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Discussions on the Aether

by Dan M. and Paul Stowe (sci.physics)

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Discussions on the Aether

Date: Threads from Tue Apr 14, 1998 for some time ...
From: shera@wt.net (Dan M) and pstowe@ix.netcom.com(Paul Stowe)
Newsgroups: sci.physics,sci.physics.relativity
Subject: **Re: Aether commentary (was Re: Senses and Physics)**

PS: (Paul Stowe) wrote:
DM: Dan M. wrote:

>>PS: I love it when nay-sayers make statements like the ones above. If there are problems, state them and specifically what these are.

>DM: Well, Pete and I have been around the block a couple times concerning these theories, and aether theories were not really central to my point. However, since I mentioned your theory, I'm more than happy to explain the context of my statement and to discuss your theory.

>DM: The aether was generally assumed to exist in the last half of the 19th century. Most physicists believed there was a medium in which light propagated. After all, with sound waves, water waves, etc., all requiring a medium, it seemed natural. Now, there were some initial difficulties, since Maxwell and company had a very hard time giving mechanistic properties to this aether from which one could deduce electromagnetic waves.

PS: What specifically was Maxwell's model? Hint, see Volume two, Section 822 of his "A Treatises on Electricity and Magnetism"

>DM: However, at this stage development of a physical theory, that is an acceptable problem to have.

PS: OK, then . . .

>DM: Then experiments were done. The first MM experiment did not detect the motion of the earth through the aether. Well, perhaps the

aether was dragged with the earth. However, if one thinks of the other fluids in which waves propagate, one sees that even with such drag, one should be able to measure the effects of the earth plowing through the aether.

PS: Hint, think superfluid, or as it was known in the ninetieth century, an inviscid, perfect fluid. Also, remove the "assumption" that "ponderable matter PLOWS" through the medium, and replace it with Maxwell's model (in its modern rendition, called string/superstrings). That is to say, assume the simpler presumption, that matter is simply a manifestation of the same medium that exhibits a distortion proportional to the "stress" applied to it by the medium state it exists in.

>DM: Results continued to be null.

PS: Are you still talking about MMX? What about Lorentz's Ether Theory (a class of theories general known as the "ansatze" [see "Theory of Relativity", W. Pauli, Dover page 142])? What about the other non MMX experiments, do you know about these?

>DM: These sorts of problems, contrasted with the consistency of the predictions of relativity with data led the scientific community to drop the idea of the aether.

PS: Not at t'all, but did constrain aether variants to those that must be Lorentz Group Invariant.

>DM: That was the context of my comment to Pete. The aether was the accepted understanding of light after Maxwell's work on electromagnetics, but was then dropped. Now, it is true, that one can keep on modifying the th
eory so that it remains consistent with the data. However, this is considered suspicious in science. As Einstein was quoted in the usenet "first the aether was made devoid of every property except its absolute reference
frame, and then it lost even that."

PS: The problem is, and away has been, that space is endowed with physical qualities. These include the ability to transfer momentum, be "stressed", manifest energy (ZPE). Therefore, in that sense there exists an aether. The physical universe as seen and manifested, is impossible without it. (paraphrasing Einstein's 1920 Leyden passage). Now one can call it "fields" or one can call it space-time, but it really boils down to what Whitaker said in his foreword to his books "A History of the Theories of Aether and Electricity":

> "A word might be said about the title Aether and Electricity. As everyone knows, the aether played a great part in the physics of the nineteenth century; but in the first decade of the twentieth, chiefly as a result of the failure of attempts to observe the earth's motion relative to the aether, and the acceptance of the principle that such attempts must always fail, the word "aether" fell out of favour, and it became customary to refer to the interplanetary spaces as "vacuous"; the vacuum being conceived as mere emptiness, having no properties except that of propagating electromagnetic waves. But with the development of quantum electrodynamics, the vacuum has come to be regarded as the seat of the "zero-point" oscillations of the electromagnetic field, of the "zero-point" fluctuations of electric charge and current, and of a "polarisation" corresponding to a dielectric constant different from unity. It seems absurd to retain the name "vacuum" for an entity so rich in physical properties, and the historical word "aether" may fitly be retained. My grateful thanks are due to Professors E., T. Copson of the University of St Andrews and J. M. Whittaker of the University of Liverpool for help in reading the proofs."

E. T. WHITTAKER Preface to "A History of the Theories of Aether and Electricity"

>DM: To give you an idea of the attitude of scientist towards free parameters of hidden theories, let me briefly discuss electroweak theory. If I remember correctly, electroweak was developed about '68. It was pretty successful in explaining a lot of data, but it included weak neutral currents. Since these were not observed, the theory was considered with suspicion. The proponents included a 4th quark, which would result in the suppression, but not elimination, of weak neutral currents.

>DM: This was regarded with quite a bit of skepticism. I think I remember Jim writing that some scientists made unflattering comparisons with the aether. Adding things that exist but are not observed is very tenuous in science. The reason for this is that, with enough free parameters, one can fit an infinity of theories to the data.

>DM: In '73, there was the October revolution, when strong evidence for the existence of the 4th quark was presented. Further, weak neutral currents were seen, at the suppressed level consistent with the existence of this quark. Data confirmed the theory, and Nobel prizes were handed out.

>DM: I would suggest that, if the aether exists, it should have experimentally testable consequences. Do you differ? Should not the aether produce results that are different from that predicted by SR and GR? If so, what are they? If not, what does it mean that the aether really exists. Remember, science is not about Truth, it is about explaining what we observe.

PS: Be advised that I had formal academic training in the physical sciences, specifically nuclear science. I was indoctrinated in the details of the standard model and bought it for the better part of my early career. But the fact that the "standard model" is acausal and provides no avenue for even beginning to attempt to approach such question as "what causes gravity", or "how can QM be integrated into our observations of gravitation", or "it nature fundamentally explained". Every time "anyone" attempts to encompass these issues, a physical medium rears its ugly head, because, if one is honest and applies Ockham's Razor, such a single underlying medium IS the simplest explanation.

I didn't seek to embrace an aether, for a noted before, I was taught to "believe" that the aether concept was definitively falsified. And I bought it. The path to the aether, was for me, a shocking experience. For I realized that, contrary to what I was taught, the falsification of the aether was a myth predicated on "assumptions", the validity of which has never been honestly re-evaluated in light of the weight of later observations. Further, Lorentz had provided an acceptable causal model (the ansatza) which was abandoned for purely philosophical reasons. This coupled with the "fact" that every aspect of modern physical phenomena are consistent with a physical medium, makes denying such a possibility IMHO an anti-science behavior.

Example, Maxwell/Boltzmann's kinetic theory provides an accepted underpinning for the macroscopic behavior of physical mediums, but was proposed before "molecular/atomic" theory supported such a state. We didn't "need" to see the individual "molecules/particles" to "know" how they would behave from purely statistical POV. The "bulk" behavior of same yields continuum mechanics. Yes, the very same continuum mechanics applied to "field" behaviors. As such I'm very hard pressed not to consider a direct linkage.

>>PS: In sci.physics.relativity there is a thread "Ether Theories" which is a good example of this type of response. If I had "perceived" problems with and physical concept I'd "at least" state them and "why" I thought said problems existed. One cannot address or answer something like the phrase above except to say that in the work that I done, I have attempted to closely look at all known problem and resolve them including all those mentioned in "Ether Theories" and Feynman's drag.

>DM: All right, I found an article with a long explanation of your theory. Let me take just one quote..

>>PS: Moreover, as Feynman pointed out (Volume 1 Chapter 7, page 9-10 of the "Lectures") this process must also produce a "drag" component on any matter that is in translational motion relative to the underlying "field". The magnitude of which is easily calculated and, at first glance, spells the death knoll for this concept. But the key to this is the phrase "that is in translational motion RELATIVE to the underlying field". For the earth, like all solar planetary bodies, the system is "assumed" to have formed from a co-rotating solar nebula. The underlying "field" thus would be also co-rotating, therefore there is no relative motion between the planetary bodies and the underlying field.

>DM: For the sake of argument, let us assume that there is an aether that is rotating around the sun in bands. These bands rotate roughly

as do the planets, so that the planets have small relative motion with respect to this motion. No motion seems a bit severe, but, I'll pass on this for a moment.

PS: Not for a superfluid.

>DM: Is it still possible to detect the drag effect of this moving aether, then? Could you suggest an experiment that would test this?

PS: Drag no, but there is the "other" Feynman observation (Lorentz also noted this). The attenuation gravity process, must of necessity, deposit the attenuated momentum/energy into the absorbing body(ies). This means that "if" gravity was produced in this fashion, all gravitating bodies must be gaining energy. This again is easy to quantify. It simply is power in minus power out. And power out is power in times the exponential of the overall attenuation coefficient. Thus, this either will, or will, not match observations. Guess what, it fits nicely, AND correctly explains the anomalous thermal emissions of the outer planets perfectly.

The resulting equation is:

q' = kM/r(1 - e^-[phi]t)

Where:

q' is in watts per square meter
k is the induction constant (2.4E-19 m/sec^3)
M is the mass of the gravitating body
r is the radius of the gravitating body [spherical]
phi is the "lump heat" response coefficient = UA/MC_p
t is the total time in seconds that the body has been around

For the gas giants, the "convective" heat transfer will may the exponential term vanish, reducing the above to:

q' = kM/r

Plug in the mass and radius and check the results against the observed anomalous thermal emissions.

>DM: As an experimentalist, a number spring to my mind. The orbits of the comets come to mind fairly quickly. These bodies do not travel as the planets do. Rather, they are on orbits that give them high relative motion with respect to the planets as they cross the orbits of the planets. The spectacular crash into Jupiter comes to mind. Since the aether moves with the planets, the comets should have high relative velocity with respect to the aether. Even with a small drag coefficient, this should be observable.

PS: OK, the "drag equation is:

a_d = GM/r^2(v^2/c^2)

Where v is the translational velocity in m/sec.

So figure out the "drag" on a typical comet, not much eh.

>DM: The second is the solar probe that went around Jupiter to be placed in a highly elliptical orbit. This satellite should have been pushed by the moving aether into a more circular orbit. We followed that satellite very closely and did not see any aether induced anomalies.

These are just a couple of examples. If interested, I can provide more.

PS: Again, what's the magnitude of the expected "drag". Measurable in our lifetimes? I think not. Don't you think I've thought of this?

Paul Stowe

From - Sat Apr 18 20:45:41 1998
From: pstowe@ix.netcom.com(Paul Stowe)
Newsgroups: sci.physics,alt.sci.physics.new-theories
Subject: Re: Aether commentary (was Re: Senses and Physics)
Date: 18 Apr 1998 16:38:00 GMT

Organization: Netcom
Lines: 508
Message-ID: <6hakt8$gsk@dfw-ixnews2.ix.netcom.com>
References: <6h94u4$tn6$1@nnrp1.dejanews.com>
NNTP-Posting-Host: val-ca3-47.ix.netcom.com
X-NETCOM-Date: Sat Apr 18 11:38:00 AM CDT 1998
Xref: news.magna.com.au sci.physics:52214 alt.sci.physics.new-theories:11396

In <6h94u4$tn6$1@nnrp1.dejanews.com> shera@wt.net writes:
Paul Stowe wrote

>>But you're forgetting that the "maximum" drag is
>>GM/r^2 "OF THE MOVING BODY" since this is the condition
>>Where v^2 = c^2 for the normal attenuation component. The
>>translational drag is this maximum, multiplied by (v/c)^2.
>
>Yes, I did indeed use GM/r^2 for the gravitation attraction
>of the sun. I see that you used the M and r^2 of the body
>itself. I don't see the reason for that, but I'll consider that
>later.

The general 'attenuation' model of gravity is analogous to the ionizing radiation transport theory and processes. This does result in the Newtonian Force equation in the 'thin shield' weak solution case. The standard "ux" term (u - linear attenuation coefficient, and x the shield thickness) becoming, in gravity's case, 2GM/rc^2. The gravitational form of (v/c)^2.

Which of course is (dc/c)^2 where the "'dc' IS the velocity change due to the field passing through the body.

So consider a body in translational motion, there is an 'added' velocity due to this motion relative to the underlying field. This IS Feynman's Drag and passes through the body in the direction of travel (a function of area exposed). The general solution is:

a = a_o(1 - e^-ux)

or

a = a_o(1 - e^-[v/c]^2)

and when e^-[v/c]^2 << 1 then the Taylor series expansion gives us:

a = a_o(1 - [1 - (v/c)^2]) = a_o(v/c)^2

and a_o equals the standard GM/r^2, IF and ONLY IF, we are considering a weak solution case. For anything less than a neutron star, we are.

>>But I can't find mass/density figures for Icarus but I'll 'assume'
>>(probably typical) values for these:
>>
>>We have:
>>
>>Mass ~ 1E+18 Kg
>>Density ~ 3600 Kg/m^3
>
>It's actually smaller than that, with a radius of 700 meters.
>
>Thus a equivalent "spherical" radius of:
>[(1E+18)/(3600)(4.189)]^.3333 = 4.05E+4 meters
>
>So our "drag" is:
>
>(GM/r^2)(v^2/c^2) or
>
>[(6.673E-11)(1E+18)/(4.05E+4)^2][(4.1E+4)^2/(2.998E+8)^2)]
>
>Which equals 7.61E-10 m/sec^2
>
>For a 700 meter object, with the same density, at this velocity
>we have about 1.3e-10 m/sec^2.

I get:

3600(4.189)(700)^3 = 5.17E+12 kg

thus

G(5.17E+12)/(700)^2(41000/2.998E+08)^2 = 1.3E-11 m/sec^2

Same number, off by a factor of ten. This means that the velocity change would be 0.016 m/sec in 40 years, or 0.002 m/sec per orbit.

So OK, the kinetic energy change is:

[(5.17E+12)/2](0.002)^2 = 1.08E+07 Joules

and the total kinetic energy was:

[(5.17E+12)/2](4.10E+4)^2 = 4.34E+21 Joules

a difference of,

1.08E+07/4.34E+21 = 2.49E-15 or 2.49E-13%

given a 2.99E+11 meter apogee orbit, the change should be linear to this or,

(2.99E+11)(2.49E-15) = 7.45E-04 meters (0.745 mm) per orbit.

This is of course based upon the total initial energy of the orbiting body.

>Actually, a 0.12 m/sec drop in velocity at perigee results in
>about a 9000 km drop in the apogee. Just look at the energy
>involved at apogee and perigee. I did both a back of the envelope
>and a FORTRAN model that calculated position, acceleration,
>and velocity every 10 seconds.
>
>I recalculated the drop in apogee with the most probable mass, the
>radius, and the actual orbit. Given your numbers, the drop in velocity
>is much smaller than 0.12 m/sec. It's about 30 km/year. However,
>the z translation is still about 21,000 km over the 40 years.

I'm quite interested in how a change of ~2E-13 % in kinetic energy will translate into such large deflections.

>>But in a superfluid, such drag would be a transitory
>>effect, with a steady state condition occurring when the "medium"
>>spun-up to match the orbit. Remember the law of superposition.
>
>A couple of statements about this:
>
>First, if angular momentum is conserved, the momentum gain of the
>medium must be equal to the momentum loss of the asteroid. The
>inertia of the medium must be very little if it can be so influenced
>by such a small mass.

Well, first we must ask the fundamental question, "What IS inertia"? But, I'll 'assume' that the 'field' has such (since this is the logical deduction based upon observation of physical processes). But given it IS a fluid, and it interacts in the manner under discussion, it will be 'influenced' by, and in turn influence all masses.

>Second, how does conservation of energy work here?

Of course.

>Third, how wide a swath is sped up by a body moving through once?
>With the drop in velocity, the same region is not encountered twice.
>If a small object like an asteroid can speed up the aether for a wide
>range, then why don't the planets leave a very wide wake that would
>change the values again?

It's like drawing your finger through the sand, the path is defined by the passage of the object. I suspect that objects that precess will experience some drag until they draw a completely symmetrical pathway. At that point, the systemic motion is fully defined.

>I would like to see an internally consistent description of a
>fluid that would give these results.

Right, agreed. It's not hard but you must remove from the picture

ALMOST all viscosity and thus, viscous dissipation. But if there was no viscosity whatsoever, then the system won't work period!

But what I truly love is that most debunkers "demand" perfection in 'alternate' proposals and expect such people to be super-human, and to answer ALL outstanding questions in physics. Since, unless the person IS miraculously super-intelligent, this shall never happen. Even the most successful of modern accepted theories have limitations and things that it cannot explain.

Feynman's drag is the one area of the conceptual model in which I, in particular, haven't completely resolved to my personal satisfaction. In fact, when Steve Carlip demonstrated the flaw in my initial drag estimation (showing it to be much more significant than I initially estimated [at that point, I hadn't done the formal derivation]), it was a crisis which almost convinced me to abandon the concept. Had I been aware of this problem before doing all the other work, I probably would have just said, "Oh well, it was a good idea at first glance" and given it up.

But by this time, we had the following correlations:

o - The derivation of Newtonian gravity
o - A consistent model & link into QM
o - A definition MLT for, and the correct relationship of charge to Planck's constant
o - The 2.8 degree temperature relationship
o - New relationship definitions

I was very hard pressed to accept that all the above was just coincidential. Thus, one looks harder at the issue.

>>>This is a pattern I've seen with supporters of alternate
>>>physics. The predictions are general, they match very
>>>complex data with theories that have plenty of free
>>>parameters. However, predictions of testable phenomenon
>>>which differ from the predictions of the standard theory
>>>are very hard to come by.

Ah yes, the old good news/bad news notion. The 'good news' Is, the theory IS compliant with all known observations and theories, The 'bad news' is the same...

In such cases, Ockham's Razor should be applied, not say "It predicts nothing new" so why should we even consider such an alternate. If, for example, the suggested alternative provides an simpler conceptual/causal foundation for a set of observations, postulates, and mathematical correlations. The 'natural philosophy' aspect argues for its consideration. Since the purpose of science (at one time) was understanding nature, not just describing it mathematically.

>On my observation that you use words like superfluid,
>quote old work on vortices, but don't grind through problems.
>My guess is that, once every constraint is placed on your
>fluid, that there will be no self consistent set of properties
>for that fluid that will satisfy the constraints.

I suggest you look up "Simply Beauty" at Pete's web-site. I think I & my colleague Barry Mingst, are ones of the very few who have 'ground through' the concept.

I did not arbitrarily choose the vortex model and superfluidic state. It came as a result of matching observations to various alternatives conceptual models under consideration. It was only AFTER we had settled on the ring vortex and superfluid did we even bother to research this topic. As you are probably well aware, the atomic vortex hypothesis is not part of normal formal physics training & education. It was when reading Whitaker's book that I discovered that this had already been suggested, and was in fact, Maxwell's model for his famous equations.

Thus its should come as NO surprise that Penrose's twistor 'topology' looks like a vortex ring, given he used Maxwell's equations as his starting point!

>>I have two specific predictions these are:

>>1) induction heating (well matched),
>
>Let see, you posted:
>>q' is in watts per square meter
>>k is the induction constant (2.4E-19 m/sec^3)
>>M is the mass of the gravitating body
>>r is the radius of the gravitating body [spherical]
>
>and for steady state
>
>>q' = kM/r
>
>It's true that this quantity gives a number within the range
>of heat produced by Jupiter. The first obvious question is
>what type of constant is k, and where did it come from?

It comes as a REQUIRED consequence of attenuation gravity. I'll include the derivation the section below.

ATTENUATION GRAVITY INDUCTION HEATING
(The LeSage Effect)

On the Macro Level (Big Picture Approach) we know that the rate of energy [power] absorbed in any attenuating body must be the incident energy entering the body minus the energy exiting per unit time. Mathematically, this is denoted by:

$$Qabs = Qin - Qout$$

Where Qabs is the absorbed power
Qin is the incident power
Qout is the exit power

Therefore, on a per unit area basis (a flux term) we have:

$$q(abs) = q(in) - q(out)$$

and q(out) is related to q(in) as:

$$q(out) = q(in)e^{-b}$$

Where b is the total attenuation coefficient.

Now for attenuation within a body (assuming conservation) the loss of momentum/energy in the field particle population acts to reduce the attenuated vector's velocity (v) component. Thus this is simply:

$$vf^2 = vi^2 - 2aS$$

Where vf is the final velocity and vi is the initial velocity which can be assumed to be the speed of light c. Now within the framework of the concept under discussion, we know that for very weak attenuators:

$$a = GM/R^2$$

and S will be equal to R.

Therefore we see that:

$$2GM/R = c^2 - vf^2 = \text{delta } v^2$$

Dividing both sides by c^2 we get:

$$1 - vf^2/c^2 = 2GM/Rc^2$$

Thus we see that the attenuation coefficient within any weak attenuator is in the familiar form of the Lorentz transformation $(1 - v^2/c^2)$

Now we also know that for values of e^{-b} where $b << 1$

can be simplified by Taylor expansion to (1 - b). Thus
equating these we see:

$$1 - 2GM/Rc^2 = vf^2/c^2 = 1 - b$$

so

$$b = 2GM/Rc^2$$

Now getting back to the power flux equations, we have:

$$q(abs) = q(in)(1 - e^b)$$

Simplifying, we get:

$$q(abs) = q(in)b = q(in)[2GM/Rc^2]$$

We can now put this in terms of acceleration a as:

$$q(abs) = q(in)(2R/c^2)a$$

So we now know the relationship of the power absorbed to the
incident power. Not very much. Indeed this cross-section of
attenuation matches very closely the estimated neutrino attenuation
cross-section.

We must now define how an object will respond thermally to such
induction. To do this we will use a common thermal model known
as the lumped parameter method of transient analysis . This says:

$$delta\ T = [q(abs)/U][1 - e^{-kt}]$$

Where U is the overall heat transfer coefficient
and k is the thermal inertia coefficient (UA/MC_p)
A is the exposed surface area
C_p is the heat capacity

inspection of this equation shows us that as time t goes to infinity,
the delta T becomes
simply:

$$delta\ T = q(abs)/U$$

We note now that we have one big unknown, which is needed
to solve this problem, namely q(in).

Now we can normalized this term by assuming that, once defined,
it should match all astronomical bodies. Since we have the most
thermal emission data for the earth, this would, at first glance be the

logical body to start with. So we know that, on average the earth
radiates 0.06 Watts/m^2 into space (See A Textbook on Geonomy,
J. A. Jacobs, Chapter 7 starting Pg 190). Thus:

$$q = U(delta\ T) \text{ and } U = q/(delta\ T)$$

So if the earth's centerline temperature is about 7,500 degrees K, U is

about 8.0E-06 Watts/m^2-K. Therefore k defined above becomes
approximately:

$$k = [(5.0E-06)(5.10E14)]/[(5.98E+24)(500)] = 1.36E-18\ 1/sec$$

Therefore the mean time interval is 1/k = 7.32E+17 secs or 23.2
billion years! This means that given as 5.2 billion year age, the
earth could not possibly be near thermal equilibrium, but would still
be heating up. There is of course high uncertainty in both the
centerline temperature and heat capacity figures making any attempt
to correlate the field's q to the earth even more uncertain.

So another, smaller body needs to be found that will at least be closer

to thermal equilibrium. Earth's moon looks to be a good candidate.
The moon, based on the heat flow experiments of Apollo 14 -16, has
a measured output of 0.02 Watts/m^2. This is twice as high as would
be expected due to estimated internal radioactive elements and all
expected thermal sources. Using the same method described above,
the moon would have reached about 97% of it's final thermal equilibrium

value in the 5 billion year history. So using 0.01 Watts/m^2 and dividing by 0.97 we get its final emission at 0.0103 Watts/m^2.

Thus we get:

q(in) = 0.0103/(2GM/Rc^2)

Plugging in the mass & radius of the moon we get:

q(in) = 0.0103/[(2[6.673E-11][7.35E+22])/([1.76E+06][2.998E+08]^2)]

q(in) = 1.66E+08 Watts/m^2

We now have all the information needed to solve for any astronomical body.

q ={2q(in)G/c^2}M/R(1 - e^-kt)

and we note that the bracketed terms are constants... which we will denote as Z.

Z = 2(1.66E+08)(6.673E-11)/(2.998E+08)^2 = 2.47E-19 Watts/m -kg

---

And the Z above is k in its final units of m/sec^3.

>Also, the heat produced in Jupiter is not an unexplainable
>anomaly. Continued contraction of Jupiter, which has a quite
>low density, is consistent with the heat production. The contraction
>does not have to be much, about 1 meter/every 1000 years is my
>calculation.

It's not that the equation, normalized to the moon, just matches Jupiter, but also matches Saturn, Uranus, Neptune, and the Earth (as long as you include the transient component) and was derived empirically for the attenuation gravitational concept.

>Now, we have not proven that is the source of the heat, but it
>is consistent with the heat produced and with the age of Jupiter.
>
>The second obvious question is why does the total energy
>produced by a given mass decrease as the density increases?

I think you mean total power?

>We have, for total power qtot=(4/3)pi*kMr.

Ah actually it is:

Q = q'A (where A is area)

Thus,

Q = kM/r(4pir^2) = 4pikMr

But, I won't quibble over a 4/3pi verses 4pi.

>Using this formula, we can see that low density gas clouds should
>generate a significant amount of energy per unit mass. Thus, a gas
>cloud with M=1 solar mass, would radiate as much energy from
>induction heating as the sun does now, if its diameter were about
>10,000 AU.

Hmm, let's see:

Q = 4pik(2E+30)(1.50E+15) = 9.05E+27 Watts

Verses about 3.0E+26 Watts (6.63E+07 Watt/m^2) for the sun.

OK, this just tells us that the gas will collapse to get to the lower potential state.

>>2) A general process match to the 'observed' galactic rotation
>> profile of spiral galaxies WITHOUT invoking 'Dark Matter'.

>Since the motion of the stars in a galaxy indicates that the
>gravitational force is higher than one can account using the visible
>matter, one certainly cannot use attenuation to explain this.

Or conversely, one could argue that as one goes towards the center,
the rotation is SLOWER than otherwise expected. This is what
attenuation gravity predicts.

>Further, any attenuation would be minimal, according to your
>model.

For each object, yes. For example, the sun only 'attenuates'
about one part in a million of that which passes through it.
However, line up a million suns, and ...

>This raises some obvious questions. Why the mass squared
>dependency of the force? Why is the total force given a 1/r^2
>dependency? Drag models that I am familiar with have an r^2
>dependence. With gravity, one could argue the 1/r^2 and r^2
>would cancel, but I would like to see a rigorous explanation for
>that dependence.

Because, one body (mass) produces a gradient in the velocity
field by its attenuating presence, but it takes a second attenuating
body to be infulenced by this gradient, to result in an observed force.

>>It does appear that you didn't even bother to consider the
>>equation provided. Which BTW is a strict, first principle
>derivation, based upon the attenuation premise. This same
>process also provides the Newtonian gravitational force equation
>in the 'weak' solution of this problem.
>
>Well, your k in the equation does not look all that tied down.
>It seems as though a fairly free parameter to me. IMHO, you
>could work around with various properties of this and than,
>until you got the number you needed, but it doesn't seem that if
>you needed a different number, that you would have accepted I
>it as evidence.

You do me a discredit sir, I do not play with numbers, and as can
be seen in the above derivation, k is NOT free to float, but is tied
to an actual heat flow measurement.

>Finally, if the fluid moving as the planets do, then that implies
>that there is a preferred reference frame for every bit of the
>fluid. That seems to imply a preferred reference frame for the
>transmission of light.

Yep, but that in not way invalidates SR/GR.

Paul Stowe

From - Wed Apr 29 10:56:32 1998
From: pstowe@ix.netcom.com(Paul Stowe)
Newsgroups: sci.physics,alt.sci.physics.new-theories
Subject: Re: Aether heating and drag (was commentary)
Date: 29 Apr 1998 03:19:48 GMT
Organization: Netcom
Lines: 294
Message-ID: <6i668k$c9u@dfw-ixnews7.ix.netcom.com>
NNTP-Posting-Host: val-ca3-33.ix.netcom.com
X-NETCOM-Date: Tue Apr 28 10:19:48 PM CDT 1998
Xref: news.magna.com.au sci.physics:54431 alt.sci.physics.new-theories:12119

>In <6hvdsj$7ao$1@nnrp1.dejanews.com> shera@wt.net writes:

In response to Dan M.'s last post,

>>Let us indeed consider the momentum flux. For simplicity,
>>let's limit this consideration to a mono-directional particles,
>>moving in the x direction. Therefore, we have an average
>>cross-sectional density of n particles of mass m per square
>>meter. Since all the momentum is in the x direction, the time
>>independent momentum passing through a unit area A in the
>>y-z plane is simply nv/A (kg/m-sec).
>
>Well, if it is classical, the momentum flux is nmv/A. If
>it is relativistic massive particle with rest mass m,
>the momentum is nmv/(A*sqrt(1-(v/c)^2). But then it won't travel

>at c if SR is correct. This won't affect the radius dependence,
>but it is something you need to define.

Pay very close attention, relativity applies to matter & EM effects,
but at the medium base level it does not apply. At this level we
have entities of finite non-zero size and linear momentum, mv.
Where m is the "apparent" mass and v is the velocity. In a very
large population of these entities in random motion, we will get
both kinetic theory and concept flux/current. The "reason"
relativity applies to matter/EM is because both arise in my
concept of a next level, consisting of vortices of this base medium.
These vortices are pliable and when stressed exhibit a strain
proportional to the stress, in a non-linear manner that leads to
the Lorentz transformation state.

Again, at the base level, the concept of "rest mass" cannot be
applied. Now why do you think I say this? Because all there is
here, is the basic medium/field. You simply can't "stop" a basic
field quantum.

>>Next let's put a momentum "attenuator" into the stream. It's
>>primary property is that it causes the momentum of the particles to
>>"weaken" as they pass through the region. Thus the change in
>>momentum is either:
>>
>>dp = n(v_i - v_f)
>>
>If it is classical, it is nm(v_i-v_f). If it is relativistic,
>the dependence on velocity is more complicated. I don't think
>we really have to work that out, but I will if necessary.

It is only the "thin shield" attenuator, which will be "essentially"
linear in the fashion shown above, the more general form is:

dp = nm(dv)

And obviously dv can never be greater than the initial particle
velocity. Further, it has to be in the general case, an exponential
form.

Since for gravitation, all non-exotic states of matter can safely
be treated as "thin shield" problems, I simply used this linearized
form.

>> Where v_i & v_f are the initial and final velocities
>>
>>Or, if the particles are absorbed:
>>dp = v(dn) and dn = m(n_i - n_f)
>>
>I get dp=vm(dn)=vm(n_i-n_f), but the idea is the same.

See above about generalization, but OK.

>>Or a combination of both the above
>
>That's fine, but I would like to use the second type of
>absorption because it simplifies the explanation.

But violates conservation of mass, unless we then
assume that matter acquires this mass at the expense
of the "field medium". I strongly think it's the former,
I'll explain below

>>From the perspective of the intensity of the current vector,
>>it really doesn't matter, since we are only interested in the
>>final resulting dp. If one assumes that the resulting dp is
>>solely based upon the "amount" of the attenuating medium
>>encountered, we are back to the standard shielding concept
>>of a fixed "mass attenuation coefficient" [us] (m^2/kg).
>
>That's perfectly fine. Even though 1/v was in my derivation
>of your equation, I was arguing towards that point.
>
>>We also have the concept of dFee/ds which is a linear function
>>of attenuator density. I.e. double the density, and we double
>>the attenuation per unit length traveled. But we see that, to
>>double the density, we must compress the attenuator to 1/2s,
>>since given a fixed cross-section, density is a linear function
>>of thickness.

>
>Actually, an example that is much closer to what we were
>discussing is a three dimensional case. If the radius of
>a sphere is doubled, the density is 1/8th of its former value.
>Thus the attenuation per unit length is also cut to 1/8th of
>its former value. The total length is doubled, so the
>attenuation in that area is cut to 1/4th the original
>value.
>
>But, the cross sectional area goes up by a factor of 4 (2^2).
>Thus, the total attenuation stays the same. In the
>slowing down case, we can see that the dp for each particle
>is 1/4th the original value (1/4th of the absorptive mass),
>but 4x as many particles are affected. In the absorption case
>1/4*4 times as many particles are affected. Thus, the dP for
>a given mass is constant.

Also true for the "thin shield" case where scatter essentially
does not affect the final results.

>>While it is true that the "residence time" or travel time through
>>the attenuator is t/v, this is not directly related to the deposited
>>energy of this system. Instead, we know that this deposited
>>energy is:

>>E = 1/2M(dv)^2 where M is the total mass of the attenuator.

>That is only true for elastic collisions. Since we are
>using shielding as an example, it does not make sense
>to use elastic collisions as our model. The physics of
>shielding, whether by absorption or attenuation, is not
>elastic collisions between a giant rigid body and the
>flux.

Sure it does, in the conceptual model I'm discussing some of
the linear momentum of the base field entities transfers into
the vorticity of the rings (a Brownian excitation of the "tube",
resulting in an oscillatory motion, which then radiates this
transferred energy in the form of waves). As per Helmholtz's
theorem, an essentially inviscid vortex is a permanent stable
structure that neither gains or losses particles, thus absorption
is out. Elastic collisions of the medium particles constituting
the vortices are in.

>A much better treatment would be parallel to the
>treatment you gave above.
>
>Let us first consider absorption. For momentum, you
>stated:
>
>">dp = v(dn) and dn = m(n_i - n_f)"
>which is, dp=vm(n_i - n_f)
>
>I stated
>"dp=vm(dn)=vm(n_i-n_f)" Looks that all we disagree
>on is the definition of terms. That's no problem.
>
>A parallel argument for energy is
>dE = 1/2mv^2(n_i-n_f). The energy deposited in the
>absorber is the energy lost by the flux.
>
>For attenuation, we have from you:
>
>">dp = n(v_i - v_f)"
>
>and me
>
>"dp = nm(v_i-v_f)"
>
>I think you just absorbed the mass into the n term.
>I was thinking of n particles with mass m.
>
>The parallel for energy is
>
>dE = n*1/2m(v_i^2-v_f^2)
>
>for small changes in v, we have
>
>dE = nmv_i(v_i-v_f)

>
>The parallel nature of the momentum and energy
>deposition is no accident. Relativisticaly, we
>know E^2 = p^2c^2 + m^2c^4. The classical approximation
>for kinetic energy is E = 1/2 p^2/m.
>
>In your model, you have a separation of momentum change
>and energy change of the flux. Above, you stated that the
>momentum drop per unit length is a function of the density.
>That makes perfect sense. Here you state that
>
>a = k[Fee(us)^2](rho)s
>
>If the acceleration is a function of the total mass, why
>isn't the change in momentum a function of the total mass?
>
>If what you said is true, the energy/momentum relationship
>for elements of the flux are dependent on whether they
>go through a long low density absorber or a short high
>density absorber. That is inconsistent with both classical
>and relativistic mechanics. If this is not a mistake,
>what relationship between momentum and kinetic energy
>do you propose?

Let's again look at the problem on the one hand we have
the momentum, but how do we translate this into energy?
So, what IS energy? Energy is the integration of the c
hange in momentum , with respect to any "change" in
velocity. That is to say:

E = integral v1 to v2 of p dv

Given that p is mv, this becomes:

E = integral v1 to v2 mv dv or if starting from rest,
v1 = 0, E = 1/2mv^2

But, we know from standard kinematics that
dv^2 = 2as so,

dE = mvdv = 2mas as shown before, then

vdv = as

or

dv = as/v = at

Gravitationally we get

dv = 2aR/v = 2GM/Rv = 2kGrhoR^2/v = (2kG/v)rhoR^2

And the terms in the parenthetical are constant. In the
case above, v is the mean speed of the particle fluence.
Assuming GR is correct, v = c, or 2kG/c we'll call this
b {beta}. Also we note that that the terms rhoR^2 yields
units of kg/m, a mass per distance term.

We have agreed that the change in momentum, acceleration,
and thus the corresponding change in pressure, in the
attenuator is constant, defined by:

dp = nmdv = nmb(rho)s^2

And gravitationally,

dp/dt = nmb(rho)s^2(c/s) = nm(2kG)(rho)s

And the acceleration generated is:

a = 2kG(rho)s

Back to the change in energy we get:

dE = nm2kG(rho)s^2 = nmcdc

Solving this, we get:

2kG(rho)s^2 = cdc

Thus,

dc = 2kG/c(rho)s^2

or

dc = 2as/c

We have thus defined the medium's response to passage
through the body, but not the attenuator's. The attenuator
of mass M gains this amount of energy and this is defined
as 1/2M(dv)^2. We see that dv in this case is appears as:

dv^2 = 2kG(rho)s^2

Back to a sphere of radius R we get:

dv^2 = 2GM/R

or

dE = GM^2/R

But, I've done all of this in an attempt to demonstrate the
strict relationship between the "escape" velocity and
gravitational acceleration so we could clearly establish
the radius dependence of the resulting energy from the
induced acceleration. Given that linearly, the acceleration
is constant, then the resulting energy and power induction
must be a function of the travel distance s.

>I'm stopping here because this is a key question that relates
>to all other discussion.

I took so long to reply because I've consulted with my
colleagues and friends and struggled on how to best present
this very complicated concept. Hopefully, the above is clearly
presented.

Paul Stowe

From - Sat May 02 23:03:07 1998
From: pstowe@ix.netcom.com(Paul Stowe)
Newsgroups: sci.physics,alt.sci.physics.new-theories
Subject: Re: Aether heating and drag (was commentary)
Date: 2 May 1998 22:53:53 GMT
Organization: Netcom
Lines: 311
Message-ID: <6ig861$h3k@sjx-ixn5.ix.netcom.com>
NNTP-Posting-Host: val-ca1-04.ix.netcom.com
X-NETCOM-Date: Sat May 02 3:53:53 PM PDT 1998
Xref: news.magna.com.au sci.physics:55093 alt.sci.physics.new-theories:12348

>In <6ib4n7$86m$1@nnrp1.dejanews.com> shera@wt.net writes:
>>
>> Paul Stowe wrote:
>>
>>
>>Pay very close attention, relativity applies to matter & EM
>>effects but at the medium base level it does not apply. At
>>this level we have entities of finite non-zero size and linear
>>momentum, mv.where m is the "apparent" mass and v is
>>the velocity. In a very large population of these entities in
>>random motion, we will get both kinetic theory and concept
>>flux/current. The "reason"relativity applies to matter/EM is
>>because both arise in my concept of a next level, consisting
>>of vortices of this base medium.These vortices are pliable
>>and when stressed exhibit a strain proportional to the stress,
>>in a non-linear manner that leads to the Lorentz
>>transformation state.
>
>So, I take it that the aether is a classical fluid. This means
>that in a region, there is one preferred inertial frame, the
>rest frame of the aether? Further, is there any limit on the
>relative velocity of one part of the aether vs. another part?
>If light is traveling in a section of aether that is traveling
>at .9c with respect to us, how do we calculate the time it

>takes to arrive?

In what sense is there a "preferred frame"? If EM & matter consist of vortices, and can only respond in a fashion consistent with the behavior of these, then how do you determine the "preferred frame"? Since there exists a certain stress/strain relationship between relative motions (energy densities) which lead to the Lorentz group for all cases involving these.

>It would also be a very good idea if you actually were to
>describe the vorticies that comprise matter and derive the
>basic relationships in SR from your equations of the vorticies.
>I'm talking about starting with a classical fluid, describing
>its properties in an internally consistent manner, and then
>deriving special relativity.

Ah, you're talking about the "Ansatze" or Lorentz Ether Theory. Also standard acoustic theory have the Lorentz group contraction. See Section 3, Chapter 8 of the "Handbook of Physics", Condon & Odishaw, McGraw-Hill Publications 1967. The term Sqrt(1 -b^2) where b = (v/c) [in this case c is sonic velocity] is a standard transform. It is a blatant myth that the Lorentz transformations are limited specifically to relativistic phenomena.

>Further, you need to show that your various assumptions do
>not lead to contradictions. From the looks of things, you use
>one set of axioms to prove A and another to prove B. The
>problem that I see, and will be documenting, is that the axioms
>used to obtain A can also be used to obtain ~B. Thus there's
>internal inconsistency in the set of axioms.

What "non-standard" assumptions from a known sense, are you talking about? How about giving me some specific examples.

>>Again, at the base level, the concept of "rest mass" cannot be
>>applied. Now why do you think I say this? Because all there is
>>here, is the basic medium/field. You simply can't "stop" a basic
>>field quantum.
>
>This sounds inconsistent with what you've said about the planets
>accelerating the fluid. If a massive body can transfer momentum to
>the aether, doesn't this mean that the aether can be brought to
>zero velocity through such a momentum transfer?

Perhaps only because you've failed to think about it. Take a perfectly reflective box 1/3 full of perfectly elastic bb's up in the space shuttle. Then shake box rigorously, stop and hold the box steady. Now, without exerting any external influence, can any of the bb's be stopped to have a "rest" state. Of course not, they have a mean speed, a definite non zero mass, and thus a mean momentum that is in conservation. But none are "at rest" relative to the box's coordinate frame and "at rest" is meaningless to this frame.

>>It is only the "thin shield" attenuator, which will be "essentially"
>>linear in the fashion shown above, the more general form is:
>>
>>dp = nm(dv)
>
>That is equivalent to what is given above by me as the classical
>case:
>
>>And obviously dv can never be greater than the initial particle
>>velocity.
>
>This is not consistent with elastic collisions. In the COM
>reference frame, an elastic head on collision has both objects
>ending up with velocities that are equal in magnitude and
>opposite in sign from their original velocity.

Please think about what is being said. Consider a "fixed" attenuator. Can the a momentum current passing through the region lose more than it had originally?

>>Further, it has to be in the general case, an exponential
>>form.
>

>Only if you are assume that F = A*v. For example, ionization
>does not work this way. You really need to pick one model and
>work out everything from that one model. Later, when we discuss
>energy, I will show why that is essential.

Ionization doesn't work that way? You're talking perhaps about
alpha/beta interactions? For gamma/neutrons interactions sure do
have an exponential form. These are also considered "ionizing".

>>Since for gravitation, all non-exotic states of matter can safely
>>be treated as "thin shield" problems, I simply used this linearized
>>form.
>
>Fine
>
>>But violates conservation of mass, unless we then
>>assume that matter acquires this mass at the expense
>>of the "field medium". I strongly think it's the former,
>>I'll explain below
>
>We know that for rest mass E=mc^2. While I'm not
>going to get into a discussion of rest and relativistic mass
>here, it is well known that the total energy of of system
>must be used to calculate the mass. The mass of a nucleus
>is not just the sum of the masses of the protons and neutrons.

It is, plus the electrons, "less" the so-called binding energy.
But in general, vortices have a distinct "apparent momentum"
which is associated with their relative motion in the fluidic
medium. They therefore have an "apparent mass". Vortices
bound together by mutual streamline interactions also exhibit
a "binding energy" or the work necessary to separate them to
infinity. There is no dichotomy here if that is the implication.

>>That is only true for elastic collisions. Since we are
>>using shielding as an example, it does not make sense
>>to use elastic collisions as our model. The physics of
>>shielding, whether by absorption or attenuation, is not
>>elastic collisions between a giant rigid body and the
>>flux.
>
>>Sure it does, in the conceptual model I'm discussing some of
>>the linear momentum of the base field entities transfers into
>>the vorticity of the rings (a Brownian excitation of the "tube",
>>resulting in an oscillatory motion, which then radiates this
>>transferred energy in the form of waves). As per Helmholtz's
>>theorem, an essentially inviscid vortex is a permanent stable
>>structure that neither gains or losses particles, thus absorption
>>is out. Elastic collisions of the medium particles constituting
>>the vortices are in.
>
>Well, lets see. For elastic collisions, the ratio of energy transfer
>to momentum transfer goes as 1/m, where m is the mass of the
>body to which the momentum is transferred. If, for example
>p = 1000 kg m/s, then for a 1 kg mass, v=1000 m/s and E=5e5
>Joules. For a 1000 kg mass, v = 1 m/s and E= 500 Joules.

dE = pdv/2 = (1000)(1)/2 = 500 Joules, yes

but dv = at = 2as/v so, since p = nmv

we get:

dE = nmv(2as/v) = 2nmas

>So, if you consider energy deposited in elastic collisions, it should
>go as 1/M.

As a function of distance yes. Which is related to the thickness of
the mass.

>Also, elastic scattering from the entire mass is typically a strong
>interaction. One does not go through a mass with elastic scattering,
>one bounces off of it.

Huh, are you reverting back to a "solid ponderable body"?

>So, elastic scattering appears to be inconsistent with the rest of
>your axioms.

No, as discussed above.

>
>Now we start having difficulties:
>
>From the discussion of momentum given above we can
>calculate a. By agreeing that the total momentum deposited
>is simply a function of the mass of the target, we establish that
>dp/ds = A*den, where A is a constant, den is the density of the
>material, and s is the path traveled. For small changes in
>velocity, we have dp/dt=A*den. We also have from you,
>dp = nmdv. So, dp/dt = nmdv/dt. Since a = dv/dt, we have
>A*den =nma. Absorbing nm into the constant A to get A',
>we have A'*den= a.
>
>You take another tact.
>
>>Gravitationally we get
>>
>>dv = 2aR/v = 2GM/Rv = 2kGrhoR^2/v = (2kG/v)rhoR^2
>
>I don't know where you get that. It has nothing to do with
>shielding problems. My understanding of the shielding model
>is that gravity occures becauses masses shield other masses
>from a universal flux...neutrinos was offered as an example.
>In this model, the flux is not subject to gravity, it is the cause
>of gravity.

Simple, if gravity is "caused" as the "result" of the loss of linear momentum of the flux passing through matter, then this "loss" in terms of the velocity change in the flux's momentum is as described. It is by definition gravitational.

>>>And the terms in the parenthetical are constant. In the
>>>case above, v is the mean speed of the particle fluence.
>>>Assuming GR is correct, v = c, or 2kG/c we'll call this
>>>b {beta}. Also we note that that the terms rhoR^2 yields
>>>units of kg/m, a mass per distance term.
>
>You need to deduce this, instead of assuming it. Given the
>shielding model, I probably could deduce these numbers.
>
>>>We have agreed that the change in momentum, acceleration,
>>>and thus the corresponding change in pressure, in the
>>>attenuator is constant, defined by:
>>>
>>>dp = nmdv = nmb(rho)s^2
>>>
>>Actually, we have determined that dp = nmdv = A*M,
>>where M is the total mass of the object. If the total
>>mometum change of the flux is of this form, than the
>>change in flux momentum is not simply a function of
>>mass. Take a give mass in a sphere of radius r.
>>Double the radius. What happens with the formula given
>>above.
>
>Two other questions. What's b here, and why is dv=b(rho)s^2?

b (beta) was described above as:

"Assuming GR is correct, v = c, or 2kG/c we'll call this b {beta}."

Then going back to dv = 2aR/v and a = GM/R^2,

And

M = k(rho)R^3

We have, if R is replaced with generic distance s,

a = kG(rho)s

Plugging in kG(rho)s for a, we get:

dv = (2[kG(rho)s)/c

Or

dv = 2as/c

>I'll stop here, because this seems like the big point
>of departure. The question I have is why do you change
>your model here? Why don't you just deduce the change
>in energy from the shielding model we have been
>discussing?

I thought that is what was done. A very long, protracted discussion in support of the very simple concept that "work" equals "force" through "distance". Which was started because of your question on the induction heating relationship q' = kM/R (where k was a constant of proportionality). This lead to the relationship Q = 4pikMR (which, BTW is a very common form) indicating a radial distance relationship of the total energy of a gravitating system.

As to sticking to the shielding model, I believe that I have done so. See le_sage.htm
for the presentation of this model. This model is based upon linear momentum transfer, not energy deposition, as is typically done in standard ionizing shielding evaluations.

>If you then want to deduce additional constants, that's fine.
>If you can deduce the dependencies you want, that would be
>perfectly valid. By positing them, you use them as axioms.
>It appears to me that this will lead to contradictions between
>theorems proven from the various axioms. This, of course,
>means the system lacks internal consistency.

I doubt that in nature there are any true "constants". Light varies it magnitude depending on the "stress" of space-time, and "if" space-time is a medium, its properties (density & pressure) will have varied accordingly. Although natural relationships won't change, and will "appear" to have a constant value to any inertial observers at a point in space, evaluated independently at two sufficiently diverse points simultaneously, the variance, like the variance in light's speed, should be noticeable.

Specifically, internal inconsistencies are what I would like to know about. But to understand, they will need to be presented in a clear, unambiguous manner.

Paul Stowe

From - Wed May 06 07:07:10 1998
From: pstowe@ix.netcom.com(Paul Stowe)
Newsgroups: sci.physics,alt.sci.physics.new-theories
Subject: Re: Aether heating and drag (was commentary)
Date: 6 May 1998 02:50:05 GMT
Organization: Netcom
Lines: 251
Message-ID: <6ioj4t$1hd@sjx-ixn8.ix.netcom.com>
NNTP-Posting-Host: val-ca2-18.ix.netcom.com
X-NETCOM-Date: Tue May 05 7:50:05 PM PDT 1998
Xref: news.magna.com.au sci.physics:55684 alt.sci.physics.new-theories:12526

>In <6im0li$n7q$1@nnrp1.dejanews.com> shera@wt.net writes:
>
>>Paul Stowe wrote:
>
>I have started to answer this post point by point, but it
>became clear that Paul and I have significantly different
>perspectives on what is clear and how things are proven.
>To attempt to resolve this, I would like to ask questions
>about the nature of the shielded flux that Paul proposes.
>Since I am middling well aquatinted with gamma, neutron,
>electron, proton, and neutrino interactions in matter, I
>think that I should be able to determine the consequences
>of each course of action.

Yes, frustration can run high (for both parties) when we can't find a common base for terminology. So Ill try to take this one step at a time.

BTW, I do understand, and, if this were a face to face discussion, I think we could get farther much quicker. But given the limitation of the medium, I'll try harder to clear things up.

>First, something else needs to be cleared up. It is whether
>the shielding involves elastic scattering: We had the
>following exchange:

>>Huh, are you reverting back to a "solid ponderable body"?

>I am trying to understand what model you are using. The
>exchange given above started when I was trying to eliminate
>each individual quantum of flux interacting with the body as
>a whole. (BTW, I am not trying to bring QM in here, I am
>merely referring to each m in your nm.

First, I agree that, for the "big picture", getting down to the level of the individual "quantum" isn't necessary to mathematically describe the attenuation gravity concept, or even it's math. So, let break these apart.

Level one, big picture: matter "attenuates a "field" in such a fashion as to result in an apparent attractive mechanism. At this level, "the specific how" isn't important, the general properties are. I thought that we did agree that for very "weak" attenuating cases (<< 1 MFP), scattering wasn't an important parameter to be tracked even if "elastic" interactions was the sole underlying mechanism.

Level two, the specific "concept" of how level one is accomplished. More on this later…

>So, the question is, what does each bit of flux interact
>with when passing through the sun? The sun as a whole
>rigid body? Regions of the sun? Electrons and protons?,
>Atoms?

Level two question, answer sub-atomic.

>If we are to use radiation shielding as an example, there are
>several possibilities. Charged particles lose energy both by
>elastic and inelastic collisions, by ionization, and by capture.

Actually, "charged particles" lose most of their excess energy by electric field interactions (mainly interacting with the electron clouds of atoms), not direct elastic or inelastic "collisions".

>Neutral particles lose energy by elastic and inelastic scattering
>and by capture/absorption. Discussing this, we had the exchange

OK

>>>>Further, it has to be in the general case, an exponential
>>>>form.
>>>
>>>Only if you are assume that F = A*v. For example, ionization
>>>does not work this way. You really need to pick one model and
>>>work out everything from that one model. Later, when we discuss
>>>energy, I will show why that is essential.
>>
>>Ionization doesn't work that way? You're talking perhaps about
>>alpha/beta interactions? For gamma/neutrons interactions sure do
>>have an exponential form. These are also considered "ionizing."
>
>Well, we look at ionizing from different perspectives. Charged
>particles lose energy as the ionize atoms they pass near.

Yep, they actually rarely "collide" with atoms. This leads to an "almost" linear math form describing their behavior

>Neutral particles do not. There are of course, interactions with
>charged particles, which then do ionize atoms, but the primary

>interaction of the neutral particles is not by ionization.

Well, for fast neutrons, these "collisions" usually "knock" the particle be it a nucleus or an electron sufficiently to result direct ionization. But, I think we're quibbling here and it's not important to this discussion.

>Why do these particles show an exponential form? The reason is
>a percentage of them are removed from the beam with every dx of
>distance that is traversed. However, you have stated earlier
>that you are looking for an energy loss model, not an absorption
>model. (With equal flux from all directions, scattering is symmetric
>with as much flux scattered toward a direction as away.)

Again, a level two concept which will be discussed later. But from the "level one" perspective, it's sufficient to note that "assuming" a net attenuation, their will result in an asymmetrical component in the flux (a resulting current) which will be directed toward the "center of mass" in the weak solution.

>Ionization as a model doesn't seem to work too well. The momentum
>transfer per unit of length for ionization does not decrease
>exponentially, as the particle goes through the material. Thus,
>the shielding effects would not be as needed to explain gravity. (By
>the way, I do realize that scattering and absorption of neutral
>particles do result in the charged particles that are given energy and
>momentum losing it via ionization, but the primary interaction is not
>ionization in this case.)

Agreed. The ionizing radiation "analogy" is just that, an analogy. A well understood process to base a description on, but the actual process to be described must, in the end, match observations.

>Absorption, of course, would work. You have suggested elsewhere
>that you do not like absorption.

At level one, thinking about it as absorption of the "field's momentum" is as good as any. At level two, the natural result of this would be the accumulation of mass in the attenuating body

>This leaves scattering. Elastic scattering has some possibilities, if
>the scattering is off the protons, electrons, and neutrons.

Level two discussion: think of it as "analogous" to how a rifle bullet would behave if fired through a tornado.

>Clearly, electrons will gain more energy than protons, but that
>shouldn't be a problem, since we are looking at momentum transfer.
>Perhaps atoms can be considered rigid bodies in this case.
>
>Anyway, I would suggest that you pick a model for how the
>shielding works and we go on and work out the relevant physics.

I'll note here, that the description below is "narrative" and devoid of any mathematical definitions or descriptions. The purpose is to convey the overall idea, not to prove the idea herein.

I have always "had" a concept of how, what I've have called "Level two" processes work. I have tried to describe these with very little success. I'll try again. I envision the base level medium as consisting solely of perfectly elastic particulate entities with an associated velocity. With sufficient numbers & overall density to constitute a medium. This "medium" has physical spacing between the entities such that the resulting medium is compressible. This condition leads to the well understood state that, at a small enough scale, there is a standard statistical "fluctuation" in the density of the particles. This of course means that not only does the particle count change, but so does the momentum/energy density. If we focus on a single "volume element" we see that this fluctuation revolves around a "normal" or mean value and does so with some periodicity. This leads to a state that in known in nature to produce natural wave motion. Thus we have, a natural causative agent for wave activity and is known in acoustic theory a "white noise" generation. Each point in the field therefore becomes a wave oscillator, and the system will naturally "resonant with harmonics that are "in tune" with the speed & spacing of the particulate population. Ross Tessien has shown graphically that the interaction of simple spherical oscillators can result in vortex motion, providing a mechanism for the formation of vortices in such a system.

The vortex form is a very efficient form to pack "pack" kinetic energy of motion. Now the vortex ring (smoke ring vortex), if formed in a inviscid perfect fluid, becomes "permanent" and cannot be dissipated (See Helmholtz's theorem).

Ring vortices interact with each other and intertwine much like rubber bands in a bag. The resulting vortex region was a state described by Kelvin as a "vortex sponge". The mathematics required to describe such behavior is very complex, and like most fluid problems, unsolvable in an analytical form.

Now, the concept I envision treats each ring vortex as a "quasi-particle" and the sponge as a super-imposed "medium" on the base medium. It is at this level, which has, by the nature of the vortices, interactions at distances by the so-call flow streamlines (Faraday's lines of force). But since each vortex is a unique permanent "entity", it qualifies as a "particle" in the sense of interactions. Thus we have the foundation for QM and QED.

Note however that a vortex IS NOT SOLID. It is a complex "energy" structure of the basic medium. It can and will "vibrate" radiating away energy in wave action. It can also "gain energy" by interacting with the base medium's momentum fluence. This process is what I perceive as "gravitation". The transference of momentum from the base medium into the "excitation" of the vortex structure. Of course, in a "steady state" condition, the "radiated" wave action must re-dissipate back into the randomized energy of the base medium. This is, descriptively, "how" I see the process behaving.

>A couple of more points along this line. We exchanged:
>>>
>>>>And obviously dv can never be greater than the initial particle
>>>>velocity.
>>>
>>>
>>>This is not consistent with elastic collisions. In the COM
>>>reference frame, an elastic head on collision has both objects
>>>ending up with velocities that are equal in magnitude and
>>>opposite in sign from their original velocity.
>>
>>Please think about what is being said. Consider a "fixed"
>>attenuator. Can the a momentum current passing through the
>>region lose more than it had originally?
>
>Momentum is a vector, thus a mass with momentum mv going
>into a collision, can have momentum -mv coming out of a
>collision. The difference is -2mv.

Given a "head on" collision into an immovable wall, yes.
A "head on" collision of two particles of the same mass yields either -2mv or mv depending on you POV. I.e. the particles just change places, going the same way at the same speed.

So, if one wants to consider an object "reflective" not attenuative, your right. But that is not what we were discussing.

>Finally, let us go back to energy and momentum, briefly. I
>propose we start with the model, choose an interaction type,
>and then determine the momentum and energy deposition.
>If the shielding model works, we should be able to determine
>the result of a shielding mass on a uniform flux, and then
>apply it to gravity. By having the gravity act on the flux
>when calculating the energy deposition, I think that you
>accidentally work the answer to your theory.
>
>BTW, with the obvious exception of drag on objects in motion,
>I can work out a shielding model for gravity that involves
>absorption. It does not have what I see in your model
>as a disconnect between momentum and energy dependence on
>mass and diameter.

Of course you can. I would of course like to see your derivation of the resulting power dissipation within the gravitating body.

Is the any other "causal" mechanism that results in a similar outcome?
I would like to hear of any.

Paul Stowe

From - Sat May 09 23:36:59 1998
From: pstowe@ix.netcom.com(Paul Stowe)
Newsgroups: sci.physics,alt.sci.physics.new-theories
Subject: Re: Gravitational shielding (was Aether heating and drag (was commentary))
Date: 10 May 1998 01:23:37 GMT
Organization: Netcom
Lines: 90
Message-ID: <6j2vip$lp8@sjx-ixn6.ix.netcom.com>
References: <6h94u4$tn6$1@nnrp1.dejanews.com> <6hakt8$gsk@dfw-ixnews2.ix.netcom.com>#1/5
<6hospdgbh1@nnrp1.dejanews.com> <6hrfot$9d0@sjx-ixn6.ix.netcom.com>
<6hvdsj$7ao$1@nnrp1.dejanews.com> <6i3rrk$k4v@dfw-ixnews8.ix.net
<6i668k$c9u@dfw-ixnews7.ix.netcom.com>#1/3 <6ib4n7$86m$1@nnrp1.c
<6ig861$h3k@sjx-ixn5.ix.netcom.com>#1/3 <6im0li$n7q$1@nnrp1.deja
<6ioj4t$1hd@sjx-ixn8.ix.netcom.com>#1/3 <6iqvb3jms1@nnrp1.deja
<6ittqcsmo1@nnrp1.dejanews.com> <3552B0E7.1E9792EF@mailhost.da
<6j1tpc$592$1@nnrp1.dejanews.com> <35544287.1E4F@magna.com.au>
NNTP-Posting-Host: val-ca1-19.ix.netcom.com
X-NETCOM-Date: Sat May 09 6:23:37 PM PDT 1998
Xref: news.magna.com.au sci.physics:56614 alt.sci.physics.new-theories:12776

In <35544287.1E4F@magna.com.au> Mountain Man
writes:
>
>shera@wt.net wrote:
>
>> Actually, the shielding model of gravity is, IMHO, a bit better
>> than complete nonsense, it is actually wrong. The shielding
>> model can explain the force of gravity and the 1/r^2 dependance.
>> The problem with it is that the planets should experience drag
>> in their orbits. Feynman has a good discussion of this in his
>> lecturces p 7-10.
>
>
> Firstly, I am not familiar with these, but would like to
> see my self acquainted with their content. I am looking,
> but if anyone has any web references, summaries, or other
> pointers, this'd be welcome in the interim.
>

Well Pete, the LeSage article derives the Newtonian from the shielding
premise. It is a general first principle derivation and doesn't
address the "Feynman Drag" issue specifically. The section from the
"Lectures" is:

Feynman's Lectures On Physics (1963), Feynman, volume 1, chapter 7, pp.
9-10

> "Many mechanisms for gravitation have been suggested. It is
> interesting to consider one of these, which many people have
> thought of from time to time. At first, one is quite excited
> and happy when he "discovers" it, but he soon finds that it is
> not correct. It was first discovered about 1750. Suppose
> there were many particles moving in space at a very high speed
> in all directions and being only slightly absorbed in going
> through matter. When they are absorbed, they give an impulse
> to the earth. However, since there are as many going one way
> as another, the impulses all balance. But when the sun is
> nearby, the particles coming toward the earth through the sun
> are partially absorbed, so fewer of them are coming from the
> sun than are coming from the other side. Therefore, the earth
> feels a net impulse toward the sun and it does not take one
> long to see that it is inversely as the square of the distance
> -- because of the variation of the solid angle that the sun
> subtends as we vary the distance. What is wrong with that
> machinery? It involves some new consequences which are not
> true. This particular idea has the following trouble: the
> earth, in moving around the sun, would impinge on more
> particles which are coming from its forward side than from its
> hind side (when you run in the rain, the rain in your face is
> stronger that that on the back of your head?). Therefore there
> would be more impulse given the earth from the front, and the
> earth would feel a resistance to motion and would be slowing up
> in its orbit. One can calculate how long it would take for the
> earth to stop as a result of this resistance, and it would not
> take long enough for the earth to still be in its orbit, so
> this mechanism does not work. No machinery has ever been

> invented that "explains" gravity without also predicting some
> other phenomenon that does not exist."
>
> There are two distinct components to this idea;
>
> 1) The actual attenuation occurring in the matter. Which, to match the
> Newtonian must be a very small percentage of the incident fluence.
>
> 2) The solid angle subtended by the attenuating body. This, is the key
> to the 1/R^2 relationship, as mentioned by Feynman above.
>
> >> Paul thinks he has a way to handle that problem; I don't. Thus,
> >> we have a thread.
> >
> > Are you referring to the outline presented at ...
> > le_sage.htm ?
> >
> > And an interesting exchange it is.
> > All the best to you both.
> >
>
> Thanks.
>
> It is indeed good to discuss an idea without the usual negativism that
> usually permeates such discussions. Dan has pointed out an interesting
> weakness in my original concept that is leading to a better
> understanding of the idea. This is what is needed. The idea could be
> fatally flawed, but unless one explores all possible aspects, I think
> it is a dis-service to science to "give up" on any such idea. Lorentz
> did, and Feynman's drag is the biggest single open issue to this
> concept. In fact, if an answer is found to address this, it would be,
> by far, the best contender for a causal agent for gravitation.
>
> Paul Stowe
> ---

From - Sat May 30 07:09:06 1998
From: pstowe@ix.netcom.com(Paul Stowe)
Newsgroups: sci.physics,alt.sci.physics.new-theories
Subject: Re: Aether, properties & gravitation ...(was, Aether Drag [Aether Commentary])
Date: 30 May 1998 04:18:21 GMT
Organization: ICGNetcom
Lines: 505
Message-ID: <6ko1ad$ag0@sjx-ixn5.ix.netcom.com>
References: <6k4g7b$bhu@sjx-ixn8.ix.netcom.com> <6kcd9i$id8$1@nnrp1.dejanews.com>
NNTP-Posting-Host: val-ca2-01.ix.netcom.com
X-NETCOM-Date: Fri May 29 9:18:21 PM PDT 1998
Xref: news.magna.com.au sci.physics:60574 alt.sci.physics.new-theories:13609

In <6kcd9i$id8$1@nnrp1.dejanews.com> shera@wt.net writes:

>>Paul Stowe wrote:
> Dan M. wrote:
>
>>>Let me get this clear. It sounds as though your idea is
>>> close to an ideal gas, but has some differences to allow
>>> for a bit of viscosity and the like? That would be reasonable.
>>> This ideal medium can be pictured as a number of particles
>>> with a distribution of energies that gain and lose energies
>>>in collisions?
>>
>>
>>Yes, that IS the entire base level concept. At this level there
>>are NO field forces acting
>
>Then I believe that we can impose the restriction that all
>behavior at the fluid level of complexity must be consistent
>with a near ideal gas.

Yes, it must be.

>This is important because aetherists attribute properties to the
>fluid that are inconsistent with a fluid that is close to an ideal
>gas. By inconsistent I don't mean minor perturbations to allow
>for viscosity, collisions, Rather, the differences are fundamental.

The concept of Aether have many permutations or varieties. This is because given the observable constraints on the physical realm, we're unable to 'directly' measure this medium. If we look at Aether as Maxwell did (only in respect to the EM phenomena in isolation), the descriptive characteristics are totally different than the physical characteristics necessary to describe the 'space-time' continuum for gravitation. Likewise, if one assumes that matter is independent of, and non-interactive with, the medium we would expect standard Galilean transformations when in motion with respect to it. This has been clearly shown to be an invalid, false assumption. However, many latch onto this aspect of the older Aether variants to claim that all Aether concepts are thus shown to be false, ignoring the best Aether model (& also the last of that era, the Ansatze of Lorentz) which removed this assumption, and according W. Pauli (circa 1958), is observationally and mathematically equivalent to GR. He states that although equivalent in this manner, the GR was preferred on "aesthetic" grounds. A good discussion of this can be found in his book "Theory of Relativity", Dover (1958) 1981. The remaining unsolved problem that could unite these two different approaches is finding the specific modification to Poisson's equation:

(delta)Fee = 4pik(mu_o) (Pauli, page 142)

which, as can be seen, is absolutely consistent with this discussion. As stated in Pauli's discussion, this definition must conform to Lorentz group invariance. And that, remains to be accomplished in a formal mathematical manner.

That electro-magnetic, and gravitational 'field' characteristics are different suggests, on its face, that either these are, at the core of their existence, totally dissimilar, or related but different characteristics of a common underlying 'something'. On both philosophical grounds and application of Ockham's Razor, I choose to think the later rather that the former. Choosing the 'idea' that they must be relational and spring from a common source (which BTW is also Einstein's opinion), at least allows one to look for, and attempt to quantify the 'how' could these be related question. If instead one takes the standard (at least as demonstrable by their posted demeanor) 'they're fields, I don't know what they are!, no-one does... attitude, surely won't lead to any answers.

Enough waxing philosophical, the key points that I'm interested in is;

- What differences exist between a basically inviscid simple compressible medium an the properties observed in nature.

- And any ideas on said modification of Poisson's Equation

>>>Well, let me walk thought this. First, let us consider an
>>>isolated, infinite uniform medium.
>>>
>>>I have no problem with assuming virtually zero damping.
>>
>>Good, but the key here is 'virtually', right!
>
>This can be handled by assuming an ideal gas, solving the
>problem and then introducing damping as a perturbation.
>
>>>The total energy in the medium is constant.
>>
>>Yes, from whence spring the conservation laws.
>
>Fine
>
>>I happen to like the idea of a Maxwellean distribution,
>>(which is supported by the CMBR thermal profile). But
>>my colleague...argued ... a Fermi distribution might develop.
>
>Well, I suggest we consider a Maxwellean dist. then, if it
>seems that Fermi-Dirac statistics would work better, going
>to that.
>
>>This would be an interesting thing to model in a
>>supercomputer to see what might develop.
>
>If we assume that the medium is large, so boundary conditions
>need not be in play and that each element is space starts with
>randomly sampled elements from the same distribution, then one

>can rigorously show that as time progresses, each volume will
>continue to have a distribution of elements that is consistent with
>randomly selecting elements from the overall distribution.
>
>I've worked with such sampling quite a bit, and am fairly confident
>of this. If you are interested, I can probably give an analytical
>expression for a three dimensional case of an ideal gas.
>
>However, thinking about it one should see the result. For the
>xn,yn,zn element in the matrix, one will have elements with
>velocity run a one dimensional analysis of this pretty easily.
>Unfortunately, I'm not enough of a mathematician to prove this
>without resorting to a computer.

OK, I just buy into the idea that a Maxwellean distribution is
maintained

>>These fluctuations are caused by the exit of elements from
>>that region to neighboring regions and by the entry of elements
>>into that region.
>
>Absolutely correct.
>
>>>To first order, the measured pressure will be seen as a
>>>random distribution of pressures around a mean, with a
>>>standard deviation determined by the mean number of
>>>elements in the volume and by the energy distribution of
>>>those elements.
>>
>>Yes, and at the limit the deviation is defined by the MFP
>>of the particles flight. The larger the interaction MFP (as
>>differentiated from an attenuation MFP), the larger the
>>deviation.
>
>I don't see the direct connection between MFP and the
>standard deviation of the fluctuation. Assuming an ideal
>gas, with no collisions at all, and one will still get a very
>easy measurement of the SD. The mean free path simply
>dictates the average distance between collisions.

Yes, and a the 'field' or medium level without collisions there's no
possibility of momentum vector changes. Moreover, any wave activity.

>For example, let us consider snapshots of a volume at
>times t0, t1, t2, t3, etc. These snapshots are taken
>far enough apart so that every particle in the volume at
>t0 is absent at t1, etc. We assume that the volume is
>uniform, except for the statistical fluctuations.
>If the average number of elements in the volumes considered
>is N, the standard deviation is sqrt(N), assuming N is large
>enough for Gaussian statistics to hold. For average energy,
>one integrates the variation in N(E)E.

While I've no disagreement with this, perhaps we're talking about two
different concepts. The concept I'm thinking about leads directly to
the definition of divergence.

>>>For consecutive short samples, the distribution of
>>>pressure is not random. Defining tm = (average
>>>velocity)/(radius of volume element).

>>I'll 'assume' that t is time so the above should be:
>>
>>tm = L / c
>>
>>where c is the root mean speed of the particle population
>>and L is the length of a side of a cubic the volume element.
>>
>>Otherwise, its a frequency (1/sec) nu?
>
>Right, I was doing it quickly in my head, and was vacillating
>between discussing frequency and time.
>
>>Either way, the limit t_l then becomes:
>>
>>t_l = MFP / c
>>
>>Or
>>

>>nu = c /MFP
>
>Why must the volume considered by a box 1 MFP on a side?
>For an ideal gas, this would an infinite volume. I think the
>box size should be fixed. Further, as I mentioned below,
>1 MFP is not the distance at which all collisions occur,
>it is the mean distance.

The mean distance between collisions, right. But, the granularity of the medium is a function of this spacing, go smaller (as in, to an absolute limit of zero volume) and the concepts under discussion are, in fact, meaningless. Go large and the 'percentage' of variation in these parameter relative to the total present, reduces. Thus the divergence (limit) is at this level. I could be wrong on this, but in all the literature I've seen, this seems to match the discussion of limits to 'continuum' based definitions.

>>>For t << tm, few particles will be seen to leave or enter
>>>the volume. Thus, the pressure is close to constant.
>>
>>This doesn't make sense to me. At the limit, the most
>>variation occur. The pressure/(energy density)
>>fluctuations are the greatest.
>
>All, right, lets analyze that. Let us take a volume 1cm
>by 1cm by 1cm. If the velocity to be considered is uniform
>at c (or close to uniform), then t = 1cm/c = 3.33*10-11 sec.
>Let us consider a shorter time, say 3.33*10-14 sec. In this
>time, a particle will only travel 1e-3 cm. The only particles
>that could possibly leave are within 1e-3 cm of the edge of
>the box we are considering. Since half are going the wrong
>way, no more than .3% can leave the box. (Actually its
>smaller than that, but its too late tonight to calculate that
>exactly. Those entering the volume must be within 1e-3 cm
>of the volume to enter in this short time. Thus, the
>fluctuations will be smaller than for time periods of
>3.33*10-11 sec, when all the elements could leave.

OK, but how does the ratio of the changes (as in the net loss or gain) to the total present change?

>>>So, if one wishes to obtain a Fourier transform of the pressure
>>>as a function of time, one would see a distribution that roughly
>>>follows the form:
>>>
>>>M = A(1-exp(a'f)*sqrt(b'+cf)
>>>
>>>where f is the frequency considered.
>>>
>>>The magnitude of this will be fairly small. Rough cut
>>>numbers for a small atmospheric volume is 10-6. Further,
>>>in a uniform environment this distribution should be fairly
>>>constant, susceptible only to small statistical fluctuations.
>>
>>In fact given the classical law of equal partition, we'll get
>>a 'thermal' profile of wave activity. However, like any
>>medium, we'll also see resonance and interference.
>
>Boundary conditions cause resonance. Superpositions of
>random sets of samples a distribution are themselves random
>sets of samples from that distribution.

But this isn't what I was suggesting or talking about. I was saying that, given an infinite system (or at least very, very large), random pressure variations will, at some point, create a point wave 'source' or activity. This is known as self generated 'noise'.

Once wave activity is occurring, waves that are out of phase with other wave fronts will destructively interfere, and those of wavelengths in phase (of multiple of 1, 2, 3 wavelength) won't. Since the only source is due to internal density/pressure fluctuations, only the family of waves that aren't in interference will last, given rise to a natural amplification of those waves that superimpose harmonically. Perhaps the term resonance doesn't apply.

>In short, there will be no standing waves of a given frequency
>that occur in such a medium. Standing waves assume correlations
>between time t and t+ n/f. There are no such correlations that
>occur in a medium that is based on collisions of particles that

>start out randomly distributed in space and distributed according to
>an energy distribution.

Perhaps, but in short time, there will appear to be. But technically,
you're probably right.

>>This should naturally lead to a quantum mechanical system.
>>Think about it, the particulate media itself is quantized,
>>harmonics will be based around the MFP, the end result
>>will be SED.
>
>I really don't know where to start with this. First of all,
>the MFP is simply an weighed average, like the mean energy.
>The mean free path is given by the integral of l*exp(-al)dl,
>where adl is the probability of interacting within a distance
>dl.
>
>Second, there is no reason for resonences based on this.
>Interacting between 0.0 and 0.1 MFP is more likely than
>interacting between 0.95 and 1.05 MFP.

First, it isn't possible to have waves in a medium who's
wavelength is less than the MFP. If you think about the
mechanics, you'll understand why. But I'll quote from the
"Handbook of Physics", Section 3, Chapter 8, Subsection 1
"Limits of Frequency and Sound Pressure"

"A gas ceases to behave like a continuum when the wavelength of sound
becomes the order of the mean free path. Strong dispersion and
absorption results, and when the sound frequency becomes considerably
greater than the collision frequency, the ordered sound motion of the
molecules will quickly be transformed into random thermal motion, and
no sound propagation can take place. At ordinary atmospheric
conditions, the mean free path is of the order of 1E-05 cm, a limit
frequency of the order of 1E+09 cps."

Given that for air, c (c is the traditional term for sonic velocity
also, and is so used here) is approximately 30,000 cm/sec, we see that:

c = lambda(nu)

This means that nu = 3E+04/(1E-05) = 3E+09.

This by the way answers the Jean's cube dilemma of classical physics
(the so-called ultra-violet catastrophe) by setting a natural upper
limit on frequencies, thus removing the ridiculous assumption of an
'infinite spectrum'. That is of course, if one assumes a compressible
particulate medium.

Second, I should have used the term amplification of complementary
frequencies instead of resonance. Given very little damping, wave
action would 'couple' over vast distances, leading to a condition
equivalent to a resonance standing wave pattern.

>> "Is there in truth, no beauty?"
>>
>>As a "Natural Philosopher" one must 'believe' there is,
>>underlying all things, a very simple basis. And that
>>science should not just strive for describing processes
>>(correlations) but truly understanding their causal nature.
>
>Beauty can be in the eye of the beholder. For a classicist,
>the work of Kepler and Galileo was an attack on the beauty of
>nature. Simplicity is to be strived for but, a theory should
>be "as simple as possible, but no simpler." I would suggest
>that established criterion of beauty can keep one from accepting
>the pattern that are suggested by the data.

To me, any physical theory must explain why, how, as well as predict
behavior and magnitude. Otherwise, it's just a correlation to data,
nothing more. And certainly NOT a physical theory. As such, for
example, Newton's gravitational equation isn't a physical theory.

>>>Well, how does this match with the standing waves needed
>>>for the vortex theory? Not very well. These waves are
>>>represented by the distribution in frequency space:
>>
>>That not the problem, the problem is formation of vortices.
>

>I would argue with that. Ross has never shown how a uniform
>medium with the expected fluctuations in density and pressure
>that is expected from random sampling from an energy distribution
>will produce regular standing waves. It just doesn't happen.
>Without boundary conditions, correlations do not spontaneously
>develop from uncorrelated events.

>>>>Of course, to this point I've failed to make you see that the
>>>>"m" in the flux we're discussing is like a molecule of air,
>>>>compared to the "mass" of a tornado (the vortex structure
>>>>of an electron).
>>>
>>>Probably because at the sub-atomic level this would be
>>>inelastic collisions and because shielding doesn't work that
>>>way. However, a model where m-electron >> m-flux >>
>>>m-element is not impossible. There are problems, I think,
>>>but we can table them for now.
>>
>>Given the definition of the inviscid medium base level, how?
>
>If you describe the electron as a vortex of very small particles,
>and also state that the elements of the flux, the shielding
>of which describes gravity, do not exhibit hard scattering,
>then there are strong restraints on the nature of the
>electron, the gravitational flux, and the elements that make
>up the electron. I was simply describing the limitation that
>are set. If one does physics, it is critical that one keeps
>track of the consequences of every postulate.

Right, but we're actually tracking the medium's momentum vectors along path-lines through such space. Asking if and how these change traversing the vortex region.

>>>Since the acceleration due to gravity is 270 m/s^2, the force on a
kg
>>>is 270 kgm/s^2 =270 Newtons.
>>>
>>>So 270 Newtons = 2.22e-6*F1. F1=1.21e8 Newtons. Lower than
>>>what I had before by a factor of 2.22, because I estimated
>>>1e-6 instead of 2.22 e-6 absorption.
>>
>>Right, but THAT force (1.21E+08) isn't what the sun experiences,
>>it's the 270. So the energy is based on 270 not 1.21E+08.
>
>I find it frustrating that after a detailed explanation,
>that you do not point out any error in my analysis, but simply
>restate your opinion. Let me try to walk through the ideas
>again, and have you point out where you differ from my
>analysis. I also left my entire analysis above intact, so
>you can point out areas of disagreement in this analysis also.

I understand your frustration in this regard (colloquially speaking, been there, done that). But I'm also confused. There is a distinct difference between the total field "potential' force (if the hypothetical attenuator absorbed ALL the incident flux) and the actual realized transfer.

>Take a 1 kg mass far away from any other object. The
>force on this mass from the x direction is 1.21E+8 Newtons.
>The force on this mass from the -x direction is 1.21E8
>Newtons.

Why? That number is the total potential if the 1 kg mass were a 'black' body absorbing all that impinged upon it. That's not what I thought we were discussing.

>The momentum loss experienced by the flux in this mass is
>1.21E8 kgm/sec per second. That is
>mdv(dn/dt) = 1.21E8 kg m/sec per sec. The energy loss of
>each particle is mncdv(dn/dt)

You have totally lost me here. The vector velocity depletion along the x direction is dv, the transferred momentum is mdv, yes. The work done by the field is mcdv, yes. So what is dv?

Gravitationally, dv is:

dv = Sqrt(2GM/R)

where M is the test mass (1 kg)
and R is the cube root of; the specific volume (m^3/kg)
multiplied by the 1 kg, divided by 4pi/3.

Assuming Iron, we get:

dv = Sqrt(2G/[3.13E-02]) = 4.26E-09 m/sec

So, we see the actual variation from the initial total potential is
1.4E-15% (given a total loss would be a dv of c)

There isn't any more interaction, the rest just 'blows' straight
through and doesn't 'see' the 1kg mass at all. I wasn't trying to
sidestep your question, I just don't understand the viewpoint.

>>>I thought I just did. IMHO what you did was work backwards
>>>from the answer you expected. Let me go through it more explicitly.
>>>Let us consider the shielding provided by a small mass: say 1 mg.
>>
>>While I did have to normalize the result (due to the unknown
>>magnitude of the incident power flux), the overall analysis was
>>based upon conservation. So, I really would be interested in any
>>logical flaws in the process.
>
>The short version of the logical flaw was that after giving
>the flux as classical particles, after establishing a means
>of determining momentum loss, you eschew using the relationship
>between momentum loss and energy loss for classical particles
>and calculate energy from a totally different set of assumptions

OK, let's look for the flaw. I started by saying,

Power deposited must equal to the field's total 'potential' power
subtracted from that which passes through untouched.

P_a = P_in - P_out

Next we said that, given the shielding form, the Power out
should be:

P_out = P_in e^-b

Where b is the attenuation term.

Any flaws so far?

Next we put this into our first equation.

P_a = P_in - P_in e^-b

This leads directly to:

P_a = P_in(1 - e^-b)

For our weak attenuation cases where b << 1 we get:

P_a = P_in(b)

I'll stop here and ask if the logic is OK so far.

>IMHO, this is a consistent pattern with developers of
>non-mainstream physics. A logical path is followed as long
>as it gives the desired results. When results start becoming
>problematic, a different path is chosen for the rest of the
>work. Thus, all the logical consequences of a set of assumptions
>are not explored to see if they are consistent with data.

Now you can fault me for perhaps making a math error or even an
inadvertent logic flaw, but I assure you, the above statement is not
true, for me at least. I consciously try very hard NOT to shy away
from such problems. That, as the fictitious Mr. Spock would say, would
be most illogical. An Ostrich's approach that helps no-one especially
ones self.

I snipped your power derivation, since until we resolve the force

issue above, the results are in debate.

Paul Stowe

From - Wed Jun 03 05:57:13 1998
From: pstowe@ix.netcom.com(Paul Stowe)
Newsgroups: sci.physics,alt.sci.physics.new-theories
Subject: Re: Aether, properties & gravitation ...(was, Aether Drag [Aether Commentary])
Date: 3 Jun 1998 01:14:26 GMT
Organization: ICGNetcom
Lines: 176
Message-ID: <6l281i$s3p@sjx-ixn4.ix.netcom.com>
NNTP-Posting-Host: val-ca3-09.ix.netcom.com
X-NETCOM-Date: Tue Jun 02 6:14:26 PM PDT 1998
Xref: news.magna.com.au sci.physics:61297 alt.sci.physics.new-theories:13775

In <6l0n77$562$1@nnrp1.dejanews.com> shera@wt.net writes:
>
>
>
> shera@wt.net wrote:
>
>> So far, it looks reasonable. What needs to follow is
>> a determination of b. For a tube with cross sectional
>> area A, length l, and density d, I get
>>
>> b= A*l*d*X, where X is a constant that is to be determined.
>>
>Actually, the A term does not enter in the calculation of b.
>It enters into the total flux through the tube. It was too late
>last night.
>
>b=l*d*X.
>

Acknowledged

Paul Stowe wrote:
> Dan M. wrote:
> >
>>>I find it frustrating that after a detailed explanation,
>>>that you do not point out any error in my analysis, but
>>> simply restate your opinion. Let me try to walk through
>>> the ideas again, and have you point out where you differ
>>> from my analysis. I also left my entire analysis above
>>> intact, so you can point out areas of disagreement in this
>>>analysis also.
>>
>>I understand your frustration in this regard (colloquially
>> speaking, been there, done that). But I'm also confused.
>> There is a distinct difference between the total field "potential'
>> force (if the hypothetical attenuator absorbed ALL the incident
>> flux) and the actual realized transfer.
>
>I accept that, and I will try to be consistent in explaining the
>actual transfer. In this post, I will limit myself to one example,
> in order to minimize the turnaround time (which has been
> growing along with the length of the posts on both ends.) I will
> not address other issues, not as a means of ignoring other
> discussions, but as a means of getting to the point quickly. If
> this doesn't clarify things for us, I'll go back to the start of your

> logic chain and try from there. Actually, I'll try to address
> the end of your last post here too.
>
>Let us again assume that a 1 kg mass feels a net force of 270
>Newtons because of the shielding effects of the sun. Rounding,
>let us say the sun absorbs 2e-6 of the flux coming through it.
>So far,this is all consistent with your shielding (allowing my
>rounding of course.)
>
>Let us then consider a body that has the same dimensions as the
>sun, only the density of this body is 10x the density of the
>sun at this point. What would be the force on a 1 kg mass at the
>surface of this body.

>
>I would suggest that the force would by 2700 Newtons, 2e-5
> shielding is still in the weak attenuation range.

OK, here lies the problem. If we "compress" the 1 kg mass to a density of 10x its original value, nothing changes. The major gradient to which this mass is responding to is still generated by the sun. The net force remains unchanged at 270 Nt. This will continue to be true until the 1kg mass is sufficiently compressed that its OWN attenuation coefficient sufficiently departs from the weak solution. This will occur when, (if the 1 kg mass is spherical) the equation: 2Gm/rc^2 (Where m is 1 kg and r is its radius) sufficiently approaches unity and the exponential form exerts itself. This is discussed in our derivation and I'll include the appropriate section of that herein. However, I won't attempt to put the full derivation herein.

"Characteristic Field Acceleration:

The above two-body equation also clearly shows the acceleration that a material body will experience in a field current (momentum flux gradient). Using the standard F=ma equation for force and acceleration gives a = F/m and:

a_1 = Fee_o u_s^2 M_2/R^2 = Fee_net u_s {A.27}

Using equation A.23 also gives:

a = Fee_o u_s^2 M/R^2 {A.28}

Note that in the case of a weakly interacting body the acceleration resulting from this type of field current is not dependent on the mass of the body. Thus any such matter body responds to a field current in the same manner (regardless of its mass). The important concept here is that field current creates an acceleration independent of mass, the resulting "force" is only a by product of this acceleration. This clearly demonstrates the derivative reason for the postulated principle of equivalence."

Just to be clear, M in the above would represent the Sun's mass, Fee_o the total potential momentum flux, u_s the standard mass attenuation coefficient in m^2/kg, and R the distance between centers of mass.

>What fraction of the momentum is absorbed? Its late to accurately
> calculate,but assuming this mass is a sphere with a density of
>1 g/cc, I get <1e-20. Definately not strong attenuation. :-)

That value does look about right for the 1 kg mass and density and yep, not a strong attenuator at'all.

Now onto the power flux derivation,

>> OK, let's look for the flaw. I started by saying,
>>
>> Power deposited must equal to the field's total 'potential' power
>> subtracted from that which passes through untouched.
>>
>> P_a = P_in - P_out
>>
>> Next we said that, given the shielding form, the Power out
>> should be:
>>
>> P_out = P_in e^-b
>>
>> Where b is the attenuation term.
>>
>> Any flaws so far?
>>
>No
>>
>> Next we put this into our first equation.
>>
>> P_a = P_in - P_in e^-b
>>
>>This leads directly to:
>>
>>P_a = P_in(1 - e^-b)
>>

>>For our weak attenuation cases where b << 1 we get:
>>
>>P_a = P_in(b)
>>
>>I'll stop here and ask if the logic is OK so far.
>>
>So far, it looks reasonable. What needs to follow is
>a determination of b. For a tube with cross sectional
>area A, length l, and density d, I get

Good, next I divided both sides by the surface area (4pir^2) to define this in terms of power flux. This was done because in the steady state condition, obviously power in from attenuation must be re-radiated back into space i.e. the standard q_in = q_out. It was this q_out that I went looking for. In standard thermal physics, q = sT^4 where s is Stefan-Boltzmann's constant and T is absolute temperature in Kelvins.

And the overall attenuation coefficient (b) is the same as determined for the momentum flux which we tentatively settled on 2GM/rc^2.

So now we get:

q = q_o(2GM/rc^2)

Rearanging those terms that are constant, for weak attenuators we get:

q = [2Gq_o/c^2]M/r

Which I then called the bracketed terms k, leading to:

q = kM/r

But we know G and c, so the only unknown in this equation is q_o.

So, in the end, where's the logic flaw?

Paul Stowe

From - Tue Jun 09 07:22:12 1998
From: shera@wt.net
Newsgroups: sci.physics
Subject: Re: Aether, properties & gravitation ...(was, Aether Drag [Aether Commentary])
Date: Sun, 07 Jun 1998 18:10:00 GMT
Organization: Deja News - The Leader in Internet Discussion
Lines: 200
Message-ID: <6lel1nkvp1@nnrp1.dejanews.com>
NNTP-Posting-Host: 208.236.21.94
X-Article-Creation-Date: Sun Jun 07 18:10:00 1998 GMT
X-Http-User-Agent: Mozilla/3.0Gold (Win95; U)
Xref: news.magna.com.au sci.physics:62194

Paul Stowe wrote:
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> There are two issues here. First, there is the "maximum"
> acceleration limit that is imposed by the attenuation process (one
> can't arbitrarily let the math go to infinity because there really
> is a physical process). Second is the issue of the "maximum"
> acceleration that can be generated by any given "gravitating"
> body.
>
>The first one is easier to deal with. In the worse case, the field
>particles are "fully" attenuated. Thus the delta v in the body is
>all their initial speed which is probably around c (in perfect gas
>mechanics, the difference between particle speed and transverse
>wave speed in vortex interactions is Sqrt(3), so particle speed
>could possible be Sqrt(3)c). This interpretation is controversial
>and thus, left to later, and c will be "assumed" for these
>discussions. This condition, in an attenuating body would never
>be achieved, that is because, while the "core" regions go "black",
>there are always grazing paths that aren't. Much like a translucent
>glass sphere of increasing radius. But yes there would be an
>upper limit to the acceleration gradient that could be produced.
>This isn't in contradiction to the process under discussion.
>
Let me repeat this view, to see if I understand correctly.
Let us assume that the flux is traveling at c and that it
can be attenuated to a speed of zero. Throughout this process,

dp/dmass_attenuator is constant for the flux. Once the speed reaches zero, any additional mass along the path the flux would have traveled sees no flux. Thus, one cannot get more than 100% attenuation, and the shielding effect of gravity is seen.

Now, considering the flux from direction x, we know that it is an isotropic flux. Thus the flux at an angle of 80 deg with respect to x has an x component cos(80 deg) and a very short path through the least dense part of each of the bodies I mentioned above. So, the flux straight through the sun shows shielding at a relatively low multiple of the sun's mass, while the flux at 80 degrees shows shielding at a much higher multiple. So there isn't a hard cutoff.

That is consistent with a shielding theory. I only neglected it for simplicity, assuming the various factors averaged out. Of course, that's not precise, but I was going for back of the envelope calculations. It appears that we don't disagree here.

>However, the real question (I really do think I understand it now)
>is that of the maximum acceleration gradient that can be generated.
>That is because, the observed "forces" in gravity are due to this
>gradient, not a "surface" pressure. Therefore, the equation:
>

Actually, my question was even simpler than that. It was, what are all the forces on a 1 kg mass. Let me look at that again.

We know that, on the surface of the sun, a 1 kg. mass has a net force of 270 Newtons. You stated that the sun provides about 2.2e-6 shielding. Thus, I understood that the 270 kg gravitational force is due to flux from the direction of the sun having momentum that is 1-(2.2e-6) of what would be observed if the sun were not there.

Let me use a metaphor to illustrate my point. Assume the flux is like water stream from two hoses hitting the 1 kg mass. One hose has the full force. The other hose has a force reduced by 2.2e-6. The net force is 270 Newtons. Thus, I would summarize that the force on the 1 kg mass from each side is app. 2.5e8 Newtons.

> P = P_o(1 - e^-b)
>
> isn't directly valid attenuation gravity. The biggest problem
> (at least it was for me) is to stop thinking of matter as something
> different, and thus separate and unique from the medium.

If its not different, then why is there no gravitational attenuation in the medium, apart from the matter? Particularly, since we can assume that any vortices are made up of a number of small "particles."

>Generally we think of attenuation as the distinct transfer of
>momentum/energy from one entity, say a photon, to another, say
>an electron. In a truly fluidic model of matter and EM transmissions,
>this really isn't the case. The "attenuation" process is the simply
>the conversion of linear momentum into angular momentum and then
>into wave action.

I thought your system was a fluid that was based on a number of very small classical elements that interact elastically with one another. If that is true then both momentum and angular momentum are conserved. Linear momentum does not get translated into angular momentum in such a system.

>
>OK, the net acceleration is simply the net change in velocity (v)
>squared divided by the distance traversed (x). And this distance
>involves the both the incoming and outgoing paths (since they are
>symmetric opposites). We thus get:
>
>a = v^2/2r
>

The definition of acceleration is dv/dt. Instantaneously,

we do have v = dx/dt, and dv^2=2vdv. So dv2/dx=2vdv/dx
= 2(dx/dt)*(dv/dx)=2dv/dt. So d(v^2)/dx = 2a.

Let us take an example. Let us consider an object with an
initial velocity v= 1000 m/s and acceleration a=1 m/sec
traveling over a distance of 1000 meters. We have at the end,
v=1000.9995 m/s ~= 1001 m/s. v^2 at the end is app
1002000 (m/s)^2. dv/dx = 2000 (m/s)^2/1000 m = 2 m/s^2 = 2a.

>And the maximum is, of course c^2/2r.

Well, that's not really true, because that assumes uniform
acceleration. If we assume that electrons, protons and neutrons
are made of tiny particles, the density of these particles
should determine the maximum acceleration.

> then get:
>
> a_g = c^2/2r(1 - e^-b)
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>Leading us back to a weak solution of:
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>a_g = c^2/2r(b)
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>Which of course becomes:
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One other problem with this. The r in GM/r^2 is the distance
from the center of mass of mass M to the center of mass of
mass m, which undergoes the acceleration a. It looks as though
you are looking for similar terms without walking through the
origin of each. One of the most tedius parts of physics is
tracking

>So the 'radial' dependence is built into the process at the fundamental
>level.
>
Actually, if this were true, the radius of a gravitating body
should affect the force of gravity exerted by it. The 1/r^2
term in a shielding model of gravity corresponds to the
1/r^2 component of the flux from a radioactive source.

Going back to the flux, if we assume a classical particle,
we have E=1/2 mv^2 and p=mv. If we are in the weak attenuation
limit dv <I must say, that for the first time in over 2 years since the
>derivation of attenuation gravity was first posted in this forum, you
>are the first to pick up on this issue. To have done so indicates a
>very good grasp of the process not demonstrated by any other.

Thank you

>Thanks for taking the time and effort to work through the idea.
>That, in the end is all that really matters, since I personally
>believe that true science is the ability to look at ALL viewpoints
>with an open, unbiased (well minimal bias, since no-one comes to
>anything truly unbiased) demeanor.

I appreciate that you will consider the difficulties with
the theory. Historically, I have gone on many circular arguments
with aether supporters, never nailing down the theory they
presented.

Finally, I would like to suggest a possibility to you. If we find that the
aether model has difficulties that we cannot correct at this time, and thus
it has significantly greater disadvantages than advantages, I would like to
suggest that you consider it reasonable to drop such a model. I realize that
having a fundamental layer as it is presently in GR and QM conflicts with
your sense of beauty. I acknowledge that beauty has been used as a means of
evaluating promising theories.

However, it has not been, and should not be a primary criterion. If
only a theory one sees as ugly matches data, and this persists for a
long time, it may be time to reconsider aesthetics. Rock and roll may

not be beautiful to someone who grew up on Irving Berlin but it does
have its own aestetics. :0).

Dan M.

From - Wed Jun 10 08:00:34 1998
From: pstowe@ix.netcom.com(Paul Stowe)
Newsgroups: sci.physics,alt.sci.physics.new-theories
Subject: Re: Aether, properties & gravitation ...(was, Aether Drag [Aether Commentary])
Date: 10 Jun 1998 04:03:05 GMT
Organization: ICGNetcom
Lines: 268
Message-ID: <6ll0hp$o8t@dfw-ixnews9.ix.netcom.com>
NNTP-Posting-Host: val-ca1-09.ix.netcom.com
X-NETCOM-Date: Tue Jun 09 11:03:05 PM CDT 1998
Xref: news.magna.com.au sci.physics:62616 alt.sci.physics.new-theories:14120

In <6l4j95$n0i$1@nnrp1.dejanews.com> shera@wt.net writes:
Paul Stowe wrote:
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>reaches zero, any additional mass along the path the flux would
>have traveled sees no flux. Thus, one cannot get more than 100%
>attenuation, and the shielding effect of gravity is seen.

Yes

>Now, considering the flux from direction x, we know that it
>is an isotropic flux. Thus the flux at an angle of 80 deg with
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> calculations. It appears that we don't disagree here.

I didn't think so, but included this to be sure.

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>>>now) is that of the maximum acceleration gradient that can be
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>>>are due to this gradient, not a "surface" pressure. Therefore,
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>> (at least it was for me) is to stop thinking of matter as something
>> different, and thus separate and unique from the medium.
>
>If its not different, then why is there no gravitational
>attenuation in the medium, apart from the matter?
>Particularly, since we can assume that any vortices are made
>up of a number of small "particles."

I think there "is" gravitation apart from "matter". In GR, EM
energy gravitates. I think this effect is the observed Hubble term.
The problem is, if the attenuation is as poor as it appears to be in
regions of high 'stress' (matter), how much would the normal
background medium attenuate? Not measurably, except over vast
distances. But this is a different issue, while interesting in the
subtle side of this process, not germane to the question at hand.

>>Generally we think of attenuation as the distinct transfer of
>>momentum/energy from one entity, say a photon, to another,
>>say an electron. In a truly fluidic model of matter and EM
>>transmissions, this really isn't the case. The "attenuation"
>>process is the simply the conversion of linear momentum into
>>angular momentum and then into wave action.
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>I thought your system was a fluid that was based on a number
>of very small classical elements that interact elastically with
>one another. If that is true then both momentum and angular
>momentum are conserved. Linear momentum does not get
>translated into angular momentum in such a system.

If that is strictly true, how do you reconcile kinetic theory with
the observed processes of fluid rotation? The 'bulk' medium does
have 'angular momentum' but, the underlying particulate entities
constituting the motion cannot.

>>OK, the net acceleration is simply the net change in velocity (v)
>>squared divided by the distance traversed (x). And this distance
>>involves the both the incoming and outgoing paths (since they are
>>symmetric opposites). We thus get:
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>Well, that's not really true, because that assumes uniform
>acceleration. If we assume that electrons, protons and neutrons
>are made of tiny particles, the density of these particles
>should determine the maximum acceleration.

But I was talking about the maximum possible for the overall process.

>> then get:
>>
>> a_g = c^2/2r(1 - e^-b)
>>
>>Leading us back to a weak solution of:
>>
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>>Which of course becomes:
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>mass m, which undergoes the acceleration a. It looks as though
>you are looking for similar terms without walking through the
>origin of each. One of the most tedius parts of physics is
>tracking

OK, it's the weak solution, so, 'morph' the mass into a shape that is a conical segment directed at a point of interest and determine the travel distance.

>>So the 'radial' dependence is built into the process at the
>>fundamental level.
>
>Actually, if this were true, the radius of a gravitating body
>should affect the force of gravity exerted by it. The 1/r^2
>term in a shielding model of gravity corresponds to the
>1/r^2 component of the flux from a radioactive source.

No, the 1/r^2 is strictly the geometric aspect of this general process, which does include distributed radioactive source mediums.

>Going back to the flux, if we assume a classical particle, we
>have E=1/2 mv^2 and p=mv. If we are in the weak attenuation
>limit dv <we assume the flux moves at c, we have dE=cdp. So, if the
>energy deposited in a body is not just dependent on the mass
>of the body, but is dependent on the radius, then the momentum
>deposited is also. Thus, gravity would not dependent on only
>the mass.

I've now tried to attempt to explain this three different ways. I thought we agreed to the following tenets:

- The medium's flux density is essentially uniform & isotropic.

- Matter acts to attenuate this flux resulting in momentum gradient in the medium.

- For the weak solution the force remains unchanged when the radius of a fixed mass is changed.

Now, (yet another way of attempting an explanation) if the force generated by the attenuation process is unchanged and the radius is reduced, the pseudo pressure (force per unit area) must intensify.

Second, the median flux density remains relatively unchanged, so the only way to intensify the pseudo pressure is to intensify the velocity gradient (acceleration). But as can be shown, the observed 'force' on any test mass is the product of this acceleration. So, the observed force on test masses is increased in direct proportion to the intensifying velocity gradient.

>>I must say, that for the first time in over 2 years since the
>>derivation of attenuation gravity was first posted in this forum, you
>>are the first to pick up on this issue. To have done so indicates a
>>very good grasp of the process not demonstrated by any other.
>
>Thank you

No, thank you for the time & effort to seriously and (more importantly) cordially delve into this topic.

>>Thanks for taking the time and effort to work through the idea.
>>That, in the end is all that really matters, since I personally
>>believe that true science is the ability to look at ALL viewpoints
>>with an open, unbiased (well minimal bias, since no-one comes to
>>anything truly unbiased) demeanor.
>
>I appreciate that you will consider the difficulties with the theory.
>Historically, I have gone on many circular arguments with aether
>supporters, never nailing down the theory they presented.

Most threads are arguments (which is really sad), since if a
discussion degenerates into an argument, it is rare that anything
can be accomplished.

>Finally, I would like to suggest a possibility to you. If we find that

>the aether model has difficulties that we cannot correct at this time,

>and thus it has significantly greater disadvantages than advantages,
>I would like to suggest that you consider it reasonable to drop such a
>model. I realize that having a fundamental layer as it is presently
>in GR and QM conflicts with your sense of beauty. I acknowledge that
>beauty has been used as a means of evaluating promising theories.

The really disheartening thing is, for me, I don't see a dichotomy in
either GR or QM to the process under discussion. In fact, GR seems
to be a very good mathematical representation of this process. One
simply has to shift their viewpoint slightly to see the curvature of
space-time as the velocity gradient in the medium. In the weak
solution, there is perfect convergence, in the strong solution, the
flux density set an upper limit that must be incorporated. This should
lead to an exponential form.

QM is the same thing, a particulate medium is inherently quantized.
You can't have it any other way. Also, it should also be clearly
evident that continuity of 'smooth' field processes break down as the
evaluation scale shrinks and the granular quantum processes become
more prevalent.

>However, it has not been, and should not be a primary criterion.
>If only a theory one sees as ugly matches data, and this persists
>for a long time, it may be time to reconsider aesthetics. Rock and
>roll may not be beautiful to someone who grew up on Irving Berlin
>but it does have its own aesthetics. :0).

Is isn't so much aesthetics, it the root question as to what
constitutes a physical theory, verses what constitutes simply a
mathematical correlation.

Paul Stowe

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