarbon sample was not part of the original cloth of the Shroud of Turin.”<sup>1</sup> While I welcome the relative increase in publicity regarding the authenticity and study of the Shroud of Turin which his article generated, Rogers has not proved his claim. And, although his repair hypothesis is neither in conflict or inconsistent with the hypothesis developed by myself and Dr. Arthur Lind, or many other hypotheses, Mr. Rogers doesn't seem to have presented any palpable or convincing evidence to support his above claim.

The most direct evidence that the radiocarbon sample was part of a repair or patch would come from the cloth itself. The Shroud was most recently examined for more than a month in November of 2002, but no sign of a repair was found when the radiocarbon site was specifically examined. Of course, the Shroud was also examined in 1997 by a group of scientists after a fire burned the dome off the chapel in which the cloth was kept. Dr. Alan Adler was among those scientists. Since he had wondered whether the radiocarbon sample may have been part of a repair, he also specifically examined this area, but did not conclude it had been repaired.<sup>2</sup> Of course the radiocarbon site was also observed for many hours by numerous participants when the sample itself was removed in 1988. If any visual evidence existed that the site was not part of the original cloth, any number of the qualified participants would have spoken up.

In 1978, at least 20 or more members of STURP examined the Shroud in various ways for 120 hours. Neither they nor any of their Italian colleagues observed that the radiocarbon site wasn't part of the original cloth, and since this site was so close to the Raes site, we know it was obviously examined by many scientists. Moreover, between 5,000 – 7,000 photographs of the Shroud were taken at this time in various wavelengths and magnifications, but no such photographs or microphotographs indicated such a repair.

In 1973, when the Raes sample was removed along with other procedures on the Shroud, none of the numerous participants observed a repair, and we know this general area was specifically examined and photographed. I observed an excellent photomicrograph of the Raes

site and the surrounding area taken in or after 1973, but before 1988, that can be found in the Centro Museum. This photomicrograph was greatly magnified and clearly covered the location that became the radiocarbon site in 1988. This photomicrograph makes it very clear that the radiocarbon site is a continuous part of the Shroud.

In the last couple of years various parties have been claiming that medieval restorers could make repairs to textiles in a way that could fool the naked eye. For purposes of debate, let us assume this is true. However, a second assumption is still required to support Rogers' above assertion, i.e. that such an invisible repair was made on the Shroud at the radiocarbon site. Such a repair would stand in stark contrast to the numerous other repairs performed on the Shroud, which are easily visible with the naked eye (and were performed after the 1260-1390 age range erroneously attributed to the Shroud.) Yet, in this case, the argument that such a masterful repair was made to the Shroud also requires a third assumption—that this repair was also kept secret for some reason from the public and from all historical record. There would certainly be no need to keep this a secret from the public or from any private records among the owners, or any number of church officials or members, or by the masterful restorer(s). To repair the Shroud is certainly nothing to be ashamed of. On the contrary, it would be something to be proud of. Assuming the repair was so excellent that no one (to this day) could even see where it occurred would be something to be very proud of. In light of this there should have been some record, as there was with most events involving the Shroud, among the notable and numerous Savoy family records, or by any other owners, custodians, priests, monks, nuns, or textile specialists or restorers.

The question of whether an assumed secretive repair was made that would fool the naked eye is actually a secondary issue. Even if you make all three assumptions (the last two of which are clearly unwarranted), they still do not address two overriding issues required to be overcome to prove a repair hypothesis. One overriding question is whether such an assumed secretive repair can also fool photomicroscopy. Since medieval restorers would not have had the extensive magnification abilities or techniques available today, it is extremely unlikely they would be able to repair cloth in a manner that would be undetected by subsequent magnification, as the Shroud has been clearly subjected to. As can be seen in the above Centro photomicrograph (and by further direct examination below) the radiocarbon site does not appear to have been repaired, but appears to be a continuation of the larger cloth (nor does Rogers illustrate, cite or allege in his paper that photomicrographs support his conclusion<sup>3</sup>).

Furthermore, there must have been a hole of some kind that required a patch or new material to be placed over it. Moreover, the patch or added material must be attached to the larger Shroud in some permanent manner. Even if you assume that some form of secretive invisible to the naked eye type of patch or "reweave" was made at the radiocarbon site, it must have been attached to the larger Shroud in some permanent manner that would have survived the very real stress, stretching and pull that the Shroud was subjected to from having been rolled repeatedly and stored on a spool for over 400 years. In 2002, the backing cloth was removed which had been attached to the Shroud for centuries. An examination of the back of the cloth confirmed what the many previous examinations, photographs and photomicrographs of the front of the Shroud revealed — that the Shroud had not been patched at the radiocarbon site. The words of Dr. Mechthild Flury-Lemberg, one of the world's leading textile experts on ancient textiles and the Shroud of Turin, and who specifically led the examination and restoration of the Shroud in

2002, are particularly illuminating on these questions. Following the Shroud's lengthy examination and restoration she stated:

I would like to add here a note on the hypothetical "reweaving done in the 16<sup>th</sup> century." There is no doubt that the Shroud does not contain any reweaving. The fabric is scattered with irregularities which are the result of faults made during the weaving process, and which could be mistaken for reweaving. But they are normal for fabrics of the early periods. Such irregularities are actually proof that a fabric has been woven on a hand-loom which points to an early date of origin of the fabric...Reweaving in the literal sense does not exist. Once the piece of fabric is taken off the loom the weaving process is finished. Afterwards one can only alter a fabric by using needle and thread. An example would be a hole which has been mended by imitating its weave structure. This process will always be recognizable as mending and in any case visible on the reverse of the fabric.<sup>4</sup>

Professor Giuseppe Ghiberti, the Vatican Scientific Advisor for the Shroud and a participant at the 2002 examination and restoration also states:

The truth is that there is no patch and no darn. During the last analysis made in 2002, when we carried out restoration and cleaning operations, Dr. Mechthild Flury-Lemberg (the leading world scholar on ancient textiles) examined the Shroud very carefully and concluded that there are no added threads. Beyond any doubt, there is no textile patch or darn in this linen....After 500 years, the (Holland) backcloth was completely removed and we were able to see the backside: there is no darn at all. Moreover, you apply a patch or a darn where there is a hole, while the samples had been removed in a corner area with no scorched holes and no medieval darns. I am astonished that a scholar such as Rogers has written so many inaccuracies in his article.<sup>5</sup>

Rogers' repair hypothesis and methodology can also be questioned in several other ways. After examining samples from the Raes threads and the radiocarbon site in different manners under the microscope, Rogers concluded, "The presence of alizarin dye and red lakes in the Raes and radiocarbon samples indicates that the color has been manipulated. Specifically, the color and distribution of the coating implies that repairs were made at an unknown time with foreign linen dyed to match the older original material."<sup>6</sup>

Yet, this conclusion completely ignores the fact that X-ray fluorescence analysis on thirteen threads from the Raes sample indicated they had roughly the same relative concentrations of calcium, strontium and iron that was found on the rest of the Shroud.<sup>7</sup> This is very significant because STURP attributed the relatively uniform concentrations of calcium, strontium, and iron that are found throughout the entire Shroud to the retting process in which the harvested flax plants acquire these elements from soaking for several days in nearby bodies of water. If the Raes threads have the same relative concentrations of calcium, strontium, and iron as the rest of the entire Shroud, this not only indicates that the entire Shroud linen and the Raes sample were woven together, but that the flax of which they're comprised could even have been retted very similarly or simultaneously.

Ironically, the X-ray fluorescence analysis is found in "Physics and Chemistry of the Shroud of Turin, A Summary of the 1978 Investigation," by L. A. Schwalbe and R. N. Rogers. This is a solid report published in 1982, which summarizes the extensive work performed and published by other STURP scientists. Unfortunately, Mr. Rogers own independent work has not measured up to that of other Shroud or STURP scientists. (In a later paper, I will discuss his sapanora/diffusion hypothesis.) This can be seen further in the extensive testing that Rogers' STURP colleagues performed on their samples prior to drawing documented conclusions, as compared to the relatively limited examination performed by Rogers prior to drawing scientifically isolated conclusions that have not been proven. In many ways Mr. Rogers' above work on Shroud samples resembles the unsupported and discredited Shroud works of Drs. Walter McCrone and Leoncio Garza-Valdez, which were primarily based upon microscopic examination of Shroud samples.<sup>8</sup>

Like McCrone and Garza-Valdez, the bulk of Rogers' examination in the above paper was based on his examination and observations of the Raes and radiocarbon samples under a microscope. Rogers largely interpreted the presence of a plant gum coating on the Raes and radiocarbon samples from his various microscopical observations of these samples, from which he then implied a repair had been made in this vicinity with the foreign linen dyed to match the rest of the Shroud or "...added by wiping a viscous liquid on the outside of the yarn."<sup>9</sup> (While Rogers does state, "Chemical tests on both the radiocarbon and Raes samples show their coatings to consist of a plant gum containing alizarin dye present in two forms,"<sup>10</sup> the reader should understand that these chemical tests do not prove Rogers' major points of a repair or of coatings on the Raes and radiocarbon samples. Furthermore, the chemical tests are not definitive for alizarin dye and involve more subjective interpretation of observations by Mr. Rogers under the microscope.)

Microscopical analysis is a valid scientific tool, but it is best used for Shroud analysis in conjunction with other more comprehensive tests. Unfortunately, depending upon the various magnifications, forms of lighting, their angles and degrees, different things can be seen or interpreted from microscopic examination, that are not confirmed by other tests that are more comprehensive and definitive. Rogers did not perform any of the extensive additional tests on the Raes or radiocarbon samples that STURP performed on the Shroud and its samples to confirm his findings. Since these tests were performed on other Shroud samples and were published, Mr. Rogers or someone on his behalf, should have performed some of them on the

Raes and radiocarbon samples and confirmed whether they were part of the Shroud or part of a repair, but unfortunately, he did not.

This was especially incumbent upon Rogers in view of the fact that X-ray fluorescence analysis of the Raes samples generally matches the rest of the Shroud. Another example where Mr. Rogers could have definitively confirmed or supported his conclusions in part, but did not, can be found in his discussion of the red lakes. Rogers claims the Raes and radiocarbon samples, unlike the rest of the Shroud, have coatings consisting of a plant gum containing alizarin dye. Rogers claims that a variable amount of the alizarin dye is complexed with hydrous aluminum oxide to form red lakes that are gelatinous.<sup>11</sup> Rogers knew that McCrone made a similar claim that gelatin was present on the Shroud (as a medium paint binder). Rogers also knew that, in addition to several other tests for the gelatin protein, STURP applied a fluorescamine test that can identify gelatin protein at the level of one nanogram – or one billionth of a gram. Rogers failed to apply or even discuss this test. Instead, he claimed to see the red gelatinous lakes under the microscope. You would think if they can be “seen” under a microscope that their gelatin could be detected at the level of one-billionth of a gram. In light of the X-ray fluorescent analysis; that no other scientists have identified coatings on any Shroud, Raes or radiocarbon samples; that fluorescamine testing had been performed on other Shroud samples; and that Rogers was claiming the red lakes that he observed were gelatinous; it’s unfortunate that fluorescamine testing was not discussed or applied to the Raes and radiocarbon samples by Rogers or someone on his behalf.

Rogers did compare STURP’s pyrolysis mass spectrometry studies of samples from the rest of the Shroud to those from the Raes sample to make a secondary point that the gum coating on the Raes and radiocarbon samples was a petrosan. (The alleged existence of the gum coating was found through Rogers’ microscopic examination which he illustrated by photomicroscopy.) Even here, this work was not very thorough or convincing and was incomplete. Of all the other tests that STURP performed on the rest of the Shroud or its samples, Mr. Rogers chose only the above which was never published by STURP. Furthermore, in figures 4 and 5 he did not even give the temperatures. Moreover, he did not perform or did not give the mass spectrometry results for the radiocarbon sample. Lastly, Rogers stated that “Cellulose pyrolyzes to produce hydroxymethylfurfural (mass 126)... the Raes fibers showed a signal for furfural at mass 96 (Fig. 5). There was no signal at mass 126.”<sup>12</sup> From this incomplete analysis we could just as easily conclude that the Raes sample did not even contain cellulose, than we could conclude it contained a petrosan gum coating.

Unlike STURP, Rogers did not conduct tests that independently identified the various elements within the Raes and radiocarbon samples, nor did he compare their elements with the elements found by STURP and other scientists on samples from the rest of the Shroud. In 1996 and 1997, Dr. Alan Adler used FTIR absorbance patterns and carried out spectroscopic investigations including scanning electron microprobe spectroscopy on samples from various locations on the Shroud, including samples from the radiocarbon site. Adler clearly demonstrated that the radiocarbon samples have a different chemical composition than most of the fibers from the rest of the Shroud; however, he understood they’re merely “an exaggerated composite of the water stain and scorch fibers.”<sup>13</sup> After studying ultraviolet fluorescent photographs taken of the Shroud, STURP’s chief photographer Vernon Miller and Alan Adler

confirmed over 15 years ago that the radiocarbon site was in the midst of a scorch mark and at the edge of a water stain.<sup>14</sup> (Since STURP's UV fluorescent photographs were taken in 1978 after the contiguous Raes sample was removed in 1973, we can't tell from the UV fluorescent photographs whether the same conditions are present on the Raes samples.)

While microscopy and photomicroscopy are valid scientific tools that are best used in conjunction with other scientific tests, Rogers even failed to utilize these limited tools and their analysis in the most appropriate circumstances. The best application of photomicroscopy to prove or consider a repair hypothesis would be photomicroscopy of the Shroud itself, particularly that performed in the vicinity of the radiocarbon site before 1988. Unfortunately, these photomicrographs were not even discussed or considered by Rogers in his paper.

One of the many inaccuracies in Rogers' article and in his methodology is also revealed by his statement that, "The disappearance of all traces of vanillin from the lignin in the Shroud indicates a much older age than the radiocarbon laboratories reported."<sup>15</sup> While I think the Shroud is much older than the radiocarbon laboratories reported in 1988, Mr. Rogers' reasoning on this point is fallacious.

Rogers argues that vanillin slowly disappears from lignin over time. He also acknowledges that, "Any heating at the time of the fire [of 1532] would decrease the amount of vanillin in the lignin as a function of the temperature and time heated." Remarkably, he also asserts "...the unscorched parts of the folded cloth could not have become very hot," that "...the cloth's center would not have heated at all in the time available," and that "...different amounts of vanillin would have been lost in different areas."<sup>16</sup> Yet, neither Rogers nor anyone else knows how long the folded Shroud was inside the burning Sainte Chapelle in Chamberry when the fire of 1532 occurred. The historical record does say, however, that the canons and monks who helped retrieve the Shroud had a frustratingly longer task than normally anticipated, for the Shroud was kept behind a grille with four locks whose collective keys were unavailable, thus requiring its rescuers to retrieve a local blacksmith to rescue the Shroud and its reliquary from this awful predicament. Further, the historical record does not reveal how long the fire had raged before anyone even awoke to its discovery. We do know that the silver lining of the reliquary melted onto the Shroud, and depending on its degree of purity, would have melted around 750°-900° C (1380-1650° F). It had to have been extremely warm inside the reliquary, as well. While we would not know the degree of heat inside the reliquary or for how long, the temperature gradient within would have been much higher than the 20-25° C constant range that Mr. Rogers used to conclude the cloth's "...vanillin loss suggests that the Shroud is between 1300- and 3000-years old."<sup>17</sup> STURP physicist Dr. John Jackson and chemist Dr. Keith Propp write that if the temperature incurred by the Shroud was just 200° C that it would lose 95% of its vanillin in a mere 6.4 seconds.<sup>18</sup>

Rogers even acknowledges that, "The major problem in estimating the age of the Shroud is the fact that the rate law is exponential; i.e., the maximum diurnal temperature is much more important than is the lowest storage temperature."<sup>19</sup> Remarkably, he fails to realize that the conditions of the fire of 1532 alone could have caused all the remaining vanillin within the Shroud to disappear. The vanillin test expounded by Rogers to determine the Shroud's

suggested age “between 1300- and 3000-years old” cannot be used to estimate the age of the Shroud.

No only do the unknown conditions of the known fire of 1532 prevent this use, but the storage conditions of the Shroud throughout the centuries are also unknown. According to a number of historians, the Shroud may very well have spent approximately 900 years in Edessa (now Urfa, Turkey), where the temperatures can become quite hot. Rogers’ calculation for his above ages of the Shroud was based upon assumptions that the Shroud was stored at constant rates ranging from 20-25° C (68-77° F). However, the temperatures inside and outside the buildings and structures within Edessa, Turkey could easily have exceeded 100° F on countless days throughout the Shroud’s many centuries within the city. (Even if the Shroud was kept in a city wall for 500 years in Edessa, it would still have been kept elsewhere in Edessa for 400 more years.) There’s simply no way of knowing how much vanillin could have been lost in such potentially prolonged exposure at such higher temperatures.

Antoine de Lalaing also makes an allegation containing several layers of hearsay that the Shroud was “boiled in oil” in 1503.<sup>20</sup> Unlike the fire of 1532, or the contention the Shroud may have spent centuries in Edessa, or the cities and countries below, this allegation lacks any physical or other first hand evidence to support it, for if such a boiling event had occurred, it would have definitely left easily detectable evidence on the Shroud. However, this unsupported allegation does serve to illustrate the point that even one unusual day or unknown instance involving the Shroud’s temperature could explain the absence of vanillin on the cloth. It should also be noted that in addition to the time the Shroud may have spent in Edessa, the cloth could also have incurred countless days where its immediate atmosphere reached temperatures above 100° F in any number of locations throughout its indicated history in Jerusalem, Constantinople, France or Italy, over a period of many more centuries. The absence of vanillin in the lignin growth joints of a textile, whose everyday storage temperatures are impossible to know, simply cannot be used to estimate its age range.

Because of the numerous errors and shortcomings in Rogers’ analysis and methodology, along with the lack of any definitive, objective corroborating evidence on the cloth itself, or in his article, Rogers has not begun to “prove that the radiocarbon sample was not part of the original cloth of the Shroud of Turin.”

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1. R. N. Rogers, “Studies on the Radiocarbon Sample from the Shroud of Turin,” *Thermochimica Acta* 425 (2005) 189-194, 189.
2. Private communication with Alan Adler 1997.
3. Rogers merely states (p. 192) “Such repairs were suggested by Benford and Marino.” These authors first asserted such repairs and that their experts could observe repairs on photomicrographs of the Shroud. However, their experts were unfamiliar with the Shroud and spoke of a “vertical seam” on the cloth, and that “medieval European weavers would typically try to match the original cloth and then hand-stitch approximately ½ inch of new material into the old...,” but both statements clearly infer

- sewing and stitching. "Evidence for the skewing of the C-14 Dating of the Shroud of Turin Due to Repairs," *Worldwide Congress "Sindone 2000"*, Orvieto, Italy, p. 4.
4. M. Flury-Lemberg, *Sindone 2002* (Torino: Editrice ODPF, 2003), p. 60. English translation: Rosamund Bandi and Susie Clavarino Phillips.
  5. According to a translation and email provided by Antonio Lombatti to ShroudScience@yahoogroups.com on February 8, 2005.
  6. Rogers, "Studies on the Radiocarbon Sample," p. 192.
  7. L. A. Schwalbe and R. N. Rogers, "Physics and Chemistry of the Shroud of Turin," *Analytica Chimica Acta* 135 (1982): 3-49, note 6, p. 47.
  8. I realize all three individuals did additional work, but the bulk of their illustrations, arguments and research consisted of microscopical and photomicroscopical analysis.
  9. Rogers, "Studies on the Radiocarbon Sample," p. 191 and 192.
  10. Ibid. p. 191.
  11. Ibid.
  12. Ibid. p. 192.
  13. A. Adler, "Updating Recent Studies on the Shroud of Turin," *Archaeological Chemistry: Organic, Inorganic, and Biochemical Analyses*, Mary Virginia Orna, ed. American Chemical Society (1996): 223-228, 225.
  14. M. Antonacci, *The Resurrection of the Shroud* (New York: M. Evans and Company, Inc., 2000) pps. 168 & 304.
  15. Rogers, "Studies on the Radiocarbon Sample," p. 191.
  16. Ibid.
  17. Ibid. p. 192.
  18. Email from John Jackson and Keith Propp to ShroudScience@yahoogroups.com on February 9, 2005.
  19. Rogers, "Studies on the Radiocarbon Sample," p. 191.
  20. I. Wilson and V. Miller, *The Mysterious Shroud* (Garden City, N. Y.: Doubleday, 1986), pps. 78 & 81.

*(Editor's Note: The next item was posted to the group on May 23, 2005 by Mark Antonacci and includes his response to various e-mail comments and criticisms that had been made by other group members, including myself).*

Dear Giulio, Barrie, Marcel, Helmut, Mario, Frank and all other researchers,

In response to your e-mails, thanks for reading and commenting on my paper "An Evidentiary Analysis of the Proofs Claimed by Ray Rogers," which I had completed before Ray suddenly died. Prior to his death, I had sent my paper to some scientists and was waiting on their comments before I released it publicly. The scientists did not find any errors in it and even commended me on some scientific points. I waited over six weeks following his death before I released my paper on a subject that has been greatly discussed publicly throughout the world.

My paper clearly shows that the X-ray fluorescent analysis on thirteen threads from the Raes sample indicated they had roughly the same relative concentrations of calcium, strontium and iron that was found on the rest of the Shroud. This is very significant because STURP attributed the relatively uniform concentrations of calcium, strontium, and iron that are found throughout the entire Shroud to the retting process in which the harvested flax plants acquire these elements from soaking for several days in nearby bodies of water. If the Raes threads have the same relative concentrations of calcium, strontium, and iron as the rest of the entire Shroud, this not only indicates that the entire Shroud linen and Raes sample were woven together, but that the flax of which they're comprised could even have been retted very similarly or simultaneously.

Ironically, the X-ray fluorescence analysis is found in "Physics and Chemistry of the Shroud of Turin, A Summary of the 1978 Investigation," by L. A. Schwalbe and R. N. Rogers. This analysis alone not only contradicts the main points in Ray's *Thermochimica Acta* paper (and the three points in Helmut's e-mail of May 19, 2005), but it also illustrates the narrow range of analysis, examination and proofs found in Ray's paper.

Ray did state in his paper that, "The disappearance of all traces of vanillin from the lignin in the Shroud indicates a much older age than the radiocarbon laboratories reported." He went on to state the cloth's "vanillin loss suggests that the Shroud is between 1300- and 3000-years old." Part of Ray's reasoning is that he speculated the center parts of the Shroud would not have become very hot in the fire of 1532. My paper clearly showed with the aid of scientific evidence that the heat from fire of 1532 alone could have caused all the remaining vanillin in the Shroud to disappear. Moreover, the Shroud was kept for centuries in unknown conditions in the Middle East and Europe where the heat frequently rises above the 20-25°C (68-77°F) constant range that Ray used to conclude the Shroud's age range. You do not have to be a scientist to understand this. The vanillin test propounded by Ray was an important part of his paper, and it must be pointed out that this major point and its reasoning are, unfortunately, invalid.

Ray not only overlooked STURP's X-ray fluorescence analysis of the calcium, strontium and iron concentrations in the Raes sample and the rest of the Shroud, but he also overlooked Adler's subsequent elemental analysis of the radiocarbon sample as compared to the rest of the Shroud, or Adler's explanation that the radiocarbon samples appeared to be an exaggerated composite of the Shroud's water stain and scorch fibers. I correctly pointed out that Ray basically looked through a microscope and subjectively interpreted most of the findings for his main points. Where he did compare his findings to STURP's work, it was on a secondary point (whose main point was not well-established or documented) and the comparison was to an unpublished study. Where he could confirm a subjective observation such as the gelatinous red lakes, he, unfortunately, did not, even though he had to have known that fluorescamine testing can detect gelatin at the level of a nanogram, or one-billionth of a gram.

Dr. McCrone made a similar observation through the microscope that gelatin was present on the Shroud (as a medium paint binder), which was not confirmed by other tests. Dr. Garza-Valdes primarily based many of his conclusions on observations through a microscope that were not seen or interpreted in the same fashion by others who have also examined Shroud fibers. Both aspects of Dr. McCrone's and Dr. Garza-Valdes' work can be seen in Ray's research. Moreover, all three researchers and their findings rely primarily on microscopy or photomicroscopy,

however, with a cloth as long and wide as the Shroud, that has incurred a variety of events and conditions over a long history, this scientific tool is best used in conjunction with tests that have also been performed on the cloth as a whole, which is another flaw these three gentlemen share.

For the main points asserted in Mr. Rogers' paper—that the radiocarbon and Raes samples are not original parts of the Shroud—photomicroscopy would best be utilized on the cloth itself. Yet, Ray did not offer any photomicrographs to support his thesis. You must keep in mind that thousands of photographs have been taken of the Shroud in various wavelengths and magnifications. Furthermore, I have seen photomicrographs that clearly appear to refute his main point. Moreover, textile experts and several other experts did not find any evidence on the cloth to support this thesis when the cloth was examined and restored for more than a month in 2002. I also pointed out there is no record whatsoever from any source for any repair, nor would there be any reason to hide the record of a repair. I noted an invisible repair would be unlike all subsequent and documented repairs to the Shroud. I further noted that even if you made both such assumptions, you couldn't explain how a medieval repairer could still fool modern photomicrographs and photographic enlargers, when they weren't available in medieval times.

For the above and other reasons, Ray hasn't come close to proving that the radiocarbon or Raes samples are not parts of the original Shroud. Because Ray's assertion has been discussed throughout the world since his paper was released in January of this year, I commented on it. I would have commented on it whether he died or not. My comments all concerned the validity of his proofs, evidence and methodology. I did not impugn his character. While I did compare his work to those of Drs. McCrone and Garza-Valdes, I did so in an accurate fashion, as they made similar mistakes. If anything, read how at the end of several paragraphs in which I validly criticized Ray's methodology, or lack thereof, I respectfully concluded: "...unfortunately, he did not."; or "it's unfortunate that fluorescamine testing was not discussed or applied to the Raes and radiocarbon samples by Rogers or someone on his behalf"; and "Unfortunately, these photomicrographs were not even discussed or considered by Rogers in his paper." I'm sure everyone who is familiar with Ray's writings to the ShroudScience Group and elsewhere knows that none of the language in my paper is as harsh or disparaging as that which was used by Ray on numerous occasions.

Barrie writes in his e-mail of May 18, 2005, that my paper was quite insulting to Ray's memory, particularly in the timing of its arrival 5 weeks after Ray's passing, when Ray was no longer able to defend himself or his work. I not only waited six weeks, but if I had waited six months or six years, Ray still wouldn't be able to defend himself. Surely, you don't mean that people can't criticize other people's works, if the author has died? This is an important paper with important assertions that are of interest to people around the world. In discussing my paper, Marcel writes in his e-mail of May 18, 2005, that "the arguments presented are so poorly minded that they don't worth specific rebuttal [sic]." In reply, I ask him what arguments did I present that you thought were invalid and why. I have focused on the evidence, proofs, methodology and arguments in Ray's paper concerning very important assertions, and welcome Marcel to focus on such in Ray's paper and mine. To the rest of his e-mail I might add, I think the Shroud is from the first century, is the burial cloth of Jesus, and much more. I just disagree with Ray's assertions and proofs.

In declining to publish my paper, Barrie's e-mail goes on to say, "To be fair, I would be happy to publish any credible criticism of Ray's work (and Ray would be the first to encourage this). . ." then adds "Since Ray's paper was written from well within his own professional expertise. . . I will only report or publish criticism from a qualified scientist, and preferably only once that criticism is also peer reviewed." Ray Rogers was not an expert in textile repairs, let alone medieval textile repairs. Ray was not an expert in the history of the Shroud. Ray was not known as an expert microscopist. Ray was not an expert concerning vanillin loss and calculating age there from. Ray did not even have a Ph.D. in any field of science. These are not meant as criticisms, but they address themselves to the question whether the subjects within Ray's paper were within his expertise. I believe Ray also stated in an e-mail to the ShroudScience Group that he was color blind. That, too, is not a criticism, but it is not necessarily a favorable quality for a microscopist to have. Ray had been away from Shroud work from the early 1980's until about the summer of 2001. After I sent him a copy of my book, *The Resurrection of the Shroud*, Ray wrote to me in July '01 that 'you have managed to 'resurrect' our (he and Joan's) interest. It helps to have had a hiatus of about 15 years.'" (In reply to Mario and Giulio, while scientists and I requested some samples from Ray, we did not receive any.)

Yet, just because Ray was not an expert on the subjects within his paper, doesn't mean he can't write about them. We should recall that Walter McCrone was one of the world's most renowned microscopists when he studied the Shroud samples. We should recall that the overwhelming number of people who criticized his findings and methodology, including myself, were not microscopists. Scores of specialties or expertise within the general fields of medicine, science, history, archaeology, textiles and art are involved in the general study of the Shroud, and a number of these particular areas are necessarily involved in any discussion or paper on the Shroud. If you have to be an expert in every field involved, a general discussion of the Shroud would be impossible.

Needless to say, anyone who has viewed Barrie's website knows that numerous comments and publications on subjects beyond individuals' expertise clearly appear on his website and they have not been peer reviewed. Ironically, my paper was peer reviewed by scientists with much longer histories and greater expertise on the Shroud than Ray. (I am not disparaging Ray by this comment.) It is also interesting to note that a Ph.D. scientist and myself submitted a short paper to Barrie for posting on his website that very professionally commented on Mr. Rogers' work two years ago, but Barrie has never posted it.

While commenting on my paper, Barrie further states, "Anecdotal attacks from individuals stepping outside their own fields of expertise do not have the credibility to challenge peer reviewed science. They tend to cloud the true issues and force us all to spend an extraordinary amount of time in discussing and disputing them." My friend Barrie is missing some very fundamental points. In my paper, the primary scientific evidence to refute Rays claim that the radiocarbon and Raes samples were not part of the original Shroud is found in the X-ray fluorescence analysis, a peer reviewed publication by Schwalbe and Rogers. Another major scientific point in my paper is that the loss of vanillin cannot be used to calculate the age of the Shroud. Scientific support for that came from John Jackson (and Dr. Propp) who is clearly qualified to comment on the fire of 1532, or any other scientific aspects related to the Shroud. The other comments regarding temperatures in the Middle East and Europe do not

require expert meteorologic evidence. Dr. Adler's elemental analysis of the radiocarbon samples and the rest of the Shroud that are found in my paper, are also relevant. Al Adler was clearly qualified to make his comments and his paper had been published for years. Probably the most damaging evidence against Ray's thesis are the first-hand observations on the Shroud itself by Dr. Mechthild Flury-Lemberg and Professor Giuseppe Ghiberti, both of whom are clearly qualified to make their observations and statements. I don't have to have conducted the scientific studies or have made the observations in order to cite these primary sources. If I have cited them incorrectly, or misquoted them, or analyzed them inaccurately, then point this out; but these papers and observations (including Ray's) were made for the benefit of the public and they should be discussed and debated.

I invite everyone to read my paper. It focuses on the merits of Ray's evidence, proofs, methodology, or lack thereof, and his claims. It does not contain any anecdotal attacks. Moreover, I have not stepped outside my own field of expertise. The Shroud necessarily involves a great amount of evidence from a wide variety of fields. Moreover, the validity of the extensive evidence, its relevance, its value as first or second hand evidence, its authenticity, reliability and comparability are all issues that lawyers are trained to analyze from a variety of perspectives. This expert analysis (if you will) can be utilized in all areas of Shroud study, as they are in all other subject matters of society. I happen to think a lawyer is an excellent background to have for Shroud studies, especially since no one could begin to be an expert in the vast number of specialties and sub-specialties involved in the total study of the Shroud.

I have extensively studied all fields of Shroud research for almost twenty-five years consecutively. It is my primary area of focus professionally. If you read my book, *The Resurrection of the Shroud* (which is easiest to obtain overseas on Amazon.com) you will see that I am comprehensive in my approach. You will also see that I have attempted to analyze and cite every major point or article concerning the Shroud. It was in that vein that I analyzed Ray's claims and methodology, and will analyze his saponaria/diffusion hypothesis, along with some of the other methods that have been proposed since my book was published. You will see that my analysis of his claims, proofs and methodology are consistent with my analysis of the works of many others. I think the more information you understand about the Shroud, makes it easier to analyze new material because you have an extensive base of knowledge (some of which is fluid) with which to compare and analyze it.

Barrie concludes his e-mail by honestly, and inaccurately, stating he is not qualified to debate Ray's work with me. Many people are qualified, including Barrie, and Ray's proofs, claims and arguments should be debated and discussed, as should all evidence concerning the important subject of the Shroud.

I have not done anything improper by analyzing Ray's paper. I have merely entered again the time honored debate and discussion on another important and public aspect of the Shroud of Turin. I think my friend Barrie was still distraught, and understandably so, at the time of his e-mail, because he lost a colleague and friend whom he had known well since the 1970's.

Mark Antonacci

*(Editor's Note: The next item was posted to the group on June 2, 2005 by Dr. Thibault Heimburger and includes a thorough and detailed response to the original Antonacci articles and postings, in which he defended Ray Rogers' work and conclusions. I should point out that Thibault's native language is French and not English).*

**REPLY TO MR. ANTONACCI'S CLAIMS ABOUT ROGERS ARTICLE**  
**By Dr. Thibault Heimburger**

Dear Mr. Antonacci,

I took note of your 2 papers in shroudScience Yahoo Group concerning the publication of Ray Rogers in *Thermochimica Acta* and this email has the aim of answering your assertions, as you propose it yourself at the end of your second paper. This email will be available in shroudScience.

In a few words, I am French (please excuse my bad English), and I have been interested in the shroud for more than 20 years. I am the author of a site in French language ([www.suaire-science.com](http://www.suaire-science.com)) which proposes a detailed and as objective as possible study about all the scientific aspects of the shroud. I studied thoroughly tens of documents, of which all those of Rogers (and also yours) and I think that the shroud is probably authentic. I have a scientific formation and I am a medical practitioner but, like you, I am neither an expert nor researcher.

I am in total disagreement with your assertion: "Mr. Rogers doesn't seem to have presented any palpable or convincing evidence to support his above claim" (i.e. the radiocarbon sample was not part of the original cloth of the Shroud of Turin)

1) Your first assertion: nobody ever could find a trace of repair in the Raes/radiocarbon sample.

This is true but does not mean that it does not exist. After having read the majority of the documents on the subject and having consulted many specialized Internet sites, I concluded that the debate on possible "invisible repair" in the radiocarbon area remains completely open, as opposed to what you write.

One of the difficulties of the debate comes from a possible confusion on the concept of "invisible repair", patch, "reweaving" etc. One finds on the Internet site <http://www.invisible-mending.co.uk/bims.htm> the following sentences:

"The term "Invisible Mending" dates back more than 200 years and describes the highly skilled and time-consuming method of repairing damage to cloth by taking individual threads from a hem, side seam or other concealed part of the garment of the same type and reweaving them over the damaged area to make as near perfect repairs as is humanly possible. But beware!... there are many other methods of repairing which are often wrongly described as "Invisible". Proper Invisible Mending skills take many years to perfect. There are no end of local tailors and seamstresses who are "handy with a needle" but that is in no way the same as genuine "Invisible Mending."

Patching, machine darns and "woven patch" techniques may have a place when speed and cheapness is all that matters but when a quality job is called for, only proper Invisible Mending will do."

Benson and Marino try, in their first paper (1), to show evidence of a medieval patch bent with original fabric. This patch, according to them, would be visible. The photographs and arguments which they present are not convincing. Such a patch would certainly have been seen by the experts. Perhaps their other paper (2) brings the solution. The questioned expert, Robert Buden (R.B), is particularly credible since his company carries out itself of invisible repairs. The technique is described in detail: each new thread is spun to match exactly the characteristics of the original, including its composition and thickness; the warp is reconstituted by respecting exactly spacing; the new and old threads are woven together to match the original pattern, the new threads are cut and will be lost in intact adjacent fabric (no seam).

Under such conditions one can really talk about "reweaving" rather than "patch" and the invisibility with the naked eye (in the excellent cases) becomes comprehensible: Another expert in medieval tapestries, from the Metropolitan Museum of Art, T. Campell, confirms it in a gripping way: "the sixteenth century weavers were magicians." True "invisible repair", the only one possible in this context, is that described by Burden. I also invite you to see on the site <http://www.roses.co.nz/repairs.htm> some photographs of modern invisible repairs.

You seem to say that very many people examined the zone to seek clues of repair. This is not true; for example, the STURP in 1978 (a long time before anyone suspect a repair in this zone) did not have any reason to be particularly interested in this very small corner of fabric and it did not do it. It was moreover one of the reasons of the difficulties of Rogers to gather the relevant data of the STURP on this zone (personal email of Rogers).

Actually, who are the experts in textile who did it? Raes in 1973, Testore and Vial in 1988 and Flury-Lemberg in 2002. Who could seriously study this zone by other means than the sight (or the binocular one)? Raes and Rogers. And it was precisely the latter who found, in a thread of the Raes sample, an end-to-end splice of two threads which could only be considered as an addition: fibers have an opposite direction, one side is coloured the other one is not, and the normal weaving of the shroud never shows splice but only overlapping to continue weaving when a batch is finished. The photograph is available (3). (Editor's note: quotes from Mark Antonacci's article are shown below in *italics*).

You wrote:

*Moreover, between 5,000 - 7,000 photographs of the Shroud were taken at this time in various wavelengths and magnifications, but no such photographs or microphotographs indicated such a repair.*

Rogers on the contrary presented several photographs which seem to show anomalies in the weaving of this zone (4). Even if one could not be convinced, the photograph in UV fluorescence (4) clearly shows a difference compared to the remainder of the shroud. Rogers has several times explained that this difference can only mean a difference in chemical composition. This fact is out of doubt and, to my knowledge, no expert chemist, to date, affirmed the opposite.

*I observed an excellent photomicrograph of the Raes site and the surrounding area taken in or after 1973, but before 1988, that can be found in the Centro Museum. This photomicrograph was greatly magnified and clearly covered the location that became the radiocarbon site in 1988. This photomicrograph makes it very clear that the radiocarbon site is a continuous part of the Shroud.*

Whatever the quality of microphotography, a perfect invisible repair is precisely characterized by the fact that its limit is not detectable (see above, R. Buden). On microphotographs of the Internet site referred to above, can you honestly recognize the repaired zone? You lengthily quote Mrs. Flury-Lemberg, uncontested and undeniable expert of the old textiles who didn't see any repair. It is obviously a serious (and the only one) argument against invisible mending. I have only 2 remarks to do: first, the irregularities which she describes must logically make more difficult the detection of an invisible repair; second, it should not be forgotten that in 2002 she obviously did not have access to the Raes/radiocarbon area removed in 1973 and 1988. Was there still on the zone which she examined threads of repair? Nobody knows it.

The question of knowing if a true invisible repair of excellent quality of the 16th century could not be detected today remains open. In all the cases, Rogers does not claim to directly prove it, but affirms it only as a logical consequence of his own discoveries.

2) Your second assertion: X-ray fluorescence shows that the Raes sample is chemically identical to the main part of the shroud

You wrote:

*After examining samples from the Raes threads and the radiocarbon site in different manners under the microscope, Rogers concluded, "The presence of alizarin dye and red lakes in the Raes and radiocarbon samples indicates that the color has been manipulated. Specifically, the color and distribution of the coating implies that repairs were made at an unknown time with foreign linen dyed to match the older original material." Yet, this conclusion completely ignores the fact that X-ray fluorescence analysis on thirteen threads from the Raes sample indicated they had roughly the same relative concentrations of calcium, strontium and iron that was found on the rest of the Shroud (...)*

Are you sure that the 13 threads of which you speak come all from the Raes sample? In note 6 p. 47 of "Physics and Chemistry of the Shroud of Turin" that you quote, it is written: "However, thirteen threads, removed from not-image, not blood areas of the shroud in November 1973 (...)". Plural employed seems to indicate different sites of taking away but I do not have access to the original document (i.e. the Report of the Turin Commission of the Holy Shroud).

*If the Raes threads have the same relative concentrations of calcium, strontium, and iron as the rest of the entire Shroud, this not only indicates that the entire Shroud linen and the Raes sample were woven together but that the flax of which they're comprised could even have been retted very similarly or simultaneously.*

But below you wrote:

*In 1996 and 1997, Dr. Alan Adler used FTIR absorbance patterns and carried out spectroscopic investigations including scanning electron microprobe spectroscopy on samples from various locations on the Shroud, including samples from the radiocarbon site. Adler clearly demonstrated that the radiocarbon samples have a different chemical composition than most of the fibers from the rest of the Shroud;*

Because of your obvious contradiction, you wrote then:

*However, he understood they're merely "an exaggerated composite of the water stain and scorch fibers."*

Adler actually showed (5) a very clear difference in the chemical composition of the radiocarbon fibers and water stain fibers on the shroud. The radiocarbon site indeed, as UV photography (4) shows it, belongs clearly to a water stain. Adler wrote: "A recent investigation comparing STURP sticky tape sample fibers with those of the radiocarbon sample by Fourier Transform Infrared Microspectrophotometry and also Scanning Electron Microprobe Spectroscopy demonstrated a clear difference in the chemical composition of the radiocarbon fibers from those of the various types of Shroud Fibers . (Note that this calls into question the accuracy of the radiocarbon date\*). In table 1 (below), it can be seen that the radiocarbon fibers, although they are from a water stain area, are "saltier" than the water stain image fibers from the rest of the cloth ".

\*Adler wrote it, not me. Table 1 that Adler quoted shows the ratio (% weight) radiocarbon/water stain: Na (8.3/0.6), Mg (0.9/0.0), Al (2.0/0.0), Cl (3.1/0.5), K (4.3/0.1), Ca (8.5/0.1).

Note the enormous contradiction concerning calcium between these data and those of the X-ray fluorescence. Which Adler to believe? That which allots these differences to external circumstances without much importance ("an exaggerated composite of the water stain and scorch fibers."); or that, which seems to think that these differences are sufficiently important to call in question the radiocarbon dating? At the end of the same paper (5), Adler wrote: "Perhaps the De Charny family decided to repair such damages at the time of their display of the Shroud. Maybe the radiocarbon sample is simply rewoven material from the time of this repair." Radiocarbon sample has not the same chemical composition than the rest of the shroud. That is a fact. The rest is interpretation.

3) Your third assertion: Rogers's claim is based on interpretations of what it sees under the microscope like McCrone or Garza-Valdez.

This assertion is simply and scandalously false.

*In many ways Mr. Rogers' above work on Shroud samples resembles the unsupported and discredited Shroud works of Drs. Walter McCrone and Leoncio Garza-Valdez, which were primarily based upon microscopic examination of Shroud samples. Like McCrone and Garza-Valdez, the bulk of Rogers' examination in the above paper was based on his examination and observations of the Raes and radiocarbon samples under a microscope. Rogers largely*

*interpreted the presence of a plant gum coating on the Raes and radiocarbon samples from his various microscopical observations of these samples, (...) the reader should understand that these chemical tests do not prove Rogers' major points of a repair or of coatings on the Raes and radiocarbon samples. Furthermore, the chemical tests are not definitive for alizarin dye and involve more subjective interpretation of observations by Mr. Rogers under the microscope.*

Didn't you read, page 3 and 4 of his paper, the many chemical tests that he carried out to characterize step by step each component of the substance which he saw under his microscope, as opposed to what made McCrone? : the tests of pH for alizarin, the tests of solubility for the aluminium mordant, the solubility in water, the tests of acid or basic hydrolysis and the tests with iodine for the plant gum. Curiously you do not quote Rogers (8) when he writes himself in 2002: "the color exchanges of the dye in the gum coating do not provide a definitive proof that it is alizarin/purpurin. Many dyes show similar color exchanges with pH, and this observation should be confirmed with spectrophotometry and additional chemical tests. The barrier to confirmation at present is the critical lack of samples. (...) however, the important point is that a dye similar to alizarin had been added to the gum coating on the Raes samples". Since 2002 Rogers could confirm it by the STURP spectrophotometric data.

Didn't you read the paper of Brown (6), who, at the request of Rogers, examined threads from the Raes sample in optical microscopy and scanning electron microscopy and who completely confirmed the conclusions of Rogers on this point?

*Rogers claims that a variable amount of the alizarin dye is complexed with hydrous aluminum oxide to form red lakes that are gelatinous. Rogers knew that McCrone made a similar claim that gelatin was present on the Shroud (as a medium paint binder). Rogers also knew that, in addition to several other tests for the gelatin protein, STURP applied a fluorescamine test that can identify gelatin protein at the level of one nanogram - or one billionth of a gram. Rogers failed to apply or even discuss this test. Instead, he claimed to see the red gelatinous lakes under the microscope. You would think if they can be "seen" under a microscope that their gelatin could be detected at the level of one-billionth of a gram. In light of the X-ray fluorescent analysis; that no other scientists have identified coatings on any Shroud, Raes or radiocarbon samples; that fluorescamine testing had been performed on other Shroud samples; and that Rogers was claiming the red lakes that he observed were gelatinous; it's unfortunate that fluorescamine testing was not discussed or applied to the Raes and radiocarbon samples by Rogers or someone on his behalf.*

It was necessary for me to read again several times what you wrote to be sure not to be wrong: you confuse the 2 meanings of "gelatinous" (resembling gelatin vs. containing gelatin)! Any reader of Rogers understands immediately that he used "gelatinous" in the first meaning (i.e. viscous) whereas you seem to believe that Rogers thought that what he saw was gelatin! Under these conditions your criticisms on the lack of specific tests for proteins (fluorescamine...) become obviously absurd.

*Rogers did compare STURP's pyrolysis mass spectrometry studies of samples from the rest of the Shroud to those from the Raes sample to make a secondary point that the gum coating on the Raes and radiocarbon samples was a petrosan (sic!). (The alleged existence of the gum coating*

*was found through Rogers' microscopic examination which he illustrated by photomicroscopy.) Even here, this work was not very thorough or convincing and was incomplete. Of all the other tests that STURP performed on the rest of the Shroud or its samples, Mr. Rogers chose only the above which was never published by STURP.*

Rogers has many times explained the true scientific method (a clear goal: to show a precise assumption, to choose the adequate tests for this assumption...): it seems not to be heard. The first goal of the STURP was to test the assumption of a painting; the tests were selected to this goal and not to seek the presence of a gum coating on a negligible part of the shroud! This is why Rogers could only use some data of the STURP (photographs, spectrometry) and only in complement of his own tests.

*Furthermore, in figures 4 and 5 he did not even give the temperatures.*

The answer can be found in *Thermochemica Acta* (p.5) : "However, it was impossible to quote an accurate, absolute temperature when single microfibrils were being analyzed, only relative sample temperatures could be compared". Moreover, the temperature and the time are dependent in the reaction rate: this is why, in "pyrolysis/mass spectrometry applied to the Shroud of Turin" (7), the graphs presented at the end use the unit "time-temperature".

*Moreover, he did not perform or did not give the mass spectrometry results for the radiocarbon sample.*

Because these data do not exist: the STURP did test some samples by Pyrolysis Mass Spectrometry (PMS), of a Raes sample, but not of a radiocarbon sample. Let us not forget moreover that the PMS is a destructive method what would have posed problem taking into account the very small quantity of threads available for his other tests.

*Lastly, Rogers stated that "Cellulose pyrolyzes to produce hydroxymethylfurfural (mass 126). The Raes fibers showed a signal for furfural at mass 96 (Fig. 5). There was no signal at mass 126." From this incomplete analysis we could just as easily conclude that the Raes sample did not even contain cellulose, than we could conclude it contained a petrosan (sic!) gum coating.*

The figures presented are only illustrations which are not enough with the demonstration of Rogers. But the whole of the text, the captions and the attentive reading of his principal article (7) on this question would have enabled everybody to understand what Rogers painfully tries to explain to non-specialists like us: by gradually heating Raes fibers, there appears a signal 96 (furfural) which cannot come from the cellulose, which did not start to break up (no signal 126). This signal represents the decomposition of the pentose which requires little contribution of heat. This fact does not exist elsewhere on the shroud. This discovery reinforces the assumption of a dye layer containing pentose. From the presentation of the 2 figures, you cannot conclude that Rogers would have made an incomplete analysis.

4) Your fourth assertion: the method suggested by Rogers to estimate the age of the shroud by vanillin is not usable in the case of the shroud.

On this point, I agree in general with you. It is clear that the assessment of the age of the shroud starting from the kinetics of decrease of vanillin is doubtful because of uncertainty on the temperatures reached during the history of the shroud and in particular during the fire of 1532.

However you forget the central fact discovered by Rogers: vanillin is detectable on the Raes/radiocarbon sample and undetectable on all the other samples tested, whatever their localization on the shroud. This is a solid fact based on a validated qualitative test. This only would be enough to invalidate the radiocarbon dating. But one can go further: if it is supposed that the lack of vanillin on the shroud comes from the heat of the fire of 1532, then one must deduce that the Raes/radiocarbon zone was not part of the shroud in 1532. Thus, even in this case, Rogers is right in his central thesis: the carbon-14 dating is invalid because the studied zone does not form part of the original shroud.

#### CONCLUSION:

The thesis of Rogers rests on at least two solid and simple facts. These 2 facts involve without any interpretation the conclusion of Rogers: Raes/radiocarbon site does not form part of the original shroud.

1) The Raes/radiocarbon zone (and it only) is coated with a dye. This fact, incontestably visible on the macro and microphotographs, is proven by the many chemical tests carried out and Pyrolysis Mass Spectrometry. Even if the chemical composition of the dye is not absolutely certain, its existence is undeniable and is enough to affirm the abnormal character of this zone: one did not apply it without reason. The following fact shows that what one wanted to handle or to hide relates to the fibers themselves (and not to an anecdotic detail):

2) The Raes/radiocarbon zone (and it only) does not contain detectable vanillin by a simple and highly sensitive test, whereas the same test is clearly positive on all the samples of the shroud, the fabric of Holland and the medieval flaxes tested. Vanillin is an intimate component of the flax fibers structure. Whatever the reason of this fact (ageing or heating), it means that the fibers from Raes/radiocarbon area are different from the fibers of the main part of the shroud (they did not share the same story) and Rogers is right.

Your paper is not credible because:

- 1) it contains too many serious errors and obvious misinterpretations.
- 2) in order to produce an acceptable critical analysis of Rogers, one must honestly hold account of all the data (cotton, lignin...) available in the other papers of Rogers. You do not even speak about it.
- 3) you claim that Rogers' work is primary based on interpretations of what he saw under his microscope. Everybody who read Rogers' peer-reviewed paper knows that it is the contrary of the truth: Rogers carefully tried to characterize what he saw under his microscope. Only experts in chemistry could comment on the accuracy of Rogers' work. This claim is not acceptable.

Finally I think that you should be thanked: you paradoxically reinforced the credibility of the central thesis of Rogers, by showing the total lack of serious argument against this one.

Sincere greetings.

Thibault Heimbürger

France

<http://www.suaire-science.com>

#### REFERENCES:

All of these references are available on the site of Barrie Schwartz [www.shroud.com](http://www.shroud.com)

- (1) Evidence for the skewing of the C-14 dating of the shroud of Turin due to repairs - Marino and Benford – 2000
- (2) Historical Support of a 16th Century Restoration in the Shroud C-14 Sample Area - Benford and Marino 2002
- (3) Scientific Method applied to the Shroud of Turin Rogers and Arnoldi 2002 (p.21)
- (4) Scientific Method applied to the Shroud of Turin Rogers and Arnoldi 2002 (p.22-23)
- (5) Concerning the Side Strip on the Shroud of Turin Adler 1997
- (6) Microscopical Investigation of Selected Raes Threads From the Shroud of Turin Brown 2005
- (7) Pyrolysis/Mass spectrometry applied to the shroud of Turin Rogers 2004
- (8) Scientific Method applied to the Shroud of Turin Rogers and Arnoldi 2002 (p.20)

*(Editor's Note: The next item is Mark Antonacci's response to Thibault Heimbürger, which was posted to the group on June 14, 2005).*

Dear Mr. Heimburger and Researchers:

Thank you for your time and consideration of my two earlier papers concerning Ray Rogers Thermochemica Acta paper. (1) Please don't apologize for your English, Thibault, as we can understand your points. By the way, your English is, at least, 100 times better than my French.

As a result of a great many items of evidence, I think the Shroud of Turin is the authentic burial garment of the historical Jesus Christ. In light of the wide variety of the evidence, this is actually an enormous understatement. I also think the Shroud is from the first century and that its 1988 radiocarbon dating is erroneous. While I think there are some excellent explanations for this aberrant dating, that I would be happy to discuss at another time, I do not think the repair hypothesis is a very likely explanation. Far more extensive and definitive textile, scientific and historic evidence exists to refute this hypothesis than exists to support it. Furthermore, the evidence presented by Ray in his above paper did not begin to prove this hypothesis, as his paper claims. I will try to summarize my reasons while also addressing the main points to Thibault's e-mail of June 2, 2005.

According to the descriptions in note six of "Physics and Chemistry of the Shroud of Turin, A Summary of the 1978 Investigation" (2), the descriptions in the Report of the Turin Commission of the Holy Shroud, Ray's description in the Thermochemica Acta paper, and a STURP scientist that I consulted with several years ago on this question, it appears that the thirteen threads that were subjected to X-ray fluorescence analysis were from the Raes sample. The X-ray fluorescence analysis is a far more specific and definitive identification of the elements within the Raes samples and the rest of the Shroud than anything found in Ray's Thermochemica Acta paper, or any of the sources that Thibault cited at the end of his paper (with the possible exception of Adler 1997, which I shall discuss below). This analysis shows that the Raes samples have the same relative concentration of calcium, strontium and iron as the rest of the Shroud. This not only indicates that the entire Shroud linen and the Raes samples were woven together, but that the flax of which they are comprised could even have been retted very similarly or simultaneously. John Jackson (and Keith Propp) confirm this analysis by stating, "I think that Antonacci's argument that the density ratios of the calcium, iron and strontium in both the Shroud and the Raes samples, as commented in Note 6 of the paper by Schwalbe and Rogers - Physics and Chemistry of the Shroud of Turin in *Analytica Chimica Acta* 1982, is a compelling argument that the fabric of the radiocarbon site is very likely not due to a fabric that is alien to the Shroud." (3) (In an e-mail to the ShroudScience group on June 2, 2005, Dan Scavone correctly notes, in part, that Al Adler asserted the Shroud acquired its calcium, strontium and iron from the retting process. Dan then seems to state or wonders whether all retted linen would produce the same identical elements. Scientist Art Lind wrote me that identical trace elements would result from the retting process only if the flax was retted in the same locality where the same trace elements are present.)

Jackson also asserts that the radiograph in Fig. 7 in the same above 1982 STURP publication also disproves the repair hypothesis. This radiograph, taken in 1978, clearly shows the site of the radiocarbon sampling ten years later, along with its surrounding area. After noting the utility of the density bands to determine that the side strip is of the same material as the main Shroud, Jackson states: "This argument can also be applied to test the hypothesis of a reweave. If a

reweave has occurred, then surely the continuity of the radiographic bands would be disrupted at the reweave intersection with the Shroud because the reweaved fabric would have different radiographic properties. Such a discontinuity is not observed anywhere in the Figure 7 radiograph, and therefore we must conclude unambiguously that there has been no reweave whatsoever surrounding the radiocarbon sample site." (4)

Adler's work in 1996 and 1997 also provides interesting definitive elemental analysis of the radiocarbon samples as compared to the rest of the Shroud. In Adler's first paper he clearly states that the radiocarbon fibers appear to be an exaggerated composite of the water stain and scorch fibers. (5) However, Thibault may have misunderstood the significance that Adler attached to the chemical differences and Al's reasons for speculating that the de Charny family may have repaired the Shroud prior to their display of it. Moreover, you seem to misinterpret Adler's data as a contradiction of the X-ray fluorescent findings, which they are not. Adler's analysis actually provides additional reasons against the invisible reweave hypothesis.

In his first paper, Adler states his findings of "gross enrichment of the inorganic mineral elements in the radiocarbon samples, even compared to the water stain fibers taken from the bulk of the cloth. In fact, the radiocarbon fibers appear to be an exaggerated composite of the water stain and scorch fibers." (6) In the next two immediate sentences he states that these differences in chemical composition could have affected the accuracy of the radiocarbon dating and that these differences could support the experiments of Khouznetsov. (7) Adler states, "How much these differences in chemical composition actually affected the accuracy of the radiodate is not clear. However, these data are consistent with a recently proposed mechanism in which it has been experimentally demonstrated that conditions comparable to those suffered by the Shroud in the 1532 fire can produce a large error in radiodating by large kinetic isotope effects." (8) Immediately following this quote, Adler cites a paper presented by Kouznetsov and his Russian associates at the American Chemical Society in 1995 (which I cited in 7). The information used by Adler to support a repair hypothesis of the radiocarbon site concerns the presence of the selvage edge. Immediately following the above quote, Adler states, "Alternatively, considering the presence of the selvage edge, this area may contain newly woven material as a repair."

In Adler's second paper, "Concerning the Side Strip," (9) he discusses the seam that runs down the length and near the side of the Shroud, and into the very near vicinity of the missing panels, the radiocarbon and the Raes sampling sites. He suggests the traditional horizontal display of the Shroud caused the ends of the cloth to tear resulting in the missing panels. He suggests strongly that the seam is really a tuck or tube that has been sewn into the cloth to alleviate this problem. He suggests that this bounded edge existed before the fire of 1532 and that it stopped the flow of water in 1532 in this area. (Adler's article was written before Aldo Guerreschi and Michele Salcito's article on the Shroud's water stains.) The table presented by Adler of the various amounts of Na, Mg, Al, Cl, K and Ca in the water stains and the radiocarbon sample shows that the radiocarbon samples that he examined came from the edge of a water stain. Adler states, "In Table 1 (below), it can be seen that the radiocarbon fibers, although they are from a water stain area, are 'saltier' than the water stain image fibers from the rest of the cloth. Since the edges of the water stains on the body of the cloth are unbounded permitting free diffusion, this implies

that missing panels were already missing at the time of the 1532 fire, as such a bounded edge would concentrate diffusing dissolved salts at such an edge." (10)

Not only does Adler's chemical analysis show the radiocarbon site was at the edge of a water stain, but ultraviolet fluorescent photographs and reflected light imagery reveals this fact according to Vernon Miller, chief photographer of the 1978 Shroud exhibition. (11) The large foldout photographic positive image in Sindone 2002 actually illustrates this point best. Adler's papers do not contradict the X-ray fluorescence analysis of the Raes samples and the rest of the cloth, and provides more specific and definitive elemental analysis of the contamination present at the radiocarbon site. Moreover, Adler's analysis confirms the radiocarbon samples were part of the cloth when the Shroud acquired its water stains! Guerreschi and Salcito (12) offer a very interesting explanation how the water stains got on the Shroud and suggest they could have been present as early as the times of antiquity. If they are correct, than the alleged repair to the radiocarbon site could not have been accomplished by a 16th century repairer who could invisibly weave the patch into the cloth. **Even if the water is from 1532, the widespread development of the technology of invisible weaving and its application to the Shroud in the first third of the century by a 16th century restorer is still problematic.** (See Marino-Benford References)

Adler suggests that since the bounded edge of the side strip existed before 1532, that perhaps the corded seam, repairs to the missing panels, and the repairs at the radiocarbon site could have occurred at the time the de Charny family displayed the cloth, which occurred in the 1350's and in 1389. (13) This, too, rules out an invisible reweave by a 16th century restorer. Adler, like myself and others, had concerns that the radiocarbon samples may have been part of a repair to the Shroud. Adler's concerns on this question were largely erased in 1997 when he got to see the Shroud for the first time and visually examined this area of the cloth, following the fire at the Shroud's temporary facilities in 1997. He told me after this examination that he wasn't nearly as concerned that the radiocarbon site was part of a larger repair. My own concerns were largely alleviated in 2000, when in the lower level of the Centro Museum, in the back of the room toward the left; I saw photomicrographs taken between 1973 and 1988 of the location that would become the radiocarbon sampling site. This was right after my long time friend Joe Marino gave a paper authored by he and Sue Benford that the radiocarbon sampling contained repaired material. You could immediately tell from the photomicrograph that the location of their alleged repair was clearly a continuous part of the Shroud and that no repair could be seen anywhere in the area within the photomicrograph. You could not also help but notice the similarity within this photomicrograph with photomicrographs taken of other parts of the Shroud cloth.

Photomicrographs and X-radiographs of the radiocarbon area prior to 1988 clearly exist. Countless other photographs in various wavelengths and magnifications were also taken of the cloth as a whole and of this area prior to 1988. Furthermore, this area, as well as the entire cloth, was examined on numerous occasions by numerous people with a wide variety of professional backgrounds prior to 1988. These photomicrographs, X-radiographs, countless other photographs, and countless individuals have all detected a great number of repairs all over the Shroud, but have not detected any at the radiocarbon site. Nor have photographs and examinations in 1997 and 2002 revealed any such evidence. The restorations and examinations in 2002 were undertaken in a careful manner over a fairly long period of time by experts who could examine both sides of the cloth.

The scientific evidence, photographic evidence, microphotographic evidence, and the evidence from many direct examinations by many different people are all contrary to the repair hypothesis. Furthermore, there is no historical evidence. Even if you assume 16th century invisible weaving was possible, you still have to assume it was actually performed on the Shroud prior to the cloth acquiring its water stains, without any evidence of a repair on the cloth itself. **You would have to demonstrate how a 1532 (or earlier) reweaver could fool photo-microscopy when he didn't have a microscope to aid him.** (Marino-Benford did this) In addition, you would also have to assume this repair was kept secret for some reason from the public and all historical records unlike all other repairs and events for the Shroud. Even if you make these assumptions, you still have to explain how this alleged reweave survived the very real stress, stretching and pull that the Shroud was consistently subjected to from having been rolled repeatedly and stored on a spool for over 400 years, while leaving no such symptoms on the cloth itself. To keep the extensive rewoven threads from being thicker than the rest of the weave on the cloth, our reweaver would have to eliminate parts of each end of the many splices, but that would make the splices weaker. The alleged repair would be somewhat like the alleged medieval painting of the Shroud: **its truly masterful artist is unknown; its brilliant technique remains undetected even today; there is no historical record anywhere; without any reason for such secrecy, and without any actual evidence on the cloth for the claims.**

The X-ray fluorescent analysis, X-radiographs, Adler's elemental analysis, the photomicrographs, photographs in other wavelengths and techniques, numerous direct examinations by a great number of people with a wide variety of professional backgrounds, and numerous possible historical records-not only fail to provide any palpable evidence of a repair-but they refute the repair hypothesis, or are quite inconsistent with it.

In light of this, the repair hypothesis is extremely unlikely. Since it is not impossible, I do not mind considerations that might further test this hypothesis; however such considerations should only be a small part of the total considerations for tests and experiments regarding the Shroud and other hypotheses. Since Rogers' paper has received a lot of worldwide attention and Thibault has raised several other points, I would like to further discuss the evidence for the repair hypothesis as presented in Ray's paper.

The only evidence supplied by Ray in his Thermochemica Acta paper are his microscopic and microchemical observations of the unquantified and, necessarily, minute samples. He does not address himself to more definitive and contrary scientific evidence discussed above by STURP or other scientists. The only reference to previous STURP studies in his paper is a very partial reference to an unpublished pyrolysis mass spectrometry study to make a secondary point (that the undocumented gum coating was a pentosan). Ray presents no other scientific evidence nor addresses any of the above contrary evidence. Incredibly, from this necessarily limited, subjective evidence he states, "Pyrolysis-mass-spectrometry results from the sample area coupled with microscopic and microchemical observations prove that the radiocarbon sample was not part of the original cloth of the Shroud of Turin."

Some of his observations under the microscope concern secondary points that characterize undocumented coatings and even these are not definitive. None of his primary observations under the microscope directly relate to a repair, nor are they definitive in establishing such a coating. The pyrolysis mass spectrometry figures (4 and 5) did not even contain the temperatures nor did either contain the results from the radiocarbon sample (and these results were merely making a limited secondary point). From these very limited subjective observations and a very incomplete reference on a secondary point to an unpublished study, Rogers concluded that he had proved the radiocarbon sample was not an original part of the Shroud. Yet, this conclusion requires such enormous leaps of logic and facts that it violates all valid logical or scientific methods.

At best, this limited, subjective secondary evidence suggests something may be present on the Raes and radiocarbon samples. Jose Botella's e-mail of June 2, 2005 wonders whether a liquid of some kind may have gotten on the Shroud in this area causing Ray's observations. According to Adler and others, the radiocarbon site appears to be at the edge of a water stain. The photographic positive image in Sindone 2002 clearly shows the edge of the water stain and the rest of the water stain extending into part of the radiocarbon site. You cannot tell from that photograph what part of the water stain or its edge also extended into the area from where the Raes samples were removed, but it looks like the stain or its edge may have extended there. (I invite Barrie, scientists and other researchers to provide us with or refer us to other Shroud photographs that may answer this question.) Jose wonders whether this stain is necessarily water and whether this liquid could be responsible for what Ray is seeing under the microscope. These are far more direct and appropriate questions to consider than the conclusions reached by Ray. Ray's conclusion not only involves a great number of unwarranted intermediate leaps or assumptions, such as those considered by Jose, but he claims to have proven the ultimate conclusion that he jumped to.

Unfortunately, Ray made numerous errors in logic and method throughout his paper. I will try to limit my additional discussion to those subjects mostly raised by Thibault in his criticism of me and in his defense of Ray. Without any basis or any evidence, Ray assumes the constant temperature of the Shroud's entire history to be 20-25oC (68-77oF) in order to estimate its actual age between 1300 and 3000 years old. (14) Thibault reminds us that, "Rogers has many times explained the true scientific method." Unfortunately, to say the above assumptions by Ray violate the scientific method is an understatement for they violate basic common sense. Almost everyone knows that temperatures would exceed 20-25oC on countless occasions over many centuries in Europe, Turkey or Jerusalem.

The above assumptions and conclusions as to the Shroud's age are even more remarkable in that Ray even acknowledges in his *Thermochimica Acta* paper that, "The major problem in estimating the age of the Shroud is the fact that the rate law is exponential; i.e., the maximum diurnal temperature is much more important than is the lowest storage temperature." And that "Any heating at the time of the fire [of 1532] would decrease the amount of vanillin in the lignin as a function of time and temperature." (15) Incredibly, without any knowledge as to how long the folded Shroud was inside the burning Sainte Chapelle in Chamberry or how hot it was in the reliquary during the fire of 1532, Rogers states ".the unscorched parts of the folded cloth could not have become very hot." and that ".the cloth's center would not have heated at all in the time

available." (16) (emphasis added) Yet, Miller's observations of the ultraviolet fluorescent photographs and reflected light imagery, which allowed details to be seen that were not visible with the naked eye, revealed that the radiocarbon site was in the midst of scorch marks. Adler's above research also indicated this.

Thibault reminds us toward the end of his e-mail that we "forget the central fact discovered by Rogers: vanillin is detectable on the Raes/radiocarbon sample and undetectable on all the other samples tested, whatever their localization on the Shroud." I admit that Ray's paper gives you that impression, but read Ray's exact words: "The Raes threads, the Holland cloth, and all other medieval linens gave the test for vanillin wherever lignin could be observed on growth nodes. The disappearance of all traces of vanillin from the lignin in the Shroud indicates a much older age than the radiocarbon laboratories reported." (17) If he means, as he implied, that lignin cannot be found anywhere else on the Shroud other than the Raes sample, than the scientific method would require him to tell us how many samples were tested? What were their locations? Did STURP or someone else do the testing? If so, in what references is their work contained? In which samples could lignin not be observed on growth nodes? **Did he or they test the radiocarbon sample?** How did he or they identify lignin on growth nodes? etc. etc. There are either numerous errors in the scientific method in this one particular area or Ray has made a very misleading statement. If Ray only did a very limited study of the amount of vanillin, he should not only say so, but he should answer the above and other scientific questions to his limited study. Whether there was a larger study, limited study, and/or a misleading statement, it illustrates very poor scientific methods.

Ray continued to make unwarranted assumptions and display poor scientific reasoning when he said, "The fact that vanillin can not be detected in the lignin on Shroud fibers, Dead Sea scrolls linen, and other very old linens indicates that the Shroud is quite old. A determination of the kinetics of vanillin loss suggests that the Shroud is between 1300 and 3000 years old." (18) Not only did he fail to realize or consider that **the radiocarbon area appeared to be in the midst of scorch marks????**, or that the fire alone, or the cloth's centuries long history in warm climates could easily account for the absence of vanillin on Shroud fibers, he appeared not to realize that the loss of vanillin for "Dead Sea scrolls linen and other very old linens" is not a significant scientific fact if those cloths have spent any periods of time in warm climates or the complete history of their storage and climate conditions are unknown.

Throughout his paper, Ray argues that loss of vanillin in other ancient objects and the Shroud has significance. Correct me if I'm wrong, but other scientists do not commonly use the amount of remaining vanillin as an estimation of the age of objects, in general. One obvious reason against this lack of common usage would be that even if you knew the location or locations that an ancient object resided in its entire history, you still wouldn't know the temperatures within its immediately surrounding storage vicinity that it incurred throughout its known existence. I doubt seriously if you could legitimately use this criteria with the "other very old linens" that Rogers uses, or with almost any old linen. This is poor scientific methodology. Moreover, this criteria could never be used to estimate the age of a cloth that is known to have been in one major fire, has scorch marks and other burn holes, and has spent centuries in very warm climates. Rogers makes numerous errors in scientific method in this one line of argumentation alone.

Ray and Thibault seem to make much significance of the unpublished pyrolysis mass spectrometry data that is incompletely given in Ray's paper. Please keep in mind that the data for the radiocarbon sample isn't even given! Thibault explains because the test is destructive it wasn't feasible to provide this. That may be true, but it certainly takes away from the significance of the analysis. Please keep in mind this analysis is a major component of Ray's proof "that the radiocarbon sample was not part of the original cloth of the shroud of Turin." (The pyrolysis mass spectrometry data also did not contain any temperatures. Thibault provides a quote from Ray's *Thermochemica Acta* article to explain. I cannot find that quote or explanation in this article. It is probably in another article that he cited. Yet the lack of specific temperatures does remove some of the significance from the study.)

Ray's failure to be precise and to also generalize without providing a basis can be seen further in this same incomplete pyrolysis mass spectrometer study. Ray's paper discusses how "linen fibers from the main part of the Shroud did not show significant product evolution until relatively higher temperatures," yet his documentation (fig. 4) is from only one single image fiber from which he appears to draw this general conclusion. It should also be mentioned that if the Raes samples (the only non-image area from which he used a sample) were in a lightly scorched area, as the radiocarbon samples were, bonds broken during the scorching of the cellulose may have allowed furfural to be released at lower temperatures.

Toward the end of Ray's paper on p. 193, when he is summarizing his microscopic and microchemical observations and his incomplete, unpublished pyrolysis mass spectrometry results to justify his conclusion that the radiocarbon sample was not part of the original Shroud, he adds the following two words as part of his evidence, "cotton content." **Nowhere else in the entire paper is cotton mentioned, let alone explained. Is he referring to the cotton Professor Raes found in his original sample from 1973? Is he referring to the cotton sample found by the Oxford lab on its radiocarbon sample from 1988? Is he referring to samples from the cotton gloves that STURP members wore in 1978? Ray is using "cotton content" as part of his ultimate proof without any description, let alone any explanation.** This is contrary to any logical method, let alone any scientific method.

Thibault frequently defends Ray's arguments better than Ray presented them, but in the end there is very little, if any, direct evidence with which to argue Ray's ultimate claims. Furthermore, there is an abundance of evidence against this ultimate conclusion (not necessarily the observations). Moreover, this evidence is far more definitive and documented. Thibault claims that when Ray used the word "gelatinous" that he really meant "viscous." That could be, but Ray uses "viscous" ten lines above his use of the word gelatinous. I'm still glad he pointed out the real possibility because there was some discussion on this site about conducting fluorescamine testing of one of Giulio's samples, which now may be unnecessary.

Thibault also mentions evidence from some of Ray's earlier papers from three years ago that were not presented by Ray in his paper in question. He mentions the earlier discussed X-radiograph, a photomicrograph of an alleged splice and coating, and an X-ray transmission photograph. **I think there were good reasons that Ray left these out of his paper three years later. They simply do not confirm the claims in his earlier papers, are quite subjective and certainly subject to several interpretations.** I know myself and many others have seen these

various photos and concluded that they failed to prove his ultimate points. He actually used coloration in these photographs to "prove[s] that the radiocarbon area has a different chemical composition than the main part of the cloth." (19) **Coloration in a photograph can mean many different things, only one of which is the cloth's chemical composition, but coloration could never prove the cloth's chemical composition.** (SEE BLUE MOSAIC) Needless to say, the identification of a cloth's chemical composition can be made far more definitively in several other ways, which Ray ignored. This is another of the many examples of the lack of valid scientific methodology that is found in Rogers's overall work. In fact, read Ray's summary of the six major elements of scientific method found at the beginning of "Scientific Method Applied to the Shroud of Turin," 2002, and summarized by him elsewhere. You will find repeated violations of all these and other scientific methods in all of Ray's post-2001 works.

Moreover, these photos actually provide evidence against Ray's ultimate points. An excellent example of this is Ray's use of the X-ray photograph in his "Scientific Method Applied to the Shroud of Turin." This is very similar to or is the same X-radiograph found in Fig. 7 of Schwalbe and Rogers '82 that Jackson uses to refute the repair hypothesis. I have not agreed with other observations made by Ray. For example, he has claimed that no coloration can be seen on the inside of Shroud body image fibers or their hollow medulla centers. **While Barrie Schwartz has not published our remarks in two years, Art Lind and I were able to point out in this forum that Ray's own photomicrographs, visible on the back cover of BSTS #54 and in Ray's articles, clearly show dark lignin growth joints in great abundance on image fibers, as well as a darkened medulla, but are not nearly as abundant or visible on non-image fibers.**

Toward the end of Thibault's e-mail, he takes an earlier misleading "central fact discovered by Rogers," and states, "if it is supposed that the lack of vanillin on the Shroud comes from the heat of the fire of 1532, then one must deduce that the Raes/radiocarbon zone was not part of the Shroud in 1532." (emphasis added) Adler's studies, Miller's photographs, and the photographic positive in Sindone 2002 show the radiocarbon site was part of the Shroud when the water stains were put on the cloth. Moreover, Miller's observations on the UV fluorescent photographs and reflected light imagery from 1978, along with Adler's above studies, strongly indicate the radiocarbon site chosen in 1988 was present when the scorch marks of 1532 were incurred. Thibault, if you accept Ray's undocumented findings and conclusions, then a 16th century restorer would have to invisibly reweave his sample in the radiocarbon area; then add coloring that continues to match the rest of the Shroud to this day; add water stains that superbly match the same large water stain and its adjoining stains; and encode scorch marks that are only visible on UV fluorescent photography and reflected light imagery. And, that would only be the beginning! Your unknown and undetected reweaver would have to somehow still make the relative amounts of iron, calcium and strontium in the Raes samples the same as the rest of the Shroud while also having all the visual properties observed by Ray in his microscopical and microchemical observations. He would have to make the repair at the radiocarbon site so well that it could not be detected by X-radiographs in the radiocarbon and surrounding area. The invisible reweave could not be attached with any stitching, cannot appear thicker at its numerous splices, and yet withstand all the stress, stretching and pull it would have incurred from the Shroud having been repeatedly rolled and stored on a spool for over 400 years. Furthermore, no photo micrographs, or any other photographs taken of the cloth in any other magnifications or wavelengths would show this repair; nor would any direct examinations during this entire time

by countless textile experts, scientists, other professionals, or any members of the public be able to detect the repair on the cloth. Lastly, the reweaver must remain anonymous throughout history, and unlike other repairs and events with the Shroud, there can be no historical record whatsoever among the notable and numerous Savoy family records, or by any other owners, custodians, priests, monks, nuns, or textile specialists or restorers; all without any reason for such secrecy.

Thibault either this almost impossible scenario occurred, or Ray is seeing something on his actual Shroud samples, but what he sees does not justify the great leaps forward in facts and evidence to claim that he has proven a repair occurred at the radiocarbon site. I believe Ray has seen something under the microscope with his samples. I think Giulio also saw something with his one sample, but that is as far as you can go at this point with the evidence. Some of Ray's interpretations of his observations may prove to be true, but he provided no direct evidence of a repair, made assumptions throughout that were unwarranted, and unfortunately, displayed very poor reasoning.

Yet, just because the repair hypothesis is extremely unlikely is no cause for alarm among Shroud enthusiasts. For what it is worth, Ray thought the Shroud's image spectrum "looks like a Maillard product; i.e., the products that form when you bring reducing saccharides (e.g., starch) into contact with the amine decomposition products of a rotting body." (20) (emphasis added) I not only think that the Shroud is the authentic first century burial garment of Jesus, I think it contains a great deal of evidence for every element of the passion, crucifixion, death, burial and resurrection of the historical Jesus Christ, exactly as they are described in the most famous historical sources of all, the Gospels.

You can read our thoughts as to the explanation of the Shroud's radiocarbon dating in my book *The Resurrection of the Shroud*. Our method not only explains how this aberrant date was erroneously arrived at, but it could also establish the Shroud's actual age to a far more specific time period than radiocarbon dating is capable of. Furthermore, the method devised by myself and Art Lind, the Historically Consistent Method, can account for every primary and secondary body image feature; the Shroud's blood marks; the coin, flower (if any) and skeletal features; the excellent condition of the cloth, and the faint images on the backside of the Shroud's frontal image, all of which no other hypothesis can claim.

It is our intention to conduct rigorous scientific tests in order to acquire evidence concerning the accuracy of the Shroud's radiocarbon dating and whether certain events occurred to the cloth while it wrapped the body that may have affected the carbon content of the cloth. As these tests and results are completed we will be happy to share these results.

Regards,

Mark Antonacci

## References:

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9. A. D. Adler, A. Whanger, and M. Whanger, "Concerning the Side Strip on the Shroud of Turin," <http://www.shroud.com/adler2.htm>, (October 27, 1997).
10. Ibid., p. 5.
11. M. Antonacci, *The Resurrection of the Shroud* (New York: M. Evans and Company, Inc., 2000) pps. 168 & 304.
12. A. Guerreschi and M. Salcito, "Photographic and Computer Studies Concerning the Burn and Water Stains Visible on the Shroud and Their Historical Consequences." IV Symposium Scientifique International, Paris, April 25-26, 2002.
13. Adler et al., "Concerning the Sidestrip," see especially the ending paragraph of the article: "Several authors have suggested that the purpose of a corded side seam might be to facilitate hanging the cloth for exhibition. Certainly many paintings of such medieval exhibitions show the Shroud being displayed in such a manner with the cloth shown along its length and held or suspended along what would appear to be the side seam. It should be noted that this mode of display places maximum stress at the end points of suspension and tearing of the fabric would be expected to proceed from the ends inward along the seam. Some historical accounts record that certain noteworthies were given pieces of the Shroud. It would be logical to assume that such samples would be taken from such torn end panels, thus providing a simple explanation for the missing panel portions of the side strip. Perhaps the De Charny family

decided to repair such damages at the time of their display of the Shroud. Maybe the radiocarbon sample is simply rewoven material from the time of this repair."

14. Rogers, "Studies on the Radiocarbon Samples," pps. 191-192.
15. Ibid., p. 191.
16. Ibid.
17. Ibid.
18. Ibid. p. 192.
19. R. Rogers, "Scientific Method Applied to the Shroud," p. 23.
20. R. Rogers, Letters to the Editor, Skeptical Inquirer, July/August 2004, p. 69.

*(Editor's Note: The next item is Thibault Heimburger's final response to Mark Antonacci, which was posted to the group on July 12, 2005).*

Dear Mark and Researchers,

Please excuse me, Mark, for this late answer to your message dated 14 June 2005.

Actually, I had to do certain thorough research and I got your excellent book "The Resurrection of the Shroud" (RS in the continuation), which I read attentively (it is unfortunately not translated into French), and which Rogers considered as the best synthesis available (until chapter 9). I entirely share his opinion.

I will not respond in detail to Mark's assertions concerning the poor quality of the work of Rogers. I think that I have shown in my first message that they rest on arguments without value.

However :

- Concerning vanillin, Mark makes again a series of assertions or questions which clearly call into question the quality of the work of Rogers in his own field of competence. For example, he reproaches Rogers for not precisely giving figures (such as number and localization of samples tested, etc). Rogers detailed the number and the localization of the samples at his disposal in chapter 2 ("Samples") of *Thermochemica Acta*. The spot-test used (phloroglucinol/HCl) is qualitative and it is carried out on the growth nodes where the lignin and vanillin concentrate. Remember that the first goal of Rogers was to show "in 5 minutes" that the theory of the repair of Benford and Marino was false. You can be certain that, to finally persuade himself that he was wrong, Rogers has worked hundreds of hours (more than 2 years) to compare the samples.

Consequently, unless calling in question the scientific competence of Rogers, the answer to these questions is clear: all the fibers tested give the test in Raes, radiocarbon sample, the fabric of Holland and the medieval linen and none gives it on the 32 samples of the shroud removed in 1978. If Rogers had found only one exception, it would have announced and discussed it. Of course, it is always possible to question the representativeness of his 14 segments of Raes threads and of the samples of (both warp and weft) radiocarbon threads. But in this case, much of other studies (including that on the calcium and iron content of the 13 Raes threads) must be called into question. Lastly, as Rogers discusses it in some of his messages to the group, if a fraud or an error in the origin of the samples cannot be excluded, it is highly improbable (the Raes and radiocarbon samples were both provided by Professor Gonella who had access to both samples and the chemical properties of the 2 samples are identical).

- Concerning the dye coating found on both Raes and radiocarbon fibers, it is not even debatable and was perfectly characterized chemically by Rogers. I also confirm all that I previously wrote concerning the pyrolysis mass spectrometry, including the question of the absence of data about the absolute temperature which comes from the method itself (the explanation of Rogers on this point:

“However, it was impossible to quote an accurate, absolute sample temperature (...)” can be found in *Thermochimica Acta*, page 5, 23 lines above the figure 4). The macro and microscopic characteristics of this coating (see Brown : "Microscopical Investigation of Selected Threads from the Shroud of Turin" on Barrie's site) indicate that it was "added by wiping a viscous liquid on the outside of the yarn" (Rogers) and not brought by the water corresponding to the waterstain (cf message of June 2 2005 of Jose A. Botella). Of course, this layer is not a definitive proof of a repair, but it means at least 2 things: 1) here, one wanted to hide something 2) if it was done in Europe, it could only be done after A.D 1291 (according to Rogers, such a dye was unknown before this date in Europe).

- Concerning cotton, it is true that Rogers does not detail this point in *Thermochimica Acta* (which is a review about chemistry and not textile). But Mark knows very well that Rogers wrote about it in a number of his other articles. The important point is that cotton was found by Raes and Rogers in the depth of the Raes sample (and the radiocarbon threads by Rogers) and was not found by Rogers elsewhere on the 1978 samples of the Shroud surface. The question which one can legitimately ask is the value of the conclusions coming from the comparison between surface samples and a thick sample (Raes). But Rogers points out one of the most important points to us: cotton was found by Raes only in one part of his sample (that pertaining to the main part of the Shroud) and there is no cotton in the other part belonging to the "side strip" (on the other side of the "seam"). Since, as Mark underlines it, it is shown that the "side strip" is in fact in continuity with the Shroud, it is highly probable that:

1) the assertions of Rogers concerning the distribution of cotton are exact 2) this distribution is indicative of an additional anomaly in the Raes/radiocarbon samples 3) if this anomaly is of European origin, it could only be done after the arrival of cotton in Europe, i.e. after AD 1350.

Finally, the conclusion of Rogers (i.e.: the radiocarbon 1988 dating is invalidated since the dated sample was not representative of the Shroud) rests on a whole of solid and convergent facts (like all the serious studies on the shroud, those of the STURP primarily).

The only scientifically admissible arguments that Mark Antonacci really provides are as follows:

1) the absence of any proof of visible repair in the disputed zone. Let us first recall that this simple fact (which is obviously subjective) does not contradict the facts shown by Rogers themselves and thus his conclusion invalidating radiocarbon dating. These two types of data are of different order. It poses however a problem concerning the interpretation of these facts. I will not return in detail on this problem which could only be solved by the mean of a new direct study of the zone.

On the other hand, Antonacci's own book referred to above (RS), very well documented, and brings many arguments against his own current assertions. Here some examples of quotations of this book (these quotations are not out of context):

- " The rectangular piece missing from the very corner of the shroud necessitated that the remaining part of the cloth be sewn down at this location. Most likely, the cloth was nearly cut off and mended at this site after having gradually torn away from years of exhibiting the cloth horizontally (...). There may have been several mendings or repairs before the present one as the cloth gradually tore away from these two corners" (p.165).

- " When the Oxford laboratory received its sample of the Shroud, fine yellow strands of cotton were found in the sample. Peter South of Precision Process Textiles in England, which assisted Oxford university in preparing its sample for the carbon dating, noted: `the cotton is a fine, dark yellow strand possibly of Egyptian origin and quite old. It may have been used for repairs at some time in the past, or simply became bound in when the linen fabric was woven in'. This is also consistent with similar cotton findings of unknown age and origin on the 1973 Raes sample".(p.169).

- " After consulting with art and textile experts, Dr. Adler has learned that medieval restorers were extremely skilled in repairing textiles. In particular, he found that medieval restorers could easily repair cloth in a way that was not visible to the naked eye. R. Adler has expressed concern whether the area surrounding and including the carbon dating sample might have been one entire repair piece. Dr. Stuart Fleming, formerly of the Oxford Laboratory for Archaeology (...), stated that a restorer `could certainly have rewoven a damaged edge to a standard not visible to the naked eye". (p.169-170).

- " The `qualified textiles experts' (quotation marks are from Antonacci) who were present to supervise the removal of the sample from the Shroud may have been textile experts, but they apparently had little or no knowledge of the Shroud. Sadly, these men were seeing the Shroud for the very first time (...) Giovanni Riggi, the technician who actually cut the sample from the Shroud, argued with these two individuals for more than an hour over where to remove the sample from the Shroud" (p.181).

- In the chapter 9 ("The scandal exposed"), Antonacci explains to us how Gove tries to plan the radiocarbon dating process without the STURP team: " One effort to disassociate STURP from the carbon dating process was to have a textile expert remove the sample from the Shroud instead of STURP(...). Their first choice was Madam Flury-Lemburg of Belgium, whom neither Tite nor Gove had met at the time. Her status as a textile expert was merely one criterion that should have been considered. The most important criterion for this task would have been her knowledge and understanding of the Shroud itself. Unfortunately, she had never seen the Shroud, and she indicated her complete lack of understanding of the complicating factors and history associated with the Shroud when she stated at the workshop that the Shroud `is the same from one end to the other.

There is no need to take samples from various places. (...)

(...) A sample was taken from the edge of the main cloth as suggested by Flury-Lemburg, but this resulted in removing a single sample from the most controversial location on the cloth" (p.196).

Antonacci, in the quoted chapters, does not affirm the repair but shows at least, before the discoveries of Rogers, its possibility. He also confirms that the location chosen for the dating was the worst possible one for the same reasons as Rogers.

Of course, everyone has the right to change his opinion. But it should be explained how, according to Antonacci, the textile experts who had no knowledge of the shroud became suddenly qualified, why Mrs. Flury-Lemberg who seems to be at the origin of the error of the single sample and did not seem to understand the complexity of the fabric must be believed without discussion in 2002, why the different experts who stated the possibility of invisible repair in the Middle Ages do not count any more, why Antonacci seems suddenly not to know anything about cotton, etc.

It is extremely illogical, that Antonacci seems to forget in 2005 his own previous arguments, while, at the same time, the discoveries of Rogers confirm the highly abnormal characteristics of this sample, directly on threads coming from the suspect Raes/radiocarbon zone.

2) The calcium, iron and strontium contents of the Raes sample are roughly identical to that of the remainder of the Shroud. This serious argument, would be, according to Antonacci, an absolute proof that the Raes sample (and probably the radiocarbon sample) and the remainder of the shroud are a single fabric, eliminating the possibility of a repair. Admitting (we have no precision) that all of the 13 threads are coming from the main part of the shroud and not from the "side strip" (or from both the 2 parts), is it an absolute proof ? In other words, like Dan Scavone points out, does the fact of finding roughly identical rates mean that the flax stems were retted very similarly or simultaneously or came from the same locality (these 3 assertions, which are not equivalent, are used by Antonacci)?

The origin of this enrichment comes, without any doubt, from the retting process, as Heller and Adler show it. The same phenomenon was found by these authors on their control fabrics (Spanish, Coptic and Pharonic linens) but they did not provide the rates.

I was not able to find, in spite of intensive research, a precise answer to this question, probably because it does not exist. I questioned (on Internet) an expert who answers me as follows:

- "1. Iron and calcium concentrations are not constant in natural waters. They can vary by season and rainfall.
2. Iron and calcium will bind at different rates, so you have to take that into account.
3. Iron and calcium concentrations can be the same at different locations. I think if you had samples with very high iron and calcium levels, you could suggest they came from a site where there was high iron and calcium or maybe rule out sites that don't."

The nearest model is probably that of ion exchange resins, a method still used to purify water. The cations (positively charged ions, primarily H<sup>+</sup>) of cellulose would be exchanged by the mean of a reversible reaction with the cations (here, Ca and Fe primarily) of the water. If the equilibrium state is reached (it is probable because the stems remained many days in water), the rate of cations bounded on cellulose depends on many factors whose principal ones are the coefficient of selectivity of the ion (according to Heller and Adler that of Ca is much higher than that of iron), the pH of water (perhaps the main factor) and the initial rate of the ions in solution in water.

Although the pH of natural water can vary considerably, it generally ranges from 7.3 to 8 with a mean of 7.7 ([www.gemswater.org](http://www.gemswater.org)).

With these basic rates of pH, the curve presented by the ion exchange resins model shows that the rate of the ion bounded on the support (here the cellulose) varies much less than for more acid pH, between 6.5 and 7 for example, more rarely found in nature. (<http://www.remco.com/ix.htm>).

Of course, this does not constitute a demonstration but simply an example of the complexity of the problem (notice the prudence of the expert). In other terms, if, as the expert suggested it, the concentrations were very different, one could conclude from it that the flax of Raes sample and the remainder of the shroud probably come from 2 different areas, but the reverse is not necessarily true, especially if one takes account of the important uncertainty of measurements of the calcium rates by X-Ray fluorescence on the Shroud, i.e. 200 + or - 50 micrograms per cm<sup>2</sup>, according to Morris and Al. (see Schwalbe and Rogers : "Physics and Chemistry of the Shroud of Turin",p.17).

The argument thus seems to me to be an important indication, perhaps an evidence, but not an absolute proof. Moreover, if an evidence seems to go against all the other evidences which converge in the opposite direction (cotton, lignin, vanillin, coating), it is advisable to continue research to try to understand apparent contradictions. To the contrary, Antonacci rejects in block all the discoveries of Rogers. In this case, while following his own reasoning, we have to state that the shroud is a medieval forgery on the only basis of the carbon-14 dating and to be unaware of the whole of the contrary arguments.

Incidentally, although the preliminary studies of Rogers in *Thermochimica Acta* about the datation of the Shroud by the mean of vanillin decay are questionable, it seems to me that Antonacci himself provides the best argument to support the conclusions of Rogers:

- 1) Vanillin is easily found on Raes/radiocarbon samples (and the Holland cloth and other medieval linen) and absolutely not on the Shroud (Rogers)
- 2) The rate of vanillin loss depends on time and/or heat (Rogers)
- 3) Radiocarbon area threads were part of the Shroud at the time of the 1532 fire (Adler, Antonacci)

Conclusion : if heating was the reason of the lack of vanillin on the Shroud, no vanillin could be found on Raes/radiocarbon threads too. It is found on it. Thus, time is the main reason of lack of vanillin on the Shroud and Rogers is right : the Shroud is much older than the Raes/radiocarbon sample and the Holland cloth and probably much older than the radiocarbon dating.

It would remain many things to say, but to conclude, for what relates to me, this discussion, I will say that:

I do not know (and nobody knows it) if Raes/radiocarbon area is a repair (although the data of Rogers strongly suggest it). I only know that this sample is different. Rogers well showed what he wanted to show. Thus, for the first time, the radiocarbon dating is really invalidated by the only means that scientists and radiocarbon experts could accept. Incidentally, the "fanatic skeptics" have seen the danger (see, for example, their current fallacious press campaign in France). This is, to me, one of the best arguments in favor of Rogers.

For the first time since 1988, a breach is open in the wall of skepticism which surrounded research on the shroud. It would be desirable that all the Shroud researchers link finally their efforts, starting from the work of Rogers, to strongly support the request of new carbon-14 dating (and other validated methods if they exist), under noncontestable conditions. The dating is and remains the final judge for science as well as for million of people.

Best regards.

Thibault Heimburger.