

Correspondence

COMMENTS ON THE “IR SPECTRA ALTERATION IN WATER PROXIMATE TO THE PALMS OF THERAPEUTIC PRACTITIONERS”

Savely L. Savva

The inspiration to find a measurable property in water that is capable of retaining a memory of an influence by a human mind is very understandable and commendable. In other words, it is an attempt to put a finger on a “prana saturated water.” Some seventeen years ago I tried exactly the same type of experiment. I tried to find micro superstructure in water that had been exposed to Ninel’ Kulagina’s “healing power.” This superstructure, I thought, might be seen in the NMR (Nuclear Magnetic Resonance) spectrum of water exposed to Kulagina’s hands. So I brought her to the NMR laboratory of the Leningrad University headed at the time by Dr. Chizhik. Three pairs of sealed glass vials were prepared, filled with chemically pure water, and solutions of paramagnetic and diamagnetic salts in this water. It was a strictly exploratory attempt. The vials were placed between Kulagina’s palm and the shoulder of one of the researchers for as long as he could tolerate the burn (perhaps, 30-50 seconds). Reddening of the “victim’s” skin was obvious. This was typical to subjects of Kulagina’s laying-on-of-hands treatment. They often developed blisters. Then the vials were placed in the cell of a NMR Spectrometer. To our disappointment, no difference in the NMR spectra of treated and untreated solutions and water was found. That is why I read with great interest the article “Infrared Spectra Alteration in Water Proximate to the Palms of Therapeutic Practitioners” by Stephen A. Schwartz, Randall J. DeMattei, Edward G. Brame, Jr. and S. James P. Spottiswoode published in the first issue of *Subtle Energies*, 1990.

I must say I was somewhat disappointed by some introductory remarks and the way the material is presented. At least eight, rather than two, theoretical models of water structure are known. No real spectra reflecting the variation

in absorption are presented at all; the final measurement result—z Score—is poorly defined (perhaps it is a difference of “ratios” or optical densities that could be expressed in percentages).

However, my main disagreement with the authors is with their selection of frequencies and the method of the optical density measurement—the multiple internal reflection method (MIR). The authors do not explain why they have chosen the frequency of 3620 cm^{-1} . I only can guess that they examined many spectra and found the highest variation at this frequency. But what they actually saw, I think, was variable absorption by water vapor.

It is well known that the IR absorption spectrum of water in the area of 3000 to 3800 cm^{-1} has a broad band assigned to absorption by polymerized liquid water, and two peaks at 3756 cm^{-1} and 3657 cm^{-1} (in vacuum) assigned to the asymmetric and symmetric oscillations of O-H bonds in monomeric (gaseous) water, respectively.¹ Should dimers or trimers occur in the system, they would show up as bumps on the right side (low frequency) of the asymmetric band; however, they were seen only in a nitrogen matrix so far.² The frequency selected by the authors to prove the healers’ effect, 3620 cm^{-1} practically coincides with the band of symmetric O-H bond oscillation in the air.

The two bands in Figure 1 are not to scale as far as intensity is concerned. Absorption of energy from the incident flux depends on the absorption coefficient and the quantity of molecules in the path of the ray, according to Bouguer’s law:

$$I = I_0 e^{-\alpha x}$$

where I_0 and I are energies of incident flux and transmitted flux, α is the absorption coefficient and x is proportional to the number of molecules in the path of the ray (or the length of the cuvette). From this equation comes optical density:

$$D = \log_e(I_0 / I) = \log_e \alpha x$$

The relative intensity of both parts of the spectrum (Figure 1) depends on the method of measurement: if the layer of liquid water is too thin or there is a

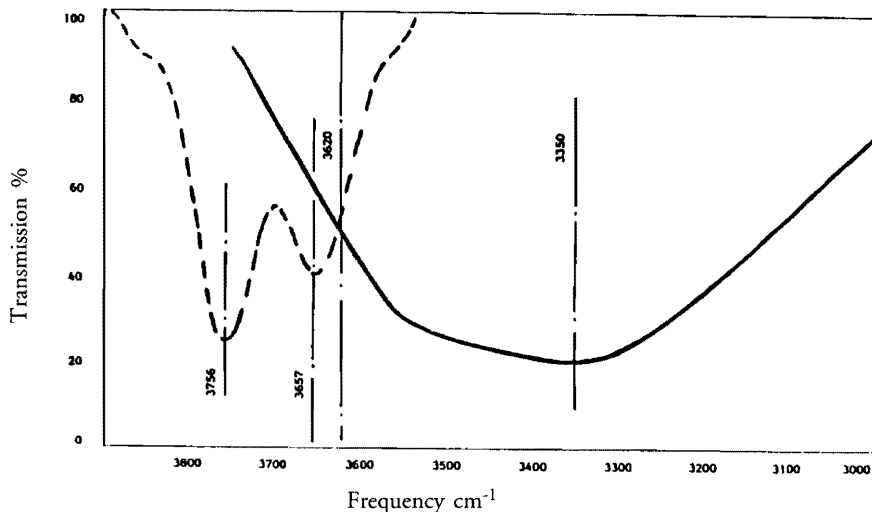


Figure 1. IR Spectra of Condensed and Gaseous (in vacuum) Water.

substantial energy loss as in the case with the multiple reflection element, the absorption by gaseous molecules can show up in the spectrum. This is especially true in a one beam spectrometer which does not compensate for absorption of water dissolved in humid air along the optical path from the source to the sensor. Curiously enough, the authors neglected to measure and even to mention relative humidity of the room air as an important factor.

The authors' remark that the germanium IRE (internal reflection element) with a higher reflective index "consistently produced lower ratios and, thus, artificially skewed the combined overall analysis"³ remarkably confirms my assumption: the greater the energy loss in the IRE, the more significant is the relative impact of gaseous water absorption.

In view of the above I do not see any relevance in the authors' discussion about the puzzling absence of healers' effect on water as compared to "session control." It would be reasonable, I think, to find out what caused the deviation of the "isolated control" spectra. It might be a result, for instance, of different conditions influencing air saturation in water.

I strongly believe that studies of matter, especially water and its solutions after it has been influenced by the most gifted individuals, should go on. But this must be researched in a more thoughtful manner. Perhaps, solubility of water in organic liquids, or solubility of organic molecules in water, or water behavior in dispersed systems may show some deviations after being influenced by a real "sorcerer." And clearly, one should expect these effects from individuals like the late Olga Worrall, Uri Geller or Thomaz Morton rather than from any "certified touch therapist."

One of the objectives of the Monterey Institute for the Study of Alternative Healing Arts (MISAHA) is exactly this: to measure whatever physical and physiological parameters can be decently measured in the process of treatment after an outstanding healing performance by an individual has been statistically established. This task (methodology and availability) will be extensively discussed beforehand with members of the MISAHA's Scientific Advisory Board (now 36 scientists from 10 countries).

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RESPONSE TO THE SAVVA CRITIQUE

Stephan A. Schwartz, Edward G. Brame, Jr., & S. James P. Spottiswoode

To begin with, we would like to thank Dr. Savva for the time and consideration he put into his letter. We also thank him for his description of his preliminary study using NMR; at the time we began designing our own experiment we considered NMR. Both our evaluation, and the recommendation of several consultants, argued against its usage and we discarded it as not being

the optimal technology by which to see any putative changes in the water samples, Treated vs. Controls. Savva's study would appear to support our conclusion since he says visual reddening occurred on the healing recipient's skin—something far more dramatic than anything we saw—yet no change was noted in the water.

In our paper we say, "The IR portion of the electromagnetic spectrum was selected for monitoring based on the assumption that, although we do not know the mechanism, what we are observing is a change in oxygen-hydrogen (O-H) bonding." As Rao points out,¹ and as we stated, "The state of O-H bonding is best observed in the infrared where the fundamental stretching frequency occurs. . ." The technique of choice for examining this, as Harrick notes, is acknowledged to be MIR.² Given this, we are at a loss as to why Savva's "main disagreement with the authors is with the selection of frequencies and the method of optical density measurement — the multiple internal reflection method (MIR)."

Savva's comments that, "At least eight, rather than two, theoretical models of water structure are known." is a misreading of our remarks. We never said there were only two models. There are, indeed, at least eight models, which by definition means there is not a critical consensus on this issue within the research community. For this reason, there seemed to be little to be gained in this context by a lengthy discussion of each theory. Instead we addressed the more fundamental issue noting in our paper, "Discussion of this subject (water structure) . . . is carried out within the context of one or another of *two broad classes of models* (emphasis added) of the structure of water: (1) mixture, and (2) continuum. . . Whichever model is used to explain these changes, IR internal reflection spectroscopy is particularly sensitive to O-H variations, and is the appropriate technique for this measurement." Hornung's, Choppin's and Renovitch's survey reference covering these issues was cited.³

After much discussion we decided not to present a single spectrum, or even a treated spectrum and its control as illustrations. We did this because we had determined to make the most conservative analysis of the data, and this was to consider only groups, i.e., Treateds vs. Controls, rather than single Treateds vs. single Controls. From this perspective what matters is not a single spectrum, but how *all* spectra were measured, and the ratio

derived. This seemed most clearly presented in the form of an example illustration, where the general spectra form, and the measurement locations could be emphasized.

According to Savva “the z -score is poorly defined.” In the paper, we defined it as: “Testing the stated hypotheses involves a comparison of the mean values of R found for the three sample populations of Treated, Session Controls, and Calibration Controls. It was determined that depending on distribution of the R values in these populations, a suitable statistical test (t -test for normal distributions, Mann-Whitney U test for non-normal distributions) would be chosen to determine whether there was a significant difference between those mean R values.”

In Figure 2 R was defined as: “We measured the absorption at two frequencies ($f_1 = 3620 \text{ cm}^{-1}$, $f_2 = 3350 \text{ cm}^{-1}$) at the peak and shoulder of the absorption band. To normalize the absorbance values, a baseline was constructed beginning at 3800 cm^{-1} across to 2700 cm^{-1} .”

Exactly how this is a “poor” definition is not clear. Savva’s suggestion that the z -score “could be expressed as a percentage” would, in our experience, be so unusual as to be confounding.

“The authors do not explain,” Savva says, “why they have chosen the frequency of 3620 cm^{-1} . . .” We pre-selected 3620 cm^{-1} (or 2.76 micrometers) as f_1 based on the survey paper we cited, by Hornung, Choppin, and Renovitch. It covers their work, as well as the work of others, in the spectroscopic analysis of water.³ We chose this reference not only for its completeness but because, for those inclined to pursue the subject, it includes spectra showing most clearly the bands which were used in our work. If our hypothesis as to what was happening, i.e., a change in the O-H bonding relationship, was correct this was the appropriate part of the spectrum to evaluate. Savva’s comment “I can only guess that they examined many spectra and found the highest variation at this frequency,” is simply incorrect.

We would also note that had we done the kind of “fishing expedition” he suggests our analyses would be post hoc, in which case they would have to be labeled as such. The reader is left in no doubt as to what was post hoc in our

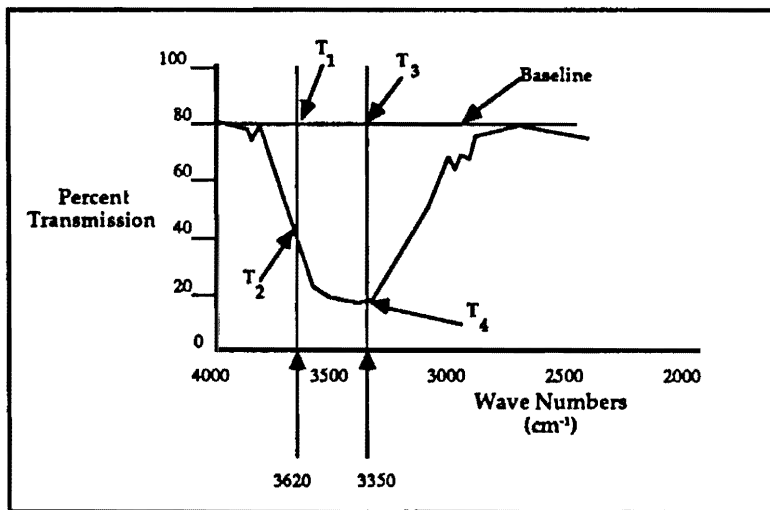


Figure 2. Example Spectrum and Measurement

The absorbance at 3620 cm^{-1} is defined by:
$$A_u = \log \left(\frac{T_1}{T_2} \right) \quad (1)$$

Similarly, at 3350 cm^{-1} is defined by:
$$A_b = \log \left(\frac{T_3}{T_4} \right) \quad (2)$$

Then, the dependent variable R is given by:

$$R = \frac{A_b}{A_u} = \frac{\log \left(\frac{T_3}{T_4} \right)}{\log \left(\frac{T_1}{T_2} \right)} \quad (3)$$

work, i.e., a sample handling issue, and what was built into the original project design. Indeed, after the initial hypotheses outcomes are reported, a complete re-analysis of the data is presented with post hoc adjusted ratios. As noted, “The z -score difference between adjusted and unadjusted ratios, in the most pronounced category, Treated vs. Calibration (Controls), changed only from -2.97 to -2.93.”

The humidity issue begins to get at the heart of Dr. Savva’s criticism. We considered a variety of factors like temperature and barometric pressure, and the paper details these environmental corrections and their effects. Re-calculated data accompanies these discussions. A superficial analysis would seem to suggest that humidity should be included. We did not mention humidity, however, because it was not a factor. The reason why it is not, also addresses Savva’s criticism that “. . . what they actually saw, I think, was variable absorption by water vapor.” The trouble with his entire line of reasoning is that it is based on false assumptions both about how the experiment operated, and how the data were analyzed.

To begin with the mechanical: As described in the paper the water went directly from a hermetically sealed 50mm vial, into a sterile single usage syringe via a needle inserted into the vial through its rubber top. It was then inserted into the Internal Reflection Element (IRE). Because of the construction of the IRE of the Multiple Internal Reflection (MIR) unit—essentially there is a small cavity which one fills—the water is always of the same thickness as the beam passes through it. There is no water vapor, because there is no airspace from vial to IRE, and within the IRE.

The issue of air saturation in the water, was explored at some length in the paper, and an analysis was reported as part of our consideration of sampling and order variation. That is, over the course of taking the measurements, the pressure on the water inside the vial changed as the vacuum increased with each sample withdrawal. Thus, the air saturation in the water might vary. As noted in the paper “To explore this we took the mean of the ratios of each measurement category, i.e., all first-of-the-day Calibration Control sample ratios, all second-measurement-of-the-day Calibration Control sample ratios and so on up to measurement six.” Variation was within error bars.

But the most critical point in this discussion lies in the effect of environmental water vapor (humidity) in the optical path. Had we used a single path instrument, as Savva seems to have inferred without checking, this could indeed have become an issue. However, the “Perkin-Elmer grating infrared model 237B” is a double beam system which corrects for distortions due to water vapor absorption by splitting the light beam in two: reference and sample. Even with the use of the MIR cell, the path length of the sample beam is essentially the same as that of the reference beam. The double beam renders water vapor absorption concerns a non-issue.

Finally, there is the appearance of the spectra themselves. Any appearance of water vapor in infrared spectra shows itself as sharp bands and not broad appearing bands. The two are easily distinguished, one from another.

Relative to analysis: In the work that preceded ours, analysis of the data was carried out by taking the difference between a single treated vial and its control. Each such calculation was treated as a test of hypothesis, with positive values presumably showing lower energy in the test samples, while negative values showed higher energy; the assumption, which we still support, being that something had either induced or subtracted energy from the test sample during the course of the healing session. Although his description is sketchy this seems to be the analytical approach Savva followed in his NMR experiment. In our preliminary report we included this analysis, following the precedent set by Grad,⁴ Dean⁵ and Dean and Brame.⁶ We presented ratio tables accompanied by the actual spectrum of a Treated and a Control, just as requested by Savva, and showed that, “In accordance with earlier studies, a healing signature appeared in the 2.5 to 3.0 micron range. Positive or negative values greater than a magnitude of .2 in the Treated minus Calibration column are significance benchmarks in this report.”⁷

However, we found this analytical approach unsatisfying and, after our preliminary presentation, we dropped it from our final report. It lacked the rigour of our statistical analyses, the threshold of significant effect seemed inelegant and arbitrary, and the two approaches, when considered together, seemed to create more confusion than clarity. Most important of all, we felt the Treated minus Calibration approach was not conservative enough. In considering anomalous phenomena, we feel the most conservative analysis should be the

one reported and emphasized. The most conservative position in this instance was to report a statistical result derived by comparing blocs or vial sub-populations, i.e., Treated vs. Controls.

Because of this bloc analysis even if water vapor were an issue, the outcome effect of any variation between any given Treated and its Control, would be significantly dampened. Further, one should remember that each measurement was taken twice, each time by a different spectroscopist, and an inter-spectroscopist analysis was done. As the paper reports, the mean in both cases was 3.27 and the Standard Deviation was 0.14 for Spectroscopist One, and 0.15 for Spectroscopist Two.

Dr. Savva's final comments, beginning with ". . . clearly one should expect these effects from individuals like the late Olga Warrel (sic), Uri Geller, or Thomaz Morton (sic) rather than from any 'certified touch therapist. (sic)'" appear to us to be inconsistent with the literature. The body of Therapeutic Touch studies strongly indicate that the ability to manifest Therapeutic Intent is widespread, although individual potential is improved through training.^{9,10} Our study certainly suggests this; when both Practicing and Non-Practicing participants were analyzed as separate sub-populations each showed independent significance ($z = -3.08/p = 0.001$ for Practicing, and $z = -1.75/p = 0.04$ for Non-Practicing). Most healers, of course, understand this, indeed, rely on it. Olga and Ambrose Worrall, for instance, told Schwartz that it was because the ability was widespread, and could be improved by training, that they could hope to see their healing center continue (as it has) beyond their lifetimes.¹⁰

And, just to close, Dr. Savva might like to know that the protocol for this experiment, as a matter of Mobius policy, was submitted to extensive interdisciplinary peer-review before the data gathering was ever carried out.

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PARADIGM BREAKING

Jerry E. Wesch, Ph.D.

Just re-read Stephan Schwartz' wonderful essay on the perils of paradigm breaking (Vol. 2. No. 1) and wanted to let you know how much I enjoyed it. It fits well with a phenomena I have noted in ISSSEEM—the problem of the “public” scientist or professional who is a “closeted” subtle energies enthusiast. For example, I'll wager more of our journals go to home addresses (as opposed to university or hospital/clinic addresses) than is usual for a scientific journal. In addition, the specific interest in allowing members' privacy in participation in local ISSSEEM affiliates seems to come from the same fear of being “exposed”—or “outed,” as the gay subjects in my research cohort call it. I would be interested in a survey of the membership about the degree of being “out-of-the-closet” so to speak in relationship to professional

roles vs. private experiences, beliefs and practices. We probably should also ask about the perceptions, experience and other forces that shape the differences. The results might give some ideas about the nature of current and future psychosocial difficulties for both ISSSEEM and its members as scientific exploration in subtle energies develops.

Somebody (maybe Ornstein) once talked about “Fear of Psi” as a personal resistance to experiencing anomalous phenomena—there seems to be a similar denial at a paradigm level to dealing with the five issues you describe in subtle energies research. (Incidentally, I might add a 6th—human consciousness may not be the only kind that exists, although this may be implicit in #3.) Regarding #5, “. . . goal of organic evolution. . .”, one of Robert Monroe’s OBE (out-of-body) teams reports a disembodied entity who said there were two kinds of human evolution—one vertical, toward merger with higher consciousness . . . the other horizontal, toward the perfection of mankind as mankind. Sounds like the mystics vs. the humanists.

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