Correspondence between
Dr. Paulo Correa, Alexandra Correa and James DeMeo
in May-October of 2001

Subject: Thanks
Date: Tue, 15 May 2001 12:16:13 -0700
From: J. DeMeo
To: Correa&Correa

Dear Paulo, Alex

[...]

Thank you all so very much for your hospitality during my visit, and once again, for sharing with me the findings and experiments which are so obviously the product of a lot of hard and focused work. As I said previously, this is the first time in 30 years where I have seen this aspect of Reich's work, with the motor, so clearly demonstrated. And, of course, you have broken into new territory which is pregnant with possibilities.

I intend to write you a letter, confirming my trip and your demonstration of the apparatus, which you can use in any manner you wish. Also, if you need to provide an outside reference, please feel free to give my name to persons whom you might wish to have an outside positive opinion of your work.

I've read the two papers you gave, and find myself in very large agreement with only a few small critical points that I can relate to you later on.

Again, seeing the motor turning with the flashing light from the vacuum tube provided a most wonderul vision into a better future.

Aside from my writing you the letter, please let me know, if there is anything I can do to assist your efforts.

Warm regards,

James

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Subject: Most welcome!
Date: Tue, 15 May 2001 22:36:37 -0400
From: Correa&Correa
To: J. DeMeo

Dear James,

Thank you for the warmest, most gracious and kind letter we have yet received - and as well for letting us know you have arrived safely. And thank you again for the gift of the 9154. [...]

You are, of course, most welcome! Your visit for us was a delight. We enjoyed it very much. And we thank you also for your courageous and generous offer. We would love to read what you might wish to write about what you have seen and perhaps, if you wish, this is something we could post on the website we are constructing.
Do also make your comments on the Einstein papers so we can discuss them.

We should let you know that we made a series of inquiries to CBC yesterday to see if we will be able to unearth this video we spoke of regarding India's nuclear power program - as we thought this might be easier for us to try to follow up on than for you. We sent email to 'Rough Cuts' and 'The Passionate Eye' (two CBC documentary programs) but have not heard anything yet. We will let you know if we are able to locate it.

Looking forward to hearing from you!

Warmest regards

Alex, Paulo - and also from Malgosia

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Subject: Letter
Date: Tue, 22 May 2001 23:42:40 -0700
From: J. DeMeo
To: Correa&Correa

Dear Paulo, Alexandra

Below is the text of a letter I will print out on OBRL stationery and mail to you. If there is anything you would rather I not say, let me know. Otherwise, I'll print and mail it on Thursday.

Regarding your email on Miller, etc., I'll reply separately.

Warm regards,

James
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Dr. Paulo and Alexandra Correa
[...]

22 May 2001

Dear Paulo and Alexandra,

In this letter, I wish to recount my observations during a recent visit to your laboratory on May 12-13. Firstly, let me express my thanks for your personal hospitality and professional openness about what surely is a most controversial set of subjects. As you know -- and I think this is one reason why you invited me -- I have been investigating the subject of Wilhelm Reich's cosmic orgone energy motor and collecting every scrap of information on the subject I could find, since the early 1970s when I firstly heard of the subject. Following my visit to your laboratory, and seeing your various demonstrations, I can state quite openly that you have done more work on this subject than anyone I have ever met, and it surely appears that you have solved the riddle of Reich's undisclosed "Y-Factor", and progressed significantly far on your own to stand at the edge of producing practical applications.

Specifically, your first demonstrations were of the PAGD device, which uses a vacuum tube configuration similar to Reich's (superficially, at least), but with much higher voltage tensions and at only moderated vacuum pressures. One set of batteries created the charge necessary for the PAGD to develop some form of cascade-effect (my words) which was tapped to charge up a second set of batteries. The computer program you developed indicated the energy required to sustain the phenomenon was less than the energy developed from it. On this particular demonstration, I would repeat a suggestion made to you at the lab, that you should try and develop the proper circuitry to make a self-sustaining reaction that would allow the recharging of a single bank of batteries which also would run the process, and simultaneously allow the excess energy to run other apparatus.

While the PAGD demonstration may in fact be the more remarkable development for eventual practical power-production, on a more personal level I found your demonstration in the smaller lab room on the second day to be even more delightful. Here, you showed me several of the "spinner" type motors originally set into motion by orgone energy by Reich, which you were also able to accomplish. The spinning of the motors from the pulsatory energy derived from the vacuum was a marvelous demonstration, and in fact appears to be a much simpler arrangement than the larger PAGD apparatus, since this latter demonstration does not require the large banks of batteries, nor any computer program to tell you something unusual is going on! I observed the spinner motor and vacuum tube as powered by the orgone accumulator, and by antenna with earth-ground, using an electronic circuit which remained undisclosed, but which constituted the Y-factor.

The simultaneous demonstration of the orgone-charged GM tube was almost equally enchanting to observe, as in my own lab, so far I have only observed spurious explosions of counts, or erratic counts, from such charging -- never the smooth effects observed at your lab, where incremental approach of the apparatus to the accumulator yielded incremental increases in the cpm. Along these lines, it might interest you to learn, that upon my return back to Greensprings, the neutron counter I had been charging in the orgone room yielded something approaching several thousand cpm (neutrons only) in a burst that lasted a full three minutes -- something only classically observed inside a nuclear reactor, I imagine. The phenomenon would probably have continued for some additional minutes had I not gotten terribly curious and removed the probe from its hard plastic sphere of moderating material, whereupon the counts immediately ceased, and would not return again after re-insertion of the probe. Such is the nature of these marvelous orgone phenomena.

Returning back to your own experiments and demonstrations, it seems clear to me, that you should make your best effort to bring these discoveries to the world. I agree with you totally about the incredibly crazy condition of the human species, but as a student of history also can see that even a clear and compelling demonstration and applications program would not automatically find acceptance. People will resist it, much as the airplane was resisted. The darker side of things, I feel you can control by deciding what to release to the public, and what not, and rely upon friends to keep the discovery clear enough to the point that the socially-revolutionary aspects connected with energy-in-space, and with even the social discoveries of Reich, are all carried along with the tide.

Upon returning home, I had a conversation with Eugene Mallove, who also is a supporter of your work, and one of his ideas I found to be excellent. This is, on the assumption you cannot get the full amount of funding you seek for full-scale applications, that with more modest funding, start to make demonstration apparatus, self-contained devices which can be sold to anyone in the public for a fee of several thousand dollars, as examples of a working free-energy device. These would serve several basic functions:
1. The sales would provide some income to yourselves,
2. The demonstration apparatus would attract increasing attention and eventually the larger funding you seek, and 3. It would build your practical experience in getting the things to work under less-than-ideal circumstances.

My own suggestion, in addition to the above, is as follows: Since you now have a patent on the central PAGD device, go ahead and publish the full information about both the PAGD and the orgone-motor Y-factor. Build demonstration devices and even let ordinary tinkerers build them for their own evaluation and home energy production, and start a new revolution in the sciences and in industry, requiring self-builders only to sign a paper saying they will not make commercial ventures without a separate contract with you. I can imagine, you would be totally ignored by industry and science for a time, until some critical persons witnessed the demonstrations, at which point the whole thing would burst upon the social scene. This is my own recommendation, having engaged in this and similar work over the long haul, and seeing too few really concrete advancements.

I am truly positively excited by what you have shown me, and you can give my name and a copy of this letter to any persons who might want an outside independent evaluation. Let

me know if there is anything I can do to assist in the development and bringing forward of your work.

With kind regards,
James DeMeo, Ph.D.
Director

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Subject: Our Plans & Miller
Date: Tue, 22 May 2001 21:55:04 -0500
From: Correa&Correa
To: J. DeMeo

Dear James -

It has been a little over a week since our last meeting and we all miss seeing and hearing you. I did not want to interrupt your heavy work schedule, but I read your Miller paper last Tuesday and made a few remarks that follow below - for you to respond when you can - and if you think they are relevant.

[...] But Gene is enthusiastic and energetic, and truly undefatigable. He knows you have seen the PAGD and Aether/OR motor demos, and I sense he is keen to talk to you at the upcoming ISSE meeting.

We have cleaned, polished and reassembled your gracious gift of the KS-9154, and maybe it works a little better and rather quietly. [...]

Before the first installment of work becomes available (ie as soon as it is mounted), we will provide you with a password for access in the hope you will enjoy reading the material. It goes without saying that we will appreciate your feedback, negative and positive. Also, if you agree, we would, at a suitable time, make your upcoming letter freely available at our website.

We have not heard yet from either Rough Cuts, nor The Passionate Eye regarding the Indian Nuclear Documentary although we have sent them several emails. In the meantime, we did find these two websites which may be of some interest -

Nuclear Map of India
http://www.anawa.org.au/india/india-map.html

The Jadugoda Uranium Mine
http://www.anawa.org.au/india/jaduguda.html

Big hugs from all three of us -

Paulo

I just finished reading your excellent paper on Miller's work - but it also left me, as you say, frustrated and astonished. Astonished, because you make a very strong case for Miller's work, which I confess I once took seriously and then abandoned. Frustrated because of the myriad of experimental work questions that it raises - not to mention the implications. I also consulted my old notes on Miller and the parts in the first volume of Aetherometry that address Miller's work. What follows are thoughts on (to my mind) critical points that I would love you to address further.

There is no doubt that the controversy with Einstein damaged Miller greatly - and it does appear that even Shankland et al acknowledged the existence of nonrandom, systematic and periodic effects in Miller's data as you point out. Nor was their analysis sufficient even for their own purposes, and it is nearly unbearable to think that hundreds of Miller's data sheets might have been burned by Shankland?, as you put it.

On the other hand, Miller himself acknowledged that there were thermal effects at work, as you also point out. But in this respect, I believe, there are also criticisms to be addressed to Miller himself. Since he wanted his apparatus to be as exposed to the elements as possible, I am convinced that it would invariably detect a diurnal variation in the start-up calibration (that sunlight might have caused spurious peaks is of little use, if complete diurnal atmospheric records were not undertaken; for instance, he should have taken control temperatures of the room, walls and roof, which apparently he did not). Nowhere does Miller seem to have controlled for this in a systematic fashion. Then, it is not just the heating effect of the sun upon the atmosphere that one should consider (even if it happened only once - can we prove that?), but equally the cooling effect of nighttime. We have seen these effects in ORACs and can easily suppose that they would affect such a sensitive interferometer as Miller's.

More disturbing still, to my way of thinking, is that the data Miller obtained - with his final and improved interferometer - yielded two very different reports of the direction of the aether drift: in 1926 he reported in Science (63:436) that the absolute motion of the earth was towards the head of Draco, RA 17h, North Dec. +65°, in the Northern Celestial Hemisphere, whereas by 1933, the motion was towards Dorado in the Southern celestial Hemisphere, nearly 180° off. Yet, the value of the drift remained at ca 200 km/sec, and the drag lowered it to the same 9 or 10 km/sec at the ca 1.8 km altitude.

Lastly come a series of considerations about what exactly did Miller thought he was detecting. One could approach this by systematizing the alternative aether models-

1. If the aether were a static fabric of space, and the earth did not entrain it, the MM experiment should have measured the translatory motions of the earth, whether solar or galactic, or both. As it did not, the hypothesis of a non-entrained stationary aether could be ruled out.
2. If the 'inertial motion' of the earth entrains a stationary aether to create an aetherosphere - thus dragging the aether along - the relative velocity between the ether and the earth may be zero (if the aetherosphere was a fixed skin) or very small (with the aether lagging behind the earth's movement of rotation, since the latter entrains it). If it were zero, then a negative result to the MM experiment should also be expected. And if it were a small lag (necessarily referenced to rotation, given that a drag referenced to translation would have to yield a lag only when the interferometry experiments were conducted during daytime), it would also fit with a nearly null result, yet it would directly contradict the West to East

motion detected by the Sagnac-type experiments - and require precisely a reverse lag (an apparent aetherospheric motion from East to West).

The two preceding alternative models are based on the notion that the rotary and translatory motions of the Earth are givens that cannot be directly explained by any form of coupling to an aether which is seen as stationary. In the second model - that of entrainment or dragging of the aether - the earth is construed to move 'like a rotating ball on stagnant water', as W. Reich put it.

Now, what to me is confusing with Miller's notion of an aether drift is that, at the end of the day, it appears to have nothing in common with the aether drag (rotary or translatory) models - since it suggests that his measurements consist of a detection of a cosmological aether drift that carries the Earth along. But it argues it detects this 'translational' drift at altitude, as a much slower velocity of the aether due to an aether drag model of the aetherosphere (otherwise the displacement fringes would be substantial).

As you well know, there is another way to construe an aether model that fits both the null result of the MM-type experiments and the results of the Sagnac-type experiments:

3. In this model, it would not be the Earth that would entrain a stationary aether, but instead a consistent motion of the aether that would propel forward the Earth, the Solar System and even the entire Galaxy or the Local Group. To again employ Reich's words, 'the analogy is that of a ball rolling on water waves more slowly than the waves'. There would still be an aetherosphere, created not by dragging a stationary aether, but by a consistent aether spin (the result of the superimposition of multiple such spins, at a cosmic, galactic, solar and planetarian levels) propelling at once both the rotary and translatory motions of the Earth. Outside of the aetherosphere, a much faster aether flux should be detectable, but the aether impulses would impart angular momentum to the planet by curving in along cycloidal paths towards the planet's surface, their energy being partially absorbed to drive the Earth's motions, as the wave impulses slow down to near the Earth's speed of rotary motion.

This third model would fit in with the notion that the MM-type experiments should yield a null result, until and unless their resolution approached measurement of that slightly faster rotation of the aetherosphere, on the order of 100 or so m/sec faster than the local terrestrial speed of rotation. And the same model would also fit in with the notion that Sagnac-type experiments should be able to measure the rotary motion of the interferometer, and when conducted as a planetarian Sagnac, should yield a faster motion of the atmosphere from West to East, in the same direction as the rotation of the planet. It follows that only the third hypothesis fits the experimental findings, and remains 'unbothered' by the small MM residuals. Moreover, unlike the previous two models of the stationary aether (undragged and dragged), the third model proposes a dynamic aether that itself explains the nearly-perpetual motions of the planet - motions which, therefore, are not treated as simply 'given'.

One might call this aether flux model, an aether drift model - where the Earth, the Sun and the other planets are dragged along by an aether drift referenced to 'the distant stars'. But the notion of drift itself conjures up the notion of an original event that impelled this drift - such as the mythical Big-Bang extracted from the New Aether Drift axed on the microwave CBR - rather than the concept of the multiple-layered superimposition of synchronous and consistent fluxes of aether spin that permanently impel astrophysical bodies, and where the lag of the motion of these bodies to their spinning aetherosphere is constitutive of the surface currents sustaining their very rotation and translation, much as the lag of drag-cup motors yields eddy currents that are constitutive of rotor motion (hence the technical concept of slip is nonsensical in drag-cups).

In accordance with this model, one should indeed be also able to detect greater motion of satellites near the shear zone when the aether impulses slow down. This is an old question that goes back to the work of Newton. And it is indeed true that, beginning at an equatorial geostationary distance of 35,862 km above the Earth, when the translatory speed of the satellite around the Earth's axis is ca 3 km/sec, satellite speed increases steadily to a value of 7.8 km/sec at ca 100 Km above the Earth, and to some slightly higher value at a slightly lower altitude still; but then, instead of continuing to increase to a theoretical 7.9 km/sec at the Earth's surface, the satellite is dragged down, suddenly decelerated, such that at tropospheric altitudes, the speed of the flux holding an imaginary satellite afloat in a trajectory parallel to the earth would not be any faster than the variable speed (0.01 to 0.1 km/sec) of the jet stream with respect to the Earth. Note also that it is along the ridges and troughs of the jet stream that cyclonic and anticyclonic systems couple themselves, much as eddy currents counter-couple themselves on the surface of a drag-cup. A suitable approximation would be ca 0.5 km/sec at altitudes of ca 10 Km, in temperate latitudes. This abrupt slowing down of the inner concentric layers of the spinning aetherosphere below 100 Km results precisely from the atmospheric and terrestrial absorption of the impulses of the 'aether stream' - and causes, of course, the illusion that free fall is a motion along the vertical.

The question then arises as to whether Miller could have detected that aether drift (and without reference to the W to E motion of the OR envelope), once it is slowed down and made to encircle the planet at a slightly faster rate of motion than the motion of the surface or the rotation of the planet. At ca 1.8 km altitude, and in light of the preceding, it seems unlikely that the value of an aether drift at 9 to 10 km/sec could be real.

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Subject: Re: Our Plans & Miller
Date: Tue, 22 May 2001 23:47:28 -0700
From: J. DeMeo
To: Correa&Correa

Dear Paulo, Alexandra

In another email I've transmitted a copy of my letter. Thanks very much for the constructive critique of the Miller paper -- I will address these points in yet a third email, as it is very late for me tonight. I've also got some points to suggest for your papers, which I read on the airplane. Very good material, educating me on many points about the Sagnac-type experiments which I did not know about.

A lot of activity since my return -- setting up for a solar panel installation, some small-scale work against the drought here, organizing a helper to repair my big cloudbuster, trying to finish the paper for SSE, etc. Sometimes, I wish I had been a "specialist" with only one thing to think about.

Regarding the electronic publication of your papers, I think without being able to offer credit-card purchases, your sales will suffer significantly. Maybe with some additional searching you can find a way to do this. I should be delighted in any case to read the materials when they are available, and will give you constructive critique as best as I can.

Regarding posting my letter to your website -- for the present time I would request only for private circulation, using your discretion. In any case, I think my letter will only have some historical importance, as for any big-money investors, my name and OBRL would simply appear as some other small-potatoes undertaking, like one heretic approving of another heretic. To say that the world's authority on cloudbusting supports the world's authority on the cosmic energy motor is somewhat humorous, from the conventional viewpoint in any case.

I also did some internet searching on the India nuclear waste situation, but found nothing of direct relevance. In any case, I am pessimistic about this project going forward at all.

More later....

Warm regards,
James

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Subject: Ether Papers
Date: Wed, 23 May 2001 14:17:31 -0700
From: J. DeMeo
To: Correa&Correa

Dear Paulo,

I give you some ideas and response below, on specific items in your email.

> I just finished reading your excellent paper on Miller's work - but it
>also left me, as you say, frustrated and astonished. Astonished,
>because you make a very strong case for Miller's work, which I confess I
>once took seriously and then abandoned. Frustrated because of the
>myriad of experimental work questions that it raises - not to mention
>the implications.
>I also consulted my old notes on Miller and the parts in the first
>volume of Aetherometry that address Miller's work. What follows are
>thoughts on (to my mind) critical points that I would love you to
>address further.
>
> There is no doubt that the controversy with Einstein damaged Miller
>greatly - and it does appear that even Shankland et al acknowledged the
>existence of nonrandom, systematic and periodic effects in Miller's data
>as you point out. Nor was their analysis sufficient even for their own
>purposes, and it is nearly unbearable to think that hundreds of Miller's
>data sheets might have been burned by Shankland?, as you put it.
>
> On the other hand, Miller himself acknowledged that there were thermal
>effects at work, as you also point out. But in this respect, I believe,
>there are also criticisms to be addressed to Miller himself. Since he
>wanted his apparatus to be as exposed to the elements as possible, I am
>convinced that it would invariably detect a diurnal variation in the
>start-up calibration (that sunlight might have caused spurious peaks is

>of little use, if complete diurnal atmospheric records were not
>undertaken; for instance, he should have taken control temperatures of
>the room, walls and roof, which apparently he did not). Nowhere does
>Miller seem to have controlled for this in a systematic fashion. Then,
>it is not just the heating effect of the sun upon the atmosphere that
>one should consider (even if it happened only once - can we prove
>that?), but equally the cooling effect of nighttime. We have seen these
>effects in ORACs and can easily suppose that they would affect such a
>sensitive interferometer as Miller's.

Miller did record temperatures of every wall, using tenth-degree thermometers, at the start and finish of each run of around 20 interferometer turns, which would fill one data sheet (see the example on p.8 of my paper). You can see the notations on the sheets. Shankland et al tried to use those notations to make a refutation of Miller's data, but as I point out, without any clear indication. As I point out, the most obvious way to evaluate for a thermal effect would have been to separate the daytime runs from the nighttime runs, and show a strong southerly (solar heat) component. This was not undertaken by them, or if so, not reported. But in fact, we have no evidence that even a strong solar heating of a wall or the roof (which had a tent-cover) would have affected the experimental result -- recall Miller's control experiments at the basement laboratory at Case School in Cleveland. He developed insulation coverings for both the iron support and light-beam path. From there, he showed it was possible to skew the results by placing strong parabolic radiant heaters in the room, aimed at the interferometer, to create a 0.07 fringe shift -- that was reported by Shankland as taken from Miller's lab book. No such intensive heat source was present at the Mount Wilson interferometer house, and I believe my speculation of a shift of only 0.007 fringe is reasonable (see p.13). That places thermal distortion in Miller's apparatus below the observational threshold, as he recorded the values only in tenths to hundredths of a fringe, with roundings only in hundredths. From my knowledge, Miller is the only one to have addressed the question of temperature effects on the interferometer through experimental measures. As such, I think the critics have to do more than merely raise a speculative objection, and run some experiments of their own, on an apparatus similar in construction and insulation to Millers, to prove a thermal artifact. Another point here, mentioned by Miller many times, is that virtually all of the past interferometer experiments, including M-M, showed similar azimuthal directions of ether drift -- were all those experiments suffering from unknown but identical thermal distortions?

> More disturbing still, to my way of thinking, is that the data Miller
>obtained - with his final and improved interferometer - yielded two very
>different reports of the direction of the aether drift: in 1926 he
>reported in Science (63:436) that the absolute motion of the earth was
>towards the head of Draco, RA 17h, North Dec. +65°, in the Northern
>Celestial Hemisphere, whereas by 1933, the motion was towards Dorado in
>the Southern celestial Hemisphere, nearly 180° off. Yet, the value of
>the drift remained at ca 200 km/sec, and the drag lowered it to the same
>9 or 10 km/sec at the ca 1.8 km altitude.

As I understand it, the interferometer can only yield a result which indicates a line of ether-drift, without value as to which direction along the line the drift is occurring. Imagine again the example of the swimmers going up/down or back/across the current, but where you cannot see the water, and the only information you have is their final time of arrival back at

the point of origin. The interpretation of the direction along the line is made by undertaking logical arguments based upon other kinds of observations, such as stellar aberration, etc. Likewise, his calculation of the final drift speed of c.200 km/s -- all that information was extrapolated and inferred from the basic set of observations which showed a ether-drift of 10 km/s along a line between Draco and Dorado. He obviously changed his mind at some point between 1926 and 1933, and I think it had something to do with various interpretations of ether-drag. The idea of Reich, of ether as a prime-mover, I feel is more to the point, but Miller did not conceive this. Somewhere in your own paper you mention that ether cannot be "dragged" in the conventional way of thinking, and I agree. There are a few places where Miller gives ambiguous statements on this as well, but I cannot tell if this was developed from his own research, or if he was merely responding to various criticisms from Michelson and others, or maybe even to the demands of an editor. I only fault Miller for not expressing his findings and calculations more explicitly, so others could follow his work more precisely, but the same is true for Michelson-Morley and other ether experimenters where a lot of technical detail is scattered across many different papers. Too bad he never wrote a book on this subject, to gather all the information in one source.

> Lastly come a series of considerations about what exactly did Miller
>thought he was detecting. One could approach this by systematizing the
>alternative aether models-
> 1. If the aether were a static fabric of space, and the earth did not
>entrain it, the MM experiment should have measured the translatory
>motions of the earth, whether solar or galactic, or both. As it did
>not, the hypothesis of a non-entrained stationary aether could be ruled
>out.

Yes, agreed. But the MM experiment did yield a small positive result.

> 2. If the 'inertial motion' of the earth entrains a stationary aether
>to create an aetherosphere - thus dragging the aether along - the
>relative velocity between the ether and the earth may be zero (if the
>aetherosphere was a fixed skin) or very small (with the aether lagging
>behind the earth's movement of rotation, since the latter entrains it).
>If it were zero, then a negative result to the MM experiment should also
>be expected. And if it were a small lag (necessarily referenced to
>rotation, given that a drag referenced to translation would have to
>yield a lag only when the interferometry experiments were conducted
>during daytime), it would also fit with a nearly null result, yet it
>would directly contradict the West to East motion detected by the
>Sagnac-type experiments - and require precisely a reverse lag (an
>apparent aetherospheric motion from East to West).

I'm a bit lost on your question here. The Sagnac experiment was table-top, and yielded no information about earth movements, only that the rotational velocity of the interferometer could be added to or subtracted from the velocity of light moving around the rotating platform. The Michelson-Gale-Pearson experiment was able to determine earth rotation, but again without information regarding the direction (W-->E or W<--E). If my memory has failed here, please correct me. In any case, there is no reason to believe that a positive result on Miller's experiment would necessitate a negative result on either Sagnac or MGP. Those

latter experiments were not designed to detect any component of drift other than within the rotational plane of the earth.

> The two preceding alternative models are based on the notion that the
>rotary and translatory motions of the Earth are givens that cannot be
>directly explained by any form of coupling to an aether which is seen as
>stationary. In the second model - that of entrainment or dragging of
>the aether - the earth is construed to move 'like a rotating ball on
>stagnant water', as W. Reich put it.

Again, I am not fully following your point #2 above, but Reich's model has significant differences from the standard "dragged ether" idea, the former having the ether in motion as prime mover, the latter having the earth push through a relatively stagnant but draggable ether.

> Now, what to me is confusing with Miller's notion of an aether drift is
>that, at the end of the day, it appears to have nothing in common with
>the aether drag (rotary or translatory) models - since it suggests that
>his measurements consist of a detection of a cosmological aether drift
>that carries the Earth along.

The suggestion is there, but I never read anything where Miller viewed the ether as in motion, pushing the planets along on their path. For awhile, I assumed he had to believe this, but now I'm certain he never believed this. He viewed the earth as pushing through the medium, not being moved by it. Which is why he ran into confusions about the earth moving towards Draco, and then Dorado. For along time, I made the error to say that Miller had claimed discovery of a "dynamic ether", which is not the case. He did not attribute dynamic properties to the ether -- only Reich did this.

But it argues it detects this
>'translational' drift at altitude, as a much slower velocity of the
>aether due to an aether drag model of the aetherosphere (otherwise the
>displacement fringes would be substantial).

Yes, faster at higher altitudes, a kind of "laminar flow" model, but where the earth pushes through the stuff. Factually, this is impossible as A) it requires some small interaction of ether with matter and yet B) such an interact would eventually bring the planetary motions to a standstill, or at least to a speed of movement equal to the ether. Consequently, if ether-matter interaction occurs, ether has to move, has to be the prime mover.

> As you well know, there is another way to construe an aether model that
>fits both the null result of the MM-type experiments and the results of
>the Sagnac-type experiments:
> 3. In this model, it would not be the Earth that would entrain a
>stationary aether, but instead a consistent motion of the aether that
>would propel forward the Earth, the Solar System and even the entire
>Galaxy or the Local Group. To again employ Reich's words, 'the analogy
>is that of a ball rolling on water waves more slowly than the waves'.
>There would still be an aetherosphere, created not by dragging a
>stationary aether, but by a consistent aether spin (the result of the

>superimposition of multiple such spins, at a cosmic, galactic, solar and
>planetarian levels) propelling at once both the rotary and translatory
>motions of the Earth. Outside of the aetherosphere, a much faster
>aether flux should be detectable, but the aether impulses would impart
>angular momentum to the planet by curving in along cycloidal paths
>towards the planet's surface, their energy being partially absorbed to
>drive the Earth's motions, as the wave impulses slow down to near the
>Earth's speed of rotary motion.

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>the previous two models of the stationary aether (undragged and
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>10 km/sec could be real.

Whatever theoretical difficulties we may have, Miller factually measured such a speed, as did Michelson-Pease-Pearson, and Kennedy-Thorndike. All were eventually dismissed as irrelevant, either due to static ether assumptions, or to temperature effects. I'd argue all the dismissals were premature, as the models being used were incomplete, or wrong. In any case, we know well, Einstein's SR demanded an empty space (his "ether" was a pure abstraction, with no real essence), and so, Miller had to die. As did Reich.

There's another set of factors at work here which we have not discussed (the subject of a paper I'm working on) which has to do with latitude dependence and atmospheric motion. You touch upon it with the concept of "atmospheric and terrestrial absorption of the impulses".

More later, a short review of your papers.

Best wishes,
James

****

Subject: Re: Letter
Date: Thu, 24 May 2001 00:54:10 -0500
From: Correa&Correa
To: J. DeMeo

Dear James -

Thank you for your kind letter, and your good advice which comes at the encounter of our hearts. We have no suggestions of any alterations. Only an observation on one of your suggestions - it is not possible to employ the same battery pack at once to drive the discharge and receive the output, because the cell chemistry is simply refractory to the simultaneous presence of two opposing reactions. If you recall, we succeeded in creating two single pack controllers (one was assembled with relays on the other large white table behind the PAGD-driven spinner, and the other was the one that Paulo brought out in the palm of his hand, which employed an inductive coupling) and have demonstrated this basic chemical fact to our heart's content. The only way therefore is to substantially delay or divert the output impulses, say to a flywheel (as in the PAGD spinner demo), which then one can use to close the loop, with or without batteries.

> Along these lines, it might interest you
> to learn, that upon my return back to Greensprings, the neutron counter I
> had been charging in the orgone room yielded something approaching several
> thousand cpm (neutrons only) in a burst that lasted a full three minutes --
> something only classically observed inside a nuclear reactor, I imagine.

> The phenomenon would probably have continued for some additional minutes
> had I not gotten terribly curious and removed the probe from its hard
> plastic sphere of moderating material, whereupon the counts immediately
> ceased, and would not return again after re-insertion of the probe. Such
> is the nature of these marvelous orgone phenomena.

That must have been quite an explosion of activity! Very interesting.

We will respond separately to the other e-mails.

Warmest regards

Alexandra & Paulo

****

Subject: Re: Ether Papers
Date: Thu, 24 May 2001 18:41:28 -0400
From: Correa&Correa
To: J. DeMeo

Dear James -

Here follow some minor thoughts on your very strong defense of Miller.

> for instance, he should have taken control temperatures of
> >the room, walls and roof, which apparently he did not). Nowhere does
> >Miller seem to have controlled for this in a systematic fashion. Then,
> >it is not just the heating effect of the sun upon the atmosphere that
> >one should consider (even if it happened only once - can we prove
> >that?), but equally the cooling effect of nighttime. We have seen these
> >effects in ORACs and can easily suppose that they would affect such a
> >sensitive interferometer as Miller's.
>
> Miller did record temperatures of every wall, using tenth-degree
> thermometers, at the start and finish of each run of around 20
> interferometer turns, which would fill one data sheet (see the example on
> p.8 of my paper). You can see the notations on the sheets.

They are illegible, so I did not realize it. However, it appears that he did not do so for floors, ceiling or roof temps, which would be more informative.

> As I point out, the most obvious way to evaluate for a thermal effect would have
> been to separate

> the daytime runs from the nighttime runs, and show a strong southerly
> (solar heat) component. This was not undertaken by them, or if so, not
> reported.

It seems to me in all fairness that this is at once an omission on the part of both Miller and Shankland et al.

> But in fact, we have no evidence that even a strong solar
> heating of a wall or the roof (which had a tent-cover) would have affected
> the experimental result

If the single instance you mention was a causal one - then one instance is evidence enough of some form of influence. Besides, since the sun itself does not transmit thermal radiation and assuming the thermal effect to be always a local one resulting from the conversion of accumulated aether energy and its interaction with solar radiation, heat will evolve where this accumulation is the highest irrespective of any thermal diffusion path. If the materials and arrangement of the interferometer facilitated that accumulation within itself, one may not legitimately conclude from room readings that there were no diurnal and weather-dependent thermal effects.

> -- recall Miller's control experiments at the
> basement laboratory at Case School in Cleveland. He developed insulation
> coverings for both the iron support and light-beam path. From there, he
> showed it was possible to skew the results by placing strong parabolic
> radiant heaters in the room, aimed at the interferometer, to create a 0.07
> fringe shift -- that was reported by Shankland as taken from Miller's lab
> book. No such intensive heat source was present at the Mount Wilson
> interferometer house, and I believe my speculation of a shift of only 0.007
> fringe is reasonable (see p.13).

It may or may not be - without knowing the power and distance of his radiant heaters, the difference in room temperatures with and without them, the outside temperature, the time of day and so on, I have no feel for it.

> That places thermal distortion in
> Miller's apparatus below the observational threshold, as he recorded the
> values only in tenths to hundredths of a fringe, with roundings only in
> hundredths. From my knowledge, Miller is the only one to have addressed
> the question of temperature effects on the interferometer through
> experimental measures. As such, I think the critics have to do more than
> merely raise a speculative objection, and run some experiments of their
> own, on an apparatus similar in construction and insulation to Millers, to
> prove a thermal artifact.

That is a tall order, James. One might equally put forth another tall order, that Miller alone had the responsibility to demonstrate exhaustively that thermal or other radiative influences were not at work, with the proper and systematic controls.

> Another point here, mentioned by Miller many
> times, is that virtually all of the past interferometer experiments,
> including M-M, showed similar azimuthal directions of ether drift -- were
> all those experiments suffering from unknown but identical thermal
> distortions?

Long ago I looked at many residuals and tables and I am not aware of this, at all. Maybe you might educate me - and you will undoubtedly have to select residuals on some objective basis that one will need to validate, no?

> > More disturbing still, to my way of thinking, is that the data Miller
> >obtained - with his final and improved interferometer - yielded two very
> >different reports of the direction of the aether drift: in 1926 he
> >reported in Science (63:436) that the absolute motion of the earth was
> >towards the head of Draco, RA 17h, North Dec. +65°, in the Northern
> >Celestial Hemisphere, whereas by 1933, the motion was towards Dorado in
> >the Southern celestial Hemisphere, nearly 180° off. Yet, the value of
> >the drift remained at ca 200 km/sec, and the drag lowered it to the same
> >9 or 10 km/sec at the ca 1.8 km altitude.
>
> As I understand it, the interferometer can only yield a result which
> indicates a line of ether-drift, without value as to which direction along
> the line the drift is occurring. Imagine again the example of the swimmers
> going up/down or back/across the current, but where you cannot see the
> water, and the only information you have is their final time of arrival
> back at the point of origin. The interpretation of the direction along the
> line is made by undertaking logical arguments based upon other kinds of
> observations, such as stellar aberration, etc. Likewise, his calculation
> of the final drift speed of c.200 km/s -- all that information was
> extrapolated and inferred from the basic set of observations which showed a
> ether-drift of 10 km/s along a line between Draco and Dorado. He obviously
> changed his mind at some point between 1926 and 1933, and I think it had
> something to do with various interpretations of ether-drag.

The problem is as you put it, but Miller's speed and direction also do not agree with the more recent measurements made with respect to the CBR (for instance, and as possibly still the most reliable one, Smoot et al give a direction that is nearly normal to the Draco-Dorado line).

> I only fault Miller for not
> expressing his findings and calculations more explicitly, so others could
> follow his work more precisely, but the same is true for Michelson-Morley
> and other ether experimenters where a lot of technical detail is scattered
> across many different papers. Too bad he never wrote a book on this
> subject, to gather all the information in one source.

I completely agree with you.

> > Lastly come a series of considerations about what exactly did Miller
> >thought he was detecting. One could approach this by systematizing the
> >alternative aether models-
> > 1. If the aether were a static fabric of space, and the earth did not
> >entrain it, the MM experiment should have measured the translatory
> >motions of the earth, whether solar or galactic, or both. As it did

> >not, the hypothesis of a non-entrained stationary aether could be ruled
> >out.
>
> Yes, agreed. But the MM experiment did yield a small positive result.

Yes it (they) did, but the residuals could never support a stationary aether model.

> > 2. If the 'inertial motion' of the earth entrains a stationary aether
> >to create an aetherosphere - thus dragging the aether along - the
> >relative velocity between the ether and the earth may be zero (if the
> >aetherosphere was a fixed skin) or very small (with the aether lagging
> >behind the earth's movement of rotation, since the latter entrains it).
> >If it were zero, then a negative result to the MM experiment should also
> >be expected. And if it were a small lag (necessarily referenced to
> >rotation, given that a drag referenced to translation would have to
> >yield a lag only when the interferometry experiments were conducted
> >during daytime), it would also fit with a nearly null result, yet it
> >would directly contradict the West to East motion detected by the
> >Sagnac-type experiments - and require precisely a reverse lag (an
> >apparent aetherospheric motion from East to West).
>
> I'm a bit lost on your question here. The Sagnac experiment was table-top,
> and yielded no information about earth movements, only that the rotational
> velocity of the interferometer could be added to or subtracted from the
> velocity of light moving around the rotating platform.

I wrote Sagnac-type experiments because, as was later referenced in the text, I am referring to planetarian (open loop) Sagnac experiments that have established the West to East difference in propagation of electromagnetic signals, not to Sagnac's own 1913 experiment.

> The
> Michelson-Gale-Pearson experiment was able to determine earth rotation, but
> again without information regarding the direction (W-->E or W<--E). If my
> memory has failed here, please correct me.

That is so, within the ambiguity of the actual values, for the MGP expt. But one may employ the results of planetarian open-loop Sagnacs to measure the rotation of the aetherosphere and determine whether it advances over the rotation of the Earth or instead lags behind it.

> In any case, there is no reason
> to believe that a positive result on Miller's experiment would necessitate
> a negative result on either Sagnac or MGP. Those latter experiments were
> not designed to detect any component of drift other than within the
> rotational plane of the earth.

As I see it, this is the crux of the matter. If Miller was detecting an Aether Drift involving some (ultimate) translational component of the Earth's motion, and if this drift motion was distorted because, above all, the rotation of the Earth through an aether entrained the latter and dragged down its speed past the Earth, then by any drag model this implies a slip of the

aether at the surface of the rotating body. Hence Miller's idea of conducting the experiment at altitude. But since the experiment never indicated the full slip to be expected, even definite residuals are unlikely to permit adequate conclusions.

A slip of the aether at the surface of the Earth should translate either into a fixed aetherosphere at or near the surface, or into a slip, with apparent E to W motion that could explain the residuals. Therefore any test for altered propagation in the rotational plane of the Earth (planetarian Sagnac and MGP) should be able to confirm either the absence of any alteration or an apparent E to W motion, and that is not the case. To my understanding, this means that what has been effectively ruled out from all these various experiments is precisely the notion that, in moving through Space, the Earth entrains the aether.

Were the speed of the drifting aether dragged down by the rotary motion of the Earth, one should expect that one might observe a cosmic variation when the light path is at 90° to the path of the earth's orbit around the sun, over a suitably long period of observations, just as Miller did. However, confronted with his results, one is hard put to see how a body rotating with surface speeds no greater than 0.46 km/sec would slow down a drift of 200 km/sec to ca 10 km/sec at 1.8 km altitude.

> > The two preceding alternative models are based on the notion that the
> >rotary and translatory motions of the Earth are givens that cannot be
> >directly explained by any form of coupling to an aether which is seen as
> >stationary. In the second model - that of entrainment or dragging of
> >the aether - the earth is construed to move 'like a rotating ball on
> >stagnant water', as W. Reich put it.
>
> Again, I am not fully following your point #2 above, but Reich's model has
> significant differences from the standard "dragged ether" idea, the former
> having the ether in motion as prime mover, the latter having the earth push
> through a relatively stagnant but draggable ether.

This is what I was getting to. Point #2 just suggested that it does not seem possible to hold onto an entrained aether model and at the same time hold onto the view that, within the troposphere, there is an aether motion at 10 km/sec pointing to somewhere along the arc joining Draco and Dorado. For this aether motion in any aether drag model would have to represent a slip with respect to the earth's rotation at such low altitudes, and be, for all purposes, nearly parallel to the surface.

> > Now, what to me is confusing with Miller's notion of an aether drift is
> >that, at the end of the day, it appears to have nothing in common with
> >the aether drag (rotary or translatory) models - since it suggests that
> >his measurements consist of a detection of a cosmological aether drift
> >that carries the Earth along.
>
> The suggestion is there, but I never read anything where Miller viewed the
> ether as in motion, pushing the planets along on their path. For awhile, I
> assumed he had to believe this, but now I'm certain he never believed this.
> He viewed the earth as pushing through the medium, not being moved by it.

That is also what I think he thought.

> Which is why he ran into confusions about the earth moving towards Draco,
> and then Dorado. For along time, I made the error to say that Miller had
> claimed discovery of a "dynamic ether", which is not the case. He did not
> attribute dynamic properties to the ether -- only Reich did this.

I agree, but I draw your attention to what Aspden wrote, for instance in Modern Aether Science, regarding the dynamic aether, even if his conception is different from that of Reich's.

> But it argues it detects this
> >'translational' drift at altitude, as a much slower velocity of the
> >aether due to an aether drag model of the aetherosphere (otherwise the
> >displacement fringes would be substantial).
>
> Yes, faster at higher altitudes, a kind of "laminar flow" model, but where
> the earth pushes through the stuff. Factually, this is impossible as A) it
> requires some small interaction of ether with matter and yet B) such an

> interact would eventually bring the planetary motions to a standstill, or
> at least to a speed of movement equal to the ether. Consequently, if
> ether-matter interaction occurs, ether has to move, has to be the prime
> mover.

This precisely what I have been suggesting -

> > As you well know, there is another way to construe an aether model that
> >fits both the null result of the MM-type experiments and the results of
> >the Sagnac-type experiments:
> > 3. In this model, it would not be the Earth that would entrain a
> >stationary aether, but instead a consistent motion of the aether that
> >would propel forward the Earth, the Solar System and even the entire
> >Galaxy or the Local Group. To again employ Reich's words, 'the analogy
> >is that of a ball rolling on water waves more slowly than the waves'.
> >There would still be an aetherosphere, created not by dragging a
> >stationary aether, but by a consistent aether spin (the result of the
> >superimposition of multiple such spins, at a cosmic, galactic, solar and
> >planetarian levels) propelling at once both the rotary and translatory
> >motions of the Earth. Outside of the aetherosphere, a much faster
> >aether flux should be detectable, but the aether impulses would impart
> >angular momentum to the planet by curving in along cycloidal paths
> >towards the planet's surface, their energy being partially absorbed to
> >drive the Earth's motions, as the wave impulses slow down to near the
> >Earth's speed of rotary motion.
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> Yes! It appears we have the same conclusions. Please note, however, that
> my paper on Miller makes only passing reference to this matter. I have a
> second paper on this subject in preparation. The goal of the first Miller

> paper was to ressurect the basic findings of Miller from the trash heap.
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> maybe one or two earth radii. This is apparent in the IMAGE satellite
> movies, showing the "magnetosphere" (which has a tail pointing towards the
> sun, not away from it).

I honestly do not see why you have to postulate this entrainment of the aether by the Earth. If you agree with model #3, all it demands is that it is the Earth which is being moved or entrained by the aether, the laminar flows and main shear zone(s) arising from the flux slowing down as a function of imparting angular momentum to the planet.

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> >resolution approached measurement of that slightly faster rotation of
> >the aetherosphere, on the order of 100 or so m/sec faster than the local
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> >the nearly-perpetual motions of the planet - motions which, therefore,
> >are not treated as simply 'given'.
>
> Yes. Absolutely. They view gravitational forces as fundamentally
> disconnected from ether. Miller broke free of the static ether model,
> which was his breakthrough, but only by a little bit.

If his approach falls within #2, then it seems he attempted to hold onto a revised stationary aether model.

> He viewed the ether as something like a fluid jelly, capable of being entrained
> by planets
> moving through it, but as far as I can tell, not in movement by itself.
> (ergo Newton "nothing moves unless something else makes it move -- the
> ether must be static to preserve God's role").
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> >satellite afloat in a trajectory parallel to the earth would not be any
> >faster than the variable speed (0.01 to 0.1 km/sec) of the jet stream
> >with respect to the Earth.
>
> I'd be interested to learn more about this, but urge you to review the
> IMAGE satellite pics. They suggest a linked movement of space and
> earth-surface even at fairly high altitudes, of one or two earth radii.

How does that depart from the toroidal disposition of the Van Allen belts that are said to structure the magnetosphere? In cross section, these belts can be shown to display a forward plume pointing towards the sun at 1.5 to 5 earth radii.

> >Note also that it is along the ridges and
> >troughs of the jet stream that cyclonic and anticyclonic systems couple
> >themselves, much as eddy currents counter-couple themselves on the

> >surface of a drag-cup. A suitable approximation would be ca 0.5 km/sec
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> Reich.
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> (the subject of a paper I'm working on) which has to do with latitude
> dependence and atmospheric motion. You touch upon it with the concept of
> "atmospheric and terrestrial absorption of the impulses".
>
> More later, a short review of your papers.
>
> Best wishes,
> James

I very much look forward to read your second installment!

****

Subject: Re: Our Plans
Date: Thu, 24 May 2001 00:53:48 -0500
From: Correa&Correa
To: J. DeMeo

Dear James -

Your letter was an unexpected and graceful offer, and we can all understand any reluctance you might have, at this stage of our relation, in having it posted publicly, especially when you do not yet know the exact content of our theoretical work. Since you mentioned that we 'could use it in any manner we wished', our thought was to have it appear side by side with letters from others who have witnessed the same demos or phenomena, like Mallove, Aspden or Soudak. The other reason is that we really believe that we shall never find the 'right' sponsor for these technologies, even though there always seems to be one or two cooking in the stove, to speak. We say this, just to explain to you what was our mindframe.

We will comply with your wishes however and will be discrete and only show it if necessary or advantageous to an open minded potential sponsor, with a sense of humour. We will also undertake to let you know who has read it under these conditions.

We are also glad that you and Gene share similar views on what to do. We will write more tomorrow, on the Miller and so on.

Warmest regards

Alexandra & Paulo

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Subject: Your ether papers
Date: Thu, 24 May 2001 20:17:00 -0700
From: J. DeMeo
To: Correa&Correa <Correa&Correa>

Dear Paulo and Alexandra,

As promised here is some feedback on the two papers you provided me with upon departure. First, I must say, I find myself agreeing with most of what you write, with my comments only on relatively small points. Most of these points are given so as to make your paper more bullet-proof from the unsympathetic critic. And a few points related to my review of the Miller materials.

A. Consequences of the Null Result of the Michelson Morley Experiment

1. On page four - please provide a citation for the Einstein quote.

2. On page four - second paragraph, you say that the MM experiment was "thereafter repeated and improved upon, again and again, by Michelson and Morley over a period of nearly 40 years..." To my knowledge, Morley worked with Michelson on the famous interferometer experiment of 1887, but never again. He quickly formed an alliance with

Miller to revisit the problem sometime around 1904, and Morley-Miller published one or two papers together on the ether-drift question before Morley finally retired. In fact, it was only Miller who stuck to the issue with such tenacity as to qualify for the "nearly 40 years" identification. Michelson only undertook the basic ether *drift* problem later on, as described in a short paper by Michelson, Pease & Pearson as cited in my "Fresh Look" paper on Miller. Michelson did approach the Sagnac question, of earth-rotation, in Michelson-Gale-Pearson, but this was not the typical MM-type experiment.

3. On the title page, as well as at the top of page 9 and in other places, you use the word "null" to describe the results of the MM experiments. This should be softened somewhat, as the emphasis upon the words "null" and "zero" have been mainly the contribution of the Einstein enthusiasts and ether-critics. Factually, the MM experiment, and most other MM-type experiments, never yielded a "null" result, a point about which Miller was emphatic, specifically as regarding his own experimental work. However, it is true that the low result, or small result of MM, did undermine the concept of a stationary ether. Again, Morley-Miller, Miller, and Michelson-Pease-Pearson, as well as Kennedy-Thorndike, all got positive results even greater than MM, but these were widely dismissed by the advocates of the static ether theories demanded by the relativists. Maybe to change the word "null" to "small" in most places, and identify the interpretation of this as "null" by those prejudiced towards static ether and relativity?

4. On page 34, in the paragraph just below the equations, you state:
"Einstein... in admitting a compatibility between Relativity and a theory of the dynamic ether..." This should read "theory of a static ether", yes? Einstein was emphatic that his ether (a mathematical abstraction) could not move and could not have any tangible properties. Or maybe you refer to some other paper Einstein wrote that I don't know about? Please inform me, as my recollection of his paper "Ether and Relativity" was emphatic about the static nature of ether. Actually, you acknowledge this yourself on p.3 of the second paper (below).

B. The Sagnac and Michelson-Gale-Pearson Experiments

5. On page 35, bottom of second paragraph, you claim no detection has been made of ether-drifting. I hope you will revise this, or at least add a footnote, in light of the materials gathered in my paper.

That's it! You've got a lot of very good information in both these papers, and I found them educational to read. Hope these points are helpful.

Best wishes,

James

****

Subject: Re: Your ether papers

Date: Sat, 26 May 2001 14:56:59 -0400
From: Correa&Correa
To: J. DeMeo

Dear James-

Thank you for your constructive and helpful comments. Follow a few notes.

A. Consequences of the Null Result of the Michelson Morley Experiment

>
> 1. On page four - please provide a citation for the Einstein quote.

This was the Pasadena speech given by Einstein at a banquet in his honour - and the occasion was Michelson's last public appearance. You find it quoted in several 1931 obituaries of Michelson and is also quoted in Jaffe's poor biography of Michelson.

2. On page four - second paragraph, you say that the MM experiment was

> "thereafter repeated and improved upon, again and again, by Michelson and
> Morley over a period of nearly 40 years..." To my knowledge, Morley worked
> with Michelson on the famous interferometer experiment of 1887, but never
> again. He quickly formed an alliance with Miller to revisit the problem
> sometime around 1904, and Morley-Miller published one or two papers
> together on the ether-drift question before Morley finally retired. In
> fact, it was only Miller who stuck to the issue with such tenacity as to
> qualify for the "nearly 40 years" identification. Michelson only undertook
> the basic ether *drift* problem later on, as described in a short paper by
> Michelson, Pease & Pearson as cited in my "Fresh Look" paper on Miller.
> Michelson did approach the Sagnac question, of earth-rotation, in
> Michelson-Gale-Pearson, but this was not the typical MM-type experiment.

Michelson by himself published other papers on the MM expt in 1897, 1904 and in 1929 (with Pease and Pearson), filling that 40 year period. Morley only worked with Michelson, as you say, in the 1887 paper, and then with Miller in two other papers, in 1905 and 1907. Miller's four papers range from 1922 to 1933. We will change the sentence to read:

"thereafter repeated and improved upon, again and again, by Michelson, Morley, Miller and others over a period of nearly 40 years...

> 3. On the title page, as well as at the top of page 9 and in other places,
> you use the word "null" to describe the results of the MM experiments.
> This should be softened somewhat, as the emphasis upon the words "null" and
> "zero" have been mainly the contribution of the Einstein enthusiasts and
> ether-critics.

That is indeed the historical case, but to us it seems that one can accept the null result and not necessarily fall into the category of adepts of relativity. Examples are Reich's theory and

Aspden's. Our own theoretical work is perfectly satisfied with a null result. Indeed, one of the lines throughout the papers is that the null result is no impediment to an adequate theory of the aether.

> Factually, the MM experiment, and most other MM-type
> experiments, never yielded a "null" result, a point about which Miller was
> emphatic, specifically as regarding his own experimental work.

It seems to us that at or near the limits of resolution of diverse interferometer apparatuses, residual values may well be shown to have little or no significance. Miller's later work is an altogether different story, as you eloquently have pointed out.

> However,
> it is true that the low result, or small result of MM, did undermine the
> concept of a stationary ether. Again, Morley-Miller, Miller, and
> Michelson-Pease-Pearson, as well as Kennedy-Thorndike, all got positive
> results even greater than MM, but these were widely dismissed by the
> advocates of the static ether theories demanded by the relativists.

The question then becomes: can you arrange these (and any other) positive residuals so that they present a consistent picture?

> Maybe
> to change the word "null" to "small" in most places, and identify the
> interpretation of this as "null" by those prejudiced towards static ether
> and relativity?

We are afraid that here we will have to agree to disagree. Obviously to this day there is no ultimate word on whether the residuals are of import or not. And the problem is made even more complicated by the more recent results of Silvertooth's experiments. Our bias is, in this respect, to assert that, if light is a local phenomenon and the aether envelope rotates with the earth slightly faster than the earth, then both a null result for MM-type experiments and the positive results of Sagnac-type and MGP experiments can be explained. That is the core suggestion of our papers. We could be wrong, of course, but draw your attention once more to the fact that Reich held similar views about the meaninglessness of these residuals: "if 'light' is due to local orgone lumination and does not 'travel through space' at all, it is quite understandable that in the Michelson experiment no phase difference could be observed (...)".

> 4. On page 34, in the paragraph just below the equations, you state:
> "Einstein... in admitting a compatibility between Relativity and a theory
> of the dynamic ether..." This should read "theory of a static ether", yes?

No, SR was never compatible with the stationary electromagnetic aether hypothesis. The context on page 34 refers not to SR/GR but to the notion of a ZPE - which anyway is sort of a never, never land: it resurrected the idea that the aether is electromagnetic in nature (which SR and GR rejected) but endowed with mobility (hence it introduces dynamic characteristics unlike Lorentz's aether). So that this comes through, we should simply subtract the 'dynamic aether' expression to have it read:

".. in admitting a compatibility between Relativity and a theory of a minimum continuous moving field..."

> Einstein was emphatic that his ether (a mathematical abstraction) could not
> move and could not have any tangible properties. Or maybe you refer to
> some other paper Einstein wrote that I don't know about? Please inform me,
> as my recollection of his paper "Ether and Relativity" was emphatic about
> the static nature of ether. Actually, you acknowledge this yourself on p.3
> of the second paper (below).
>

Einstein's argument (in Ether and Relativity) is as you say, that Lorentz had left only one mechanical property to the electromagnetic static aether, that of immobility. As Einstein saw it, SR had to abstract this quality itself from Lorentz's aether and transpose it to the constancy of the speed of light, while at the same time "be on guard against ascribing a state of motion to the [Lorentzian] ether". This was so as to lay the basis of GR as a theory based, not on Lorentz's, but paradoxically on Einstein's conception of "Mach's ether" (!), where it is suggested that the real aether is gravitational. Yet, this aether too is reduced to a fixed geometry, "devoid of all mechanical and kinematical properties", as the idea of motion may not be applied to it. Much later, this contention too would change somewhat, with the notion of a spin to Spacetime, but would retain the principle that the 'ether of Space' is devoid of kinetic activity.

> B. The Sagnac and Michelson-Gale-Pearson Experiments
>
> 5. On page 35, bottom of second paragraph, you claim no detection has been
> made of ether-drifting. I hope you will revise this, or at least add a
> footnote, in light of the materials gathered in my paper.

We can certainly insert a parenthesis reading:
...detectable (excepting the controversial claims of D. Miller, as is argued and documented by J. DeMeo's recent paper reappraising the value of Miller's work [reference to your paper]).

Let us know your thoughts on all of the above. We are very glad you enjoyed the two papers.

Warmest regards

Alexandra & Paulo

****

Subject: Re: Your ether papers
Date: Sat, 26 May 2001 14:20:52 -0700

From: J. DeMeo
To: Correa&Correa

Dear Paulo,

Again, a quick reply to a few of your replies.

>It seems to us that at or near the limits of resolution of diverse
>interferometer apparatuses, residual values may well be shown to have little
>or no significance. Miller's later work is an altogether different story, as
>you eloquently have pointed out.
>

The question then is, what is the "limit of resolution"? The interpretation of the variation answers this question. Static ether means small results are insignificant. Temperature argument means small results are insignificant. But in both of these cases, only assumptions are made, without empirical testing or independent lines of evidence to back them up. Entrained or dynamic ether means small results may be significant. And Miller's control experiments suggest, for his insulated apparatus, shifts approximating 0.07 fringe can only be produced by strong thermal heat sources, and consequently are spurious objections.

>> However,
>> it is true that the low result, or small result of MM, did undermine the
>> concept of a stationary ether. Again, Morley-Miller, Miller, and
>> Michelson-Pease-Pearson, as well as Kennedy-Thorndike, all got positive
>> results even greater than MM, but these were widely dismissed by the
>> advocates of the static ether theories demanded by the relativists.
>
>The question then becomes: can you arrange these (and any other) positive
>residuals so that they present a consistent picture?

Miller addresses this in his 1933 paper, showing that the original MM results, and the Morley-Miller results yield consistent sidereal azimuthal readings.

>> Maybe
>> to change the word "null" to "small" in most places, and identify the
>> interpretation of this as "null" by those prejudiced towards static ether
>> and relativity?
>
>We are afraid that here we will have to agree to disagree. Obviously to this
>day there is no ultimate word on whether the residuals are of import or not.
>And the problem is made even more complicated by the more recent results of
>Silvertooth's experiments. Our bias is, in this respect, to assert that, if
>light is a local phenomenon and the aether envelope rotates with the earth
>slightly faster than the earth, then both a null result for MM-type
>experiments and the positive results of Sagnac-type and MGP experiments can be
>explained. That is the core suggestion of our papers. We could be wrong, of
>course, but draw your attention once more to the fact that Reich held similar

>views about the meaninglessness of these residuals: "if 'light' is due to
>local orgone lumination and does not 'travel through space' at all, it is
>quite understandable that in the Michelson experiment no phase difference
>could be observed (...)".

OK, but keep aware that "null" means nothing. The question for me is, does the use of this word flow from reasonable interpretations of the results, or from theoretical necessity. The former is a good reason assuming it can be supported logically, the latter not so good.

>> 4. On page 34, in the paragraph just below the equations, you state:
>> "Einstein... in admitting a compatibility between Relativity and a theory
>> of the dynamic ether..." This should read "theory of a static ether", yes?
>
>No, SR was never compatible with the stationary electromagnetic aether
>hypothesis. The context on page 34 refers not to SR/GR but to the notion of a
>ZPE - which anyway is sort of a never, never land: it resurrected the idea
>that the aether is electromagnetic in nature (which SR and GR rejected) but
>endowed with mobility (hence it introduces dynamic characteristics unlike
>Lorentz's aether). So that this comes through, we should simply subtract the
>'dynamic aether' expression to have it read:
>
>".. in admitting a compatibility between Relativity and a theory of a minimum
>continuous moving field..."
>
>> Einstein was emphatic that his ether (a mathematical abstraction) could not
>> move and could not have any tangible properties. Or maybe you refer to
>> some other paper Einstein wrote that I don't know about? Please inform me,
>> as my recollection of his paper "Ether and Relativity" was emphatic about
>> the static nature of ether. Actually, you acknowledge this yourself on p.3
>> of the second paper (below).
>>
>
> Einstein's argument (in Ether and Relativity) is as you say, that Lorentz
>had left only one mechanical property to the electromagnetic static aether,
>that of immobility. As Einstein saw it, SR had to abstract this quality
>itself from Lorentz's aether and transpose it to the constancy of the speed of
>light, while at the same time "be on guard against ascribing a state of motion
>to the [Lorentzian] ether". This was so as to lay the basis of GR as a theory
>based, not on Lorentz's, but paradoxically on Einstein's conception of "Mach's
>ether" (!), where it is suggested that the real aether is gravitational. Yet,
>this aether too is reduced to a fixed geometry, "devoid of all mechanical and
>kinematical properties", as the idea of motion may not be applied to it. Much
>later, this contention too would change somewhat, with the notion of a spin to
>Spacetime, but would retain the principle that the 'ether of Space' is devoid
>of kinetic activity.

Well, my ignorance of the fine points of Einstein's theory is showing here. But the concept of immobility is what he was emphatic about. Sometime we should discuss the issue of Newton's emphasis upon this same point, a theological premise.

>> B. The Sagnac and Michelson-Gale-Pearson Experiments
>>
>> 5. On page 35, bottom of second paragraph, you claim no detection has been
>> made of ether-drifting. I hope you will revise this, or at least add a
>> footnote, in light of the materials gathered in my paper.
>
>We can certainly insert a parenthesis reading:
>...detectable (excepting the controversial claims of D. Miller, as is argued
>and documented by J. DeMeo's recent paper reappraising the value of Miller's
>work [reference to your paper]).
>
>Let us know your thoughts on all of the above. We are very glad you enjoyed
>the two papers.
>
>Warmest regards
>
>Alexandra & Paulo

You are doing good work here, and I think if we can challenge each other's ideas to find the weak points, out of it will come a very clear resolution. I also again raise the issue, of a meeting or maybe even a conference on the ether, where we could present and give and receive critical review on the spot, giving drawings and models for clarity's sake.

Warm regards,

James

****

Subject: Re: Ether Papers
Date: Sat, 26 May 2001 14:20:44 -0700
From: J. DeMeo
To: Correa&Correa

Dear Paulo and Alexandra,

Lots of work to do before I depart to New York next week, so I must keep my answers on the short side. Thanks much for the thoughtful replies. I think, it would be valuable if we could meet again in the future to have a working meeting on this issue of ether and ether-drift, how it specifically relates to Reich's discovery and theory, and to classical astrophysics.

For myself, I need drawings and even 3-dimensional models to keep my thinking clear, and you are now among the few persons who are both sufficiently educated in both Reich and classical ether findings, as to ask penetrating and important questions. I feel there probably will be some ambiguity and uncertainty raised by giving Miller a serious consideration, but this is the nature of research. My role in the matter was basically to have noted the political manner in which his findings were dismissed, and to expose that set of facts. Actually, I

never considered myself a physicist, and yet I now grapple with questions central to that discipline.

Specifically regarding the temperature issue, I still must refer you to the available material from Miller's control experiments in the basement at Case School, undertaken prior to his final Mount Wilson experiments. Shankland had the original lab books, and repeated Miller's statement that a 0.07 fringe shift was observed when a radiant heater was aimed at one of the cross-arms of the insulated interferometer. If there had been anything in Miller's records more damning, I'm sure Shankland would have mentioned that as well. In fact, the only other issue raised was the one data sheet where the "sun shown on interferometer", probably the only time when the shading tent was not properly installed and caught them off-guard.

Factually, there was no heat source in Miller's actual experiments comparable to that radiant heater. Let's review the facts here. My own estimate is that heat variations under intense solar heating of the outside of one of the walls (which by no means is a proven matter -- I simply assume this for the sake of argument) *might* have produced a distortion in the readings of perhaps 1/10th the amount noted with the radiant heater. Remember, the interferometer was being turned at the rate of 1 rotation per second, which means one of the four arms would come into proximity of a hypothetical solar-heated wall every 15 seconds, and this source would then produce a hypothetical artifact of around 0.007 shift. Again, since his readings were made of tenths to maximum of hundredth's of a fringe, such a distortion would not be detectable. And we have no assurance even this small shift would have occurred, or if it occurred, that it would have been systematic through all seasons and hours of the day.

I would extend this argument and point to the Michelson-Pease-Pearson third experiment undertaken in the dome of Mt. Wilson, where Michelson constructed an interferometer far more sensitive than his original one with MM. In that Mt. Wilson experiment, MPP simply declared that "temperature factors were addressed". Nobody challenges that statement, or the result obtained because, while they got a reading of around 10 km/s (in close agreement with Miller) they continued to assume a static ether and therefore dismissed it as inconsequential.

The person who did the very most practical and experimental investigation of the effects of temperature on an interferometer was Dayton Miller. None of his critics, to my knowledge, undertook experiments to show, for example, that temperature X produces effect Y under conditions Z. Miller did, and therefore controlled for those factors, as he did for altitude and building materials. It is from this perspective that one must appreciate his somewhat irritated comments to the critics who constantly raised the temperature argument, but who never personally investigated the matter. They raised this argument purely because, it was necessary that Miller's work be swept aside for the sake of Einstein. And Shankland resurrected this argument in a most distorted manner. So I would encourage you, to not make the same temperature argument on the basis of a theoretical necessity -- if something substantial can be found in this regard, that is fine. Unfortunately, none of the original data are available to allow an independent checking.

One also has to appreciate, Miller may have undertaken even more stringent temperature controls, maybe even the southerly-daytime approach I mention, but simply never published it. After all, when he was alive, the temperature arguemnts were not raised very strongly

against him in any published articles -- I think Joos was the only one -- but mostly by rumor campaigns. Other than a paragraph or two in his published papers, his control experiments at Case School, which occupied him for several years, were never published.

Then there is the issue of the systematic nature of the findings: Winter, Summer, Spring, Fall, daytime, nighttime, all pointing to the same general sidereal azimuth. And what do we make of Miller's claim, that the original MM experiment, as well as that of Morley-Miller, all showed similar sidereal azimuths, though of lower magnitudes? And the various papers cited by Miller, and now also by Allias, showing astrophysical phenomena with similar azimuths or axes? Added to the above, I think it makes the temperature argument only a theoretical speculation, far weaker in nature than the systematic patterns in Miller's data which cannot be explained by temperature. My feeling is, all these materials noted by Miller, with the newer findings of Allias and from classical astrophysics, needs to be evaluated for common elements and astro-geographies. There are a plethora of facts and observations which need to be plotted on cosmic maps -- something which I started doing back in the 1970s, but abandoned until only recently due to other work matters -- before any firm conclusions can be drawn. But I think we can both agree, out of this procedure of extracting empirical facts from the dismissals of the relativists and big-bangers, a new cosmology will emerge for which Reich ideas will be largely validated.

I must educate myself more on the Sagnac-type experiments before I can fully understand some of your arguments, and so will obtain the papers you cite over the next weeks. Maybe then I can have some "Ah Ha" experiences, and engage that aspect with more detail.

Well, this was a *moderately* short reply.

Best wishes,
James

****

Subject: Re: Ether Papers
Date: Sun, 27 May 2001 20:51:23 -0400
From: Correa&Correa
To: J. DeMeo

Dear James -

We will keep it short, in light of your NY trip and given that we too are quite busy preparing to leave - hopefully, at present, with all the repeated postponements,. this will be on the week of June 11th. [...]

We too prefer to visualize the astrophysical problems raised by the interferometry experiments. Our first volume of aetherometry ("Aether wars in the XXth century") goes into the matter in much greater detail, with several maps and illustrations - and has separate sections for Tesla, Miller, Reich, Silvertooth, Aspden and others. The papers on Einstein were taken from two chapters of this book.
You raise an interesting problem with the MPP experiment - but our examination turned out so many other problems with it, many of which Michelson himself acknowledged - even before the final runs, that it is another can of worms. Of course ambiguity, uncertainty and even confusion are bound to arise from any re-evaluation of Miller - since, at the end of the

day, the subject matter is always the same: what is the import of those residuals - and can one find a pattern to them, as Miller claimed one could or should. From past discussions, we had been led to conclude that it is far more difficult to create such a pattern with the residuals of various light speed experiments performed by other methods after the death of Miller (the maser, laser, CBR, Silvertooth, etc experiments).

> So I would encourage you, to not
> make the same temperature argument on the basis of a theoretical necessity
> -- if something substantial can be found in this regard, that is fine.
> Unfortunately, none of the original data are available to allow an
> independent checking.

You should notice that we have not made any such argument. The reason is simple: what we deem is critically incomplete and we cannot assess by repeating the experimental set-up, we tend to avoid. This rule has been cardinal to our process of discovery. The truth is that we have no set position vis a vis Miller. We neither feel we need to defend him, nor that we need to attack him and take the defense of Shankland et al. You see, for the nature of the measurement that Michelson sought, there was no need to be concerned with thermal and other influences, since the magnitude of any residuals could never resurrect the stationary aether hypothesis. But in the case of Miller, what he proposed to do and find required, to our mind, still more stringent controls than those he actually and factually undertook. Add to this disappearing notebooks, faulty analysis on both sides of the fence, an incomplete public record, and what one gets is a stew. Such stews are, of course, part of the history of science - but without experimental involvement, it is impossible to find one's way out of them. Furthermore, since they are staring at one in the middle of the road, so to speak, one has to address them one way or the other, which is what we think you courageously did, trying to sort out what you think is or is not valid in the work and the criticism, and placing the entire affair in its historical context.

An aside - when you say Allias, you mean Maurice Allais? If so, it has been two decades since we looked at his work - but we remember that he identified precisely a diurnal variation in his pendulum studies (exquisitely analogous to the diurnal oscillation of atmospheric parameters in our ORAC studies) and proceeded to conclude that he could exclude all sorts of parameter correlations such that the phenomenon required one to assume the action of a new energy field. But the fact is that he never controlled for simple parameters, such as humidity, intrinsic potential energy of the atmosphere, atmospheric electricity, and a few others that, as we have shown you in the drafts of the OR/Aether motor, are critical. You refer to the newer findings of Allais - would you summarize them for us (has he considered these other parameters since his earlier papers?)? We just checked your Miller paper and found the last reference - it must be the same fellow.

> But I think we can both agree, out of this procedure of extracting
> empirical facts from the dismissals of the relativists and big-bangers, a
> new cosmology will emerge for which Reich ideas will be largely validated.

Eventually this might happen - though we would not count on seeing it in our lifetimes. In any event, it is always better to be surprised.

We wish you a good talk in NY and a good event at the SSE. Let us know how both went.

Warmest regards from all of us

Alexandra & Paulo

****

Subject: Re: Your ether papers
Date: Sun, 27 May 2001 22:10:43 -0400
From: Correa&Correa
To: J. DeMeo
References: 1 , 2

Dear James,

Just a fast note

> OK, but keep aware that "null" means nothing. The question for me is, does
> the use of this word flow from reasonable interpretations of the results,
> or from theoretical necessity. The former is a good reason assuming it can
> be supported logically, the latter not so good.

I fully agree - so much so, that the question in the subject matter at hand is almost entirely one of how reasonable one is in interpreting the residuals as meaningless or as meaningful. Theoretical necessity is not what I think was of concern either to Reich or to Aspden when they accepted the residuals as reasonable nulls. It certainly is not a necessity at present for our theoretical work, even if I think my own reasonable interpretation biases me towards the null result. Now, however, consider another angle - if one assumes that the earth is driven by aether energy impulses that slow down to nearly the earth's speed at the surface, then one is forced theoretically to assume that the MM experiment must yield a null result. So, in my mind, it is a fine line between the two faces of the edge. Personally, I think that the more important attitude is to remain open to listen even to those who are rabid about their theories and one already knows they are wrong because, if one knows how to listen and pry them open, I find one always learns something new that might well change this or that critical detail in one's own thought. But I agree with you that theoretical consistency should never be taken as a substitute for the scientific process.

> Sometime we
> should discuss the issue of Newton's emphasis upon this same point, a
> theological premise.

Yes, one might say that all the misunderstandings regarding inertia stem from Newton's theological premise that the motion of planets is inertial (always the notion of an original impulse). I think that there is much Einstein had to teach us - and that is why Reich sought him also - even if his theoretical edifice will one day be proven to be a sham. I learned from Einstein that when all was said and done with GR, he himself was forced to conclude that there never were, are or will be any such things as inertial systems. That, of course, did not make him any smarter - all the less as he died shortly thereafter.

>
> You are doing good work here, and I think if we can challenge each other's
> ideas to find the weak points, out of it will come a very clear resolution.

It is always joyful to engage you and find our strong, indifferent and weak links. We should keep it up on any issue of common interest - and according to our schedules.

> I also again raise the issue, of a meeting or maybe even a conference on
> the ether, where we could present and give and receive critical review on
> the spot, giving drawings and models for clarity's sake.

[...] But a meeting with you is always welcome - even for no reason at all. Let us plan on that sometime soon.

Warm regards

Paulo

PS - Have you read/perused the Clastres book yet?

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Subject: Re: Ether Papers
Date: Wed, 30 May 2001 14:26:48 -0700
From: J. DeMeo
To: Correa&Correa
References: 1 , 2

Dear Paulo and Alexandra,

[...]
I'd be happy to secure the other 2/3's of my energy via your PAGD or the antenna-ground system you've worked to propell the OR motor.

>We too prefer to visualize the astrophysical problems raised by the
>interferometry experiments. Our first volume of aetherometry ("Aether wars in
>the XXth century") goes into the matter in much greater detail, with several
>maps and illustrations - and has separate sections for Tesla, Miller, Reich,
>Silvertooth, Aspden and others. The papers on Einstein were taken from two
>chapters of this book.

Sounds fascinating. After our meeting, I decided to read Aspeden again, and will take his books on the airplane.

>You raise an interesting problem with the MPP experiment - but our examination
>turned out so many other problems with it, many of which Michelson himself
>acknowledged - even before the final runs, that it is another can of worms.
>Of course ambiguity, uncertainty and even confusion are bound to arise from
>any re-evaluation of Miller - since, at the end of the day, the subject matter
>is always the same: what is the import of those residuals - and can one find a

>pattern to them, as Miller claimed one could or should. From past
>discussions, we had been led to conclude that it is far more difficult to
>create such a pattern with the residuals of various light speed experiments
>performed by other methods after the death of Miller (the maser, laser, CBR,
>Silvertooth, etc experiments).

I've not made a review of so many of these, but wonder if the problems of low-altitude, basement locations, etc., which Miller emphasized, might be at work in their studies.

>> So I would encourage you, to not
>> make the same temperature argument on the basis of a theoretical necessity
>> -- if something substantial can be found in this regard, that is fine.
>> Unfortunately, none of the original data are available to allow an
>> independent checking.
>
>You should notice that we have not made any such argument. The reason is
>simple: what we deem is critically incomplete and we cannot assess by
>repeating the experimental set-up, we tend to avoid. This rule has been
>cardinal to our process of discovery. The truth is that we have no set
>position vis a vis Miller. We neither feel we need to defend him, nor that we
>need to attack him and take the defense of Shankland et al. You see, for the
>nature of the measurement that Michelson sought, there was no need to be
>concerned with thermal and other influences, since the magnitude of any
>residuals could never resurrect the stationary aether hypothesis. But in the
>case of Miller, what he proposed to do and find required, to our mind, still
>more stringent controls than those he actually and factually undertook. Add
>to this disappearing notebooks, faulty analysis on both sides of the fence, an
>incomplete public record, and what one gets is a stew. Such stews are, of
>course, part of the history of science - but without experimental involvement,
>it is impossible to find one's way out of them. Furthermore, since they are
>staring at one in the middle of the road, so to speak, one has to address them
>one way or the other, which is what we think you courageously did, trying to
>sort out what you think is or is not valid in the work and the criticism, and
>placing the entire affair in its historical context.

This clarifies your position greatly, and I can appreciate your viewpoint much better for it. Yes, there is much truth to what you say, about the presence of ambiguity making it difficult to embrace a given set of conclusions. What set me on the path to dig deep into Miller's work was the parallel between his observation that dense building materials and metal shields blocking the ether-drift, to Reich's work on the accumulator, that orgone's "reflectibility" or "attraction and quick repulsion" might be the product of a functional identity between ether and orgone. What I will consider to be a more telling proof of Miller's work, however, is the identification of his measured drift-axis in the astrophysical observations of other scientists. My sense of this is, however, that psychological defense mechanisms in orthodox science lead to a suppression of findings which go in this direction. Like a priest who is against anything too lively or pleasurable, knowing it all leads to sexual pleasure.

>An aside - when you say Allias, you mean Maurice Allais? If so, it has been

>two decades since we looked at his work - but we remember that he identified
>precisely a diurnal variation in his pendulum studies (exquisitely analogous
>to the diurnal oscillation of atmospheric parameters in our ORAC studies) and
>proceeded to conclude that he could exclude all sorts of parameter
>correlations such that the phenomenon required one to assume the action of a
>new energy field. But the fact is that he never controlled for simple
>parameters, such as humidity, intrinsic potential energy of the atmosphere,
>atmospheric electricity, and a few others that, as we have shown you in the
>drafts of the OR/Aether motor, are critical. You refer to the newer findings
>of Allais - would you summarize them for us (has he considered these other
>parameters since his earlier papers?)? We just checked your Miller paper and
>found the last reference - it must be the same fellow.

Yes. I'm afraid I chronically mis-spell his name. His latest findings are the presence of anomalous perturbations of pendulum behavior during solar eclipses. Since most of it is in the French language I cannot get into the fine details, but he claims the findings of Miller are in agreement with the force that perturbs his pendulums.

>> But I think we can both agree, out of this procedure of extracting
>> empirical facts from the dismissals of the relativists and big-bangers, a
>> new cosmology will emerge for which Reich ideas will be largely validated.
>
>Eventually this might happen - though we would not count on seeing it in our
>lifetimes. In any event, it is always better to be surprised.

One wishes science could change as quickly as European politics did when the Berlin Wall came down. Perhaps thats possible with discoveries as unquestionable as the airplane, but surely would be similarly so regarding "free" energy devices that really work.

>We wish you a good talk in NY and a good event at the SSE. Let us know how
>both went.

Thanks very much.

All the best,

James

Subject: OBRL New Article on Reich and To-T Experiment in Infinite Energy Mag.
Date: Wed, 30 May 2001 14:44:27 -0700
From: J. DeMeo
Reply-To: obrl-news@lists.village.virginia.edu

To: *OBRL_News <obrl-news@lists.village.virginia.edu>

From: OBRL-News <J. DeMeo>
To: obrl-news@lists.village.Virginia.EDU
Subject:- New Article on Reich and To-T Experiment in Infinite Energy Mag.

Orgone Biophysical Research Lab <J. DeMeo>
http://www.orgonelab.org
Forwarded News Item

Please copy and distribute to other interested individuals and groups

****

Fascinating New Article on Reich and To-T Experiment in Infinite Energy Magazine

"The Reproducible Thermal Anomaly of the Reich-Einstein Experiment Under limit Conditions" by Dr. Paulo Correa and Alexandra Correa Infinite Energy, Issue #37, 2001.

The article revisits the Einstein Affair from an experimental viewpoint, and provides an excellent methodology (derived from Reich's original approach, but with important additions) for reproducible To-T demonstrations.

The editor of Infinite Energy, Dr. Eugene Mallove, has taken an interest in Reich's work, and will have several articles supporting Reich's discoveries, and critical also of Einstein's special relativity theory, in the next several issues.

An article by James DeMeo: "Dayton Miller's Ether-Drift Experiments: AFresh Look" will also appear in the next issue of Infinite Energy. It is a significant revision and extension of what currently exists at the OBRL web site. http://www.orgonelab.org/miller.htm

Now would be a good time to subscribe, if you want all these new materials.
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Fax. 603-224-5975
http://www.infinite-energy.com
Subject: Your Paper on To-T
Date: Fri, 1 Jun 2001 10:32:00 -0700
From: J. DeMeo
To: Correa&Correa

Dear Paulo and Alexandra,

Yesterday I received the latest issue of Infinite Energy, which contained your paper on To-T measurement. A very good protocol and results, and I'm glad Eugene is now making the connections between the cold-fusion thermal anomaly and Reich's work. As per Eugene's editorial, it was an interesting psychological phenomenon, that Leon Jaroff (science editor of Time Magazine and former Stalinist and CSICOP founder) put Pons, Fleischman and Reich all on the same page, though Reich's larger discovery holds much more import.

A question for you regarding the issue of thermal IR radiation, which may be addressed in your second paper. Years ago when I was engaged in solar energy and conservation design, everyone was advocating use of insulating materials lined with aluminum foil -- they still do -- on the basis that thermal IR frequencies are "reflected" by the metal foil. Particularly in Florida where I lived at the time, lining the underside of the roof with such foil-backed insulation created a cooling effect below the metal layer, and a heating effect above it. The contractors and product literature claimed the thermal energy was "held above" the metal layer above the roof line. A nearly identical insulated roof without the metal layer would allow the IR energy to radiate through the materials of the roof construction into the home, creating a slightly warmer home environment. The point of this may already be obvious for the To-T measurements made above the accumulator in daytime, that re-radiated solar IR might be considered the source of at least some of the heat at the top of an accumulator. Of course, in a deep basement location, and at nighttime, this argument would not hold, but I wanted to run this idea past you, as you seem to follow a line of my own thinking, that there is some deep relationship between the "radiated" component of heat energy and orgone, or orgonotic excitation as Reich might call it. Since none of my homes or labs have ever had a suitably shielded basement location, given the above consideration I always made my to-t inside the top of the accumulator, which yielded a very small effect I never felt happy about, ans do never pursued the matter. But from this experience, I today question if a 1 - 2 degree temperature difference above an accumulator can appear as the consequence of any warming inside the accumulator which is then conducted upward above the accumulator, especially if the upper interior temperature is maybe only 0.1 - 0.3 deg to begin with. It seems to me, if the temperature effect above the accumulator is truly a real phenomenon, then it must be due to some kind of energetic expression originating above the accumulator, and not inside it. This consideration is partly what led many persons in the USA to make their measurements inside the accumulator instead of on top of it. I think you might get some critique from a physicist or two on this issue, in which case the reference to nighttime measurements, and the basement location, would counter it nicely.

Also, I wonder if you have seen the paper on To-T by Hanspeter Seiler, which used suspended oracs and open-air thermometers, roughly similar to Reich's original experiment, except that everything was conducted outdoors under a shaded structure, with a second series of measurements with glass shields surrounding the experiment (each series lasted about a year). He used fairly small single-ply accumulators (around10 cm) to lower heat capacity and allow for rapid response to environmental temperature changes, and obtained pretty good results of around constant 1-2 deg. C., without so much of the commonly-reported negative readings sometimes attributed to dor conditions. (H. Seiler: "New Experiments in Thermal Orgonometry", Journal of Orgonomy, 16(2):197-206, Nov. 1982.)

The paper suffers from an incomplete presentation of the measurements, but is otherwise very much in agreement with your own findings -- of a fairly constant positive To-T. I also mention this because, one quotation attributed to you on p.2 of Infinite Energy dismissed all prior efforts, which I think is too sweeping. I also have little respect for persons who behave like a "Reichian devotee" or "disciple", while trying to maintain the attitude of a scientist -- and we both know such people -- but quite a lot of work confirming various aspects of Reich's findings has been undertaken and published in the years since his death, in both Europe and the USA.

Again, an excellent paper, and my congratulations. I will probably change our demonstration To-T apparatus here to more closely match your own protocol, for evaluation and demonstration to students this summer. Since I don't have a basement location, however, varying environmental temperatures will be be a greater problem. The lab building is constructed for passive solar heating, which creates a diurnal variation even in summer. So I may have to take it outdoors under the trees, in some variation between your approach and that of Seiler. Interesting how these same questions of temperature keep popping up, for ether-drift, cold fusion, and orgone energy.

Best wishes,
James

cc: Eugene Malove

****

Subject: Re: Your Paper on To-T
Date: Fri, 01 Jun 2001 18:36:05 -0500
From: Correa&Correa
To: J. DeMeo

Dear James -

It made us very happy to know that you enjoyed the paper on the RE expt. And we agree wholeheartedly - Reich's body of work greatly extravasates the scope of cold fusion, but your comment on Jaroff brought home the full import of Gene's counterpoint.

> A question for you regarding the issue of thermal IR radiation, which may
> be addressed in your second paper.

This second paper will be the lengthy monograph #5 at our website.

> Years ago when I was engaged in solar
> energy and conservation design, everyone was advocating use of insulating
> materials lined with aluminum foil -- they still do -- on the basis that
> thermal IR frequencies are "reflected" by the metal foil.

Yes, they still do, and we see the material regularly up here at various hardware stores.

> Particularly in

> Florida where I lived at the time, lining the underside of the roof with
> such foil-backed insulation created a cooling effect below the metal layer,
> and a heating effect above it. The contractors and product literature
> claimed the thermal energy was "held above" the metal layer above the roof
> line. A nearly identical insulated roof without the metal layer would
> allow the IR energy to radiate through the materials of the roof
> construction into the home, creating a slightly warmer home environment.
> The point of this may already be obvious for the To-T measurements made
> above the accumulator in daytime, that re-radiated solar IR might be
> considered the source of at least some of the heat at the top of an
> accumulator. Of course, in a deep basement location, and at nighttime,
> this argument would not hold, but I wanted to run this idea past you, as
> you seem to follow a line of my own thinking, that there is some deep
> relationship between the "radiated" component of heat energy and orgone, or
> orgonotic excitation as Reich might call it.

In that monograph #5, we present a novel method (we want to surprise you a bit) that permits identification of where, within the ORACs, the heat evolves. Generalizing from our results, we found that most of the photon heat (IR heat) evolves on the outer layers, at the dielectric interfaces.

> Since none of my homes or
> labs have ever had a suitably shielded basement location, given the above
> consideration I always made my to-t inside the top of the accumulator,
> which yielded a very small effect I never felt happy about, ans do never
> pursued the matter.

Yes, we also started, decades back (1978 to be exact), in making measurements in this way and found also a diminished effect that could easily be made negative if one were not careful to pay attention to all sorts of location and environmental factors (including the distance from the inner walls and top). Reich, as we point out in this monograph #5 (which you will soon be able to read at our website - we hope!), was also far from clear as to the different significance and levels of the readings done inside and above the ORAC.

> But from this experience, I today question if a 1 - 2
> degree temperature difference above an accumulator can appear as the
> consequence of any warming inside the accumulator which is then conducted
> upward above the accumulator, especially if the upper interior temperature
> is maybe only 0.1 - 0.3 deg to begin with. It seems to me, if the
> temperature effect above the accumulator is truly a real phenomenon, then
> it must be due to some kind of energetic expression originating above the
> accumulator, and not inside it. This consideration is partly what led many
> persons in the USA to make their measurements inside the accumulator
> instead of on top of it.

This is very interesting indeed, since Reich was also tricky (both intentionally and unwittingly) on this matter. In that monograph #5 we also find another novel methodological way to separate the heat evolved inside, above and outside (at the outer surface) the ORAC: which is why we employed white and black ORACs with dielectric

interfaces throughout painted in inverted 'colors'. In fact, one should not make one's measurements inside _instead_ of above the top - as you say, that being the way that most other investigators have done - but _both_ in and above, and as well out, and do so in different devices that permit the controls we will discuss in that #5 monograph. What we found is that, whereas most photon heat evolves in the outer layers, at dielectric interfaces, there is an irreducible quantity of heat that can be stringently shown to evolve inside the metal box alone - and which we can trace as it slowly exits to the outer chamber and outer layers.

This, however, is a related phenomenon, though different from what happens above the top: here, across the top plate and above it, is where we believe (from other experiments in much later papers) that the flux of latent heat, not sensible heat, is the strongest, sensible heat evolving in that space above the metal cage by transformation from the latent heat flux that 'wants' to escape from the top.

> I think you might get some critique from a
> physicist or two on this issue, in which case the reference to nighttime
> measurements, and the basement location, would counter it nicely.

Indeed. And wait until they see what is in that monograph #5.

> Also, I wonder if you have seen the paper on To-T by Hanspeter Seiler,
> which used suspended oracs and open-air thermometers, roughly similar to
> Reich's original experiment, except that everything was conducted outdoors
> under a shaded structure, with a second series of measurements with glass
> shields surrounding the experiment (each series lasted about a year). He
> used fairly small single-ply accumulators (around10 cm) to lower heat
> capacity and allow for rapid response to environmental temperature changes,
> and obtained pretty good results of around constant 1-2 deg. C., without so
> much of the commonly-reported negative readings sometimes attributed to dor
> conditions. (H. Seiler: "New Experiments in Thermal Orgonometry", Journal
> of Orgonomy, 16(2):197-206, Nov. 1982.)

We perused it a long time ago indeed. But, as far as we were concerned, we wanted to replicate as much as possible the RE expt, with the same kind of box volume and structure that Reich himself had indicated, and with the effect being noted (or not) in the naked metal cage. And we wanted to do it indoors under disadvantageous conditions, with a simple control that addressed Infeld's objections. Moreover, the use of glass is very problematic: its heat insulation properties impose a tremendous delay in response and it easily acquires positive electric charge (the reason why in general glass electroscopes are so poor at holding negative charge). What we wanted was really a stringent, no-frills approach to the basic phenomenon, as Reich put it to Einstein.

> The paper suffers from an
> incomplete presentation of the measurements, but is otherwise very much in
> agreement with your own findings -- of a fairly constant positive To-T. I
> also mention this because, one quotation attributed to you on p.2 of
> Infinite Energy dismissed all prior efforts, which I think is too sweeping.

We thought you might. But we hold onto our view that, with respect to the RE expt, defined as we do as a function of the actual events and the need for a simple, incontrovertible control, no strict repetition - to our knowledge - has ever been undertaken.

> I also have little respect for persons who behave like a "Reichian devotee"
> or "disciple", while trying to maintain the attitude of a scientist -- and
> we both know such people -- but quite a lot of work confirming various
> aspects of Reich's findings has been undertaken and published in the years
> since his death, in both Europe and the USA.

That was not the intention of our statement, which qualifies the repetition of the RE expt as we defined it - and Gene was careful here to keep it in context.

> Again, an excellent paper, and my congratulations. I will probably change
> our demonstration To-T apparatus here to more closely match your own
> protocol, for evaluation and demonstration to students this summer. Since
> I don't have a basement location, however, varying environmental
> temperatures will be be a greater problem. The lab building is constructed
> for passive solar heating, which creates a diurnal variation even in
> summer. So I may have to take it outdoors under the trees, in some
> variation between your approach and that of Seiler. Interesting how these
> same questions of temperature keep popping up, for ether-drift, cold
> fusion, and orgone energy.

Excellent. Keep in mind that this report in IE is just the tip of the iceberg with respect to the question of heat inside the ORAC, as far as the results we are about to present are concerned.

Thank you for this thoughtful response.

Best wishes to you too

Alexandra & Paulo

cc: Eugene Mallove

****

Subject: Re: Ether Papers
Date: Fri, 01 Jun 2001 16:46:31 -0400
From: Correa&Correa
To: J. DeMeo

Dear James -

[...]

> >You raise an interesting problem with the MPP experiment - but our examination
> >turned out so many other problems with it, many of which Michelson himself
> >acknowledged - even before the final runs, that it is another can of worms.
> >Of course ambiguity, uncertainty and even confusion are bound to arise from
> >any re-evaluation of Miller - since, at the end of the day, the subject matter
> >is always the same: what is the import of those residuals - and can one find a
> >pattern to them, as Miller claimed one could or should. From past
> >discussions, we had been led to conclude that it is far more difficult to
> >create such a pattern with the residuals of various light speed experiments
> >performed by other methods after the death of Miller (the maser, laser, CBR,
> >Silvertooth, etc experiments).
>
> I've not made a review of so many of these, but wonder if the problems of
> low-altitude, basement locations, etc., which Miller emphasized, might be
> at work in their studies.

That is one possibility - Aspden raises still another set of possibilities in Phys Essays, vol 3:1:p. 39, going as far as arguing that MM-type experiments and laser and ring laser gyro experiments suffer from a standing wave condition that entrains standing wave energy. He argues that it is possible that only Silvertooth bypassed this problem. Above we were of course thinking of those experiments performed at altitude, preferably aboard satellites, balloons and airplanes, with lasers or masers, or even better, with modified Dicke radiometers (like Smoot et al).

> >> So I would encourage you, to not
> >> make the same temperature argument on the basis of a theoretical necessity
> >> -- if something substantial can be found in this regard, that is fine.
> >> Unfortunately, none of the original data are available to allow an
> >> independent checking.
> >
> >You should notice that we have not made any such argument. The reason is
> >simple: what we deem is critically incomplete and we cannot assess by
> >repeating the experimental set-up, we tend to avoid. This rule has been
> >cardinal to our process of discovery. The truth is that we have no set
> >position vis a vis Miller. We neither feel we need to defend him, nor that we
> >need to attack him and take the defense of Shankland et al. You see, for the
> >nature of the measurement that Michelson sought, there was no need to be
> >concerned with thermal and other influences, since the magnitude of any
> >residuals could never resurrect the stationary aether hypothesis. But in the
> >case of Miller, what he proposed to do and find required, to our mind, still
> >more stringent controls than those he actually and factually undertook. Add
> >to this disappearing notebooks, faulty analysis on both sides of the fence, an
> >incomplete public record, and what one gets is a stew. Such stews are, of
> >course, part of the history of science - but without experimental involvement,
> >it is impossible to find one's way out of them. Furthermore, since they are

> >staring at one in the middle of the road, so to speak, one has to address them
> >one way or the other, which is what we think you courageously did, trying to
> >sort out what you think is or is not valid in the work and the criticism, and
> >placing the entire affair in its historical context.
>
> This clarifies your position greatly, and I can appreciate your viewpoint
> much better for it. Yes, there is much truth to what you say, about the
> presence of ambiguity making it difficult to embrace a given set of
> conclusions. What set me on the path to dig deep into Miller's work was
> the parallel between his observation that dense building materials and
> metal shields blocking the ether-drift, to Reich's work on the accumulator,
> that orgone's "reflectibility" or "attraction and quick repulsion" might be
> the product of a functional identity between ether and orgone. What I will
> consider to be a more telling proof of Miller's work, however, is the
> identification of his measured drift-axis in the astrophysical observations
> of other scientists.

Yes, precisely.

> My sense of this is, however, that psychological
> defense mechanisms in orthodox science lead to a suppression of findings
> which go in this direction. Like a priest who is against anything too
> lively or pleasurable, knowing it all leads to sexual pleasure.
>
> >An aside - when you say Allias, you mean Maurice Allais? If so, it has been
> >two decades since we looked at his work - but we remember that he identified
> >precisely a diurnal variation in his pendulum studies (exquisitely analogous
> >to the diurnal oscillation of atmospheric parameters in our ORAC studies) and
> >proceeded to conclude that he could exclude all sorts of parameter
> >correlations such that the phenomenon required one to assume the action of a
> >new energy field. But the fact is that he never controlled for simple
> >parameters, such as humidity, intrinsic potential energy of the atmosphere,
> >atmospheric electricity, and a few others that, as we have shown you in the
> >drafts of the OR/Aether motor, are critical. You refer to the newer findings
> >of Allais - would you summarize them for us (has he considered these other
> >parameters since his earlier papers?)? We just checked your Miller paper and
> >found the last reference - it must be the same fellow.
>
> Yes. I'm afraid I chronically mis-spell his name. His latest findings are
> the presence of anomalous perturbations of pendulum behavior during solar
> eclipses. Since most of it is in the French language I cannot get into the
> fine details, but he claims the findings of Miller are in agreement with
> the force that perturbs his pendulums.

In the earlier papers he did not bring this up. We will see whether we can get a hold of his latest book.

We also want to thank you very much for the mention you made of our recent IE paper and the upcoming ones, in your OBRL list. We were all very pleasantly surprised.

Warmest regards
Alexandra & Paulo

****

Subject: Thank you!
Date: Wed, 06 Jun 2001 00:39:23 -0500
From: Correa&Correa
To: J. DeMeo
References: 1

Dear James -

We know from your silence that you are on the road, deep in preparation. We wish you the best and want to thank you very much for your gift of Saharasia (which we are looking forward to read), the dedicatory you included and the hard copy of your letter - even if this note will only find you after you return to Oregon.

We would love to hear how your trip and lecture went.

Warmest regards and a big hug from all of us -

Alexandra and Paulo

****

Subject: Re: Thank you!
Date: Sun, 10 Jun 2001 23:05:01 -0700
From: J. DeMeo
To: Correa&Correa

Dear Alexandra and Paulo

I return today from a week of travel, and 180 emails! So I will be brief.

[...] The SSE, however, was a very valuable and worthwhile conference. So many new discoveries on the cutting edge, that even my presentation on cloudbusting was greeted openly.
Eugene Mallove was there, distributing copies of his latest issue, so many persons asked me about your article which I took as a very good sign.

Thanks much for the email hug, which I send back three times.

best wishes,
James
Subject: Welcome back!
Date: Mon, 11 Jun 2001 19:44:43 -0500
From: Correa&Correa
To: J. DeMeo

Dear James -

Welcome back!

[...]

> The Psychohistory conference was somewhat interesting -- I doubt if I shall
> present to that group again, as they appear quite lost in the
> psychoanalysis Reich left behind in the 1930s.

However backward, one could still see that as a possible jumping board to approach your work. We guess that the ossification is simply too extreme.

> The SSE, however, was a
> very valuable and worthwhile conference. So many new discoveries on the
> cutting edge, that even my presentation on cloudbusting was greeted openly.
> Eugene Mallove was there, distributing copies of his latest issue, so many
> persons asked me about your article which I took as a very good sign.

Excellent - these are all people that we most likely do not know or have never met. Were they, for the most part, familiar with the subject matter?

[...] We have been rather busy preparing for two upcoming meetings, as well as with our work on the website and preparing to leave. Soon we will be able to give you access at the site.

[...]

There's no need to reply other than at your leisure.

Warmest regards from all of us -

Alexandra & Paulo

****

Subject: Re: Welcome back!
Date: Tue, 12 Jun 2001 11:34:18 -0700
From: J. DeMeo
To: Correa&Correa

>> The SSE, however, was a
>> very valuable and worthwhile conference. So many new discoveries on the
>> cutting edge, that even my presentation on cloudbusting was greeted openly.
>> Eugene Mallove was there, distributing copies of his latest issue, so many
>> persons asked me about your article which I took as a very good sign.
>
>Excellent - these are all people that we most likely do not know or have
>never met. Were they, for the most part, familiar with the subject
>matter?

I think mostly not, but many were appreciative of your article in any case, and it seems a larger opening up to Reich's ideas is taking place within this group. Dick Blasband also presented there, on treatment of cancer mice with the orgone accumulator, and his charts paralleled a paper presented just before of "laying of hands" treatment of cancer mice. Since the SSE has a lot of interest in this latter subject material, many parallels were made.

[...]

Best wishes,
James

****

Subject: Re: Welcome back!
Date: Fri, 15 Jun 2001 00:49:50 -0500
From: Correa&Correa
To: J. DeMeo

Dear James -

Sorry for the delay, but it has been hectic for us to find sometime to even get near the computer.

What you report sounds like a good meeting at SSE. We never met Blasband, but are aware of his studies dating back to the 70's - and very much appreciate his contention about testing the evolution of spontaneous tumors. How has his work progressed - any new approaches?

[...]

Eugene is writing an account of his experiences in his two visits to our lab, for our website (we are aiming to give you access on the week of the 25th of June). Is it alright with you if we show him a copy of your letter?

Warmest regards - from all of us

Alexandra & Paulo

****

Subject: Re: Welcome back!
Date: Fri, 15 Jun 2001 12:22:01 -0700
From: J. DeMeo
To: Correa&Correa

>Eugene is writing an account of his experiences in his two visits to our

>lab, for our website (we are aiming to give you access on the week of
>the 25th of June). Is it alright with you if we show him a copy of your
>letter?

Yes, and you can copy it and show it to anyone else you want. I was only a little hesitant about having it posted to the web site until I could see more of what you were going to do there. Probably, my hesitation is silly, but I tend to go into these things a bit slowly.

My solar panels are up -- 16 of them on a large platform, mounted 10' in the air on a heavy pole. Looks impressive. I mentioned to the man who runs the solar company [...] I might supplement them in some years with a free energy device, and without mentioning your name or any details, simply mentioned I had seen demonstration of a working device some weeks back. He lit up and said if such a device was available, he would start installing them using the existing network and renewable-energy hardware already available. I tell you this, as an example of how such apparatus could eventually be distributed and marketed, going around the big energy companies. The more I think about it, the more I get worried about your personal safety if this thing becomes public before you have a chance to secure the precise mechanism, either via patents or in the making of practical demonstration devices available as per Eugene's excellent idea. I don't think the big companies already invested in oil or coal would respond favorably, but perhaps a company like Simens, which made our solar panels?

I had a wild idea: If we could get such a demo apparatus up and working here, I'd be willing to host you for a special weekend seminar, to demonstrate the device and talk about it. I've a lot of contacts in the USA and Europe, and perhaps one of them -- upon getting an announcement -- might be able to open doors on the matter, especially if connected with your making demo units available for sale. First, however, would be to get one up and working, maybe to let it run for some weeks uninterrupted, so the demonstration would be an unquestionable thing.

Warm regards,
James

****

Subject: Re: Welcome back!
Date: Sun, 17 Jun 2001 14:34:36 -0500
From: Correa&Correa
To: J. DeMeo

Dear James -

Thank you for your very warm letter and honesty. And thank you for your permission to use your letter. We'll show it to Eugene as soon as we have a chance.
>I was only a
> little hesitant about having it posted to the web site until I could see
> more of what you were going to do there. Probably, my hesitation is silly,
> but I tend to go into these things a bit slowly.

On the contrary, your hesitation is well-intentioned and proof of your carefulness - and we ourselves have suggested it and appreciate it. The truth is that we suspect that there may be

much at our website that you will disagree with. We are not referring to experimental or theoretical criticisms of Reich - those exist also, but they are not there as a matter of belief but of science or experimentation; we think it more likely you may object to our analytical orientation. But you will judge these and other matters for yourself as soon as we have it ready for test trials.

We would like to restate that we invited you up here because we could not think of any other scientific researcher in the field of Orgone Physics that took Reich seriously and deserved to witness the event other than you. You have been kind to us, gracious, honest and most generous - we are very happy that we met you.

> My solar panels are up -- 16 of them on a large platform, mounted 10' in
> the air on a heavy pole. Looks impressive. I mentioned to the man who
> runs the solar company (Bob Maynard of Energy Outfitters) I might
> supplement them in some years with a free energy device, and without
> mentioning your name or any details, simply mentioned I had seen
> demonstration of a working device some weeks back. He lit up and said if
> such a device was available, he would start installing them using the
> existing network and renewable-energy hardware already available. I tell
> you this, as an example of how such apparatus could eventually be
> distributed and marketed, going around the big energy companies. The more
> I think about it, the more I get worried about your personal safety if this
> thing becomes public before you have a chance to secure the precise
> mechanism, either via patents or in the making of practical demonstration
> devices available as per Eugene's excellent idea. I don't think the big
> companies already invested in oil or coal would respond favorably, but
> perhaps a company like Simens, which made our solar panels?

We approached Siemens in Germany six years ago, in the context of the PAGD technology; they never responded to our three queries. To give you an adequate response to the problems you raise is difficult by letter, and risky via e-mail. We too are convinced that 'big companies' are not where we should be looking for support. We believe we have proven that to ourselves with the efforts at commercializing the PAGD. [...] We should apprise you that the Aether Motor is not, by any means, the key thrust of our website publication effort - and will not even concretely figure on a first tempo. It will only emerge, in its strict theoretical context, after we have laid the critical groundwork for understanding the principles of its operation. There are other even more fundamental surprises which we are preparing - one with respect to weight cancellation, and still others.

> I had a wild idea: If we could get such a demo apparatus up and working
> here, I'd be willing to host you for a special weekend seminar, to
> demonstrate the device and talk about it.

We would love to do this, but after those other landmarks we spoke of above have been crossed. It would be much easier to discuss these matters if we lived nearby.

> I've a lot of contacts in the
> USA and Europe, and perhaps one of them -- upon getting an announcement --
> might be able to open doors on the matter, especially if connected with

> your making demo units available for sale. First, however, would be to get
> one up and working, maybe to let it run for some weeks uninterrupted, so
> the demonstration would be an unquestionable thing.

Unfortunately, James, we know that this is the moment in our process where we are still in a position to give these demos over here. And for now, we are intent on not letting any of this technology get out from under us. Now is the time for interested parties to come on board, for we are about to make some very irreversible decisions regarding our own future that will preclude us getting involved in further business ventures. We really are at a critical crossroad.

If, however, we happen to secure the financing we seek, then as soon as a kit would be ready we would be very happy to give you one or more.

Warmest regards

Alexandra & Paulo

****

Subject: Q?
Date: Mon, 25 Jun 2001 20:02:42 -0700
From: J. DeMeo
To: Correa&Correa <Correa&Correa>

Dear Paulo and Alexandra,

I'm working more on Pulse #5, and recently revisited the article on the Orgone Motor which I gave you a copy of. After witnessing your own demonstration, I wanted to give you the opportunity to have something added to them -- in the manner of a postscript which you or I could write. There could be as much, or as little detail as you would desire. Perhaps, if you are going to make an internet posting of information on this question, we could include a note about that forthcoming information. Or we can not mention anything about it at all.

If you'd like to have a mention of your internet materials included, simply send me the web address.

Best wishes,

James

****

Subject: Re: Q?
Date:
Tue, 26 Jun 2001 01:31:30 -0500
From: Correa&Correa
To: J. DeMeo

Dear James-

It was good to hear from you. Here go some fast lines.

> I'm working more on Pulse #5, and recently revisited the article on the
> Orgone Motor which I gave you a copy of. After witnessing your own
> demonstration, I wanted to give you the opportunity to have something added
> to them -- in the manner of a postscript which you or I could write.

There are several ways in which one can go. First, you know we would love to write something together with you on the subject - but the timing for a complete unveiling is not yet now, and we think you will soon understand why we think so. Essentially, we plan to give you access to the files by the weekend - if we are lucky. We have been just a little delayed in readying ourselves. Then you can make your assessment of the situation and of what we are trying to accomplish.

However, we are certainly not opposed to have you write something on what you observed for the next Pulse, even in some detail, which we could review and suggest changes if any would be necessary. In fact, as we have discussed it over here, we appreciate your offer to do so - and if you want we can send you negatives of the shots we took while you were here (it seems like so long ago!). You might find some of them could be used. By the way, when are you intending to release this #5?

> There
> could be as much, or as little detail as you would desire.

We would be delighted to do this indeed, but just not so soon. There is much that we did not talk about - even if for no other reason than because our time was limited. But we would like to do this indeed, with you, in a time not so far from now.

> Perhaps, if you
> are going to make an internet posting of information on this question, we
> could include a note about that forthcoming information.

We would be happy to oblige you on this also - we have made a draft announcement of our website for Gene - do you want to see it and see if you want to use it, or would you prefer if we composed something else? [...]

> Or we can not
> mention anything about it at all.

We all think it is only fine that you do mention it - since you desire to do so. We do not know how far we want to push the envelope, but we all think that you know enough about us to know we will push it very far. We just did not want you to get involved with us openly in public, until you had had a chance to see what we were up to on the website. And this is just around the corner. So we leave it up to you - if you would like to write something about your experiences up here, we would be glad to review it.

[...]
Warmest regards

Alexandra & Paulo

****

Subject: Re: Q?
Date: Thu, 28 Jun 2001 17:36:28 -0500
From: Correa&Correa
To: J. DeMeo
References: 1 , 2

Dear James -

Time zero is approaching for us. We will send you the ad pdf file separately this evening. And we will send you the website password in a matter of days now. [...]

> Pulse #5 is crashing towards completion, and so I only have a bit of space
> available. Mostly I was thinking to give a bit of information, and then
> direct people to your own writings in Infinite Energy and on your web site.
> I think it may take another month, or even two, before I get all the
> typesetting done, so there's still time.

This is good - that there is still some time. Here is what we suggest - we think it is fine that you want to give a bit of news. We would still prefer if you wrote it - and then we could add to the structure and the idea you have, weave something to it if you want, or simply review it, as you prefer. And, if you want any of the pictures, let us know. Let us say, that we will revisit this in a couple of days after we give you access to our site, which should now be very, very soon indeed.

> The delay is my own articles
> which I continue to fine-tune and revise. A terrible habit, but necessary.

We know only too well what you mean. The worst thing is to see it published and still want to do that - the necessary revision that comes from having learned more or better since the time of writing.

Your view of medical doctors is the same as ours. But what saddens us even more is that in this case there is a whole other dimension of slow suicide, not so far from the notion that Artaud once proposed for the suicide of Van Gogh - that he was a 'suicided' of society. [X] wanted to die, such was the rage inside of him, and he chose this dead-end street with full cognizance of the outcome. It all happened in a scant two years from the first episode.

Warmest regards
Alexandra & Paulo
Subject: Re: Q?
Date: Thu, 28 Jun 2001 10:19:58 -0700
From: J. DeMeo
To: Correa&Correa

Dear Paulo and Alexandra,

Some fast lines in return...

Yes, please send the pdf file, and let me know when the actual materials will be up and available for review. [...]

Pulse #5 is crashing towards completion, and so I only have a bit of space available. Mostly I was thinking to give a bit of information, and then direct people to your own writings in Infinite Energy and on your web site.
I think it may take another month, or even two, before I get all the typesetting done, so there's still time. The delay is my own articles which I continue to fine-tune and revise. A terrible habit, but necessary.

Most bitter, these experiences with dying relatives and friends who adopt only the orthodox approaches. I view the modern medical profession like the arrogant beasts who wore powdered wigs and bled people to death, but too many still view them as gods. Very sad, but glad to hear of your own hurricane force. That's necessary to survive with life in this deadly world.

Best wishes,

James

****

Subject: IE Ad
Date: Thu, 28 Jun 2001 18:21:38 -0400
From: Correa&Correa
To: J. DeMeo

Dear James,

As promised, please find enclosed a copy of the pdf piece that will appear in the next edition of IE.

Warm regards,

Alexandra & Paulo

IE ADF.pdf
Name: IE ADF.pdf
Type: Portable Document Format (application/pdf)
Encoding: base64
Description: Unknown Document

****

Subject: Re: IE Ad
Date: Sun, 1 Jul 2001 16:23:46 -0700
From: J. DeMeo

To: Correa&Correa

Cool graphic. Let me know when its up and running.
Best wishes,
James

>Dear James,
>
>As promised, please find enclosed a copy of the pdf piece that will
>appear in the next edition of IE.
>
>Warm regards,
>
>Alexandra & Paulo
>
>Content-Type: application/pdf
> name="IE ADF.pdf"
>Content-Description: Unknown Document
>Attachment converted: IE ADF.pdf

****

Subject: ?
Date: Mon, 09 Jul 2001 12:25:53 -0400
From: Correa&Correa
To: J. DeMeo

...that bad, eh?

A&P

****

Subject: Re: ?
Date: Mon, 9 Jul 2001 13:10:30 -0700
From: J. DeMeo
To: Correa&Correa

>...that bad, eh?
>A&P

Dear Alexandra and Paulo,

My apologies for the silence... it had nothing to do with your materials. [...]

I did go to your web site, and printed out one the first of the article series to read -- The Use and Abuse of Physics -- but have not read it yet. I find I cannot read so well from the computer screen. The web site itself is very good, well organized and so on, and some of your mateirals (not requiring the password) I had seen previously at your other web site. Thank you very much for allowing me access to the materials, and my apologies for the delay in giving you this thanks.

I can tell you, after a brief scanning of the one paper, that you make reference to a lot of philosophical writings about which I am ignorant, and so may not be able to judge so easily. The later scientific figures you discuss, I have more familiarity with. In any case, give me some additional time to digest the material!

I've also finally ordered copies of the books you recommended, by Clastres, Anti-Oedipus, etc., and so will soon get a view of the inner mind of the Correas. Regarding my Saharasia, I assume that will also take a lot of time to digest, but I should warn you in advance, that an article now being finished ("Update on Saharasia: Ambiguities and Uncertainties Regarding 'War Before Civilization'") addresses some of the claims of intensive social violence prior to my marker date of c.4000 BCE. Much of that evidence is not confirmed in the archaeology, only in the interpretations by advocates of genetic determinism.

[...]

Best wishes,
James

****

Subject: Re: ?
Date: Tue, 10 Jul 2001 00:44:02 -0500
From: Correa&Correa
To: J. DeMeo

Dear James -

[...] you are right - there was no time obligation involved. We also thought you would have more fun with the other papers, since the philosophy ones are more sweeping. It is just that you got us wondering about your reaction, seeing all the pressing time factors involved.

We were thinking of sending you the Anti-Oe, but it seems you got ahead of us. We hope you enjoy it. However, you will find that, as usual, we disagree in much with Deleuze and Clastres - specifically about war and the State. But we will revisit that once you check those texts and we will have read Saharasia. We were also wondering whether to publish some of our work on the origins of war and the State, but finding time to do all this is the difficult matter.

Wishing you out of your current nightmare of number numbness soon!

Alexandra & Paulo
Subject: Re: Aetherometry web site
Date: Mon, 30 Jul 2001 10:26:30 -0700
From: J. DeMeo
To: Correa&Correa

Dear Paulo and Alexandra,

The first of the weekend seminars went very well, and I am now recovering from the exhausting long days for preparation and carrying it out. Dr. Grad's new work on cell-like

forms from basic chemical reactions was a highlight, and everyone was quite excited and satisfied. I'll have some additional hours for myself from now on, as the preparations for the bion seminar are always the most grueling.

Over the last several weeks, with help from several assistants, a lot of preparatory work and organization was done in and around the lab. I've constructed an open and airey outdoor to-t structure, under the trees, and two bare-metal accumulators for suspension underneath, with intention to measure inside, on top, and all around with mercury thermometers. Celotex shielding panels were placed around to eliminate stray sunlight, and for several days the temperatures inside stabilized to within 0.2 deg. C. between the front and back of the structure -- but the readings on top and bottom of the accumulator gave similar readings as the background environment. Only the interior-top of the accumulators gave higher readings, about 0.5 deg. higher than the background environment. I assume once the top thermometer is enclosed wtihin a container, it will also heat up, as will the overall effect when an insulating layer is added later on. At present, I have not measured them around the clock, to rule out thermal lag effects. Last night, it rained a bit, and the celotex started "melting", breaking two of the thermometers and requiring a dismantling of everything. I will rebuild it with weather-proof materials tomorrow, but the basic design is good, I feel, stimulated in large measure by your article.

Given my time constraints, I haven't been able to read through your materials with the speed I would like. Aside from the work the telephone is constantly ringing in reference to the seminars, with the students and assistants popping in and out, that I have to flee to the local restaurant and hide in the corner to have any quiet privacy. This AM, over a quiet breakfast, I plan to read your latest as published in IE, however. Your first paper in IE, and the Aetherometry web site, are the subject of some discussion among many persons, however, students and professionals in orgonomic work. I think you probably will get a few critical letters on methodology sent to Mallove. Nearly everyone acknowledged your basic points, however, about the need to revisit the experiment.

Good to see the orgonomic tree getting a good shake.

I'll communicate more with you, as I get the opportunity to read.

Warm regards,

James

>Dear James -
>
>How are the seminars on biogenesis going? Did you get a good attendance
>this year?
>
>We look forward, of course, to your review of our materials at your
>convenience - positive and negative will be welcome from you. Did you have
>any further thoughts on the Pulse of the Planet matter that you previously
>raised?

>
>Warmest regards
>
>Alexandra & Paulo

****

Subject: Re: Aetherometry web site
Date: Wed, 01 Aug 2001 01:21:53 -0400
From: Correa&Correa
To: J. DeMeo

Dear James -

It was good to hear from you again. We were particularly delighted that you are having fun with the intensive seminars on biopoiesis and Grad's new work on it - of which we are unaware. Maybe you might educate us on this, everything else permitting. This is most fascinating. Some time ago we finished all the announced papers, and we have been for the past four months concentrating on writing up our experiments on the very same topic. In fact, we are leaving this weekend for an open ended vacation of 2 to 4 weeks (at last!), and taking this material with us, together with Saharasia. We will do some diving and sunning and - in between - bounce between the two. [...]

Your letter, however, left us a bit worried that you are working too hard.

We thought about your outdoor experiments and concluded that you should read our S2-05 - since you have just about all that it takes to understand it. There we perform outdoor measurements both in the shade and in sunlight which are not reported in the paper that Gene published. Remember, this paper was solely a methodological paper (precisely what we think has been missing in the field) - trying to establish the phenomenon under the most disadvantageous and stringent conditions (indoors basement), with the right control to address Infeld's objection. Note that it claims nothing more. Also, it is important to realize that under these conditions the phenomenon needs the space above the ORAC where the thermometer is placed, to be insulated - otherwise, under the limiting conditions, the residuals become erratic. If one is picky, one can also suspend a thermometer at the same height and surround it with the same housing - it will not present the temperature difference, and is therefore also an excellent control. [...] - for us, it is only of importance that these matters attract the necessary attention and shake that ossified tree!

You can skip reading the latest paper in IE, since it is the same as the manuscript we gave you (save for corrections). We are stunned with the interest in the aetherometry web site - with so many readers from all parts of the globe. Incidentally, since we are leaving this weekend - in case you wanted us to compose any announcement for your OBRL list, we still have some time left to do that.

Warmest regards,

Alexandra & Paulo

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Subject: Re: Aetherometry web site

Date: Tue, 7 Aug 2001 01:36:17 -0700
From: J. DeMeo
To: Correa&Correa
1

Dear Paulo and Alexandra,

You will be on vacation by this time, while I just finished the second of three seminars. Another week and my time will again be my own. The students are deeply interested, and most of them interesting to know -- a few nuts or contracted people, but thankfully these are a minority. I hope your vacation will be refreshing.

Yes, the article in IE is the same one you gave me during my visit -- but as things turned out, my peaceful trip to the Cafe for reading the paper was accompanied by six other persons, so I was pursued with questions and no time to read anyhow. You will know what I mean.

Our accumulator shelter was reconstructed with plywood panels over celotex. The energetic feeling and thermal dynamics were changed a bit, but only slightly, so we will have another go at it next week. Once I understand fully the bare accumulators, the insulation and top chambers will be added, to increase the orgonotic effects. I have not yet read the item you suggest -- S2-05 -- but tried with great difficulty to download them tonight. Finally I succeeded and printed the 05 document on to-t, and hope to read it in the next days. A quick scan suggested many good points, but also a few question areas. Will communicate with you in maybe a week on it.

Also tried to download S2-01, 02, etc., but my computer continually gets hung up due to slow loading (rural telephone line) or printer complications (your photo covers are beautiful, but memory intensive). Is it possible I can simply pay you for a photocopy of the entire set of materials? I have a small budget for publication costs, and would be happy to purchase them from you, if this is possible. I find reading the printed copy the best way, as I cannot make margin notes on the computer, or sip a coffey so easily while reading from the computer.

Meanwhile, we have found that or-charged water yields a slightly higher surface tension. This was expected theoretically. The task now is to validate it with a larger number of samples, to determine how long the charge lasts after the water is removed from the orac, and how long is required to induce the change, and to what magnitudes.

I've just reviewed your full Table of Contents for the atherometry site, and am glad to see the materials on the orgone-aether motor "coming soon". Put me down for a copy of the videotape on "The Improved Aether Motor", when available. Also, what is the "Aetheroscope analytical DA system"?

Best wishes,

James

****

Subject: OML nonsense

Date: Fri, 17 Aug 2001 11:05:55 -0700
From: J. DeMeo
To: Correa&Correa

Dear Paulo, Alexandra and Malgosia,

Today a friend sent me information about the recent nonsense on the Ogg email group list. Don't let that little pest bother you -- his list has only a small number of persons marginally affiliated with Reich. My own past difficulties with him are given some small detail at: http://www.orgonelab.org/oggpore.htm

To update you, during my seminars (now thankfully finished) the students and I erected a well-shaded and well-ventillated open structure composed of celotex and plywood, under a dense patch of trees on my property. Inside we suspended by string, two bare-metal 27 gauge galv. steel boxes, measuring temperatures in the open air all around, as well as just above, below, and at the inside-top of the boxes. An inverted paper cup made a chamber above the accumulators, sealed with a thin layer of silicone caulk. The boxes were taped together with strips of duct tape. My goal was to, in part, replicate your approach as given in the recent IE article, but also to slowly build up the accumulator layers in hopes to understand the overall thermal dynamics of the accumulator -- or rather, of a simple metal box on its way to becoming a stronger accumulator.

The results from four days of round-the-clock measurments made every two hours, suggested we only were recording the effects of thermal lag. In the morning hours when solar heat warmed the air, the top-interior and above-box temperatures lagged behind the environment by up to half a degree, yielding a negative to-t, while in the afternoon and night, a reversal took place, where the interior and above-box temperatures showed a positive to-t by the same approximate half-degree as outside air cooled. All environmental thermometers and below-accumulator thermometer showed variances of from 0.1 to 0.2 degrees maximum.

The two metal boxes, one large 12" cube and a smaller 10 cm cube, showed nearly identical thermal dynamics. No control enclosure was used, only background air temperatures.
I then added a layer of acrylic felt to the exterior of one of the metal boxes, and around the exterior of the paper cup as well. This did not observably change the situation, though I have not yet plotted those data to a graph as yet. More could be done to equalize the thermal dynamics inside the structure, but the absence of any positive to-t in the morning hours, I am presently led to conclude there is either a problem with my set-up, or some environmental problem exists (we are currently suffering under a drought, so dor-levels may be suppressing the effect), or as any good scientist must admit, the phenomenon is artifactual. I intend to wait until we regain some cloudy and even rainy weather and then run the measurments once again, to be sure the issue of dor is not a factor. On the other hand, we ran some seed-sprouting experiments in the orgone room during the same time as these thermal tests, and the accumulator group did very well, as usual (this biological response I find to be systematic and repeatable). If you have any suggestions, please let me know.

From my own review of the to-t experiment, I must confess to not ever getting so excited about it, as most everyone has got small results over the years, which the typical skeptic-physicist would always find an argument against. There is an electrical anomaly inside the

accumulator (discovered independently by myself and a Spanish physicist) related to the use of electrical thermistors or thermocouples, but that is a different question altogether.

In your graphs of to-t I note you did not record measurements from the period after midnight to approximately 10 AM or noontime -- I cannot be sure about the times as the data points are so close together on the graphs. While your initial daily measurement at 10 AM or noon does show a positive effect, suggestive it is not due to thermal lag, the issue of thermal lag in the morning hours is nevertheless raised by the absence of those data -- on your graphs the data points from midnight to 10 AM or noon are connected straight across, but in fact thermal lag might have reduced them considerably, or rendered them negative at those hours. Have you considered this aspect?
Also a small note on your Thermal Anomaly paper from the Aetherometry web site -- on page 61 the discussion of relative humidity inside the accumulator gives the impression that you expected the higher interior temperature to correlate with a higher relative humidity. From the classical expectation, the reverse is anticipated (higher temp = lower RH, all else being equal), though my own experiments on water evaporation in the accumulator, supplemented by sling-psychrometer measurements inside a larger orgone room, indicated a higher humidity spontaneously develops within the accumulator than in the outside air.

I've made margin notes -- some other questions and constructive critique -- on your Thermal Anomaly paper (the only one I have reviewed in detail so far), and will share them with you at a later time, but at present am playing catch-up with all the things neglected during the seminars. Assuming you are back from your vacation and "catching-up" yourselves, have you considered to allow me to purchase photocopies of your full set of papers?[...]

Again, don't let Ogg get under your skin. He's a nuisance pest and simply isn't worth it.

Hope your vacation was a big refresh. I go away for a few days myself next week.

Best wishes,

James
Subject: Success with downloads
Date: Sat, 18 Aug 2001 21:16:58 -0700
From: J. DeMeo
To: Correa&Correa <Correa&Correa>

Dear Paulo and Alexandra,

I finally was able to download your papers, and will now be able to print and read them without difficulty (hopefully). The problem was stupidly simple, I should have saved them to hard disk before trying to print them
out.

James

****

Subject: Matters at hand

Date: Mon, 20 Aug 2001 03:55:00 -0400
From: Correa&Correa
To: J. DeMeo

Dear James

We just got back in today - and confess we are almost ready to get back out again! On our side there is very little news to report. We read about a third of Saharasia - and thought much about your theses there, did a lot of land searching and some sunning and bathing. The best part was that it was very quiet, even when the weather was not so good.

[...] We were pleased that you were able to sort out the printing problem of the monographs and your experience indicates that perhaps on the system requirements section of the site we should add a note in the downloading section that it is only after having downloaded that one should attempt to print.

We will now move to respond to your two e-mails you sent while we were away-

> Meanwhile, we have found that or-charged water yields a slightly higher
> surface tension. This was expected theoretically. The task now is to
> validate it with a larger number of samples, to determine how long the
> charge lasts after the water is removed from the orac, and how long is
> required to induce the change, and to what magnitudes.

How do you know that the effect is not an electrostatic one?

> I've just reviewed your full Table of Contents for the atherometry site,
> and am glad to see the materials on the orgone-aether motor "coming soon".
> Put me down for a copy of the videotape on "The Improved Aether Motor",
> when available.
[...]
>Also, what is the "Aetheroscope analytical DA system"?

This is, quite frankly, an amazing device which provides all sorts of analytical parameters for every physical interaction (electrostatic, electrodynamic, thermodynamic, gravitokinetic, etc), down to fine structure and wave functions, if you can believe it. In principle it will run on a dedicated station much like a DAQ (Data Aquisition System) does, and will function as the concrete embodiment of aetherometric theory - employing both new software and hardware material. We wrote the algorithms and Malgosia is now doing all the programming. The original device was written for the Superscope, but soon outlived its capabilities. We have produced a few basic modules just to show its potentiality. The PAGD motor results you witnessed processed on a Mac were obtained with just such a module, operating to give results in the usual units - which all aetheroscope modules do, giving the results either in aetherometric notation or conventional notation with the aetherometrically corrected results.

> >We thought about your outdoor experiments and concluded that you should read
> >our S2-05 - since you have just about all that it takes to understand it.
> >There we perform outdoor measurements both in the shade and in sunlight which

> >are not reported in the paper that Gene published. Remember, this paper was
> >solely a methodological paper (precisely what we think has been missing in the
> >field) - trying to establish the phenomenon under the most disadvantageous and
> >stringent conditions (indoors basement), with the right control to address
> >Infeld's objection. Note that it claims nothing more. Also, it is important
> >to realize that under these conditions the phenomenon needs the space
> >above the ORAC where the thermometer is placed, to be insulated - otherwise, under
> > the limiting conditions, the residuals become erratic. If one is picky, one can
> >also suspend a thermometer at the same height and surround it with the same
> >housing - it will not present the temperature difference, and is therefore
> >also an excellent control. At any rate, Gene will send us all the criticisms
> >- for us, it is only of importance that these matters attract the necessary
> >attention and shake that ossified tree!

>To update you, during my seminars (now thankfully finished) the students
>and I erected a well-shaded and well-ventillated open structure composed of
>celotex and plywood, under a dense patch of trees on my property. Inside
>we suspended by string, two bare-metal 27 gauge galv. steel boxes,
>measuring temperatures in the open air all around, as well as just above,
>below, and at the inside-top of the boxes. An inverted paper cup made a
>chamber above the accumulators, sealed with a thin layer of silicone caulk.
>The boxes were taped together with strips of duct tape. My goal was to, in
>part, replicate your approach as given in the recent IE article, but also
>to slowly build up the accumulator layers in hopes to understand the
>overall thermal dynamics of the accumulator -- or rather, of a simple metal
>box on its way to becoming a stronger accumulator.
>The results from four days of round-the-clock measurments made every two
>hours, suggested we only were recording the effects of thermal lag. In the
>>morning hours when solar heat warmed the air, the top-interior and
>above-box temperatures lagged behind the environment by up to half a
>degree, yielding a negative to-t, while in the afternoon and night, a
>reversal took place, where the interior and above-box temperatures showed a
>positive to-t by the same approximate half-degree as outside air cooled.
>All environmental thermometers and below-accumulator thermometer showed
>variances of from 0.1 to 0.2 degrees maximum.
>The two metal boxes, one large 12" cube and a smaller 10 cm cube, showed
>nearly identical thermal dynamics. No control enclosure was used, only
>background air temperatures.

Speaking to these objections mentioned in your first message - and in your second message also: we have responded to Baker in the upcoming issue of IE. We would really like you, of all people, to understand what we were trying to do here, in what we have called the Reich-Einstein experiment - it is not about defending Reich or Einstein. It is about completing what they started but did not finish. And if you read the record - it is simple what this amounts to: does one still see an anomalous temperature difference when one minimizes the effect of solar radiation, the consequent effect of diurnal variation of atmospheric temperature, the amount of sensible thermal radiation from walls, the heat convection from floors and ceilings (the major Infeld objection), and removes any direct heating of the closed large room where the experimental apparatuses are placed?

It is a completely different experiment from the one you are trying to do (in the above excerpt). To minimize the effect of solar radiation, we performed the experiment indoors, as Einstein did under Reich's guidance. To minimize the diurnal variation of atmospheric heat - which, thank fate or not!, still comes through loud and clear - we performed the experiment around the clock in a large basement room surrounded by three cold rooms. The distance of all experimental apparatuses to the walls was very nearly the same, and they were arranged in a circle, with a minimum distance several feet from each other, suspended from the ceiling by thin nylon strings. The room was not heated directly, nor were the adjacent rooms, and the experiment was conducted in the winter. The convection argument can then (at last!) be addressed with the naked Faraday cage, and the result is - that under stringent and most disadvantageous conditions - the thermal anomaly is irreducible.

What you are trying to do is an entirely different matter. In outdoor experiments (where the set ups also matter), there is no way to determine whether or not what you are seeing is merely a lagging sensible thermal ordinary difference (any outdoor exposure to the elements will exhibit this lag) caused by solar-induced blackbody radiation, unless you directly confront blackbody radiation theory on its own terms using aetherometric tools: does solar blackbody radiation account for the amount of heat and the manner in which it is evolved? etc. This was in fact the purpose of the AS2-05 report, as it demonstrates that you can get negative temperature differences regularly around the clock (the data is profuse with readings taken in the middle of the night AND with negative readings in the early morning - which effectively do not mystify us, as they did Reich), and even employ the ever so slightly biased (yes, it is true, there are fewer points at night) DWLS curves (that nevertheless give an unambiguous net positive result), only to demonstrate that the evolved amount of observed sensible heat in the thermal region of the electromagnetic spectrum remains unexplained by the amount of photon radiation absorbed by the devices, even when they are directly exposed to the sun!!

Incidentally, all of our thermometer stands in the indoors-winter-basement Reich-Einstein experiment were made of PVC tubing having a plug of Basalt glass wool wrapped around enough times inside the top of the PVC tube to hold the thermometer stem quite fixidly, such that the thermometer bulb is exposed to free air a good two inches away from the Basalt wool above.

We also would like you to know that we do not know about celotex per se. Reich employed it as a 'revetement', in place of wood or cardboard, but what the brand name chemically encompasses has changed over the last sixty years. Some celotex brand name material up here is simply a new type of extruded styrofoam-like material, and it bears no resemblance in color or chemical composition to the original material Reich employed.

Incidentally : are you employing 0.1 to 0.05 °C resolution mercury thermometers, and placing them at the same exact bulb heights in the setups?

>I then added a layer of acrylic felt to the exterior of one of the metal
>boxes, and around the exterior of the paper cup as well. This did not
>observably change the situation, though I have not yet plotted those data
>to a graph as yet.
>More could be done to equalize the thermal dynamics inside the structure,

>but the absence of any positive to-t in the morning hours, I am presently
>led to conclude there is either a problem with my set-up, or some
>environmental problem exists (we are currently suffering under a drought,
>so dor-levels may be suppressing the effect), or as any good scientist must
>admit, the phenomenon is artifactual.

We would have been more than happy to have admitted that, under the stringent conditions of what we called our Reich-Einstein experiment, the phenomenon was artifactual. But that is neither what we found, nor what you legitimately can conclude from your outdoor setups - independently from whatever else might be at work, including a DOR emergency.

>I intend to wait until we regain
>some cloudy and even rainy weather and then run the measurments once again,
>to be sure the issue of dor is not a factor. On the other hand, we ran
>some seed-sprouting experiments in the orgone room during the same time as
>these thermal tests, and the accumulator group did very well, as usual
>(this biological response I find to be systematic and repeatable). If you
>have any suggestions, please let me know.

>From my own review of the to-t experiment, I must confess to not ever
>getting so excited about it, as most everyone has got small results over
>the years, which the typical skeptic-physicist would always find an
>argument against.

We wager, from this statement, that you still have not understood our endeavours, nor what we said above. Under the correct stringent conditions, the phenomenon is irreducible. Reich was right. But you must first understand the difference between indoor stringent experiments and outdoor maximalizing (or not) type of experiments.

>There is an electrical anomaly inside the accumulator
>(discovered independently by myself and a Spanish physicist) related to the
>use of electrical thermistors or thermocouples, but that is a different
>question altogether.

Do not worry about this James - nothing of the aether motor is of this nature (the direct sensible thermoelectric conversion is a very minor part indeed). Alex and I also discovered years ago that ORACs exposed to the sun develop sizeable potentials between the layers that can be exploited with thermocouple arrangements. Harold did a lot of work on ideal magnetothermocouple layer arrangements with Strachan, without even realizing quite what they were doing.

>In your graphs of to-t I note you did not record measurements from the period after
>midnight to approximately 10 AM or noontime -- I cannot be sure about the times as the
>data points are so close together on the graphs. While your initial daily measurement at
>10 AM or noon does show a positive effect, suggestive it is not due to thermal lag, the
>issue of thermal lag in the morning hours is nevertheless raised by the absence of those
>data -- on your graphs the data points from midnight to 10 AM or noon are connected
>straight across, but in fact thermal lag might have reduced them considerably, or >rendered
them negative at those hours. Have you considered this aspect?

We thought we had in a most eloquent fashion. To begin with, we need to know what Figure you are talking about. Are you on the IE paper or on AS2-05?

>Also a small note on your Thermal Anomaly paper from the Aetherometry web site -- on
>page 61 the discussion of relative humidity inside the accumulator gives the impression
>that you expected the higher interior temperature to correlate with a higher relative
>humidity.

Yes, we were mindful of what Tromp wrote and what we have observed - that, in the warm spring and summer months relative humidity is normally higher inside buildings and enclosures than outside, while their temperature is also warmer in general.

>From the classical expectation, the reverse is anticipated (higher temp = lower RH,
>all else being equal),

The 'all else being equal' is what we are having trouble with: for higher temperature to mean lower RH, dewpoint and absolute humidity must indeed remain constant, but the centigrade difference between the dry and wet thermometers of a psychrometer must increase.. This is what happens typically in a dry incubator - it gets drier as it heats up - and also, as it turns out, what happens in the ORACs. Whereas if the d-w difference remains the same, then relative and absolute humidities, as well as dewpoint, will increase with increasing temperature.

>though my own experiments on water evaporation in the accumulator, supplemented by
>sling-psychrometer measurements inside a larger orgone room, indicated a higher
>humidity spontaneously develops within the accumulator than in the outside air.

We have not conducted systematic psychrometer measurements inside and outside our OR room, but our systematic hygroscopic measurements inside the ORACs show smaller RH oscillations and are in general substantially lower than the outside measurements. This also makes sense since the effect of the ORAC is clearly one of slowing down the electroscopic leakage or seepage, which we also know is accelerated by increasing humidity.

We noticed that you did not mention anything about our website on your list so far (nor for that matter your own or our paper in IE #38). What is your current thought on this? If it ties in with your reaction to the material, are you only going to put forth an announcement if you are in full agreement with what we have put forth, or are there other considerations in your mind at present?

Warmest regards from all of us

Alexandra & Paulo

Subject: Re: Matters at hand
Date: Tue, 21 Aug 2001 12:06:39 -0700
From: J. DeMeo
To: Correa&Correa

Dear Paulo and Alexandra,

[...] Very glad to hear of the more detailed success with your motor experiments and filming, but your "Aetheroscope analytical DA system" will probably remain a mystery to me until I could see it in use.

Regarding your points on the experimental work:

1. Regarding the water surface tension measurements, these are so very preliminary. However, even assuming the surface of the charged water has somehow acquired a "charge" greater than the control, I could not imagine what other than the OR energy could impart such a charge. The orgone room is totally isolated from the other buildings, with no line voltage of any sort inside. Even in the lab where the measurements are made, there are only extremely weak fields present, which in any case would have affected both control and OR-charged water samples identically. The samples were handled identically once being removed from the accumulator and control boxes, and left on a lab table to equalize in temperature for more than an hour -- and temperatures were taken just before measurements. I am accustomed to seeing the accumulator "charge things up" with electricity even if only slightly so (ie, non-frictional charging, as with the electroscope), and am actually more concerned about chemical surfactant contamination as a possible source of the difference, but in any case have a lot of control testing to do before it could be announced with certainty. What did you have in mind specifically, regarding an electrostatic influence?

2. Regarding to-t... Certainly, I very much appreciate your offering of a more rigorous control to the "debate" between Reich and Einstein, and so will do whatever is necessary to experimentally test your approach. But I do have my own questions on your methodology, and so will engage you on the subject with some detail.

A. You are correct, that this first trial in my outdoor shelter did not replicate all aspects of your outdoor tests, but it did duplicate a crucial part of your outdoor arrangements. Nevertheless, I am convinced my own outdoor experiment could not have detected any subtle orgonotic influence upon temperature, as the environmental variations were too great. Morning thermal variations in my outdoor structure were generally from 16 deg to 32 deg C., a 17 deg swing over the course of only five hours (morning), with a similar downward swing in the evenings (though with a more shallow slope). At most only 0.5 deg. C increase was observed in the metal box, or above the metal box, and then only after the air temperature peaked around noon, and then declined more rapidly than the temperatures inside or above the boxes. Definitely thermal lag. But it raises a question about the differences between our two shaded outdoor structures, as your outdoor enclosures did not show this kind of thermal lag effect, but rather remained positive to-t around the clock. You used a simple double-nylon canvas structure, which I am trying to imagine -- was it placed into your open backyard area? Was there snow on the ground, or could the sunlight penetrate from the sides to warm the ground underneath in the morning hours? And could some diffuse light filter through the canvas from the top and from the sides, or was the canvas totally opaque, extending to the ground with side-flaps as well? A thick and heavy canvas, or a light canvas? Irrespective, some thermal IR effects surely must have been at work in your outdoor setup, penetrating through the canvas. In my own outdoor setup, the

dense forest canopy, plus the totally opaque plywood and natural celotex panels (doubled-up, with the celotex on the interior), all around, totally shielded the boxes, the thermometers, and even the ground below, from even a trace of stray direct sunlight over the course of the entire day, and I would anticipate that even IR was dramatically reduced in my shelter as compared to your canvas setup, given the moist condition of the trees.

If I am correct here, then perhaps some of your outdoor to-t effects under the canvas were due to solar orgonotic excitation or incident IR radiation penetrating through the canvas (yes, they are functionally related!), in a manner similar to your unshaded experiments in the open sun (at least for IR frequencies). I believe I mentioned to you my work years ago in solar energy construction and home design in Florida, where it was usual to recommend a metal foil layer above the insulation layer just under the roof surface, so as to "reflect IR radiation back upwards" to help with summertime cooling. A few experimental homes were constructed with basically a roof-top water layer, which totally absorbed the solar IR, which was the main problem with keeping homes cool in summer. The IR was known to penetrate through roof tiles, tar paper, plywood and eventually into the upper floors of the home, unless there was a metal or water layer at the top surface of the home. This principle, nobody following Reich has considered except for myself, and you also in your discussions on black-body radiation, but with a different emphasis than I am giving it here. I also just noted a new method of fire-fighting being employed by some homeowners in the Pacific Northwest forests, wherein they wrap their entire home in a heavy insulated foil, with the reflective foil on the outside to "reflect the heat" from nearby forest fires. From this effect alone, of incident IR radiation, one could anticipate some interior and top heating of a metal box in the open sun, or even under a canvas shelter, but probably not under a living and moist tree canopy (of around 20-30 m height) supplemented with plywood and celotex insulating panels -- and the celotex I used is the original natural material, as described below. As you say, the point can probably only be resolved by "confronting blackbody radiation theory on its own terms", but it does appear to explain why you got some postive effects from your outdoor structure (maybe 5 degrees positive to-t?) whereas I could show only thermal lag effects.

B. Regarding the question of missing data in the mornings, I refer to the graphs in both your IE article, and in the S2-02 article. When I look at them, there are no data points for the morning hours, and the data points at midnight are connected straight across to the first data point of each day, which appears to be around noontime or 10 AM. As mentioned before, some of the graphs are too compressed to be absolute about this, but it appears to conflict with your statements of having made measurements around the clock. The morning hours, where data is absent, is precisely when one anticipates thermal lag effects with potentially negative to-t readings.

C. I would imagine, if I increased the layerings on my outdoor accumulators, and made a matched control box, that the to-t would appear and also be fairly regular, in the manner of the experiments of Seiler. This would show, as was my intention, the gradual overtaking of the effect of orgonotic heating over that of thermal lag. The problem, however, is the setup might require a considerable amount of layering solely to act as insulation against environmental variation -- which suggest to simply bring the existing setup indoors. In reviewing your paper and email, I'm convinced to now explore the indoor effects, as the outdoor thermal lag is simply too great to allow the effect to be easily observed.

D. I would not wish to experimentally investigate the matter of blackbody radition, until such time I could confirm the bare-metal box OR influence you have reported. A

spectrometer of some sort would be helpful here, to measure what actually reaches the boxes, in the IR frequencies.

E. You are correct about the Celotex confusion, and the new edition of my Handbook printed last year made a note about this. The original Celotex was made from crushed sugar-cane residue, glued together and spread into a thin sheet, with one side painted white. Today, you can still find it under the name "builder's board" or "acoustical ceiling tile", and it has a very good orgonotic feeling. Excellent accumulators can be made using this as the final exterior, though one must stabilize it with shellac. I've seen the styrofoam and foil-backed "celotex", which is a noxious material. It was introduced sometime around 1997.

F. My thermometers are mercury 0 - 100 deg. NTSC tracable, marked with 0.1 deg. divisions. They were suspended at the same heights, save for the small variation of the ones above and just below the top metal layer, and the one directly below the boxes.

3. Regarding the thermocouple (and also thermistor) accumulator effect, I was speaking about a non-thermal influence upon the current flow in the wires or thermocouple junction, as the observed effects occurred when the to-t as measured independently by thermometers was only around 0.1 deg. Calibrated thermocouples lost calibration quickly in the accumulator, skewing off with a negative t-ot at around 0.2 deg. while the thermometers (and initial readings with the thermocouples) were a positive 0.1 deg. The effect was repeated many times here. It may be an artifact of more normal origins, but my contact in Spain made a completely independent observation of the phenomenon.

4. Regarding the RH issue, perhaps you could review the paragraph self-critically to see if the language could be clearer. Or to add a footnote as per your clarifying sentences in your email. My sling psychrometer measurements were made inside Dick Blasband's large orgone room at the old Elsworth Baker Laboratory in Buck's County, Pennsylvania, during summertime afternoons. The door and ventillation window were always left open, and the door was sufficiently large as to allow a very free flow of air. Both the "exterior" and interior locations of measurement were only about 2 meters distance, both being within the shade of a large barn. Basically I would make a measurement outside, then step inside the room and make another, spaced out over the day. The results were never published, though my pilot study on water evaporation did mention it. I should undertake another review of the question in my own orgone room here. Relative humidity is actually not so interesting as absolute humidity, in this context.

5. Regarding OBRL-News, I did post out an announcement of the IE articles, yours and mine both, on May 30th, but have not done anything as yet on your new web site -- so much work to do, it is one of many things that has simply slipped by. No, I do not feel the need to be in full agreement in order to do so, but would like to gain some experimental confirmation of your approach so that I could add a note about it to the posting.

I'm struggling to understand your aetherometric theory, which appears to be a replacement for orgone in many respects, but I don't think I could make a decent evaluation until such time I've gone through all of what you've written. So far, however, I don't see anything so essentially new as to require a re-think of the basic orgonomic theory Reich developed... but I will read on and remain open. I do note, however, some needlessly provocative statements in your papers directed against the followers of Reich. On one level, I can appreciate what you are saying, as there are some really stupid and arrogant people laying claim to his

heritage (what I call the "fight over Reich's bones"), and I share your outrage. However, since you don't make explicit citations of whom you are referring to (as with the claim there are "no studies worth referencing") it suggests an across-the-board dismissal of everyone save for Reich, including myself. Probably that's not your intention, but I question the need to include such statements at all. They are a needless distraction from your work, your experimental results and contributions. In any case, the followers of Reich (myself included, and you also my friends) are so marginalized and outside the mainstream, that the people you should be addressing your work to won't even know who those persons are. My advice is, to make your criticisms of other people's work open and explicit, with full citations and credit where it is due, but without any of the personal element even if they have been insulting to you in the past -- otherwise, don't even mention or refer to them. Everyone I know who has done any significant work has quite a few arrows sticking out of their back-side, and has also been bruised by the mud-throwers. Such wounds are a good way to recognize the genuine workers -- but the average sloth will have no understanding about it.

Warm regards,
James
PS. The Indo-aryan root of the word "character" is "gher" which is basically a "slash or wound" -- so those who have done great battles, are possessed of great "character" (gashes and wounds).

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Subject: Re: Matters at hand
Date: Wed, 22 Aug 2001 14:15:09 -0400
From: Correa&Correa
To: J. DeMeo
References: 1

Dear James -

We all felt sad at receiving your letter, and somewhat sad responding to it as follows, because - and solely because - we feel that there is no other way of keeping these exchanges honest other than at times to appear brutal.

[...]
>Very glad to hear of
> the more detailed success with your motor experiments and filming, but your
>"Aetheroscope analytical DA system" will probably remain a mystery to me
>until I could see it in use.
There is no reason why it should. Since we present our rediscovery of the conversion of mass into length and wavelength in AS2-01, and the critical aetherometric equations for the gravitational field in this same monograph, these already provide an example of the scope of the device: it deploys everywhere new units of measurement for physics based solely upon

length and time parameters, and it can be applied to all physical interactions. This can be the analysis of gravitons from simple pendulum experiments, or the determination of the exact velocity of massbound charges in an ordinary DC or AC circuit, or the determination of the number of photons truly responsible for the ejection of an electron in the photoelectric effect, and so on. It is a modular tool, with distinct hardware elements and software routines that operate with a new and common mathematical language (aetherometry), and can also give conventional values where they apply or are correct. It is, in a very real sense, an analytical tool for aetherometric engineering and diagnostic of existing physical devices. The routines we have written encompass all known physical effects, whether thermodynamic, gravitational, electric, photonic, magnetic or mechanical.

Now, we address your points -

>1. Regarding the water surface tension measurements, these are so very
>preliminary. However, even assuming the surface of the charged water has
>somehow acquired a "charge" greater than the control, I could not imagine
>what other than the OR energy could impart such a charge.

We cannot imagine exactly what you mean by OR energy. Is this energy electric - in the sense that one speaks of massbound charges, eg electrons, which create electric monopoles, as when one charges an electroscope negatively with an excess of negatrons? Because if it is, then your statement is nonsense: you are just saying that monopolar electric energy charged electrically the water with a certain monopolarity. Or is this OR energy you speak of, a nonelectric form of energy - thermal, perhaps - since it has been well shown that you can get sensible heat or IR electromagnetic energy (ie IR photons) to convert into electric energy? And if it is not massbound electricity, nor sensible heat, then what is it? You see, James, we hope you will not be offended, but the fact is that we think you currently have very little idea of what exactly you mean when you say OR energy. Even Reich may well not have been so sure of what he meant. But that is alright - so long as one does not bluff amongst allies.

>The orgone room
>is totally isolated from the other buildings, with no line voltage of any
>sort inside. Even in the lab where the measurements are made, there are
.only extremely weak fields present, which in any case would have affected
>both control and OR-charged water samples identically.

We have never made assumptions about the presence or absence of fields other than by actually utilizing batteries of various detectors. And we have observed, under many varied conditions, the electrostatic charging of water, whether achieved by mechanical motion, or by electrostatic influence, or by long-term friction of the microatmospheres of water molecules. But before we engage in such trivial matters that others long ago observed without use of any ORAC, for instance, it would be more adequate to wonder about just which water you employed. We can, for instance, assure you that very curious things indeed occur in water experiments: triple distilled and deionized water has entirely different sets of properties from any other water, and it does not stay as such for very long, unless kept in a scrupulously sterile and filtered atmosphere. If you want an explanation for what you have observed, you would have to better describe the conditions (we do not even know _which_ water you are working with). But this is of little importance, because it was not the

objective of our question in the previous message. Instead, we were wondering exactly what you meant - in physical terms - by OR charging the water. Especially so when it is known that charging water electrostatically goes hand in hand with increased surface tension.

Moreover, if in fact what you are doing is charging ultrapure water electrostatically, and if you were then to demonstrate that this was not a thermal, baroscopic, or other known phenomenon, etc, and that the negative charges you were observing were actually coming from an aether source, you would then have to conclude to what we just presented in our AS2-09 - in its most explicit mathematical and physical form: the creation of negatrons from the secondary superimposition of massfree aether energy of specific energy and frequency characteristics.

>The samples were
>handled identically once being removed from the accumulator and control
>boxes, and left on a lab table to equalize in temperature for more than an
>hour -- and temperatures were taken just before measurements. I am
>accustomed to seeing the accumulator "charge things up" with electricity
>even if only slightly so (ie, non-frictional charging, as with the
>electroscope),

We have never observed such a phenomenon that was not an artifact. Let us explain: we have never observed any such spontaneous charging with naked electroscopes inside or outside ORACs. We have made such observations rather unexpectedly, at the start, and they could be traced to two simple conditions: the electroscope case was floating, and it was whatever dielectric or insulated metal it contacted with which underwent a sudden change of potential ((charging or discharging). In the instances we were able to explore the phenomenon, we found either thermoelectric conversion that changed the potential of the floating case, or an abnormal ion flux of charges in the environment, or an unexpected friction event which, in one instance, was the fact that the well insulated body of the experimenter touched the metal stand where the case rested and imparted a substantial negative charge quantum to the electroscope case, resulting in induced charging with a lag of a minute or so. So, despite these observations, we have never observed any real spontaneous charging of the electroscope. In fact, even though we keep our mind open, aetherometric theory simply suggests that it does not exist.

> and am actually more concerned about chemical surfactant
>contamination as a possible source of the difference, but in any case have
> a lot of control testing to do before it could be announced with certainty.
>What did you have in mind specifically, regarding an electrostatic
>influence?

We think we have answered this above. At bottom, we wanted to understand that what you are checking is a correlation of some accumulation of electrostatic charge with increased surface tension of (pure?) water. For, indeed, if you exclude any of the other known sources of such electric charging, then you would have to invoke asymmetric creation of massbound charges. Since Reich did not provide that recipe, you might want to consider taking recourse to aetherometry.

You see, it seems to us that you have not understood what are the immense differences towards Reich in what we are saying, doing and hoping to introduce you to. Suppose Reich had checked both leakage and seepage rates inside the ORAC and found that the ORAC slows down the former but not the latter. Then the enclosure would indeed constitute a source of negative charges and, if these were massfree, then the aether would also be composed of monopolar charges, but they would just be massfree. The aether would duplicate the world of matter, or electrons. But none of that is what is observed in reality. The electroscopic anomaly affects an electroscope whether it is positively or negatively charged - and therefore, most inevitably, one has to conclude that the effect of the ORAC upon the electroscope is a nonelectric one.

Question: if you are doubtful whether there is a real and unexplained thermal anomaly inside the ORAC, and if you then refuse the difference of our treatment of the electroscopic anomaly towards Reich's most incomplete explanation, what then becomes of the physical OR effects of the ORAC, or the substance of a nonelectric, massfree energy?? What does this OR energy become then, save for another mystical notion??

We contend our experiments are unequivocal - even if difficult to wrap one's arms around: they clearly state that Reich was correct - there are two unexplained anomalies within the ORAC, one thermal and the other electroscopic. But neither can be explained by creation of massbound or massfree monopolar charges, anymore than by any passive absorption of electromagnetic energy. The energy accumulated inside the ORAC is neither primarily thermal, nor electric. In the electroscope, it is detected by the mediation of the trapped monopolar massbound charges - and it is what permits these trapped massbound charges to oppose local gravity: hence the critical gravitokinetoregenerative phenomenon we (and not Reich) discovered, as reported in AS2-01. Likewise, with respect to the sensible thermal anomaly, we demonstrate that (1) under stringent conditions the anomaly is retained by simple Faraday cages (How correct Reich was in this!), and (2) that even when directly exposed to the sun and elements, the observed evolution of heat is not accounted for by external absorption of sensible heat or photon energy, and remains to be explained instead by the conversion of latent heat into sensible heat at the interfaces of the devices and in the core.

>2. Regarding to-t... Certainly, I very much appreciate your offering of a
>more rigorous control to the "debate" between Reich and Einstein, and so
>will do whatever is necessary to experimentally test your approach.

It did not seem to us that this was what you were up to. For the stringent experiment is to be conducted indoors, in a basement, preferably, in the winter, preferably, and so on.

>But I
>do have my own questions on your methodology, and so will engage you on the
> subject with some detail.

We have not yet seen one of these questions. It appears rather that you have been avoiding them.

>A. You are correct, that this first trial in my outdoor shelter
>did not replicate all aspects of your outdoor tests, but it did duplicate a

>crucial part of your outdoor arrangements. Nevertheless, I am convinced my
>own outdoor experiment could not have detected any subtle orgonotic
>influence upon temperature, as the environmental variations were too great.

So then, what you are trying to do is to replicate the outdoor experiments. But this is an altogether different matter than the Reich-Einstein experiment - now you are on AS2-05, or the thermal anomaly of ORACs placed outdoors: but here, as we have said, how do you hope to demonstrate that the heat trapped - independently of whatever variations and whichever is the smoothing employed - is not a function of the heat absorbed or the so-called blackbody radiation of the sun?

>Morning thermal variations in my outdoor structure were generally from 16
>deg to 32 deg C., a 17 deg swing over the course of only five hours
>(morning), with a similar downward swing in the evenings (though with a
>more shallow slope). At most only 0.5 deg. C increase was observed in the
> metal box, or above the metal box, and then only after the air temperature
>peaked around noon, and then declined more rapidly than the temperatures
> inside or above the boxes. Definitely thermal lag. But it raises a
>question about the differences between our two shaded outdoor structures,
>as your outdoor enclosures did not show this kind of thermal lag effect,
> but rather remained positive to-t around the clock.
Then what you want to know is how come we obtained no negative readings when the devices were in the shade outdoors. After several attempts at purchasing a decent enclosure and testing one or two tents, we created a large tent from nylon tent material on ceiling and walls, with fine nylon mesh at the entrance and openings, exposed in a field far from any trees, so that only its shade counted. The apparatuses were mounted on a large steel wire frame which rested on the ground. As we said in that paper, they were directly exposed to radiation emanating or reflecting from the soil, even if exposed to air drafts and winds. The tent provided shade and protection from rain and so on, but undoubtedly also a luminous environment through which 'some form of solar radiation' penetrated. This is why we do not think we made much of it, other than using the experiments done in the shade as a positive stepping stone to those directly exposed to 'solar radiation'. Since we could not be sure whether or not, when the ORACs are exposed directly to the sun and elements, the thermal anomaly is simply the result of absorption of electromagnetic energy, at their modal maxima, we thought we might as well 'bite the bullet' and directly address the blackbody question which has damned this problem for too long. To address the problem of absorption we measured also the temperature of the external envelope of the box and applied aetherometric tools to the problem of heat - which is possible to do because we provided, in aetherometry, an obvious alternative to Reich's mistaken understanding of what temperature and sensible heat is all about (see AS2-07). What we found is that at the IR modal absorption maxima for the devices employed, the absorbed IR most clearly does not account for the sensible heat evolved.

>You used a simple
>double-nylon canvas structure, which I am trying to imagine -- was it
>placed into your open backyard area? Was there snow on the ground,

The canvas structure is explained above. As for snow on the ground, the paper explicitly states that these experiments in the shade which you refer to were conducted in the summer.

>or
>could the sunlight penetrate from the sides to warm the ground underneath
>in the morning hours?

Answered above already as yes, some amount of sunlight and whatever else creates sunlight traverses the nylon tent, through ceiling or walls, and that is why we pushed the experiment to direct exposure conditions.

>And could some diffuse light filter through the
>canvas from the top and from the sides, or was the canvas totally opaque,
>extending to the ground with side-flaps as well? A thick and heavy canvas,
>or a light canvas?

Ditto.

>Irrespective, some thermal IR effects surely must have
>been at work in your outdoor setup, penetrating through the canvas. In my
>own outdoor setup, the dense forest canopy, plus the totally opaque plywood
> and natural celotex panels (doubled-up, with the celotex on the interior),
>all around, totally shielded the boxes, the thermometers, and even the
>ground below, from even a trace of stray direct sunlight over the course of
> the entire day, and I would anticipate that even IR was dramatically
>reduced in my shelter as compared to your canvas setup, given the moist
>condition of the trees.

No argument here, but it is still luminous in your enclosure during the day, is it not?

>If I am correct here, then perhaps some of your outdoor to-t
>effects under the canvas were due to solar orgonotic excitation or incident
>IR radiation penetrating through the canvas (yes, they are functionally
>related!), in a manner similar to your unshaded experiments in the open sun
>(at least for IR frequencies).

If the solar orgonotic excitation is related to the incident IR photon energy (incidentally, it would be good if you came clean on this and just told us what it is you are saying: do you know _what_ exact energy and frequency of solar OR radiation produces _which_ exact frequency of IR photon? And do you know how? And do you know whether this solar OR radiation is charged or not, and in what manner?) this does not mean, in any way , shape or form, that incident IR radiation accounts for either the heat evolved inside or above the core cage, or for the electroscopic anomaly which Reich half-identified. Precisely AS2-05 proves that it doesn't - and what you must address at the end of the day is not whether your shade experiments give the same results as ours, we think, but whether these devices display a thermal anomaly even in full exposure to the sun that cannot be explained by any blackbody radiation theory.

>I believe I mentioned to you my work years
>ago in solar energy construction and home design in Florida, where it was
>usual to recommend a metal foil layer above the insulation layer just under

>the roof surface, so as to "reflect IR radiation back upwards" to help with
> summertime cooling.

Yes you did.

>A few experimental homes were constructed with
>basically a roof-top water layer, which totally absorbed the solar IR,
>which was the main problem with keeping homes cool in summer. The IR was
> known to penetrate through roof tiles, tar paper, plywood and eventually
> into the upper floors of the home, unless there was a metal or water layer
>at the top surface of the home. This principle, nobody following Reich has
>considered except for myself, and you also in your discussions on
> black-body radiation, but with a different emphasis than I am giving it
> here.

We beg to disagree: nobody, you or Reich included, bothered to compare white versus black ORACs and thus deploy precisely a tool that permits cancellation of the effect caused by absorption of those IR photons, and employed that experimentally to differentiate just what is the sensible thermal energy from the latent thermal energy. It is one thing is for you to understand our method, the other is for you to make statements that are scientifically inaccurate: we demonstrate experimentally and mathematically, that is rigorously and formally, that the IR absorption in open air, direct exposure experiments cannot be accounted for by absorption of blackbody photons locally caused by whatever is the nature of solar radiation. And with respect to those experiments - there are plenty of negative values for To-T and points taken in the middle of the night.

>I also just noted a new method of fire-fighting being employed by
>some homeowners in the Pacific Northwest forests, wherein they wrap their
>entire home in a heavy insulated foil, with the reflective foil on the
>outside to "reflect the heat" from nearby forest fires. From this effect
> alone, of incident IR radiation, one could anticipate some interior and top
> heating of a metal box in the open sun,

This is a gratuitous statement, James - we know what the energy density of solar-induced blackbody radiation is - and that is what matters. If you are instead making measurements of fires and their blackbody profiles, very well, but that is an altogether different can of worms. Where are the quantitative tools that permit you to make a comparison?

>or even under a canvas shelter, but
>probably not under a living and moist tree canopy (of around 20-30 m
>height) supplemented with plywood and celotex insulating panels -- and the
>celotex I used is the original natural material, as described below. As
>you say, the point can probably only be resolved by "confronting blackbody
>radiation theory on its own terms",

Yes, yes, and for this one needed aetherometric tools, and most specifically a treatment of thermodynamics that learned from Reich (and others) but also surpassed him. His notion of temperature and the energy value of the thermal anomaly were simply wrong, and so was his

OP interpretation of the electroscope anomaly. That does not diminish him in our eyes (maybe it does in yours)- it instead proves him correct in his discoveries.

>but it does appear to explain why you
>got some postive effects from your outdoor structure (maybe 5 degrees
> positive to-t?) whereas I could show only thermal lag effects.

In light of the fact that the direct exposure experiments are unequivocal with respect to blackbody contentions, it becomes irrelevant whether the shade experiments can be explained by the same, with luminosity and IR reduced by whatever factor. So, no, the critical experiments are not those performed in the shade but those performed either under stringent conditions indoors, or in full exposure outdoors.

>B. Regarding the question of missing data in the mornings, I refer
>to the graphs in both your IE article, and in the S2-02 article.

We are at a loss to understand what you mean: in AS2-02 there are no To-T measurements. Only in AS2-05, and here, as we said above, there are sufficient nighttime and early morning points (for ex Fig.s 20-27) to confront the blackbody problem in its own terms.

>When I
>look at them, there are no data points for the morning hours, and the data
> points at midnight are connected straight across to the first data point of
>each day, which appears to be around noontime or 10 AM.
No, typically 8 to 10 am.

>As mentioned
>before, some of the graphs are too compressed to be absolute about this,
>but it appears to conflict with your statements of having made measurements
> around the clock. The morning hours, where data is absent, is precisely
>when one anticipates thermal lag effects with potentially negative to-t
> readings.

We confess we like to sleep - but, even putting nightfall at 9 pm, you have three to four points every day spanning the night to early morning period. But, what we do not understand is your repeated emphasis on something we ourselves pointed out. For, what matters is whether, when you place those devices under direct sun, solar quantum blackbody can or cannot explain the observed evolution of heat during daytime! You can lower the values of the means by putting more points in the nighttime period, but you cannot thereby remove the insufficiency of any argument that purports to explain the heat evolved during daytime based upon passive absorption of photon energy generated by some OR radiation from the sun that you do not even know the frequency of.

> C. I would imagine, if I increased the layerings on my outdoor
> accumulators, and made a matched control box, that the to-t would appear
>and also be fairly regular, in the manner of the experiments of Seiler.
>This would show, as was my intention, the gradual overtaking of the effect
>of orgonotic heating over that of thermal lag. The problem, however, is

>the setup might require a considerable amount of layering solely to act as
> insulation against environmental variation -- which suggest to simply bring
>the existing setup indoors. In reviewing your paper and email, I'm
>convinced to now explore the indoor effects, as the outdoor thermal lag is
>simply too great to allow the effect to be easily observed.

Alright - this a good move; but now you must minimize all other conditions, daytime light, wall radiation, convection, and so on. Excellent.

>D. I would not wish to experimentally investigate the matter of
>blackbody radition, until such time I could confirm the bare-metal box OR
>influence you have reported. A spectrometer of some sort would be helpful
> here, to measure what actually reaches the boxes, in the IR frequencies.

Surface temperature readings and the tools of aetherometry and blackbody theory are all one needs, if that is the purpose.

>E. You are correct about the Celotex confusion, and the new
>edition of my Handbook printed last year made a note about this. The
>original Celotex was made from crushed sugar-cane residue, glued together
>and spread into a thin sheet, with one side painted white. Today, you can
>still find it under the name "builder's board" or "acoustical ceiling
>tile", and it has a very good orgonotic feeling. Excellent accumulators can
> be made using this as the final exterior, though one must stabilize it with
>shellac. I've seen the styrofoam and foil-backed "celotex", which is a
>noxious material. It was introduced sometime around 1997.
>F. My thermometers are mercury 0 - 100 deg. NTSC tracable, marked
>with 0.1 deg. divisions. They were suspended at the same heights, save for
>the small variation of the ones above and just below the top metal layer,
>and the one directly below the boxes.

Good.

> 3. Regarding the thermocouple (and also thermistor) accumulator effect, I
>was speaking about a non-thermal influence upon the current flow in the
>wires or thermocouple junction, as the observed effects occurred when the
>to-t as measured independently by thermometers was only around 0.1 deg.
>Calibrated thermocouples lost calibration quickly in the accumulator,
>skewing off with a negative t-ot at around 0.2 deg. while the thermometers
> (and initial readings with the thermocouples) were a positive 0.1 deg. The
>effect was repeated many times here. It may be an artifact of more normal
>origins, but my contact in Spain made a completely independent observation
>of the phenomenon.

Unless you provide more detail, we have no idea of what it is you are looking at. Work with thermistors and thermocouples is exacting and demands very precise and calibrated tools. Junction thermometry is an art all on its own, but your description could be anything.

>4. Regarding the RH issue, perhaps you could review the paragraph

>self-critically to see if the language could be clearer. Or to add a
> footnote as per your clarifying sentences in your email.

Yes, we certainly could do so, but do you think it would really help?

>My sling
>psychrometer measurements were made inside Dick Blasband's large orgone
>room at the old Elsworth Baker Laboratory in Buck's County, Pennsylvania,
>during summertime afternoons. The door and ventillation window were always
> left open, and the door was sufficiently large as to allow a very free flow
>of air. Both the "exterior" and interior locations of measurement were
>only about 2 meters distance, both being within the shade of a large barn.
>Basically I would make a measurement outside, then step inside the room and
>make another, spaced out over the day.

One thing we never did in our psychrometer studies was transport the instrument outside of its environment for alternate measurements in different environments. Usually we leave the instruments in their environment throughout a course of readings.

>The results were never published,
>though my pilot study on water evaporation did mention it. I should
> undertake another review of the question in my own orgone room here.
>Relative humidity is actually not so interesting as absolute humidity, in
> this context.
>5. Regarding OBRL-News, I did post out an announcement of the IE articles,
>yours and mine both, on May 30th, but have not done anything as yet on your
>new web site -- so much work to do, it is one of many things that has
> simply slipped by. No, I do not feel the need to be in full agreement in
>order to do so, but would like to gain some experimental confirmation of
>your approach so that I could add a note about it to the posting.

Dear James - when we invited you up here it was not to place you in any debt whatsoever. Your counter-potlacht to ours has been generous and we have not asked you for any support. We welcomed your offers and witnessed you retreating from them - for good or bad reasons - it does not matter, all the more so as time elapsed from our meeting. Now, we even feel at times that what you are writing to us is more a question of establishing some form of priority with respect to what we are doing than anything you want to pursue aside from, let us say, putting us on some sort of probation where you reevaluate us as a peer. But you are not our peer, not per se. You are a dear friend who is interested in these matters seriously and made his lifelong effort their study, but you have not made the discoveries we have or have even understood them enough to be able to criticize them as a physicist, as you yourself said often enough. So, here is the thing that strikes us as odd, that somehow you are making any announcement of aetherometry.com based upon your reproduction of one or a variety of experiments (this could run you up to a few years, if you are going to be decent about it, ie about the experiments). We think you would be more honest if you said that you do not want to do it for whatever reasons, our non-Reichian orientation would suffice - even if we believe it is closer to the spirit of Reich than any Reichianism has been. This is something we can live with - your disagreement in whatever analytical or scientific matters. But we think that this never stopped you in the past from announcing a variety of matters.

So, it is odd - and we would like to withdraw our previous agreement to such an announcement precisely not to embarrass you any further.

>I'm struggling to understand your aetherometric theory, which appears to be
> a replacement for orgone in many respects, but I don't think I could make a
>decent evaluation until such time I've gone through all of what you've
>written.

This is fine.

>So far, however, I don't see anything so essentially new as to
>require a re-think of the basic orgonomic theory Reich developed... but I
>will read on and remain open.

We will gladly submit a summary list of what is different and new:

AS2-01 - Isolates the anti-gravitic kinetoregenerative phenomenon of the electroscope, and formally and experimentally demonstrates how it is not accounted for by the electrostatic energy with which the device was charged, as well as applies Carnot's, Aspden's and Reich's treatments of the gravitational pendulum to provide the mass to length conversion (that we all accept Reich discovered but nowhere left it written as the exact equivalence, as we did) and a pendulum treatment of electroscopic work. If Reich or anyone else has understood this property of electroscopes and isolated it, we would like you to tell us where any record of it appears.

AS2-02 - This monograph demonstrates that in open air there are a variety of factors (not just a mythical orgone that serves as a blanket for everything) that affect the electroscopic discharge rate, some independent of charge, others clearly electric. It introduces the reader to the new concepts of leakage and seepage, following the route traced by very classical electrostatic theory, and experimentally proves that one of these factors is nonelectrical and arrests the spontaneous discharge of the electroscopes irrespective of charge. We will not summarize any further these differences - suffice it to say that it provides an integrated functionalist theory of the operation of the electroscope, with respect to both electric, electromagnetic and antigravitic field actions. Where did Reich enunciate such a theory? Or detail the concepts of leakage and seepage, and report spontaneous arrest outside ORACs?

AS2-03 - Demonstrates the error in Reich's concept of the OP and proposes a novel method to measure effectively what amounts to latent heat (nonelectric, nonelectromagnetic latent heat). Where did Reich criticize his own critical orgonometric tool, the OP?

AS2-04 - Demonstrates how no one, Reich included, nor Le Bon - if one wants to read Le Bon - has to this day fully understood the charged states of the electroscope. Reich came very near to a full understanding but failed to wrench it. This paper demonstrates why, and above all, how, we have nevertheless a clear response of living fields to any trapped quantum of massbound charges, that confirms Reich's notion of an energy draw for active processes: the living field draws both the kinetic and the electric energy of charges, and this we have formally demonstrated. Reich, as you know did not believe that electroscopes were charged with massbound charges but with orgone charges. It seems the onus is on Reich or you, if that is a challenge you want to take, to demonstrate that one can trap massfree charges in an electroscope. The entirety of aetherometry to us proves the contrary: one

cannot trap massfree charges because only massbound charges are monopolar. That is simple and easy for us to understand, and demystifies much of what confused Reich.

AS2-05 - Reich did not perform the critical control that addressed Infeld's objection - irrespective of whatever contempt one holds for Infeld. We did, and performed the experiment in as stringent a set of conditions as we could without creating a new and different experiment. And then, through the insufficiency of the shade experiments to address the blackbody problem, we formally demonstrated that the heat evolved inside of the ORACs directly exposed to the sun is not accounted for by any blackbody radiation theory. In fact, we produced the first two spectra of ORACs. We do not recall anyone trying to do this - neither Reich, nor you, or anyone else.

AS2-06 - We applied our aetherometric functionalist theory for the integrated responses of the electroscope to further study the slowing down of leakage discovered by Reich. The result is a totally new way of employing the electroscope to indirectly measure and quantitate the nonelectric latent heat accumulated inside ORACs. With electric means we prove the phenomenon is nonelectric, precisely as Reich claimed but failed to prove to any intense scrutiny. For, had he fully understood it, you would be able to derive the same aetherometric understanding we have, that can be summed up in this way: the OR effect of the ORAC is caused by the trapping of latent massfree thermal energy, fed to trapped massbound charges in order to regenerate their gravitational work, and is transformed into the sensible thermal heat that evolves in the various layers of the ORACs. This OR effect of latent heat is not an electric, thermal or gravitational (not per se) phenomenon. As such it is an effect of the aether in a massfree state, before it feeds the Van der Waals atmospheres of molecules, but it is not directly the effect of OR energy. OR energy, we contend, carries electric charge, which is always ambipolar and deployed in longitudinal waves, and it produces all blackbody radiation by interacting (mostly) with matter. The production of latent heat, sensible heat and all other forms of blackbody photon energy in our atmosphere is a local process, or set of processes, driven by both OR and DOR. But OR and DOR are just subtypes of aether radiation, they do not exhaust the latter, since they are aether radiation in an electric state. If you found these words and explanations in Reich's work, then you might be legitimized in thinking - as you are presently - that there is nothing new in aetherometry. But we wager you cannot find in orgonomy, such as Reich legated it and you understand it, any such aetherometric understanding as we propose. We have provided this, including the exact knowledge of the frequencies of OR and DOR and their cut-off points.

AS2-07 - Here we propose a new method to determine the org Reich sought, employing all the new tools developed above.

AS2-08 -Here we report the photo-induced antigravitokinetic phenomenon which we discovered - and which no one else has! - that could threaten the entirety of the foundations of Reich's claim that OR energy is not electromagnetic, since the electroscopic anomaly would then stand explained by absorption of electromagnetic energy. Yet, we tease that notion out, and show experimentally and with aetherometric tools that the ORAC phenomenon is not photo-induced.

And with this we have barely started laying the foundations of aetherometry. We think your statement above was deeply unfair.

>I do note, however, some needlessly
> provocative statements in your papers directed against the followers of
>Reich.

Here we thought you liked to shake the ossified tree - and now you are chiding us!

>On one level, I can appreciate what you are saying, as there are
>some really stupid and arrogant people laying claim to his heritage (what I
> call the "fight over Reich's bones"), and I share your outrage.

And this is all we meant ever, all those that have turned Reich and his living memory into museums or last-ditch churches of psychobabble where there is no science and no bridges either with the outside world, other than through all the mystical invocations of OR energy or Chi and so on.

>However,
>since you don't make explicit citations of whom you are referring to (as
> with the claim there are "no studies worth referencing") it suggests an
>across-the-board dismissal of everyone save for Reich, including myself.

It is difficult for us to see you having identified yourself in what we say - since we scrupulously avoided commenting on Saharasia or your lifelong cloudbusting experiments, etc (save for your seed experiments) for various reasons, including the fact that you have wanted to keep our relation, so far, away from any public scrutiny. On the contrary, we mentioned that there are lone wolfs that serve as exceptions to the churches, even if ever so few. Now you ask us to state whether or not we include you in any of these comments, and the answer should have been obvious by now that we did not. However, the degree of care with which you have read our material so far - and this recent way of seemingly setting yourself up for some form of imaginary competition with what we are trying to do with aetherometry - we think is not the most productive, nor what one would expect from someone of your caliber. Somehow we sense that you are not acting straightforwardly, as you did before - and one cannot help but sense peer pressure at work. For, if it is a matter of science, you should not act as if we are stepping into your turf, or that you have a turf to defend. You should note instead - as you do with more evasion now - that all that we wrote about the followers of Reich is strictly true.

>Probably that's not your intention, but I question the need to include such
> statements at all. They are a needless distraction from your work, your
>experimental results and contributions. In any case, the followers of
>Reich (myself included, and you also my friends) are so marginalized and
>outside the mainstream, that the people you should be addressing your work
>to won't even know who those persons are.

This is why this way of posing these matters, ie your approach, is not very constructive. We do not see ourselves as followers of Reich, anymore than we are followers of Anaxagoras, and so on. We see Reich as a remarkable factory of desire, one to learn from and work with, but we are followers of nobody. We are in fact disagreeable people who disagree with everybody, Reich, Nietzsche, Deleuze, Aspden, you. And if we do it, it is because we think we do know better and further. We do not think it is fair for you to reduce us in a way we explicitly reject, if for no other reason than to honour and respect the memory of Reich himself who doggedly opposed churches and mysticisms. Aetherometry is new, whether you see this or not. And it is very different indeed from orgonomy as Reich left it and as we all know it. You may disagree from aetherometry and our findings, but if you are choosing to set yourself up as our peer and judge then the least you owe to that same memory is to try

first to understand what we are saying and to reproduce the experiments first with fidelity. Walk steadily before you run.

>My advice is, to make your
>criticisms of other people's work open and explicit, with full citations
>and credit where it is due,

This is sort of a doublebind you are putting us in. Maybe one day we shall name names in that Dictionary of Praised and Insulted Persons, but is that really what you are after? Besides, we are not judges of the field, only analytical operators who state what they see and have found in the areas they work in. With all the biases that must be addressed scientifically but just as well emotionally.

>but without any of the personal element even if
>they have been insulting to you in the past -- otherwise, don't even
> mention or refer to them.

James - you felt our generosity and ease, or so we hope. These attacks or statements are not personal, they do not come from deep wounds or gashes, nor from resentment or vengeance against the world. They are the most impersonal markers of our trajectory - the uncompromising that permits science and its libidinal economics to go on unfettered and fully stated. They are the sign of the irrecuperability of our work - that it does not pactuate with the many just because we live in a dictatorship of silent majorities with entrenched interests that pose as the interests of truth and knowledge. Our statements are analytical ones, and maybe one day we will do more than footnote them and expose each and every one of them. But right now, that is not where our desire lies. Simply put, we do not want any amalgamation with any last-ditch church.

We can only hope that you take this in the best of characters. After all, many of the things you have said are creating a divergence between us which we cannot help notice. If you cannot remain completely honest about your motivations in all this, then there is little sense in continuing what we started as a hopeful alliance, irrespective of our final views on matters.

For all it is worth to you and in the hope of clearing the air,

Warmest regards from all three of us
A, P &M
Subject: Re: Matters at hand
Date: Wed, 22 Aug 2001 21:10:08 -0700
From: J. DeMeo
To: Correa&Correa

Dear Paulo and Alexandra,

Its going to take me some time to read through your materials and undertake replication experiments, and until I do, it won't be possible for me to come to any clear conclusions about your larger theoretical standpoint. You know, I came to be convinced of the essential

correctness of Reich's orgonomy by virtue of extensive experimental evaluations, and by its internal logic and agreement with observed facts. Now, you wish to present new facts, and I am listening. However, at present, I've only read your S2-05 and S2-01. Regarding S2-01, I find your discussion of the electroscopical discharge retardation in terms of antigravitation function to be fascinating... perhaps brilliant, but do not yet see where you go with it, and if something new or deeper emerges from the new theoretical formulations. If so, then you really have something remarkable. If not, then its only an interesting theoretical reformulation, which is much less remarkable.

I have not backed away from your work in the slightest, nor retreated in any manner. Most assuredly, I understand the necessity for pointedly direct critical review -- but from my viewpoint, it is your overall theory which is at issue and being put to the test, and not Reich's. So a question for me is "Does Correa's Aetherometry present new empirical findings and experimental proofs which necessitate the abandonment of Reich's purely orgonomic theory?" So far, from everything you have shown me, and from my readings of the two documents (S2-05 and S2-01) and the IE articles, I must answer "insufficient evidence to render an opinion". So I now tackle S2-02.

I will hold off on detailed replies to your critique below, however, until I can go deeper into the experimental testing -- but will make the following brief points:
* I know exactly and precisely what I mean by the term OR energy.
* I have seen the accumulator charge up and increase the deflection of a slightly-charged electroscope, and likewise have seen discharged capacitors and batteries significantly increase their charge while in the accumulator, while never doing so outside the accumulator. (Burr's work also anticipates fluctuating changes along these lines, but I speak to measured increases of a substantially higher voltage.) Likewise the charging of fluorescent tubes and high-vacuum tubes to spontaneous glow, or glowing merely from being held, with no electrical fields applied and with full groundings, shortings, etc. I consider the non-frictional orgonotic charging of insulators (as per Reich's observations as early as the bion experiments) to be solidly demonstrated -- and so the appearance of small electrical charges from a strong orgonotic charge is to be expected within the framework of Reich's theory. This might also be so within the surface tension measurements of OR charged water (which remains only preliminary). I cannot dismiss the observations merely because of a theoretical objection, or suggestions of artifacts which have no reasonable expectation or independent proof of their exisence. Be assured I am well-versed in classical parameters, so as to run a controlled experiment.
* Re-writing your paragraph so as to indicate you know that increased temperature leads to lowered RH would be advised, as the sentences as written suggest you believe increased temperature will also increase RH.
* The graphs of your temperature measurements do clearly show the absence of measuremnets in the morning hours -- I'll photocopy a page and mail it to you to clarify the matter. If you have the measured data entered on those graphs, then the time-placement of the data appears off.
* My points about solar IR creating a mechanical heating of the top metal plate should not be so easily dismissed as it is a parameter of classical thermodynamics which stands as an objection to any other interpretation -- but on this matter, I concede the possibility we are talking past each other, and maybe have the same thing in mind. The IR signatures of forest fires versus the sun is not the issue, as both create a significant IR which gets partly reflected

by sheet metal, but also partly absorbed and converted into sensible heat, which can create slight thermal convection both above and below the metal.

I'm off for a few days of relaxation, but will thereafter return to the temperature measurements. This will probably take more time than before, as there are no student helpers.

Warm regards,
James

PS. On the matter of "shaking up the orgonomic tree", what I meant to say is, this is best done by letting the facts of one's work speak for themself.

****

Subject: Re: Matters at hand
Date: Thu, 23 Aug 2001 16:43:11 -0400
From: Correa&Correa
To: J. DeMeo
References: 1 , 2

Very well James, we await such time as you can respond in detail to the points of our last message, once you will have read through our materials.

James DeMeo wrote:

>Dear Paulo and Alexandra,

>Its going to take me some time to read through your materials and undertake
>replication experiments, and until I do, it won't be possible for me to
>come to any clear conclusions about your larger theoretical standpoint.
>You know, I came to be convinced of the essential correctness of Reich's
>orgonomy by virtue of extensive experimental evaluations, and by its
>internal logic and agreement with observed facts. Now, you wish to present
> new facts, and I am listening. However, at present, I've only read your
>S2-05 and S2-01. Regarding S2-01, I find your discussion of the
>electroscopical discharge retardation in terms of antigravitation function
>to be fascinating... perhaps brilliant, but do not yet see where you go
>with it,

Alright, you will see the applications in AS2-02, -06, -07 and -08.

>and if something new or deeper emerges from the new theoretical
>formulations. If so, then you really have something remarkable. If not,
>then its only an interesting theoretical reformulation, which is much less
>remarkable.

You tell us.

> I have not backed away from your work in the slightest, nor retreated in
>any manner.

Our perception is that you have - for instance, your original offer of permitting us do whatever we wished with your letter re the Aether Motor demo, became later qualified as a matter for private investors only; your offer to write something, with you or not, for the Pulse of the Planet, was suspended; and so was your offer to run our ad in your list. Anyway, we are not in any way upset with you because of this, but you must concede with some honesty that here and there you have retreated. This is, by the way, fine with us because we ourselves hope to have acted in such a way that we were not in any way forcing your hand.

> Most assuredly, I understand the necessity for pointedly
>direct critical review

We welcome that; what we do not appreciate are comments and criticisms based on an inattentive reading of what has been said. This only muddies the water and wastes time unnecessarily.

> -- but from my viewpoint, it is your overall theory
> which is at issue and being put to the test, and not Reich's.

Undoubtedly our theory and experiments are at stake - but so are Reich's theory and the entirety of currently accepted Physical theories. Moreover, whatever it is we claim to have found or discovered in the present monographs, was found in the process of testing Reich's theory and sorting what is error from fact in it. So, one way or the other, Reich's theory is also at stake.

>So a
>question for me is "Does Correa's Aetherometry present new empirical
>findings and experimental proofs which necessitate the abandonment of
>Reich's purely orgonomic theory?" So far, from everything you have shown
>me, and from my readings of the two documents (S2-05 and S2-01) and the IE
>articles, I must answer "insufficient evidence to render an opinion". So I
> now tackle S2-02.

The ball is in your court. Do read on.

>I will hold off on detailed replies to your critique below, however, until
>I can go deeper into the experimental testing -- but will make the
> following brief points:
>* I know exactly and precisely what I mean by the term OR energy.

Then it should not be a problem for you to provide the frequency and energy spectrum of orgone energy, since you know what you mean in an exact and thus quantitative way.

>* I have seen the accumulator charge up and increase the deflection of a
>slightly-charged electroscope, and likewise have seen discharged capacitors
> and batteries significantly increase their charge while in the accumulator,
>while never doing so outside the accumulator.

This can hardly be taken as acid 'proof' of orgone, James. And besides, it is simply not true. The phenomenon of 'spontaneous regeneration of charge' was first discovered in capacitors by Faraday and that of the regeneration of batteries was discussed by Thomson. Whittaker describes Thomson's principle as applying to certain classes of batteries where the whole electric energy is ""procured at the expense of the thermal energy of the cell's surroundings" and, in AS2-07, we extend the principle to encompass not just sensible heat but also latent heat. Charge regeneration in either capacitors or batteries has been regularly observed by us outside of any ORAC or Faraday cage structures, as it was by those who first observed them. And what is clearly established since Thomson's equation is that whatever feeds the regeneration of electric charge, is not electric energy itself. Whether the phenomenon involves charge condensation or regeneration of the kinetic energy of charges trapped in dielectrics remains an open question, specially in light of the fact that capacitors operate by setting up a reverse energy potential that sequesters charge. We ourselves have put forth (in our AS2-09) a physical and mathematical process precisely for the asymmetric generation of condensed massbound charges of one polarity or another. As for the transformation of sensible heat and electromagnetic energy, or latent heat for that matter, into the kinetic energy of massbound charges, we too have put forth new physical and mathematical methods in aetherometry, beginning with AS2-01, and not being complete until we publish our work in electrodynamics.

Moreover, it seems to us that what you are claiming is indeed that the ORAC is a source of negative charges (presumably massfree?). This flies in the face of what Reich himself wrote in the Cancer Biopathy, that "orgone energy is something other than negative electricity" (whether massbound or massfree). Incidentally, he was not legitimized in making this conclusion because he never studied whether or not the ORAC accelerated the discharge rate of positively charged electroscopes; hence he was only entitled to conclude that orgone was not positive electricity. More importantly, it seems that if this is so - as you argue it - then the onus is on you to show that an ORAC can charge an electroscope with negative charges, slow down or arrest a negatively charged electroscope (ie the leakage rate) and accelerate the rate of a positively charged electroscope (ie the seepage rate). And if you claimed instead that the ORAC was a source of positive charges, you would have to demonstrate just the opposite. We have done these experiments extensively and our results are conclusive: whatever accumulates inside the ORAC is not electric energy, and is certainly not monopolar electric energy of one polarity or the other. Moreover, we contend that there are no massfree monopolar electric charges. We hope you will address this simple set of problems when you are ready (after all, as you are ready in principle as a scientist to attribute the thermal anomaly to a lag response in absorption of IR electromagnetic energy - and thus dissolve the very existence of the thermal anomaly per se - so should you be ready to demonstrate that the electroscopic anomaly is after all just the result of the accumulation of one type or the other of electric charges inside the ORAC; if you were right on both counts, because effectively you demonstrated such and such quantitatively, then you should also explain to us what possible need you would still have for the notion of an OR energy??).

> (Burr's work also
>anticipates fluctuating changes along these lines, but I speak to measured
>increases of a substantially higher voltage.) Likewise the charging of
> fluorescent tubes and high-vacuum tubes to spontaneous glow, or glowing
>merely from being held, with no electrical fields applied and with full

> groundings, shortings, etc.

Yes, we too have done these experiments, and have remarked that, unless the body is insulated and can electrically charge an electroscope, it does not trigger any such lumination. It is solely a matter of whether the body is charged or not with those Burrian electrostatic potentials which are, when all is said and done, merely secondary byproducts of what is effectively the energy of the living.

>I consider the non-frictional orgonotic
>charging of insulators (as per Reich's observations as early as the bion
>experiments) to be solidly demonstrated -- and so the appearance of small
>electrical charges from a strong orgonotic charge is to be expected within
> the framework of Reich's theory. This might also be so within the surface
>tension measurements of OR charged water (which remains only preliminary).
> I cannot dismiss the observations merely because of a theoretical
>objection, or suggestions of artifacts which have no reasonable expectation
> or independent proof of their exisence.

Surely that is not what we have put forth - on the contrary, what we have done is a rigorous experimental demonstration (not performed by Reich) that the effect of the ORACs is not an electric one. As for the framework of Reich's theory, maybe you could point out where Reich provides a physical and mathematical process for the creation of monopolar electric charges (massfree? massbound?) from orgone energy? Yes, he argues that orgone is both a source of electricity and inimical to it, depending on the experimental arrangement, but he never argued that the ORAC effect is to produce electric charge of any type whatsoever. He is so convinced of this that he renames the electroscope orgonoscope, to argue that it traps orgone charges. What are these orgone charges? Maybe you can answer - since we hold, as does the rest of physical science, that the charges trapped in the electroscope leaf-system are monopolar massbound negatrons or charged lattice atoms.

>Be assured I am well-versed in
>classical parameters, so as to run a controlled experiment.
> * Re-writing your paragraph so as to indicate you know that increased
> temperature leads to lowered RH would be advised, as the sentences as
>written suggest you believe increased temperature will also increase RH.
>* The graphs of your temperature measurements do clearly show the absence
>of measuremnets in the morning hours -- I'll photocopy a page and mail it
> to you to clarify the matter. If you have the measured data entered on
> those graphs, then the time-placement of the data appears off.

It would help if you referred concretely to specific figures, as we did in our last message to you. It would also help if you addressed in detail our previous points in that message, since the argument we put forth with respect to blackbody radiation already arises for an ORAC directly exposed to the sun in daylight hours alone.

> * My points about solar IR creating a mechanical heating of the top metal
>plate should not be so easily dismissed as it is a parameter of classical
> thermodynamics which stands as an objection to any other interpretation --
>but on this matter, I concede the possibility we are talking past each

>other, and maybe have the same thing in mind. The IR signatures of forest
>fires versus the sun is not the issue, as both create a significant IR
> which gets partly reflected by sheet metal, but also partly absorbed and
>converted into sensible heat, which can create slight thermal convection
> both above and below the metal.

When we are trying to make measurements, significant means nothing. The profile in question is that of the solar blackbody and not of any fire. The solar blackbody is rather well known. And it cannot quantitatively explain the evolved heat inside the ORACs. If you think it can, you must be the one demonstrating that it does.

>I'm off for a few days of relaxation, but will thereafter return to the
> temperature measurements. This will probably take more time than before, as
>there are no student helpers.

>Warm regards,
>James

>PS. On the matter of "shaking up the orgonomic tree", what I meant to say
> is, this is best done by letting the facts of one's work speak for
> themself.

They are speaking for themselves (provided, of course, that they are publicized, and read with understanding). But it would be astonishing if you gave Reich the same advice at any time in his life - to limit himself to publishing his work and neither attack false theories and enshrined reactionary interests, nor defend his work from attack, whether it was a public or a surreptitious attack. He too made a point of surrounding his science with precisely the same politics of deconstruction and uncompromise that we desire to pursue, and are firmly convinced this synthesis is one of the conditions preventing the recuperation of our own work by last ditch churches.

In the hope of a more enlightened interchange with you in the near future, and wishing you a good vacation -

Alexandra & Paulo

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Subject: Re: Matters at hand
Date: Tue, 28 Aug 2001 12:34:09 -0700
From: J. DeMeo
To: Correa&Correa
CC: <webmaster@aetherometry.com>

Dear Paulo, Alexandra

I just returned from several resting days at the oceanside, and had the opportunity to read through most of your papers in the S2 series. My thought is to write up a review for you, and email it in about a week. I can give some initial impressions here, however.

Firstly let me say, you have undertaken an enormous amount of work and careful thought on this subject, and with an ambitiously broad scope which is rare in any scientific circle. Personally, I very much enjoy and like this broad approach, and take it myself also, but as you know it also "increases the stakes" across the board. As Reich said "we're not exactly playing for peanuts here". So it becomes all the more crucial to get it right. And so, I reviewed your materials in detail, looking for new findings and lines of evidence, as well as problems from both the conventional points of view, and from the viewpoint of Reich's orgonomy.

Some questions naturally came up, which I'll ask you about. However, your conclusions retain their significance only if the basic data upon which they rest is well-founded and secure. And that is where I have some serious and significant criticisms to make. There is probably much more to come in your publication series, which might force the dropping away of some of my concerns. However, as things now stand, there appear to be methodological errors which, if I am correct, undermine several key assumptions of your overall Aetherometry theory, which then loses its impetus as a critique against Reich's original orgonomy. I also have concerns about your presentations of Reich's viewpoint, and areas where it seems you have simply re-named phenomenon aetherometrically which were originally described by Reich orgonomically -- but these concerns either float or sink based upon the methodological arguments.

I will present the details to you in a subsequent email composed from my margin notes, and that will take some days, but here wish to address a few points in your most recent email.

>>I have not backed away from your work in the slightest, nor retreated in any manner.
>
> Our perception is that you have - for instance, your original offer of
>permitting us do whatever we wished with your letter re the Aether Motor
>demo, became later qualified as a matter for private investors only; your
>offer to write something, with you or not, for the Pulse of the Planet,
>was suspended; and so was your offer to run our ad in your list. Anyway,
>we are not in any way upset with you because of this, but you must concede
>with some honesty that here and there you have retreated. This is, by the
>way, fine with us because we ourselves hope to have acted in such a way
>that we were not in any way forcing your hand.

As I recall, my only hesitation, expressed from the very start, was about internet posting of my letter. Regarding Pulse of the Planet, since the seminars started some months ago, I have been forced to suspend work on it, or even thinking about it. As was the case with OBRL News. Nevertheless, I concur that it is probably best to simply wait on the matter and see what develops from our discussions. I clearly do not feel comfortable making an endorsement of such a large theoretical departure from Reich's original positions, especially when major errors appear to be underlying that departure.

>>Most assuredly, I understand the necessity for pointedly direct critical review
>
> We welcome that; what we do not appreciate are comments and criticisms
>based on an inattentive reading of what has been said. This only muddies
>the water and wastes time unnecessarily.

I shall try to be as clear as possible, asking for the same in return.

>>* I know exactly and precisely what I mean by the term OR energy.
>
> Then it should not be a problem for you to provide the frequency and
>energy spectrum of orgone energy, since you know what you mean in an exact
>and thus quantitative way.

Here, you would require a response which is in keeping with your own theoretical framework. Since I do not accept that orgone possesses (or has to possess) any special frequency or "energy spectrum" easily defined from the classical viewpoint, I cannot accept the validity of your question-challenge. However, as an aside, a good argument can be raised for orgone's relationship to the 3 deg. cosmic background, as the source of "dark matter" estimations, of "neutrino" misinterpretations, as a possible source of natural sferics frequencies, and a few other such things which are highly mysterious from the classical viewpoint (or only "explained" mystically, as per big-bang creationism), but these are as yet only theoretical formulations. More on this later.

>>PS. On the matter of "shaking up the orgonomic tree", what I meant to say
>>is, this is best done by letting the facts of one's work speak for
>>themself.
>
> They are speaking for themselves (provided, of course, that they are
>publicized, and read with understanding). But it would be astonishing if
>you gave Reich the same advice at any time in his life - to limit himself
>to publishing his work and neither attack false theories and enshrined
>reactionary interests, nor defend his work from attack, whether it was a
>public or a surreptitious attack.

While we both have suffered a lot of insults and censorships, neither of us have gone through what Reich did, nor can Reich's followers be compared to the Nazis or CP or FDA fascists who tried and eventually succeeded in destroying him. There are other problems with Reich's "followers" but they are structurally different. I would ask, do you consider Reich's orgonomy to be a "false theory" (as per your comparison above)? Yes, it does alarm me to read sentences which suggest you hold all the published verifications of Reich's work in exceedingly low esteem. Do you really believe, the various published papers in the Journal of Orgonomy, Annals of the IOS, and my Pulse of the Planet, as well as the German journals Emotion and Lebensenergie, are worthless? Factually, these are filled with verifying empirical reports on nearly every aspect of Reich's orgonomy, going back many years. None of it is beyond scientific critique, but that's a different matter from simply tarnishing everything as worthless. That's a different matter from responding to criticisms in a separate context, as you did with Marrett on the PAGD work. Or to Ogg by Malgosia in the recent email. That, I have done myself many times, and it is important to do so, to set the record straight. The only "Reich followers" openly cited and critically discussed in your S2 series was Mann and Barth (on the UV question), whom I would hardly consider to be scientific advocates of Reich. Paulo, Alexandra, you know I also had some horrible experiences with

the Reich-groups, and also have my own serious questions about some of the things they have published in their journals, but the journals mentioned above are filled with validating material, and to say there is "nothing worth referencing", that is simply not a factual statement.

That's all for today -- I hope you share with me the attitude and approach, to let the empirical and experimental facts determine the truth of the matter, whatever that may be. While you probably will disagree, I consider your aetherometric theory to be of a far lesser importance than the work you have done on the orgone motor (and possibly also on PAGD, but my knowledge on that matter is more limited). If you are successful to realize the social application of this motor force, and if the principle is not lost once again as it was when Reich died, you will have made a powerful contribution to the well-being of our small planet, irrespective of the theory used to explain it.

Best wishes,
James

****

Subject: Re: Matters at hand
Date: Thu, 30 Aug 2001 22:27:28 -0400
From: Correa&Correa
To: J. DeMeo

Dear James,

We can certainly confirm two matters raised in your letter of Tuesday: - what we have published to date is, in fact, just the tip of the iceberg - and we are indeed "not exactly playing for peanuts here".

Our comments in AS2-05, which seem to be causing you an inordinate amount of difficulty, concern the lack of physical studies relevant to this (AS2-05) study of the thermal anomaly in ORACs, in the context of the Reich-Einstein experiment and the direct solar exposure ORAC experiments, with their implications for blackbody theory. So your question regarding what you present as our assessment of the worthlessness of "all the published verifications of Reich's work" makes little sense to us. If you think that there is to be found in the Journal of Orgonomy, Annals of the IOS, your Pulse of the Planet or the German journals Emotion and Lebensenergie any pertinent rigorous, scientific papers which we overlooked in preparation of this paper, perhaps you would be so kind as to send us a copy (or copies). We will be more than pleased to evaluate whether or not our statement - in the context in which it appears - needs to be reevaluated.

Your tentative appraisal that we "have simply re-named phenomenon aetherometrically which were originally described by Reich" strikes us as such a preposterous reductionism - that we will just leave our response to it - for now - at that. As for what you allude to as our significant 'methodological errors' which may 'undermine several key assumptions' - we must say that, based on the scant and fragmentary commentary you have so far provided us, which

has seemed to us to be based more on what we have not said than on what we have said- we have greater reason to suspect that these are more likely errors in your reading and comprehension of the subject matter than in our methodology. Nevertheless, we will keep our minds open and if you are true to your word and provide us a careful, annotated set of precise responses which concretely address our protocols and analyses, we promise to give them our full attention.

Now if you carefully reread our last letter, which you quoted, you will notice that what we wrote was:

>But it would be astonishing if
>you gave Reich the same advice at any time in his life - to limit himself
>to publishing his work and neither attack false theories and enshrined
>reactionary interests, nor defend his work from attack, whether it was a
>public or a surreptitious attack.

We made no comparison here of our lives with that of Reich's. Nor do we see the relevance of doing so. ("While we both have suffered a lot of insults and censorships, neither of us have gone through what Reich did". We would, however, caution you against making unfounded assumptions regarding our lives, of which, in fact, you still know very little.) Be this as it may, our comment above addressed solely what we perceive as somewhat of a double standard you were proposing in questioning our practice of 'frontal assault' which for us is integral to any healthy process of discovery as it confronts well-entrenched belief systems. And, unless one chooses to impute to Reich's life some divine, or some unapproachable element, it seems obvious that the myriad of forces which - in one way or another - arise to oppose any knowledge of the functions of nature (yes, even from the camps of 'decent people') are no less real, no less violent or malevolent in the life of one thinker than in the life of another. And no less worthy of an intractable fight.

> I consider your aetherometric theory to be of a far lesser importance than the work you
>have done on the orgone motor

You might wish to consider instead the possibility that Reich was able to bring his OR motor to turn precisely because his understanding of the complex functions he was dealing with had begun to move beyond his (always tentative) earlier premises regarding 'orgone'. (To this, we would add, that no one, to our knowledge (aside from Aetherometry), has any idea of what he was beginning to drive at in his Contact with Space). That no one, since the time of Reich's death has substantially advanced upon these early premises - and that they have instead remained frozen, fixated and enshrined as a final truth - or untruth, depending on one's perspective - is only underlined precisely by the fact that the motor has remained (to this day) a perplexing riddle. The fact is, without Aetherometry, neither the OR motor nor the improved aether motor would today exist. They have arisen only and solely as a result of the understanding provided by Aetherometry, and not the other way around. And we can assure you that working solely on the scant written records left behind by Reich will never directly lead to either one - nor to any functional understanding of the notion of a 'motoric force' nor what the extraordinary significance of 'cosmic engineering' might entail. The fact of the matter is that the aether motor is only one of a constellation of small signposts which, to us, unequivocally indicate both the accuracy and the utility of Aetherometric theory - it is only a tip of the iceberg.

You wrote -

>I would ask, do you consider Reich's orgonomy to be a "false theory"

We find this such a curious question that we are somewhat at a loss as to understand how you could ask this - if you are indeed reading the monographs - for we think our position is rather clear. Do we think Reich's theory qualifies effectively as a scientific theory? Yes, it does. Can that theory be put to good uses - yes it can. Does it make mistakes and is it incomplete. Yes it does and yes it is. Because it is a useful theory, one can employ it as a lens to read some of the finer text of nature. But the task of science is to come up with better lenses that have learned from the experience of previous lens-makers. In doing so, science must err as much as correct the errors of previous lenses. However, what we here say about Reich's theory hardly applies to his followers in general - in their mouths, most of the concepts, the functions and the subtleties were lost along with the exact knowledge. And their interpretations have most often objectively falsified what was right in Reich's theory, not to mention what was noble and shone so brightly.

We would also encourage you to carefully rethink your position with respect to our question concerning the frequency and energy spectrum of orgone energy.
>Here, you would require a response which is in keeping with your own
>theoretical framework. Since I do not accept that orgone possesses (or has
>to possess) any special frequency or "energy spectrum" easily defined from
>the classical viewpoint, I cannot accept the validity of your
>question-challenge.

For the answer we sought could be compatible with any theoretical framework, except perhaps for one defined by a truly classical viewpoint when energy was poorly understood and the quantum frequency unknown. What is somewhat more troublesome - for science in principle - is to refer to energy and not accept that it must have magnitude, or that there is somehow energy and no characteristic functions, be these wavespeeds, wavelengths or frequencies, etc. If these functions exist at all, then, whether we like it or not, energy forms spectra. If the only spectrum there was were electromagnetic, orgone energy would simply not exist. How you wrap your arms around these facts - so much emphasized by Reich himself - which our question-challenge poses, escapes us. If orgone energy exists it must also have the physical characteristics of energy, no? What then do you imagine Reich was wrestling with in his mathematical descriptions of the 'energy of the swing'? What could possibly be the relevance of the mass to length conversion?

Finally, you wrote:

>I clearly do not feel comfortable making an
>endorsement of such a large theoretical departure from Reich's original
>positions, especially when major errors appear to be underlying that
>departure.

It cannot but strike us as funny that, on the one hand, you say that we "have simply re-named phenomenon aetherometrically which were originally described by Reich" and, on the other, you impute to us "such a large theoretical departure from Reich's positions" that

you have trouble, as you put it, "endorsing" it. How do we manage to do both at the same time?

Of course, as we said above, we will wait to see what you perceive to be these 'major errors' in our departures. But in closing, let us say only that we never expected you would give or should give any sort of an 'endorsement' of any part, let alone the whole, of our effort. We thought, following your visit here, solely that it might have been interesting for all of us to use the OBRL announcement list to draw some attention to our site- since it puts forth systematic new approaches that concern your own fields of interest, as well as those of your readers - and which others might want to know about - - regardless of whether they or you would ultimately agree with its contents or not. In other words, to generate discussion. This, it would seem to us, is the point of the open exchange and discussion of information. In any case, it is forgotten now and we need not speak of it further.

Best Regards,

Alexandra & Paulo

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Subject: Constructive Critique
Date: Mon, 3 Sep 2001 21:36:08 -0700
From: J. DeMeo
To: Correa&Correa

Dear Paulo and Alexandra,

Attached is a summary critique developed after an extensive review of your S2 publications at the Aetherometry web site. It is much longer than I had intended, some 13 pages, but then, your own materials were quite extensive and detailed, raising many issues. The two versions are the same, one being sent in rtf.

I find your work to be fascinating with some real contributions, and certainly it has stimulated my own thinking and revisiting of some older experiments which I've left idle since my university years. Many of the questions you raise have not been fully clarified in the years since Reich's death, and this alone is an important contribution. I truly want to see your work succeed, to help bring about a radical shift in both academic and industrial power, and to provide additional bullet-proof validation of Reich, even if it comes with a differing set of theoretical premises. However, as metioned previously, there are problem areas and points of debate. I've covered these, and given some of my own insights as well -- I hope they are useful.

Please take your time on these materials, as for the next several weeks I've got a ton of work to do, with a working trip to Europe at the end of the month.

I hope your summer is pleasant, and that the articles in IE have stimulated some new contacts to bring you closer to your goals.

Best wishes,

James

Correa_Critique
Name:
Correa_Critique
Type: Macintosh BinHex Archive (application/mac-binhex40)

Correa_Critique.rtf
Name: Correa_Critique.rtf

Subject: Re: Constructive Critique
Date: Mon, 08 Oct 2001 20:03:13 -0400
From: Correa&Correa
To: J. DeMeo

Dear James,

We hope you had a good vacation and stay in Europe. We have now had a chance to read over your thought-provoking Constructive Critique. Since the critique is laid out as if it were intended for publication, we gather that, unless you have already published it, this is the text meant to accompany your announcement of our website? Are you planning to do so -- in OBRL or Pulse of the Planet or other?

Best wishes

Alexandra & Paulo

****

Subject: Re: Constructive Critique
Date: Thu, 11 Oct 2001 20:36:36 -0700
From: J. DeMeo
To: Correa&Correa
References: 1

Dear Paulo and Alexandra,

I have no plans to publish my critique, and have not given it to anyone else to read, save for my wife D. At present, it is for your files only. If pressed by scientific people, I would privately express my opinions, but could only anticipate publishing the materials if your own papers on aetherometry, as they exist now, became widely known and thereby stimulated a crisis against Reich's original orgonomy. Unfortunately, even with Eugene Mallove's support, I doubt if anything defending or even significantly referencing Reich will get such attention, no matter what theoretical formulations are offered. From the strictly classical perspective, your aetherometry will be as distasteful as Reich's original orgonomy, and it therefore would serve no useful purpose for my critique, and any rebuttal you might generate, to proceed publicly. Probably, the only way you'll get significant public attention is by making a stand-alone, practical demonstration unit of the aether-orgone motor, as per Eugene's suggestion -- and on that matter you continue to have my support.

It is really an open field for you to proceed however you wish, for better or worse -- as I've said before, Reich's orgonomy is sufficiently robust and supported to withstand such controversy, and I would defend your right to undertake a new theoretical reformulation along the lines of aetherometry even while making criticisms of it. Over the next months, I hope to get back to To-T evaluations, and possibly to the electroscope-humidity question, in line with what's in my critique. Even a few days of tests during the critical AM periods, and measurements with proper IR-frequency sensors, will cut through a lot of theoretical discussions.

The trip to Europe was relaxing and good to get away from the chaos of work, though the social catastrophe cast a shadow over everything and everyone.

Best wishes,

James

****

Subject: PS to my last email
Date: Thu, 18 Oct 2001 16:09:25 -0700
From: J. DeMeo
To: Correa&Correa

PS. Much sooner than anticipated, I am now getting inquiries from various scientists interested in Reich's works, asking questions about your work which require a straightforward answer. Consequently, I must ask if you intend to openly address the points in my Critique, making appropriate changes in your papers and revisiting the experimental work to clarify things sufficiently, or if you intend to disregard it? My impression, from recently reading your reply to Dr. Baker in IE #38, is that you must believe there's nothing worth considering in my Critique.

****

Subject Re: PS
Date: Sat, 20 Oct 2001 00:49:52 -0400
From: Correa&Correa
To: J. DeMeo

Dear James -

We have been terribly busy demonstrating to prospective sponsors, and did not have a chance to respond to you until now. Your last e-mail also indicated that you were in no hurry to make your criticism of our work public. But now you say:

>PS. Much sooner than anticipated, I am now getting inquiries
>from various scientists interested in Reich's works, asking
>questions about your work which require a straightforward answer.

We presume these questions concern the publicly available material, and not the subject of our confidential disclosures to you, no?

If they concern the public material, we see no problem with you being straightforward with them. We cannot see what would stop you. However, you add:

>Consequently, I must ask if you intend to openly address
> the points in my Critique, making appropriate changes in your
>papers and revisiting the experimental work to clarify
>things sufficiently, or if you intend to disregard it?

We had intended to spend some time to address the points in your critique, but for your consumption alone. We have not yet had the chance to do so. It was not our intention to disregard it.

>My impression, from
>recently reading your reply to Dr. Baker in IE #38, is that you must
>believe there's nothing worth considering in my Critique.

You might want to note that the response to Baker was written in early August, while we were bathing and sunning abroad, and well before we received your extensive critique.

But since you insist on making it public, we could follow your advice:

"It is really an open field for you to proceed however you wish, for
better or worse -- as I've said before, Reich's orgonomy is sufficiently
robust and supported to withstand such controversy, and I would defend
your right to undertake a new theoretical reformulation along the lines
of aetherometry even while making criticisms of it."

Let us then say that we publish both your critique and our counter-critique on our website. Will that satisfy you?

Best wishes

Alexandra & Paulo

****

Subject: Re: PS
Date: Sat, 20 Oct 2001 22:27:08 -0700
From: J. DeMeo
To: Correa&Correa

Dear Paulo and Alexandra,

The questions I am getting are about the publications on your web site, and in Infinite Energy. The primary thing that is alarming people, myself included, is the misrepresentation of a very large body of published scientific literature supporting Reich as being worthless. And it is being done on the foundation of your own to-t and electroscopical experiments, which as discussed in my critique, have major problems and are themselves inadequate to support your conclusions. If those problems didn't exist, you'd have a stronger argument to make -- but they do exist.

This is an entirely different matter from your re-discovery of the orgone motor principle. I've told a few persons about witnessing your positive demonstration of the KS-type spinner motors, but no previously unknown details were discussed.

There are no plans to publish my critique, but given the importance of the issues, when asked I will give people my opinion about both the strong and weak points in your papers. I look forward to reading your counter-critique, and hope that you are getting the support you need for bringing the motor principle into commercial production.

Best wishes,

James

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Subject: Re: PS and more.
Date: Sun, 28 Oct 2001 01:49:29 -0400
From: Correa&Correa
To: J. DeMeo

James -

Like the proverbial lady, you do protest too much - and this is starting to seriously annoy us. Who are these people that are so alarmed?? Why do they hide behind your skirts? Don't they have a name and a face? Are they cowards or are they figments of your imagination??

>The questions I am getting are about the publications on your web
> site, and in Infinite Energy. The primary thing that is alarming
> people, myself included, is the misrepresentation of a very large
> body of published scientific literature supporting Reich as being
> worthless.

We again (presently) challenge you (as we did in our letter to you of August 30th) to provide a single reference (Reich included!!!) to the following, prior to our own publications:

1. A stringent replication of the Reich-Einstein experiment with a naked ORAC.

2. A demonstration of the nonequivalence of the work performed by charge against charge with the work performed by trapped charge against terrestrial gravity.

3. An electroscopic study of atmospheric leakage AND SEEPAGE rates.

4. A single experimental and comparative test of leakage AND SEEPAGE rates inside ORACs.

5. A physical and mathematical demonstration of the uselessness of the concept of OP.

6. A formal and experimental demonstration of two different actions of reverse potentials at work in living systems, the ground and water - radiative nonelectric draw versus electric contact draw.

7. A readily reproducible method for the determination of the charged states of the electroscope - including the identification of the novel contact-control charging method.

8. A demonstration, both experimental and formal, that blackbody radiation does not account for the sensible heat observed to evolve inside and above ORACs.

9. A comparative study of black versus white ORACs.

10. A demonstration of the functional energetic equivalence of the org.

11. The experimental discovery of the kinetoregenerative power of LFOT photons.

12. A re-examination of the Hallwacks effect, and the discovery that it also affects seepage rates, unlike what is accepted by the current theory of the photoelectric effect.

13. A study demonstrating that OR energy induces LFOT photons and DOR energy induces HFOT photons, both of which are distinct, formally and experimentally, from ionizing radiation, with their limits being provided along with a new functional mathematical theory of massfree energy, latent heat, gravitation and sensible heat.

14. A chemical study of the allotropic cycle of oxygen and water, where enthalpies are balanced and DOR-induced HFOT photons as well as OR-induced LFOT photons are identified.

15. A physical and mathematical process for the asymmetric creation of massbound monopolar charges arising from secondary superimposition of identified Aether energy elements.

Even one reference for each of the above would suffice! To this day, of course, you have failed to do so. We could also provide you with a list of the theoretical discoveries of Aetherometry. We will not bother. Save for challenging you to provide a single reference that gives the quantitative transformation of mass into length, such as we do!

>And it is being done on the foundation of your own to-t and
>electroscopical experiments, which as discussed in my critique,
>have major problems and are themselves inadequate to support your
>conclusions. If those problems didn't exist, you'd have a stronger
>argument to make -- but they do exist.

Whatever problems remain with our work affect none of the above claims. Your 'critique' was neither an adequate critique, nor a constructive one. It was thought-provoking alright, but by the movement of its recoiling into an irrational orthodoxy - by its exemplification in vivo of the very ossification of Orgonomy that we spoke of when you visited our laboratory. It was, in this respect, quite mesmerizing to see you perform such a treacherous volte-face.

>This is an entirely different matter from your re-discovery of the
>orgone motor principle. I've told a few persons about witnessing
>your positive demonstration of the KS-type spinner motors, but no
> previously unknown details were discussed.

In accordance with the NDA you signed with us - and confirmed verbally on several occasions during those days you spent here - you should have informed us of who these people are and when they were contacted. Since we are not contemplating doing business with you, we cannot see why you would have to speak about this at all with anybody - save Dr. Mallove. Moreover, you are also misrepresenting both us and Reich - what we demonstrated to you was both our rediscovery of the Orgone Motor and our invention of an Aether Motor that differs in fundamental ways from Reich's Orgone Motor. It is this sliding about of your positions and statements that worries us, given that it stems, at best, from your lack of clarity in general, and at worse, from a desire to harm us, harm the cause of a science of the Aether, and unwittingly harm Reich's work itself.

Which leads exactly to another sliding about or oscillation of yours:

>There are no plans to publish my critique, but given the importance
>of the issues, when asked I will give people my opinion about both
>the strong and weak points in your papers.

First you send a PS saying that 'matters are degenerating' because you are accosted by people (sounds like you are some People's Kommissar) and you virtually demand that we 'openly' (sic) address your 'critique'. Next, you simply say you have no plans to publish, when you have already said that you would publish if you were pressed...'by people'. Which is which? This is like the offers to write something together for your PoP, or to put a note about our

website on the OBRL. So much sand, smoke and mirrors, all to mask your oscillations, or worse.

> I look forward to reading your counter-critique, and hope that you
>are getting the support you need for bringing the motor principle
>into commercial production.

No thanks to you.

Indeed, as you are well aware, it is our intention to make our papers on Aetherometry, whether experimental or theoretical, _as widely known as possible_. We have just started this task. Further, it strikes us that, had you intended your criticism to be reflected in modifications to our publications (as you now demand, with some gall!!!), you would have made it during the month or so you had a chance to read the material before it went public - and you would not have formalized it the way you have, nor expressed a condition for its publication that hinges on making our materials, as they are, _widely known_. Curiously, you are convinced that, were Aetherometry to become _widely known_, it would "stimulate a crisis against Reich's original orgonomy". This is not only a very thinly veiled insult but a falsity as well. Like all disinformation, it contains a grain of truth: that Reich's orgonomy would be integrated and superseded. But that is not what you mean -which is, rather, that the pathological versions of Reichianism now extant will be obliterated. And this is, in fact, what you fear.

For, we are entirely convinced that what is harming an understanding of Reich's orgonomy are the misinterpretations that surround his work, the mysticism and the confusionism that go hand in hand with precisely the kind of poisonous behaviour and bogus 'criticism' that you have exhibited in your 12 page attack on Aetherometry. Though you pose as defending Reich's original Orgonomy, the fact is that what you put forth is only another anorgonomic version of the same - and when it comes to being defended, Reich is better defended by Aetherometry than by such bankrupt iterations of Anorgonomy.

Particularly significant and disingenuous in your most recent letter is the cynicism of your statement:

"Unfortunately, even with Eugene Mallove's support, I doubt if anything defending or even significantly referencing Reich will get such attention, no matter what theoretical formulations are offered."

First, you gloss over that this is not about theory, but about science, which means theory validated by experimental facts. And these are the same facts that your supposedly 'constructive' criticism purposefully glosses over (matters like the leakage-seepage comparison, the antigravito-kinetoregenerative phenomenon, the identification of a solar-sourced variable that tends to arrest the spontaneous electroscopic discharge, etc, etc).

Secondly, you gloss over your own desire not to support our endeavour. Indeed, when you first announced you were going to write your letter of support, you wrote (May 15):

"I intend to write you a letter, confirming my trip and your demonstration of the apparatus, which you can use in any manner you wish."

Then, you withdrew that offer, by asking us to present it only to private investors. We complied, even though - we remind you - you signed an NDA with us that gives us the right to employ all pertinent information to enure to our benefit, including its publicization whenever and however we see fit.

In the next e-mail, you state:

" Returning back to your own experiments and demonstrations, it seems clear tome, that you should make your best effort to bring these discoveries to the world."

Yet, you are clearly and presently determined not to help this come about, and to do, instead, whatever you _can_ to block them. This even though we made several openings to write either back to back items, or something together with you, on either the AM or the then upcoming Aetherometry.com publications, for either the PoP or the OBRL. For instance, you wrote (June 25):

"I'm working more on Pulse #5, and recently revisited the article on the Orgone Motor which I gave you a copy of. After witnessing your own demonstration, I wanted to give you the opportunity to have something added to them -- in the manner of a postscript which you or I could write."

To which we responded (June 26):

"There are several ways in which one can go. First, you know we would love to write something together with you on the subject - but the timing for a complete unveiling is not yet now, and we think you will soon understand why we think so. (...) However, we are certainly not opposed to have you write something on what you observed for the next Pulse, even in some detail, which we could review and suggest changes if any would be necessary."

Likewise, you wrote (June 25):

"Perhaps, if you are going to make an internet posting of information on this question, we could include a note about that forthcoming information."

and we answered (June 26):

"We would be happy to oblige you on this also - we have made a draft announcement of our website for Gene - do you want to see it and see if you want to use it, or would you prefer if we composed something else?"

To which you answered (June 28):

"Yes, please send the pdf file"

as if you actually intended to announce the website and were contemplating using the announcement we had composed and given to Dr. Mallove. Then you added:

"Pulse #5 is crashing towards completion, and so I only have a bit of space available. Mostly I was thinking to give a bit of information, and then direct people to your own writings in Infinite Energy and on your web site. I think it may take another month, or even two, before I get all the typesetting done, so there's still time."

To which we responded (June 28):

"This is good - that there is still some time. Here is what we suggest - we think it is fine that you want to give a bit of news. We would still prefer if you wrote it - and then we could add to the structure and the idea you have, weave something to it if you want, or simply review it, as you prefer. And, if you want any of the pictures, let us know. Let us say, that we will revisit this in a couple of days after we give you access to our site, which should now be very, very soon indeed."

We gave you private access to the website materials on July 1st. From then on, all these pending questions were systematically ignored by you in our correspondence. In fact, you found all sorts of excuses not to address them, or even to glance at our papers - from taxes to trips to the local cafe, to computer problems. When - after long silences from you and before leaving for holidays - we offered (Aug. 1):

" Incidentally, since we are leaving this weekend - in case you wanted us to compose any announcement for your OBRL list, we still have some time left to do that"

you responded by ignoring our message. By the time you sent your response, we had long departed - which you knew. Upon returning, we asked you, after a lengthy message on Aug. 20:

"We noticed that you did not mention anything about our website on your list so far (nor for that matter your own or our paper in IE #38). What is your current thought on this? If it ties in with your reaction to the material, are you only going to put forth an announcement if you are in full agreement with what we have put forth, or are there other considerations in your mind at present?"

To which you responded (Aug 21):

"Regarding OBRL-News, I did post out an announcement of the IE articles, yours and mine both, on May 30th, but have not done anything as yet on your new website -- so much work to do, it is one of many things that has simply slipped by. No, I do not feel the need to be in full agreement in order to do so, but would like to gain some experimental confirmation of your approach so that I could add a note about it to the posting."
You have written a 12 page diatribe criticizing our theoretical and experimental Aetherometry, that you intend for publication if our work becomes _widely known_ (as if it were a sword over our heads...), but it slipped your mind to make an announcement in OBRL, even though you were eager to get the announcement early on to see whether it, or something else, would do for just such purpose??

But then, just in case we would not swallow such a fish dish, you explained your motives as depending upon you gaining some experimental confirmation - as if the discussion on the table was about your endorsement and not simply your publicization of a fact - the

launching of our website - with respect to which you could have kept any and all distances you wanted.

Eventually, it becomes clear that what you want is for us to suppress our criticism of the diverse followers of Reich, all these 'good, hard-working, honest people' that never went out on a limb and will, one day, be understood as having simply 'followed orders':

"However, since you don't make explicit citations of whom you are referring to (as with the claim there are "no studies worth referencing") it suggests an across-the-board dismissal of everyone save for Reich, including myself.((...) My advice is, to make your criticisms of other people's work open and explicit, with full citations and credit where it is due, but without any of the personal element even if they have been insulting to you in the past --otherwise, don't even mention or refer to them."
As our correspondence from then on shows even more than before, the issues for us are not personal but scientific - and what you were smarting from was your own Reichianism, as you recognized it in our criticism.

To your message we responded (Aug 22):

"So, here is the thing that strikes us as odd, that somehow you are making any announcement of aetherometry.com based upon your reproduction of one or a variety of experiments (this could run you up to a few years, if you are going to be decent about it, ie about the experiments). We think you would be more honest if you said that you do not want to do it for whatever reasons, our non-Reichian orientation would suffice - even if we believe it is closer to the spirit of Reich than any Reichianism has been. This is something we can live with - your disagreement in whatever analytical or scientific matters. But we think that this never stopped you in the past from announcing a variety of matters. So, it is odd - and we would like to withdraw our previous agreement to such an announcement precisely not to embarrass you any further."

Finally, to prove to us that you are our peer - you chose then, at last (?!),to try to read the materials, and the result is the present critique, prefaced on Aug. 22 with these words:

"I have not backed away from your work in the slightest, nor retreated in any manner. Most assuredly, I understand the necessity for pointedly direct critical review -- but from my viewpoint, it is your overall theory which is at issue and being put to the test, and not Reich's. So a question for me is "Does Correa's Aetherometry present new empirical findings and experimental proofs which necessitate the abandonment of Reich's purely orgonomic theory? "So far, from everything you have shown me, and from my readings of the two documents (S2-05 and S2-01) and the IE articles, I must answer "insufficient evidence to render an opinion".

We remarked (Aug. 23):

"Our perception is that you have - for instance, your original offer of permitting us do whatever we wished with your letter re the Aether Motor demo, became later qualified as a matter for private investors only; your offer to write something, with you or not, for the Pulse of the Planet, was suspended; and so was your offer to run our ad in your list. Anyway, we are not in anyway upset with you because of this, but you must concede with some

honesty that here and there you have retreated. This is, by the way, fine with us because we ourselves hope to have acted in such a way that we were not in anyway forcing your hand."

So, you went on to read and the result is the 'Constructive Critique'.

If it were what it purports to be, we would have welcomed your interest at last. But the truth is that what comes through is a precise exercise in Reichianism run amok. You consistently do not read the texts, decontextualize statements and appear clearly handicapped in reading mathematical language; you defend outrageous nonscientific mystical viewpoints, misinterpret graphs and their significance as well as our claims of significance, and by the time you reach AS2-07, you have no more time or patience even to glance at the material. What you succeed in with your enterprise is demeaning to both Reich and yourself - even if the not-so-covert objective of this supposedly constructive critique was to demean us.

Independently of the fact that we now suspect you have already passed around this 'Constructive Critique', and that your present statement is a veiled reference to your present intention of doing just that ("when asked I will give people my opinion about both the strong and weak points in your papers ") there is the _ridiculous and intolerable_ notion that _you will be the judge_ of when it is that Aetherometry has become known enough_ for you to publish your unconstructive and uncritical attack. You are not doing us any favour by this, such as you pretend you are. And we do not intend to let our hard-won success give you and others like you any form of red carpet. Consequently, we hereby put you on notice that we intend to publish our rebuttal of your 'Constructive Critique', as soon as we have finished drafting it.

Likewise, regarding what you will publish in upcoming Pulse of the Planet issues, we reserve all rights of response. Our advice to you is to stick to the agreement you have signed and not to divulge any of what we have told you or demonstrated to you.

We regret that our relationship has come to this. We were obviously mistaken in having taken you into our confidence. If anything, your reaction to having witnessed the Aether Motor is plagued with false fears and malevolent intentions - including statements of future actions you never intended to commit, but made solely to gain our trust.

What's worse, much worse, we cannot even fathom what you have intended to achieve with all these devious oscillations, these half-words. Sooner or later, this behaviour of yours was bound to induce our loss of trust. You could have been frank with us and have admitted that you did not understand our papers or even bother to read them with the care they deserve, and the care needed to write a serious appraisal (let alone a critique!) of them. That would at least have secured our friendship. But this silly tone of demands you have recently adopted - and specifically your demand that we address openly your critique and modify our papers (!) so that you can reply to queries of these masked men you call 'people' - are simply too much. Did you seriously expect we would just lie down and play dead?

Paulo Correa, Msc, PhD
Alexandra Correa, HBA