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The Allais Effect and Majorana

by Miles Mathis

In the 1950's, Maurice Allais discovered a variation in pendulum motion during his work with the anisotropic paraconical pendulum. The most important variation was discovered during a solar eclipse, but he also found a standard variation that recurred about every 24 hours, 50 minutes. Allais used these variations to propose that neither Newtonian nor Einsteinian gravitational mechanics were complete. He presented a theory that centered on the idea of a sort of ether, or what he called an anisotropy of space. According to him, this required a reassessment of the experiments of Michelson/Morley and Miller, and most importantly of the theory underlying Relativity.

Three things have conspired to keep Allais' theories and findings out of the mainstream. First is that he became more famous as an economist, winning the Nobel Prize in Economics in 1988. Physics does not currently take kindly to autodidacts, amateurs, or other non-tenured "non-professionals" in physics. Second is that he has made some rather politically incorrect comments about Einstein, calling him a plagiarist. I have no intention of entering a debate about the politics of priority here, so we will move on. Third is that he contradicts the standard model, and that is his real offense. The first and second faults here would be overlooked if not for the third.

I discovered Allais in a rather roundabout way. I was reading a book by Webster Tarpley, and he mentioned Allais. I am not an economist and not much interested in economics, so I had never heard of him. Being universally curious, however, and in full control of my computer, I did a quick search. In this way I learned not only about his economic theories, which I find fairly compelling, but also about his physical theories, which I am still discovering. My French is not really good enough to read at speed through scientific papers, and only a part of Allais' physical output has been translated.

At any rate, I saw fairly quickly that Allais' effects fit into my theory like a hand in a glove, with or without taking his theories along with his effects. And that is why I have rushed this paper into print, even before I fully understood Allais' own ideas about his effects. It may be that he and I agree on many things; or not. My opinions on Relativity and Michelson/Morley are well known to those who want to know them, so I will have to let others compare my opinions to those of Allais. For now it is more important that I tie Allais' pendulum to my theory of gravity and E/M, and specifically my correction to tides.

I have shown in a series of papers that what has been called the gravitational field is in fact a compound field, one that includes both the field of gravity and the foundational E/M field. I have even presented equations A href="http://milesmathis.com/uft.html that show how to separate the two fields out of Newton's equation. In this way I claim that I have found the unknown field of Allais, and the cause of his effect.

This brings me to Majorana. In researching Allais, I also stumbled on another experiment that is now mostly hidden, that of Quirino Majorana. His nephew Ettore is somewhat better known, both for being called a genius by Enrico Fermi and for his mysterious disappearance. Quirino is now mentioned only in regard to "gravity shielding". He has no page of his own at Wikipedia, and the page on gravity shielding is a whitewash. Nor is it only Wiki that has been cleansed. Majorana's original papers from 1920[1] are no longer available at *Philosophical Magazine*, although the journal does exist. You can't even order them online, since the back issues stop at 1925. Other papers relating to gravity shielding have also been removed, for example a paper that was until recently at adsabs.Harvard.edu called "An Experiment Concerning Gravitational Shielding" by L.B. Slichter, M. Caputo and C.L. Hagar, 1965, *Journal of Geophysical Research*.

The only thing I could come up with on the internet at short notice is the response to Majorana by H.N. Russell from 1921.[2] The Wiki page on gravity shielding neglects to mention that Majorana came to his theory, like Allais, via pendulum experiments, but Russell is nice enough to tell us this himself. He also tells us, "Professor Majorana, in a very interesting series of papers, has proposed a new theory of gravitation, with experimental evidence supporting it." Notice that last bit. Although Russell contradicts all of Majorana's conclusions, he is gracious enough to admit that some evidence was presented. Wiki wants us to think Majorana just pulled his ideas out of the air, proposing a new extinction coefficient simply to be contrary. By extinction coefficient, they mean h in this equation:

$$F = [GMm/r^2][e^{-h\int \rho\, rdr}]$$

But Majorana and Russell wrote it like this

$F = [GMm/r^2][e^{-\int h\theta\, dr}]$

Where h is "a second universal constant, that of absorption of gravitation" [quote from Russell] and θ is the density of matter along the line of attraction.

Because the best laboratory experiments have produced "an upper limit for shielding of 4.3×10^{-15} m²/kg" [Wiki], we are told that Majorana cannot be correct. Russell puts it this way: he says that any new field must create a force, and that current equations and data have no room for such a force. Take the Moon as an example, he says. If e takes on any measurable value in the equation above, F must change and with it any and all orbital numbers.

Russell is quite right about this, so rather than defend Majorana, I will defend my own equations. I will push both Majorana's findings and Allais' into line with mine, showing that neither Russell's nor any of the standard model's historical complaints touch my theory. None of the critiques against Majorana or Allais concern me, since my equations and theory are not theirs.

Before I show this, I will make a short diversion into LeSage. I have to admit that I just discovered LeSage during this research into Allais and Majorana. I have been making my own way since the beginning, and have not always been aware of those who had made similar paths in the bush before me. Lesage (c1750), following Duillier (c1690), is famous for proposing what is called push gravity. This is an explanation of gravity by which all objects are radiating a field. Attractions are then caused by shielding this field. I have been mistaken as a proponent of this field, and I now see why (especially considering my new paper on Cavendish, whereby the attraction is caused by shielding). But my theory is not push gravity.

Majorana has been tied to LeSage, since he also proposes shielding. But, as you can see from Russell's description above, Majorana is proposing a *second* field, and only the second field is a shielding field. This is closer to my theory, since we have two fields here, but Majorana's theory and equation are not equivalent to mine either. Majorana does not tie his second field to E/M, or explain how it fits into Newton's equation. As you can see, he wants to *add* an effect to Newton's equation, which must push it out of line. But I take Newton's equation to be correct, then *insert* my E/M field directly into it. This means that with my equations, no variance from current numbers will be found, unless the E/M field is blocked. This is how my equations answer both Russell's paper and all more recent critiques of push gravity, shielding, and Allais effects. I don't propose any variance from current numbers, since I show that (minus Relativity) Newton's equation is correct. Rather, I show that Newton's equation *already* describes a compound effect, and that the numbers we have now are compound numbers, achieved by a differential field. We could not find variance unless we blocked one or other of the two constituent fields.

However, I have also denied that gravity can be blocked. I have defined gravity as acceleration only, and acceleration cannot be blocked. In this way I concur with standard model assumptions and the equivalence principle of Einstein. The gravitational component of the compound field cannot be blocked or shielded in any fashion. Only the E/M component of the field can be

blocked, and this is what is happening in Cavendish. Once the E/M field is blocked, you get a variance in the total field. But this would not affect orbital numbers or any other astronomical numbers. All our experimental values would be expected to stand with my equation, *except values achieved during eclipses*. During eclipses, we would have shielding by solar system bodies; and only during events like this would our equations behave like I have shown the Cavendish equations behave. In normal situations, my equations are not at all like those of LeSage or Majorana, neither numerically nor theoretically.

This is all to say that neither LeSage nor Majorana nor Allais proposed the correct differential field. The correct differential field would show no variance from Newton's equation under normal circumstances, confirming current data, but show the reason for variance during an eclipse, confirming their own data. Allais gives us no specific equations, that I have found. Majorana's equation fails as I showed above, since it would show variance in all situations. In his equation, the variable F changes with any and all field densities for θ. Variations in his second field must cause a variation in F, and these variations in his second field would always be present. All real fields have real densities, so that all real equations would have to deviate from Newton. In my equation, this is not so. My equation is

$$F = E + H$$

Where E is the foundational E/M field and H is the gravitational field caused only by acceleration. F is then the compound field found by Newton's equation. So neither of my fields is an addition to Newton. Newton's equation *already* included them, so no variance will be found, short of an eclipse.

This is just to say that Majorana's field density is external to the classical and currently accepted value for F. This is because the variable θ is not part of the term GMm/R^2. His field density is external to that term, and therefore outside of F. But my field density is a part of F. In my theory and equations, the field density is important only regarding E, but E is part of F, as you can see by the equation F = E + H. If the field density is part of E, it is also part of F. My density is not external to F.

Many of the so-called "crank" solutions posted on the web to the Allais Effect and other anomalies have the same problem Majorana had with his equation. I do not think most of these solutions come from cranks, but they are mistaken. The standard model is also mistaken, so calling people that are wrong "cranks" is hardly to the point. It would be better to show where they are wrong. Let's look at one: the highest listed on a current web search (Jan. 2008). This belongs to Joseph Nduriri http://blackholethermodyn.site.voila.fr/enter.html, who has been very active recently, posing his solution in many forums. Mr. Nduriri's solution is no worse than Majorana's, and Majorana was no crank. But they fail for the same reason. Mr. Nduriri's central equation is this one:

$$F= (-Gm_1m_2 + kq_1q_2)u_r/r^2 + (\mu gm_1m_2 + \mu_o q_1q_2)V_1\times(V_2\times u_r)/r^2$$

Not a bad looking equation, except that you can immediately see that this will give a straight addition to Newton's equation. Mr. Nduriri's F, like Majorana's F, will not match Newton's in

normal circumstances. And Mr. Nduriri's addition is not a relative one. He is trying to add the E/M field to the equation, in the same way I did. The problem is that his equation gives us variation even when we don't want or need variation. It predicts variance in situations that we know, experimentally, have no variance. What Mr. Nduriri wants is an equation that includes his E/M field *within* the old F, that is to say, within GMm/r^2. But the form of his equation makes this impossible. He has Newton's equation, and then some. But Newton's equation is already correct. It doesn't fail. It only fails to include the E/M field. So Mr. Nduriri's equation can't be the correct one.

LeSage proposed that gravity was a push. I show that our field of data is both a real push and an apparent pull. Two fields. Majorana proposed two fields like me, and in opposition like me, but he added the opposition outside of F, as another term in the equation. I have shown that it must be included within F, as a part of the classical measured force.

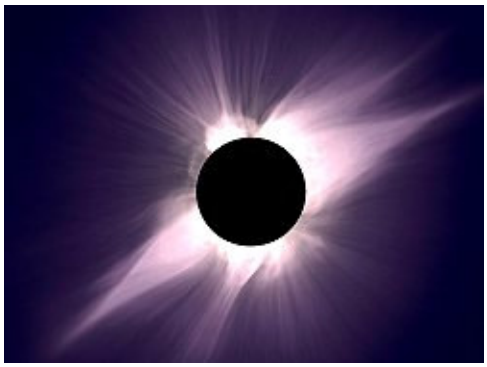
But notice that my inclusion of the second field within F does not change the mass of either object. You will think that it must change either the mass or the radius, since those are the only variables we have in the term GMm/R^2, but I have shown that it doesn't work that way. Russell proposed, like me, that the variation must be within Newton's equation itself, within F, and he was correct in that. But he thought the only other place the variation could take place is within the masses.[3] This is not true. Look at my segregated equation again:

$F = E + H$

$H = m(A + a)$

$E = [GMm/R^2] - [m(A + a)]$

Where H is the gravitational part of the equation and E is the foundational E/M field. Obviously, F is not going to change from the values of Newton's equation, unless you block E. If you block E, then the F will become H. But the masses in F and H and E are the same. I don't propose any change in mass, whether E is blocked or not. The m in H and in E is exactly the same. Variance is not caused by a change in mass or by an absorption of gravity. It is caused by blocking the E/M field, and this causes the full expression of H. Which means that gravity is never absorbed. In normal circumstances, gravity (H) is partially offset by E. *Offset*, not absorbed. But during an eclipse, H would express itself without the presence of E, becoming greater.



Now let me show you how this solves the Allais Effect. In a Solar Eclipse, we don't have a straight blockage of an E/M field, so the effect is not the simple one I have just glossed. Yes, in a Solar Eclipse, the Sun's E/M field would be blocked. But I have shown in other papers that the Sun's foundational E/M field is negligible at the distance of the Earth, so this won't help us explain anything. And in a Solar Eclipse, we still have the E/M field of the Earth and Moon, unaffected by any blocking or shielding. So we cannot find the Allais Effect by a simple differential. What is blocked in a Solar Eclipse is the Solar Wind, and the importance of this is shown in the third part of my tidal series http://milesmathis.com/tide3.html.

Up to now, given standard model equations, blocking the Solar Wind would not have explained anything. As long as tidal and pendulum effects were explained only as gravitational, the loss of the Solar Wind could not have been of any import. But now that I have shown that Newton's equation always includes the foundational E/M field, the Solar Wind must play an important role in many motions that were always thought to be gravitational.

Concerning tides, I showed that the main cause of influence was the foundational E/M field, not gravity. That is to say, the same field that mediates the force between the proton and electron mediates the tides on the Earth, and is a player in all forces at all levels in the universe. Up to now, QED has ignored the mechanics of quanta interaction, studying only probabilities. The mediator of the field has not yet been discovered, and has not really been sought or even hypothesized. What we have now is only the skeleton of a theory about a messenger photon, one that can cause both attraction and repulsion. I have shown that this theory is non-mechanical and theoretically useless, and I have replaced it with one that is more consistent and useful. I don't need to restate that theory here; it is enough to show that any such field (even the one we have now consisting of messengers photons), once shown to exist, must exist not only at the quantum level, but at all levels. Any field that is more than an intellectual abstraction must exist in space. If it exists, it cannot stop existing once you leave the quantum field. Modern physicists have assumed, without doing any math, that this quantum field must be too small to effect macro-objects, but this assumption is false. Actually, no one that I know of has even thought to ask about the macro-presence of this field, since the field has never yet moved beyond being a mathematical postulate. If physicists have not found it necessary to give this field a meaningful mechanical expression at the quantum level (and they haven't), why would they think to apply it to the macro-world? Due to specialization, most quantum physicists have forgotten that the macro-world even exists. Many of them state out loud that it doesn't, really: we wouldn't even *have* a macro-world if it weren't for decoherence, according to them.

At any rate, it is this field that I am calling the foundational E/M field, to differentiate it from the E/M field mediated by electrons. The foundational E/M field controls the more obvious field of electricity and magnetism, but the two fields aren't the same thing. One is mediated by electrons, the other by some kind of photon (we assume). But it is this latter field that causes tides. And it is this field that I will show must effect a pendulum.

In my tidal papers, I have actually shown a number for this field on Earth. At the surface of the Earth, the field causes an acceleration of $-.009545 m/s^2$, in vector opposition to the acceleration of gravity. If this field is blocked, then the compound field increases, bumping the number for gravity, 9.8, up to 9.81. So if you block the foundational E/M field, your weight would *increase*, throwing a wrench into the anti-gravity efforts of NASA (see below).

But, as I already said, an eclipse will not block this field. This field arises mainly out of the Earth. The Moon's field affects it, but the Sun's field does not affect it appreciably. So even during an eclipse, the field is present. Neither the gravitational field nor the foundational E/M field is being blocked during an eclipse, which is what makes my theory and equations so interesting. I am not proposing some simple-minded affect, I am proposing a complex mechanical explanation, complete with equations and numbers. And, as it turns out, my explanation confirms the most orthodox explanation of the Allais Effect, according to NASA's own website. NASA lists possible causes of the anomaly, and "solar radiation" is one of them--the least controversial of all. It is nice to be slightly non-controversial for once.

I have shown that with tides, the Solar Wind is the main cause of spring and neap variations. In the third part of my tidal series I even provided the numbers http://milesmathis.com/tide3.html. I showed the acceleration from the Moon to be 4.7×10^{-10} m/s^2, and the acceleration of the Solar Wind to be 1.87×10^{-10} m/s^2, providing a 40% variance between spring and neap positions. You will say that is a very small acceleration, but it is applied to a kilogram, so it is not a negligible force. In fact, I just showed in my last paper before this one, http://milesmathis.com/caven.html that it is about the force acting in modern Cavendish devices: enough to move lead balls in a very short time. In considering pendulum variations, we will look at the same factors, but change the positions of the players.

Basically, the Solar Wind draws off energy from the Earth-Moon E/M system. Both bodies must use energy blocking the Solar Wind, and this energy comes from the E/M field. But the Earth's use of its E/M field to block the Solar Wind varies depending on the position of the Moon. This is simply because the Moon is an important player in the combined field. In fact, I have shown the numbers that prove that the Moon is a very large player in this drama, having a vastly greater foundational E/M field than would be expected according to the standard model. This is obvious, regardless, since the standard model doesn't even know about the existence of the field I am talking about; but even if they did admit its existence, they would never expect the Moon to have a field as active as it has. In a nutshell, this is because the foundational E/M field is emitted by quanta, and it increases in strength with smaller spherical macro-objects. That is to say, the emitted field becomes denser very fast, with decreasing size and equivalent object density, due simply to the nature of a spherical field. I have shown that the Moon has a stronger foundational E/M field than the Earth, not just relatively, but absolutely.

At any rate, I have shown that it is this field that causes tides. It is this field that allows for a force at a distance, since this field is a real bombarding field. The Moon does have a gravitational field, but this field is an acceleration field, and cannot affect the Earth at a distance (except as it directly and mechanically affects the material E/M fields between the Earth and Moon). Given that fact, it is clear how the Solar Wind interrupts this field. As I said, it draws off energy from this field. The field is required to expend potential fending off the Solar Wind, and it expends this potential whether parts of the Wind are positive or negative. I show this in more detail in my tidal papers.

However, during an eclipse, the Solar Wind is blocked partially or completely over extended areas of the Earth. This must give rise to a temporary increase in the total potential and total E/M field in those areas. The E/M field isn't being sapped as much by the Solar Wind, so it increases. If we plug that effect into my differential equation F = E + H, we must find a

temporary decrease in F, or in what we now call gravity. Since E is always negative to H, it might be clearer to write the equation F = H - E. H never changes, since it is an acceleration and depends only on the size of the Earth; nor can it be blocked. So if E gets larger, F gets smaller.

You can see that this immediately gives us not one, but two visible effects during eclipse. We have a more active E/M field, which will cause motions due to that variance; and we have a separate but related decrease in weight of all objects in the vicinity.

My theory also explains why gravimeters are less likely to find a variation during eclipse than a pendulum. Old-style gravimeters would have been more likely to find the same effects as a pendulum, since the mechanism of these gravimeters was a spring. A spring, like a pendulum, is a macro-object. It is a rather large object, one that you can hold in your hand. This is important because these large objects create surfaces that interact with the E/M field. But newer gravimeters are fancier things. They work with laser interferometers, measuring fringes, and lasers cut right through a photon field. More importantly, these new machines are very small. A modern drop machine has a tiny object falling only 20cm, and a rise and fall machine may have an even smaller object moving through only 4cm. This makes the time of measurement on the order of 200ms. The interaction with the field has been minimized, you see, and so it should not surprise anyone that variations are difficult to measure with a modern gravimeter. Current scientists are assuming they are measuring an acceleration field only, with no emitted particles. Strictly, they are measuring a motion, not a field. But if the E/M field is involved, as I have proven, then the gravimeter should be made to test larger objects over greater times, not the reverse. A useful device, measuring a compound field, must allow for a substantial interaction with the emitted field. Otherwise it will under-report this field. If you under-report this field, you have failed to find the existing variation. This is what is happening with small, fast gravimeters.

Now let us return to Allais' pendulum. I have shown that F should decrease under an eclipse, according to the mechanics of my fields. If we decrease F, we decrease g.

$T = 2\pi (\sqrt{} L/g)$

That is the equation for the period of the pendulum. If we decrease g, we must increase T. Allais' pendulum should swing wider. Allais reported no such increase, since he was not measuring the period.

But Saxl & Allen reported precisely that in their famous torsion pendulum experiment of March 7, 1970, published in PRD. This experiment was done in Boston, just north of the eclipse line (96.5% total), and nowhere near either end of the line. I have been able to find nothing wrong with their experiment, according to my fields and theory.

Luo Jun and his team published results in PRD in 1991 of their measurements with a torsion pendulum. They traveled to Bjelomorsk in Russia in 1990, and claimed to have refuted the results of Saxl & Allen. The problem with their experiment, according to my explanation of the fields, is that they were in the wrong place. It would have been a much shorter trip for them (from China) to have gone straight north to a town like Aliskerovo, Siberia, which was at greatest eclipse, directly in the line. Instead they traveled thousands of miles west, almost to

Finland, and there the eclipse line was at an end (it was the beginning of the line, actually). You can see this by going to the Google eclipse map of July 22, 1990.

The reason this is a problem is that the blocking is going to be greatly decreased at either end of an eclipse line, since the effect of the Solar Wind is going to be "leaking" back in (from the West, in this case). You can be above or below an eclipse line and still expect an effect: meaningful percentages of the Solar Wind will be blocked in those positions. But you cannot be beyond either end of the line, for obvious reasons. The normal Solar Wind exists in those positions. Jun was quite near the line, which normally would be good, but he was too close to the *end* of it. From the published map, I cannot tell how near the end he was, but he was very close. It is not at all clear to me why he would pick this position over a greatest eclipse position that was nearer to him. Even without knowing of any Solar Wind effects--even accepting the standard model *in toto*--there is no reason for him to have gone to Bjelomorsk. One suspects, after the fact, that he may have done so specifically in order to refute Saxl & Allen and to bolster the standard model.

Beyond this, Jun begins his paper by stating that Saxl & Allen found a period of relative increase of 2.7×10^{-4}. But Jun's abstract tells us he found any relative change must be less than 5.2×10^{-5}. According to my calculator, that difference is only a factor of 5. So he found at least five times less effect than Saxl & Allen. Not a very strong refutation, given that he traveled 6000 miles to find a location with a weaker effect. It is not so surprising that he found it.

Another Chinese paper published in PRD is that of Wang et. al. from 2000. This is a gravimeter test at eclipse with a decrease in weight. The experiment has been repeated by them many times in many places, the last in 2002, all showing a gravity decrease. This team has been attacked by the standard model police due to these results, finally forcing them to stage PR responses in 2003 and 2004.

The key element of this experiment, in my opinion, is their use of the LaCoste-Romberg D gravimeter, a well-known, widely used, and respected instrument. To date, no one has been able to spot a flaw in the instrument or the procedure. But I spotted the key fact immediately. If you go to the LaCoste-Romberg website, almost the first thing you discover is that the gravimeter is all metal. We are told, "Few ferrous metal parts are used in the meter. The meter is demagnetized or compensated, then installed in a double μ-metal shielding to isolate it from magnetic fields." But this means, 1) some ferrous metal parts are used, 2) metal parts that are demagnetized or shielded from magnetic fields are not thereby shielded from the foundational E/M field.

I am not saying that this means the machine is flawed. It means it is a good measurer of F *precisely* because it is a good measurer of both H and E. Due its makeup, it fails to respond to magnetic fields; but due to this same makeup, it responds particularly well to E. Remember that E in my equation is neither the electric field nor the magnetic field. It is the sub-field that creates them both. The electric and magnetic fields are caused my the motion of electrons. E is caused by the motion of photons. Lacoste and Romberg shield their gravimeter from electrons, but not from photons, which is perfect here. We get a machine that is a lovely conductor of photons and a lousy conductor of electrons, just what we want to measure small fluctuations in the

foundational E/M field.

The second reason the Lacoste-Romberg would be expected to work well in finding eclipse variations is that it is neither a drop machine nor a rise-and-fall machine. It is a spring and hinge machine. It is small, but it isn't measuring an event like a fall, that takes place over 4cm or 200ms, so the interaction with the field is not limited.

In *Current Science* [June 10, 1997] we have another gravity decrease found with a metal Lacoste and Romberg, by the team of Mishra and Rao, in India. This one was slightly easier to ignore than the one of Wang, but now that it has been confirmed multiple times by Wang and made the pages of PRD in that way, it should no longer be ignored.

As for the article in *Nature*, 1955, by Tomaschek, I think he himself admits his machines were useless under the conditions. I have said that old style gravimeters might give better results in response to the variation in the E/M field during an eclipse, but this is assuming they are in optimal conditions, unaffected by other more banal interruptions. Tomaschek used three meters, a Frost 32, a Frost 54, and a Worden. He tells us that the Worden and the Frost 32 responded poorly to temperature and pressure extremes in the Shetlands, and that the Worden showed an "elastic drift". His best machine, the Frost 54, had an accuracy of ± 3 percent. I think we can therefore leave these gravimeters out of it.

Then we have Kuusela's torsion pendulum experiment of 1991, published in PRD, which found no eclipse effect. The problem here is that the eclipse was on the horizon. If you draw a simple schematic of the required positions of Sun, Moon, and Earth, to create this eclipse for the viewer, you quickly see that very little of the Solar Wind would actually be blocked from the Earth in this case. Like the light, it would be blocked for the viewer. But the largest part of the Earth facing the Sun would get no blocking at all. Only a tiny "heel" of the Earth is experiencing the block at any moment; the rest of the blocked area is out in the atmosphere, above the viewer, where the E/M field strength of the Earth drops off. This is why little or no pendulum effect would be expected.

If that seemed a bit vague, let me put it another way. If you are above or below the line, you can expect a sizeable effect, given my fields. But if you are very near either end of the line, you are going to get leakage. You are very near the normal field--where the eclipse is zero--and there are no field walls. If the Solar Wind is creating normal effects just a few miles from where you are, then that is going to affect your measurements, too. Which is just to say that anomalies caused by an eclipse must drop off near either end of the line.

But the same is true here with eclipses right on the horizon. When the eclipse is right on the horizon, it is like always being at the end of the line, wherever you are. This is proved by looking at the eclipse map for Kuusela's experiment. He measured the same eclipse as Luo Jun's Chinese team: July 22, 1990. Remember that I said Jun went almost to Finland, and was very near the end of the line? Well, Kuusela is Finnish, and he was in Finland. Specifically, he was in Turku, 25 km from the zone of totality. Problem is, he was 25km *past* the end of the line of totality. Due to the movement of the Sun, he was actually 25km past the *beginning* of the line, but the idea is the same. He tells he was at 99.8%, which sounds great until you remember that his .2% was right out in the breeze, so to speak.

What I mean by that is that if you are somewhere along the middle of the line on the great eclipse map, and you say you were at 99.8%, that means that an area in front of you is at 100% and an area behind you is at 99.6%. Areas to the side of you are also at 99.8%. But if you are slightly off the end of the line, like Kuusela is, then when you say you are at 99.8%, that means that an area in front of you is at 100%, but areas pretty close behind you and to both sides are at 0%. This must make a big difference according to my fields. This is what I mean by "leakage." Your results are literally going to be leaking away to the fields nearby.

Now lets look at Ieronim Mihaila's experiments, briefly. These were done in 1999, 2003, and 2005, in Bucharest, Romania. Mihaila used both Foucault pendulums and Allais pendulums. With both he found a slowing of the period, confirming Allais. They also showed a movement of the plane of motion toward the eclipse line. Both findings confirm my theory. On August 11, 1999, Mihaila was very near the point of greatest eclipse, which was between Belgrade and Bucharest. On May 31, 2003, Mihaila was above the eclipse line but nowhere near the end of it. His experiments are a standard confirmation in all ways, and only merit an extended mention here as politics. His papers were published in the Romanian Academy, but refused by the French Academy. Although Maurice Allais himself scolded the French Academicians for this, the standard model--arising from Harvard and MIT and such places in the US--has used this refusal as basis for dismissing Mihaila *in toto*, as little more than a crank. This despite the fact that he is a professor at the University of Bucharest, where he has taught both astronomy and mathematics, as a member of both departments. Other papers by him can be found at Springerlink and adsabs.Harvard.edu. In this regard, he is clearly a victim of worldwide blacklisting for his threat to the standard model.

Before I move on to the discussion of the sideways motion of the pendulum, I want to touch on one further experiment which appears to confirm the weight change, or the effect on g. This is the experiment of G.T. Jeverdan, also of Romania. He was in Jassy in Feb. 15, 1961, just north of the line (which went through Bucharest), and fairly near the greatest eclipse, which was in the eastern Ukraine. He was nowhere near either end of the line. He used a very long Foucault pendulum. The problem for me is that he found a decrease in the period and an increase in g, the opposite of what I predict.

Jeverdan's report is very brief, and it doesn't divulge enough details to make a full analysis. Its brevity makes it difficult to find what may be the cause of the variance. However, rather than dismiss Jeverdan out of hand simply because I don't like his findings (as the standard model does now, over a broad swath of topics), I prefer to assume that he was correct and see if I can incorporate his data into my theory with the rest. It looks very difficult to do at first, which has kept anyone else from attempting it. But I will press on regardless.

In analyzing the pendulum equation $T = 2\pi (\sqrt{L/g})$, I have noticed that we have not one but two possible variances, due to my new theory. Which is to say, an E/M theory like I propose will actually enter that equation in two separate places, since a mass change will enter it in two different places. I have shown above that it enters through the variable g, which causes a variation in T. But the tension on the wire or other hanging apparatus will also vary. This second effect will be in opposition to the first.

If we block the Solar Wind, we increase E, which decreases g and increases T. This is the effect

I have shown above. But if we decrease g, then we also decrease m, and this must effect the tension of the wire and the weight on the wire. If we decrease the tension and weight of the wire, we decrease T (simply because we make the swinging motion easier: if it is easier, it is faster). Now, above, with the other pendulum experiments, I simply assumed that the effect on g would trump the effect on the tension, giving us an increase in T. But this may not be so with all pendulums. Let's look at the difference between Jeverdan's pendulum and the other pendulums.

As I said, Jeverdan had a very long Foucault pendulum, with a wire 25 meters long and a weight of the bob of 5.5kg. We don't know the diameter of his wire. If we look for equations to apply to the tension, we have problems. The equation above has no mass or tension variable, as you see. And, as one expert recently said, "The manner in which the wire tension varies with the motion [of the Foucault Pendulum] is unknown, and its elimination from these equations only serves to further complicate matters. It is possible, however, to derive an approximate solution for small amplitude oscillations."[3] He then gives us some extensions of Hooke's law, which would dirty rather than clarify this paper.

It is better to keep it simple here, and state that tension is a measure of force per unit length. As usual, the standard model gives the important dimensions to the constant, and it is actually the constant in Hooke's equation that has these dimensions; but here it is better to think of these dimensions applying to the tension. This is because it allows us to see very quickly that a longer wire, in eclipse, is going to lose more tension and weight than a shorter wire. Every unit length of the wire loses weight and tension. You will say that all pendulums with bobs of equal mass will lose the same amount of weight from the bob, and this is true; but they don't lose the same weight from their wires, since their wires don't weigh the same. This must affect the tension. Even if their wires weigh the same per unit length, a longer wire must have more total weight, and therefore more variation in eclipse. This will be especially true of long thin wires, since we have less mass per unit length. A shorter or thicker wire will resist changes in force more easily, since it has more mass per unit length. Molecular forces will resist the full expression of variation: some changes will be absorbed, or will not overcome molecular forces. But in a long thin wire, with little mass per unit length, the force will be more fully expressed.

So, you will have a greater force per unit length, due to the greater mass of the wire itself, and a fuller expression of this force, due to the lower mass per length. But there will also be a third factor, and that is the change in the gravitational field itself. The two ends of a wire 25 meters in length aren't even in the same gravitational field, precisely. The bob, at the bottom, is feeling more acceleration than the ceiling, from which the wire is hung. This compounds the effect, since we have a small inverse square effect here. If we measured the tension from the top, we wouldn't get the same number as the tension from the bottom. To get the right answer for what the wire is feeling itself, we have to sum as we move along the wire, rather than sum from one end or the other. But as we go up the wire, the bob will seem to weigh more and more (very very slightly, of course). And this effect is in addition to the weight of the length of wire below us. Even if the wire weighed nothing, we would still measure a heavier bob as we go up, due only to the change in acceleration from the field.

If the bob seems to weigh more from the top, then any *change* in weight will also be greater, summed over the length of the wire.

You will say, according to my new theory, gravity is only an acceleration, not a force. I have said elsewhere it doesn't even obey the inverse square law. I have said that Newton's equation obeys the inverse square law only because it contains E, and E obeys the inverse square law. And yet here I am finding an inverse square difference. How is that consistent?

It is consistent since we have the compound field in effect here, in this problem, not just my field H. Yes, my field H does not obey the inverse square law (except in transforms). But the pendulum here is not swinging through H only. It is swinging through F, as I said above. F works in the classical way, since it contains both H and E. F obeys the inverse square law. Therefore the pendulum will be obeying the inverse square law. A very long pendulum will find a variance due to this inverse square law, even minus any relativity transform.

Now, I admit that all three of these effects I have just listed are very small. I don't have equations to balance any one of them or all of them with the variation I have proposed for g. I can't prove at this time, mathematically, that tension becomes the greater effect in the pendulum equation with Jeverdan, explaining his reverse effect. I can only suggest that this may be the case. It is certainly interesting to note that all the other pendulums, including those of Saxl & Allen, Mihaila, Allais, and the rest, are much smaller than that of Jeverdan. Allais pendulums and torsion pendulums are normally much shorter than Foucault pendulums, and Mihaila's Foucault pendulum, although large [14 meters], is only a little over half the size of Jeverdan's. And, though it was shorter, it had a greater mass at the end [7.3kg].

It is my opinion that the most intelligent, as well as the most gracious thing to do, is to assume that Jeverdan's data is correct like all the rest. Then we try to show how his reverse effects may be logically and mathematically explained, with no contradictions. In this line, I assume that the mechanisms I have enumerated here are all at work, and I will try to develop equations to show precisely why and where the effect of tension overcomes the effect of g in the pendulum equation. It seems to me that since we don't have a full understanding of the pendulum, we should welcome this data with open arms. It is precisely data like this that will allow us to develop a full equation that includes tension, mass, g, inertia, and so on, all in the same equation. Therefore, we should start with the assumption that we have useful data here, data that may be telling us something. Only once we have exhausted all possibilities in that line should we begin stating authoritatively that either Jeverdan or Mihaila must be wrong. One of them may be wrong. But we should also consider that they may both be right.

Now let us move on to the other pendulum motion seen, the change in the plane of swing that is the main outcome of Allais' data. The first thing to notice here is that Allais was south of the line, and the plane of his pendulum moved north. He tells us it moved 15 centesimal degrees; and while I only count about 11 from beginning to top, the more important feature, in my opinion, is that it moved northward, from approximately 173 to 184. Allais tells us he was "measuring azimuths from the south, in a direct sense." Therefore, we may deduce that his pendulum moved toward the line.

In all the other experiments that measured this motion, we see the same thing. If the experimenter is south of the line, he sees a deflection of the azimuth north. If he is north of the

line, he see a deflection of the azimuth south. For example, in all three experiments, Mihaila was north of the line, and all three times he found a deflection of his plane of oscillation toward the south. The only experiment I haven't mentioned yet, of Oct. 3, 2005, Mihaila was well above the line (the annular being in Africa). He was nowhere near either end of the line. He found a deviation of about one degree, to the south.

Another experiment also confirms this, without the use of a pendulum. Latham and Last performed gyroscopic experiments during the eclipse of June 20, 1974, in Perth. They were north and east of the line and near the center. The gyroscope was unaffected, but a Talyvel electronic level placed on the base of the gyroscope measured a five arcsec change. Since the level was mounted E/W, and the change was negative 5, we may take it that the change of direction of g they thought they were measuring was toward the eclipse line.

It is interesting to discover that Latham proposes an aether http://home.t01.itscom.net/allais/blackprior/latham/latham-rep28-2.pdf traveling at 500km/s to explain his findings, since this is now the speed given to the Solar Wind by the standard model, based on direct measurements by satellites. It is not clear where Latham pulled this number from, but it is a lovely estimate, especially if he based it only upon the effect seen. Against this is that he says, directly afterwards, that it is not clear why the effect is so small, considering that an "aether flow of 500km/s would cause enormous mechanical changes."

Yes, it would if it were not blocked, but we know it *is* blocked, in large part. The standard model proposes it is blocked by the magnetosphere, but they haven't proposed what creates the magnetosphere. I propose my E/M field creates it directly, with emission from quanta, without the need of iron dynamos in the core of the Earth. In either case, it explains the tamping down of the Solar Wind, or of Latham's aether, in normal circumstances. But because Latham did not include the foundational E/M field in his theory, he did not come to the correct equations or conclusions. However, I must say that there is much in his theory to be commended, and that it is close enough to the truth to have diverted me if I had come upon it sooner. I am fortunate, I think, that I did not.

Although there are points to like in Latham, there are other points against him. One is his explanation of why the gyroscope showed no deflection, while the level did. The correct answer is that his level has a little pendulum inside it.[5] But the gyroscope is not set up to respond to either of our postulated variations here. It is not a gravimeter, so it will not register a change in mass or g. And, since it is fixed both top and bottom, it cannot swing. It is spinning, so the only thing it can measure is a change in tilt. We have no change in tilt here. Latham proposes that the direction of g is changing, but that is not what is happening. Nothing is happening to the centripetal acceleration of the Earth. That is clear no matter what theory you are following. Only that would cause a change in tilt. What is drawing off the pendulums and the level is not the gravitational acceleration vector, it is the imbalance of the E/M field. That field is stronger toward the line and weaker away from it. But that loss of balance cannot affect the gyroscope. It could only tend to push the scope as a whole away from the line. Since the scope has no freedom of motion in that direction, it shows none. Latham's longwinded explanation concerning couples is not to the point.

Which brings us to the mechanics of the motion of the pendulum toward the line. What causes

it? In my mechanics, it has nothing to do with H and everything to do with E. A lack of balance has been caused in the foundational E/M field, by the loss of the Solar Wind. That E/M field is stronger along and near the line, and more normal away from it. But this does not set up some electric or magnetic potential. No, it sets up a simple density difference. In that sense, the analogy is not to the electrical field, but to the wind. In fields of real particles, field density differences cause particle motions, away from denser areas and toward less dense areas, due only to what is called entropy (but which is simply a statistical matter[6]). What we have is a photon wind blowing out from the eclipse line. This is why I said the only thing that the gyroscope could feel is a push away from the line. And this wind explains the pendulum motion quite simply. The pendulum acts like a weather vane. It is being bombarded by real particles, with a definite "wind" direction away from the line of eclipse. If the eclipse could be made to last a bit longer, the pendulum would point right at the line. Since the eclipse is always of short duration, and the photon wind is very weak, we get only a partial motion in that direction.

I think I have hit all the major theoretical additions I have to this problem, and it is time to conclude. I will do so by once again attacking the presumption of the standard model. Anyone who researches this question as I have will find that mainstream physics has set up yet another wall here. This is most obvious at Wikipedia, where the discussion page behind "Allais Effect" is little but transparent propaganda against Allais and all forms of openness. Thomas Goodey, whose very fine Allais website http://allais.info/ supplied me with a great deal of my evidence here, is treated with unveiled contempt when he appears on that page to make comment. The Wiki mandarins actually allow themselves to say, "Ugh!" when his name is mentioned. They also throw mud at Chris Duif, whose only crime is that he posted an overview of these experiments http://arxiv.org/ftp/gr-qc/papers/0408/0408023.pdf, claiming that the standard model had not successfully incorporated them into its theory. Although his article is published at ArXiv, and they must know that ArXiv requires a sponsor, they state that Duif is "not a reputable source…according to Wiki guidelines or standard scholarly practice." Of course they don't want to have to consider what Duif actually wrote, they just want an excuse to revile it, without actually reading it. But I suspect that what really bothers them is Duif's list of standard-model failures, complete with publications:

The observation of an anomalous acceleration on spacecrafts in the solar system (the Pioneer Anomaly [AndL02]), anomalous velocity increases of spacecrafts during Earth flybys [AntG98, AndW00, NieT04] and even discussions about whether we understand gravity at laboratory scale [MbeL02, Mel99, GerG02] may have contributed to this renewed interest. Also the (unfinished) project by NASA [NASA99] in 1999 played a role as well as several publications which hypothesized conventional causes for the eclipse phenomena [UnnM01, FlaY03, RuyS03, YanW02].

The biggest of the mandarins in this squabble at Wiki is Steven G. Johnson, a young professor in the mathematics department at MIT who thinks it is his job to stand guard at Wiki. He says,

In fact, the evidence is shrinking in the literature as the recent experiments were refuted by multiple papers, and apparently the only people who still believe it is an anomaly are so far outside the mainstream of science that they don't even publish.

That comment is true only to the extent that mainstream science now disallows and blocks publication of contrary evidence or opinions. But it is false to a greater degree, since Duif's list above contradicts him. Many people agree that this and other anomalies exist and are important, including the Task Group On Gravity Probe B, National Research Council, Washington, D.C., which said,

Despite its omnipresence, gravity remains the least well tested of all the fundamental forces.[7]

And also including NASA, which recently co-sponsored a worldwide test of the Allais Effect during eclipse, including nine separate pendulum experiments and 13 separate gravimeter experiments in the US, France, Italy, Austria, Germany, Russia, Belgium, and the United Arab Emirates. The NASA Marshall Space Flight Center took part with both pendulum and gravimeter experiments.[8, 9] Is Johnson suggesting that all these parties are cranks and crackpots, "far outside the mainstream of science"?

He must also feel the same way about Ron Koczor, assistant director for science and technology at the Space Science Laboratory in NASA's Marshall Space Flight Center, http://www.space.com/businesstechnology/technology/advanced_projects_000621.html, Huntsville, Alabama, who is involved in anti-gravity experiments, and through whom NASA awarded a $600,000 contract in 2000 to Superconductive Components Inc. in Columbus, Ohio, to build an anti-gravity device.[10] It would seem that NASA is one of the few places that still maintains enough money and independence to ignore the standard model police. If it were up to Johnson at MIT, no doubt NASA would also be written off as a pesky nest of cranks.

It may be that NASA is indeed being written off or taken over by standard model police. To start with, the "Allais Effect" page at Wiki is doomed, as far as I can tell. In my experience with Wiki, a discussion page in this form is an immediate precursor to a lockdown and a whitewash, if not a complete delete. The standard model is very uncomfortable with questions like this, and the standard model owns Wiki. Most other pages like this one have been locked and/or deleted. Johnson admits as much when he links this page to the "relativity crackpots." The standard model has closed relativity and it is in the process of closing gravity, as a subject of discussion.

And strange things are going on at NASA as well, in this regard. The Allais Effect experiments were done in 1999 and these pages[8, 9] still stand on NASA's website. But we have no results, *almost nine years later*! It is as if the entire thing went down the memory hole, except that someone forgot to flush these initial PR pages (I predict they won't last much longer, which is why I saved them). This means that either 1) they obtained absolute confirmation of the effect, and they don't want to admit it; 2) they obtained confirmation of something even stranger, and had to hide it out at Area51; 3) the experiments tied in to the anti-gravity device, which works and must now be guarded as a military secret; or 4) NASA has been taken over by the standard model, and now no longer does positive work--it just crouches in the corner and shoos off "cranks".

In my opinion, number 3 is the most likely, and I will tell you why. In doing so I will be able to tie up the last loose string in this paper. Far above I mentioned that NASA's anti-gravity device could not really be blocking gravity, since gravity is not blockable. Their device cannot be

blocking the E/M field either, since if you blocked the E/M field, your object would weigh more, not less. But this is not to say that they do not have a device that is levitating objects. Based on the accidental findings of the Russian engineer Yvegeny Podkletnov, superconducting devices now apparently exist that can partially levitate small objects. I will show in a moment how this happens, but right now I want to point out a very strange "coincidence." In researching the Allais Effect, I discovered that the scientist in charge of NASA's pendulum and gravimeter experiments at Marshall Space Flight Center was a man named David Noever. Noever is now AWOL from NASA, and this, we are told, is one reason we have no data from the 1999 experiments (it doesn't, however, explain why we have no data from the other places). In researching the anti-gravity experiment of Podkletnov, taken up by Marshall in 2000, I discovered that physicists Torr and Li at the University of Alabama, near Marshall, had predicted that superconducting magnets would have an anti-gravitational effect. They reported this to a friend at Marshall. Although the press kits report that Ron Koczor is the NASA director of the anti-gravity experiments at Marshall, the friend of Torr and Li is none other than…yep, David Noever.

Since 2000, both the Allais experiments and the anti-gravity experiments have gone underground. With the Allais experiments, we have a complete cover-up. It appears that a worldwide order went out from high up, CIA or DoD, to sit on or lose the data from all Allais experiments. The anti-gravity experiments, which early on were used as part of the PR package of new physics, have since changed directions, and no more "sci-fi" updates were given to the magazines or websites. Once the experiment was shown to work, it was no longer a colorful joke, it was deadly serious.

In this way I think Allais' Effect has become a victim of the Podkletnov Effect. Because they are seen to be related, the secrecy surrounding one has affected both. The Allais Effect would not appear to have any National Security importance, but because it is linked to this anti-gravity experiment, it has been lost down the same memory hole.

This in unfortunate, since it will deny open data just when this problem looked like being solved. All the insiders here, the ones now locking their suitcases and being tailed by black cars and taking jobs with "new companies," are being schooled by the standard model, which appears to have decided this effect has something to do with blocking gravity. But it doesn't.

These superconducting disks and magnets are not blocking fields, they are accelerating fields. That is clear from the first look at the data and the tools. It is also clear from the by-products. Podkletnov has told of dangerous radiation from the backside of his disks, as one example. What is happening, in the first instance, is that the superconductor is allowing the E/M field of the Earth, which is already going straight up, to go straight up faster than it was. The E/M field I am talking about is emitted radially out from the Earth, in a summed sense. It is a real field made up of real particles. These particles are photons, not electrons, but they have real momenta. They bombard and cause real forces of repulsion. Under normal conditions, this field has to move through the atmosphere, and this slows the field. What happens with the superconductor (without spin) is that a small portion of the atmosphere is cooled to near zero. This allows the E/M field to move through it with less resistance. This is precisely what "superconducting" means. Resistance gets very small, and it is because the motion of the particles in the atmosphere or object has been stopped, or nearly stopped. So you have fewer collisions. Fewer

collisions means greater transparency. The E/M field meets less resistance, so it moves more quickly through the atmosphere or object. Quite simple, really. Since it is moving more quickly, it has greater momentum. And so it causes a greater force. It pushes any non-supercooled mass it does meet with a greater force. So the object rises. This is what was happening with Podkletnov's smoke, above his superconductor. The E/M field was pushing it up.

Under normal circumstances, the E/M field doesn't cause smoke to rise, because under normal circumstances, all objects, including smoke, have reached a place of balance with the E/M field and gravitational field. The gravitational field impels them down, the E/M field impels them up, and they seek a level of balance. For most objects, this level is on the ground. For smoke, it is some level in the atmosphere, based on the weight of the smoke.

But Podkletnov's superconductor changes this balance. The E/M field is suddenly moving faster in that vicinity, and things begin to move, seeking a new level of balance. Smoke rises, objects lose weight, and so on. Straightforward mechanics.

Now, in the second instance, Podkletnov adds spin to his superconductor. The effect is increased. What is happening? Any field being blocked now? No. The E/M field is simply being accelerated once more. The superconductor acts like a large fan, blowing the field up. Why up? The old right hand rule of electricity. Every object emits an E/M field, and transmits an E/M field. The E/M field is ubiquitous, as plasma research and other modern research has shown. I assume Podkletnov's superconductor was spinning CCW, which creates a force up.

This would be the first assumption, but the force can be explained even without the right hand rule. For instance, it may be that the photon field does not act precisely like the electron field. We have no hard-and-fast knowledge that the foundational E/M field obeys a right hand rule. We may have to come up with other rules, based on experience, by looking at experiments like this. We do know, from QED, that we have orthogonal spins creating orthogonal fields, even with the foundational E/M field. This is what Schrodinger's equations are telling us, among other things. Therefore it is no great difficulty explaining an orthogonal force from a spinning field. We would expect a spinning field to create a force either up or down, and we must look to nature to tell us how things actually work. We spin Podkletnov's superconductor both directions, collect the data, and then we know. But I hope you can see that it is much easier, not to say much more logical, to propose the mechanics I have proposed here, rather than to propose mysterious and non-mechanical blocking of fields. Besides, blocking the gravitational field breaks so many Newtonian and Einsteinian rules. I am not one for obeying rules for no reason. But I am not one for breaking rules for no reason, either. Einstein's rule of equivalence is a lovely rule, one that is both logical and backed by a century of data. Why break it when you can keep it by a simple mechanical manipulation?

[1]Majorana, Q., (1920). "On gravitation. Theoretical and experimental researches", *Phil. Mag.* [ser. 6] 39, 488-504.

[2]Russell, H.N. (1921). "On Majorana's theory of gravitation". *Astrophys. J.* 54, 334-346.

[3]Russell actually proposed a change in mass due to the presence of other mass, to answer Majorana's evidence.

[4]Beatty, Millard F., "Principles of Engineering Mechanics, Vol. 2", p. 184, *Springer,* 2005.

[5]Webster, John G., "The Measurement, Instrumentation and Sensors Handbook", p. 15.2. *Springer*, 1999.

[6]See my paper on Entropy.

[7]books.nap.edu/html/gpb/summary.html

[8]science.nasa.gov/newhome/headlines/ast06aug99_1.htm

[9]science.nasa.gov/newhome/headlines/ast17jun99_1.htm

[10]space.com/businesstechnology/technology/anti_grav_000928.html