Critical assessement of the YARK theory

Klaus Kassner 3 March 2018

I will mostly base my discussion on Refs. [1] and [2]. Of the latter I have, however, only found a preprint version on the web, so there may be some differences from the published paper. Finally, I will have a very brief look at the application of the theory to the Mössbauer experiment.

The first thing to note when looking at the YARK theory is that it is *not* a complete theory of gravity, contrary to general relativity (GR). The YARK theory never produces any field equations that would allow to calculate the gravitational field for arbitrary mass distributions. Instead, its consideration of gravity is restricted to the case of a point mass or to the field outside a spherically symmetric mass distribution (anticipating that a variant of Birkhoff's theorem also holds under the assumptions of the YARK theory). The title of [1] exaggerates, because only two results of general relativity are reproduced,[1] i.e., the weak-field results for light deflection and the perihelion precession. The Shapiro effect is not considered nor is there any discussion of geodetic precession (de Sitter effect) or rotating systems with their frame dragging effects (Lense-Thirring effect). These predictions of GR have been confirmed as well as some strong-field effects – gravitational waves from the Hulse-Taylor binary (and a new one where both neutron stars are pulsars) or gravitational waves from the merger of black holes or neutron stars. If the YARK theory makes deviating predictions for these cases, it is falsified. But the predictions apparently have not been worked out in the more than 10 years since the inception of the theory. Nor have the authors come up with field equations so far.

Energy conservation

The title of the paper [1] seems strange, insinuating that there is no energy conservation in GR, which is clearly wrong. The field equations satisfy energy conservation locally, but this is not the kind of energy conservation considered by Yarman anyway. He considers energy conservation of a test mass in a static gravitational field. This kind of energy conservation is also satisfied in GR. I will later comment on the use of "quantum mechanics" (QM), appearing in the title as well.

Yarman starts from the obsolete concept of rest mass. Had he ever switched to the modern language, in which this is called *invariant mass*, he might have avoided some conceptual pitfalls. Invariant mass means that the mass does not change under a change of the frame of reference. It does not mean that the mass of a combined system cannot be smaller than the sum of its constituent masses. Invariant mass is not a conserved quantity, so it can change dynamically. But it is a scalar in special relativity, so it is the same in all inertial systems. It remains a scalar in general relativity, thus it does not change in a gravitational field either. The *rest energy*, which sometimes still is a useful concept, does change in a gravitational field.

Here are the mathematical details in GR, because I want to compare points of view a bit: the four-momentum of a particle may be written

$$P \equiv (p_\mu) = \left(-\frac{E}{c}, p_1, p_2, p_3\right) \,, \tag{1}$$

[1] And with so little detail that it is difficult to verify whether the calculation is correct or not.

where E is the energy of the particle and p_1, p_2, p_3 are the components of its (ordinary) momentum along the three coordinate directions. This is a four vector, i.e., its length is unchanged under a change of reference system, and this holds also if the change is to an accelerating or gravitating system. But that length is $\mathrm{i}mc$,[2] where m is the invariant mass. Using the standard formula for the scalar product of P with itself, we have

$$P^2 = -m^2c^2 = p_\mu g^{\mu\nu} p_\nu = g^{00}\frac{E^2}{c^2} + g^{11}p_1^2 + g^{22}p_2^2 + g^{33}p_3^2\,, \tag{2}$$

where Einstein's summation convention has been used after the second equal sign. To obtain the last equality, I have assumed that the metric is diagonal (which is the case for the standard form of the Schwarzschild metric). Assume now that the particle is coordinate stationary, i.e., its spatial coordinates do not change. Then the p_i, $i = 1\ldots3$, are zero and E becomes the rest energy:

$$E^2 = -g_{00}\, m^2c^4 \qquad \Rightarrow \qquad E = \sqrt{|g_{00}|}\, mc^2 = \sqrt{1 - \frac{r_s}{r}}\, mc^2\,, \tag{3}$$

where I first use that, in the case of a diagonal (or just time-orthogonal) metric, $1/g^{00} = g_{00}$ and have set g_{00} equal to the value it has in the Schwarzschild metric in the last formula. $r_s = \frac{2GM}{c^2}$ is the Schwarzschild radius. What equation (3) tells us is that $E = mc^2$ does not hold in gravitational fields, which should not come as a surprise, given that it does not hold for moving particles either.

We may then – sometimes – interpret the difference $E_B = mc^2\left(1 - \sqrt{|g_{00}|}\right)$ as the binding energy of the particle in the gravitational field. When can we do so? Whenever we make sure that the particle does not accelerate, otherwise energy conservation from GR tells us that the total energy of a particle dropped from rest at infinity will remain $E = mc^2$, with the difference between that energy and the rest energy being now kinetic energy (and the particle will remain unbound if it does not get close to the event horizon, i.e., it will escape back to infinity). One way to avoid the accumulation of kinetic energy is to slowly lower the particle in the gravitational field at the end of a tether. Then it will exert a force on the other end of the tether and lowering it in the field will release work that might be stored in a spring. This way it can get rid of its binding energy and its total energy will be $mc^2 - E_B$.[3]

Let us return to the YARK theory. I have not found the "derivation" of Newton's law from special relativity (SR) that the author claims to have achieved. This claim can only be false in my opinion, because on the one hand, SR is compatible with other forms of static fields, e.g., the Yukawa potential, and because, on the other hand, I know where the form of Newton's law comes from and this has nothing to do with SR. Both Newton's universal law of gravitation for a point mass and Coulomb's law for the electrical force exerted by a point charge have a $1/r^2$ dependency. This is easy to explain as a consequence of Gauss's theorem. If we assume that the gravitational (electric) field has no sources in vacuum, i.e., field lines of a static field can end only in masses (charges), then the divergence of the force field must be zero there. A static spherically symmetric field has necessarily vanishing curl and if we require the field to vanish at infinity (sufficiently fast) then Helmholtz's theorem tells us that the only possibility is the $1/r^2$ dependence outside the central mass (charge).[4] Nowhere does this argument require SR.

[2] We take the signature of the metric to be $(-, +, +, +)$, so spatial components of four vectors have positive sign with respect to their 3D counterparts.

[3] Note that a local observer will still assess its energy to be mc^2, due to the slower rate of his proper time, compared with that of an observer at infinity.

[4] That is, we have a one-parameter set of solutions, with the parameter determining the mass or charge.

Helmholtz's theorem implicitly assumes Euclidean space. So deviations from the flatness of space allow deviations from Newton's law. But an appropriately defined divergence in the curved space still vanishes, because in a static field, there are no sources of gravitation in vacuum. Contrary to the Newtonian case, this observation is not quite sufficient to derive the full Schwarzschild metric [3], but is nevertheless helpful.

My conclusion is that Newton's law, corrected by a binding energy modification, is not really derived in Yarman's development. It is an assumption. That assumption is justified in the weak-field limit, but it will miss higher-order corrections ($1/r^3$, $1/r^4$ etc.) [4]. Not necessarily all of them, because the binding-energy argument gives a deviation from $1/r^2$, but it is not guaranteed to get the exact deviation. In fact, given what we know from GR, we can do an exact calculation on the basis of energy conservation. We know that the exact potential of the Schwarzschild gravitational field is given by

$$\Phi(r) = \frac{c^2}{2} \ln\left(1 - \frac{2GM}{rc^2}\right) \qquad \Rightarrow \qquad -\frac{\mathrm{d}\Phi}{\mathrm{d}r} = -\frac{GM}{r^2}\frac{1}{1 - 2GM/rc^2}\,. \tag{4}$$

Note that for large r, the first expression reduces to the Newtonian potential and the second to the magnitude of the Newtonian force per mass unit. Assuming then that the energy of the particle in the field is $mc^2 - E_B$, we get in the same style as Yarman:

$$E_B(r) = \int_r^\infty \left(mc^2 - E_B\right) \frac{1}{c^2}\frac{\mathrm{d}\Phi}{\mathrm{d}r}\,\mathrm{d}r \tag{5}$$

$$\frac{\mathrm{d}E_B}{\mathrm{d}r} - \frac{E_B}{c^2}\frac{\mathrm{d}\Phi}{\mathrm{d}r} = -m\frac{\mathrm{d}\Phi}{\mathrm{d}r}\,, \tag{6}$$

which is a first-order differential equation for E_B that can be solved easily, noting that the left hand side is just $\mathrm{e}^{\Phi/c^2}\frac{\mathrm{d}}{\mathrm{d}r}\mathrm{e}^{-\Phi/c^2}E_B$ and that after multiplication with $\mathrm{e}^{-\Phi/c^2}$ both sides of the equation are total derivatives:

$$E_B = mc^2\left(1 - \mathrm{e}^{\Phi/c^2}\right) = mc^2\left(1 - \sqrt{1 - \frac{2GM}{rc^2}}\right)\,, \tag{7}$$

where we have set $E_B = 0$ at infinity, as a boundary condition. So we do obtain the correct value for g_{00} here, if we use the exact potential. Yarman's result is an approximation valid in the limit where the potential can be replaced by its Newtonian value. To this order, the coefficient g_{00} can, in fact, be obtained from the equivalence principle [5]. There is no need to invoke the field equations, and that is, of course, the reason why the simple considerations of Yarman give the correct approximate result here.

A few words on the equivalence principle may be in order. Yarman asserts in different places that the gravitational mass and the inertial mass are different in his formalism, thus violating the weak principle of equivalence. He does not care too much, because he thinks he has something better. However, there are theorems that make such an attitude careless.

First, there is a theorem saying that any metric theory of gravitation[5] must obey the equivalence principle. Since Yarman essentially constructs the approximate g_{00} from energy conservation and "derives" a first-order approximation to g_{11} from QM, as we shall see, he should obtain agreement with GR to that order, unless he seriously blunders in the equations of motion. Therefore, his theory must satisfy the equivalence principle. I have rederived his

[5] I.e., a theory where the equations of motion of test particles are derived from a metric using the standard Lagrangian obtainable from it.

equations of motion and compared them with those from GR. At lowest, i.e., Newtonian order, there is agreement; the first post-Newtonian order does not seem to agree, but I would have to check that in more detail and this would be a lot of work. That he gets light deflection and the perihelion precession right would suggest agreement. But then his theory must satisfy the equivalence principle. What he overlooks in denying its validity is, in my opinion, that with his introduction of a new definition of rest mass there are *two* rest masses now – the rest mass "according to the observer at infinity" and the rest mass according to a local observer, obtained by dividing the former quantity by the time dilation factor $\sqrt{|g_{00}|}$. The equivalence principle is a *local* statement for inhomogeneous gravitational fields, therefore it is the latter mass that figures in the equivalence principle, not the former one. With this taken into account, presumably the equivalence principle will continue to hold in his theory.

Second, the converse is also true, i.e., if the equivalence principle holds[6] and we can find a set of global coordinates for the system, then the theory can be expressed as a metric theory of gravitation, because the equivalence principle is sufficient to derive the equations of motion locally and all we then have to do is to transform them to the global coordinates. Thence, of the two problems in a theory of gravitation which are to find equations that determine the gravitational field given the mass-energy distribution and to find equations of motion for test masses in the field, the second is solved by applying the equivalence principle.[7]

Quantum mechanics

Let us next discuss the use of quantum mechanics for the evaluation of the "stretching of lengths". Basically, the argument suggests that time dilation lowers the energy of a quantum mechanical system (because of the energy-frequency relationship $E = h\nu$). This must then change the size of atoms, because their radii are inversely proportional to their energy. Instead of using an atom, it is easier to argue with a photon, where we get the de Broglie wavelength $\lambda = h/p = hc/E = c/\nu$, which goes up as the frequency decreases.

Now the idea that this may be used to explain a "size increase" predicted by the metric is based on a misconception that roughly corresponds to assuming that Greenland is about one third the size of Africa, because it looks so large on maps.

Inspecting the map on a globe instead, we note that Greenland is much smaller and we realize that the size distortion on the flat map arises due to the fact that meridians (lines of constant azimuthal angle φ) are piling up towards the pole, so when they are drawn as constant-distance lines on a flat map, size distortions necessarily must occur.

The proper radial length element in the Schwarzschild metric is $\mathrm{d}\ell = \sqrt{g_{11}}\,\mathrm{d}r = \left(1 - \frac{r_s}{r}\right)^{-1/2}\mathrm{d}r$. If a ruler of this length is moved towards the center of the coordinate system, then $\mathrm{d}\ell$ does not change. But the density of constant r coordinate surfaces changes. As $\mathrm{d}r$ becomes smaller, fewer surfaces fit into a length $\mathrm{d}\ell$, i.e., the (proper) distance of constant r surfaces separated by equal intervals $\varDelta r$ increases. Note that basically the same thing happens in SR with a ruler that is set in motion. Its proper length does not change. The length with respect to an observer at rest in the original inertial system contracts, and this can be easily measured by

[6] But this should be the Einstein equivalence principle, which is a little stronger than the weak equivalence principle and says that special relativity applies locally in freely falling frames.

[7] SR and the equivalence principle imply local energy conservation for the test masses. If the global coordinate system is stationary, this also implies global energy conservation. So Yarman's energy conservation postulate cannot give more than the equivalence principle.

comparing the end points of the ruler with marks on a measuring rod next to it and at rest in the original inertial system. So there is a meaning to saying the length has been contracted. But we cannot compare the length of a ruler that has been moved into the gravitational field with that of a rod next to it and *outside the field*. The nearby rod cannot avoid getting into the field, too. Moreover, it would be wrong to state that the length for the "distant observer" is $\mathrm{d}r$ and therefore, the size *decreases*. That would be similar to saying that Greenland really has a bigger area for an observer at the equator than for a local observer.[8]

Our considerations suggests that a possible size change of atoms or photons in a gravitational field cannot be discussed in these simple terms. We must think a little more carefully. If we wanted to apply quantum mechanics directly, we might have to solve the Schrödinger equation in a gravitational field. That looks difficult. Can we proceed in a simpler way? Let us see.

However, the first point that I would like to debunk here is that the proportionality between energy and frequency is of quantum mechanical origin. Actually, the relationship $E = h\nu$ was first derived[9] theoretically using the correspondence principle, i.e., as a *requirement* on quantum mechanics to make it *agree with classical physics* in the limit of large quantum numbers! In fact, a relationship between energy and time or frequency follows from the classical Hamilton-Jacobi equation. If energy is conserved (i.e. the Hamiltonian is constant in time), the action function solving the Hamilton-Jacobi equation can be obtained by separation of variables and has the form $S(q,t) = -Et + f(q)$. If we assume that the action between two points on the world line of a particle is a relativistic invariant (there are good reasons to assume this), then we get, for fixed spatial coordinates infinitesimally apart, that $E\mathrm{d}t$ is invariant. Therefore, if we have time dilation between two frames, then the ratio of energies E_1/E_2 of the same object observed in the two of them should be inversely proportional to the ratio of their temporal rates $\mathrm{d}t_1/\mathrm{d}t_2$. Another approach would be to look at the phase space trajectory of a classical harmonic oscillator. We find $\oint p\mathrm{d}q = E/\nu$ and the integral represents the action for a full oscillation. Requiring it to be invariant, we have $E \propto \nu$, without ever invoking quantum mechanics.

Second, if instead of atoms we just consider photons and treat them as electromagnetic waves with wavelength $\lambda = c_{\mathrm{ph}}/\nu$, where c_{ph} is the phase velocity, there is no need to invoke quantum mechanics either to obtain length relationships. What I would like to say with these remarks is not that I object to using quantum mechanics in making statements about size relationships but that I would like to avoid the "mystic" impression that these *follow* basically from quantum mechanics. They are obtainable from classical mechanics alone but they must of course hold in QM, too, for consistency reasons.

Let me then discuss the statements made by GR about frequency and wavelength changes in the metric, to contrast this with the YARK theory. Consider a photon sent from radius r_0 to infinity and arriving there with frequency ν. What was the frequency it had when leaving its sender, as described in the global coordinate system? We have energy conservation. Gravity is just the curvature of spacetime, so there is no extra potential describing it. Therefore, the frequency of the photon will be ν along its entire trajectory. That is the point of view of the "observer at infinity".

Actually, one has to be careful describing things this way, because it is not uniquely determined what will be the results of the observer at infinity when we are referring to events far from that

[8]For lines of constant φ on the sphere, the opposite happens as a ruler oriented parallel to circles of latitude is moved towards the north pole: their density gets higher.

[9]It was first *used* by Planck as a *postulate*, and that was of course a quantum mechanical approach. But derivation is not the same as postulating.

observer. It is better to speak of the description in terms of a certain global coordinate system, because that gives cleaner definitions. For example, when we describe events in terms of the Schwarzschild coordinates on the one hand and in terms of Painlevé-Gullstrand coordinates on the other hand, then both coordinate systems become Minkowskian at infinity and both have the same rate for the global time at any set of fixed spatial coordinates. Nevertheless, the foliations of spacetime are different for the two sets, meaning their notions of simultaneity differ. As a consequence, an infalling particle will cross the event horizon of a Schwarzschild black hole only after infinite time for an observer at infinity describing the course of events in Schwarzschild coordinates, but after a finite time for the same observer, if he chooses Painlevé-Gullstrand coordinates instead. Therefore, the statement that something happens this or that way for an "observer at infinity" is less precise than the statement that it happens this or that way in these particular coordinates. Anyway, I will refer to Schwarzschild coordinates here only, and this should clarify what is meant by the views of an observer at infinity.

Given that the frequency of our photon is ν everywhere in terms of the global time coordinate, we can easily calculate its frequency at finite r as observed by a *local* observer. Such an observer has a proper time that is slowed down by a factor of $\sqrt{|g_{00}|}$ with respect to the observer at infinity, so the emission frequency at r_0 was

$$\nu_{L0} = \frac{\nu}{\sqrt{|g_{00}(r_0)|}} = \frac{\nu}{\sqrt{1 - \frac{r_s}{r_0}}}\,, \tag{8}$$

and a local observer at r will see the photon at the frequency $\nu_L = \nu/\sqrt{|g_{00}(r)|}$. In order to have energy conservation with this varying frequency, a potential is needed. That is $\Phi(r)$ from Eq. (4), and the energy of the photon along its course is given as the sum of its "kinetic energy" $E_\nu = h\nu\,(1 - r_s/r)^{-1/2}$ and its potential energy $E_{\text{pot}} = h\nu\left[1 - (1 - r_s/r)^{-1/2}\right]$.[10] So the local frequency of the outgoing photon decreases and we have a redshift. The local wavelengths are then given by

$$\lambda = \frac{c}{\nu} \tag{9}$$

at infinity and by

$$\lambda_{0L} = \frac{c}{\nu_{L0}} = \sqrt{|g_{00}(r_0)|}\frac{c}{\nu} = \sqrt{1 - \frac{r_s}{r_0}}\lambda \tag{10}$$

at r_0. Suppose now we measure, at infinity, the spectrum of a hydrogen atom that emits light at r_0. We will receive all the spectral lines redshifted by some factor $1 + z$, meaning their frequency is lowered by a factor $1/(1 + z)$ by comparison with the corresponding frequency of a local hydrogen atom. The wavelength is increased by a factor $1 + z$. From Eq. (8), we read off that $1 + z = 1/\sqrt{1 - \frac{r_s}{r_0}}$. The lowered frequency would correspond to the size of an atom that is larger by a factor $1 + z$ (energy $\propto 1/a_B$, where a_B is the Bohr radius). But lengths at the emission point are smaller by a factor $\sqrt{1 - \frac{r_s}{r_0}} = 1/(1+z)$ – if all lengths behave the same way as the wavelength of a photon. So the atom at the emission site actually has precisely the same size as an atom at infinity.

Hence, we cannot argue with a size change due to quantum mechanics to obtain the factor g_{11} or g_{rr} of the metric. In fact, this kind of argument is similar to that of Schiff [6], in

[10] It is easy to calculate E_{pot} from an integral similar to Eq. (6).

which he tried to derive the metric factor for the radial-radial component from the length change of rods dropped in the metric. This kind of argument typically gives the inverse of the time-time component of the metric as the radial-radial component. That it does not work, can be seen from a counterargument given by Rindler [7], in which he demonstrates that the same kind of argument applied to the parallel gravitational field (arising in an elevator accelerating uniformly with Born rigid motion) gives the wrong metric, i.e., the argument does not reproduce the Rindler metric, in which all spatial metric coefficients are one (whereas the argument would suggest the one of them corresponding to the direction of acceleration to depend on the time-time component).[11]

Now, the Yarman paper does not argue with metric coefficients, but it changes radial and, apparently, also azimuthal lengths "as assessed by the distant observer". While this, if transferred to a description in terms of a metric might give a reasonable result for the g_{rr} component, an additional change of the $g_{\vartheta\vartheta}$ and $g_{\varphi\varphi}$ components would be wrong. Neither of the changes would have been well justified.

To summarize, the "derivation" of the spatial part of the metric in the YARK theory is flawed. It is an argument without a sound basis, but since the author knew what the solution of the field equations in GR looks like, he was able to fabricate a description that seems to agree to first order in the Schwarzschild radius with the known exact solution.[12] Therefore, the YARK theory should reproduce GR results at lowest post-Newtonian order, if the equations of motion are correctly derived. According to the author, it reproduces light deflection and the perihelion precession, which would not be surprising given the way it was constructed with an approximation that is correct for the piece of the metric that can be derived via the Einstein equivalence principle and an alternative version of the Schiff argument, known to be wrong but giving the right answer.

Then the application of the YARK theory to the Mössbauer experiment should reproduce the GR result ($k = \frac{1}{2}$), because that is definitely a weak-field situation. Since it does not, according to [8], the theory must have been incorrectly applied. I will briefly look at this in the next section.

Application of the YARK theory to the Mössbauer experiment

The calculation given by the authors of [8] is lengthy, so rather than trying to redo every step, I glanced through it attempting to spot blatant errors immediately. And I found one that I will report here. Whether that will destroy the result, I do not know. I suspect it does, because it happens in the middle of the calculation and anything that follows cannot be trusted.

The authors here work with a metric explicitly and transform it in a number of ways. They have expressions in Eq. (23) of [8] which I suspect to be already wrong, because the metric is conformally flat instead of flat as it should, but that is not my point. What is definitely wrong is the transformation from (23) to (24). In fact, it is difficult to understand how the authors could produce such a beginner's error.

[11] The size-stretching argument from quantum mechanics would also give a spatial metric component different from one in the Rindler metric. So that metric works as a counterexample for the YARK theory as well.

[12] Since he does not give a metric, it is not clear whether he effectively changed more than the radial coefficients, in which case his result would disagree with GR even at lowest nontrivial order. But then he would not get the correct results for light deflection etc., unless a second error compensated the first.

I consider only the transformation of the time variable and the spatial variable in the local direction of motion. These are called $\mathrm{d}t$ and $\mathrm{d}x$ for a disk observer and $\mathrm{d}t_L$ and $\mathrm{d}x_L$ for an (inertial) observer located outside the rotating system. The authors then state that "involving the known relativistic effects of time dilation and contraction of a moving scale along the direction of motion, we obtain"

$$\begin{aligned} \mathrm{d}t_L &= \mathrm{d}t/\gamma \,, \\ \mathrm{d}x_L &= \mathrm{d}x/\gamma \,, \\ \gamma &= 1/\sqrt{1-\frac{u^2}{c^2}} \quad \Rightarrow \quad \frac{1}{\gamma} \approx 1 - \frac{u^2}{2c^2} \end{aligned} \tag{11}$$

and they plug this into the metric and peacefully continue their calculation.

This strongly suggests that they do not understand time dilation nor length contraction!

What is the problem? The coordinate differentials in the line element of a metric are independent. We may plug in total differentials to get the line element for arbitrary coordinate increments. However, the differentials for time dilation and length contraction are not total differentials. In fact, they are derived from the Lorentz transformations, say

$$\begin{aligned} \mathrm{d}t' &= \gamma \left(\mathrm{d}t_L - \frac{u}{c^2}\mathrm{d}x_L\right) \\ \mathrm{d}x' &= \gamma \left(\mathrm{d}x_L - u\mathrm{d}t_L\right) \,. \end{aligned} \tag{12}$$

Time dilation is then obtained by considering a clock at *fixed* spatial coordinate in the primed system, i.e., we set $\mathrm{d}x' = 0$ and get first $\mathrm{d}x_L = u\mathrm{d}t_L$ and, on inserting this in the equation for $\mathrm{d}t'$, we find

$$\mathrm{d}t' = \gamma \left(\mathrm{d}t_L - \frac{u}{c^2}u\mathrm{d}t_L\right) = \mathrm{d}t_L/\gamma \,. \tag{13}$$

We could identify this with $\mathrm{d}t$ and insert it in the metric,[13] but then we would be obliged to replace $\mathrm{d}x$ by zero and would not have any $\mathrm{d}x_L$ dependence left in the metric. Because the time dilation formula requires a certain relationship between $\mathrm{d}x_L$ and $\mathrm{d}t_L$, they are not independent.

Length contraction is obtained from (12) by setting $\mathrm{d}t_L = 0$ (the length must be measured at a *fixed time* in the system where it is moving). This gives immediately

$$\mathrm{d}x' = \gamma \mathrm{d}x_L \,, \tag{14}$$

which corresponds to the relationship between $\mathrm{d}x_L$ and $\mathrm{d}x$ given by the authors. In principle, it is possible to use $\mathrm{d}x_L$ in the line element, but then the dependency $\mathrm{d}t' = -\frac{u}{c^2}\gamma\mathrm{d}x_L = -\frac{u}{c^2}\mathrm{d}x'$ following from the condition on $\mathrm{d}t_L$ must be used and again we would not have four independent differentials.

Even if the differentials are *not* inserted into a metric, we are *not* allowed to use the formulas $\mathrm{d}t_L = \gamma\mathrm{d}t$ and $\mathrm{d}x_L = \mathrm{d}x/\gamma$ together *in the same expression*, because they hold under different conditions. When the second formula holds, we must have $\mathrm{d}t_L = 0$, so the first does not hold!

[13] Note that this result differs from $\mathrm{d}t_L = \mathrm{d}t/\gamma$ obtained by the authors. Clearly, time must run more slowly on the rotating disk than in the laboratory, so the authors are wrong here regarding the "direction" of time dilation. We must have $\mathrm{d}t_L > \mathrm{d}t$.

The correct transformation automatically including time dilation and Lorentz contraction effects is of course the Lorentz transformation itself. But using that will most likely not lead to the desired result $k = \frac{2}{3}$.

In any case, given that the YARK theory should, in the weak-field limit, reproduce the predictions of GR,[14] a correct calculation would yield $k = \frac{1}{2}$, which is the GR result.

That leaves us with not having *any* correct calculation predicting $k = \frac{2}{3}$. Given that many early experiments indeed gave $k = \frac{1}{2}$ with precisions excluding $k = \frac{2}{3}$, it would perhaps be good to reconsider the re-analysis [9] of Kündig's experiment [10] by the authors.

I have read both papers and may read some of the other accounts of similar experiments (e.g. Champeney). What I noted is that the authors of [9] base their criticism of [10] largely on a calibration curve (Fig. 5 in [10]) that, as they say, does not describe the data. They then recalculate the calibration curve from the digitized data and obtain a different result.

I would like to point out that Kündig does not really say that the calibration curve given in Fig. 5 was used to analyse the data from Figs. 3 and 4. Rather he says that calibration measurements were done every few days. Certainly, it would not be good scientific practice to publish a calibration curve that was actually not the one used to generate the measurement data. But it is not impossible that this happened and that the data used for the calculation of k were evaluated using another one of the several calibration curves produced.

Moreover, it should be noted that the re-analysis of Kündig's results gave $k \approx 0.6$ rather than $k = 2/3$ and that the later experiment, in which the extra energy shift was confirmed [11] gave a k value between 0.68 and 0.69. So the Kündig result is almost midway between $k = \frac{1}{2}$ and $k = 0.68$. Moreover, it is not clear that the newer experiment was better. Kündig had a nifty way of eliminating the effect of vibrations, whereas apparently all other experiments had to somehow remove their influence on the result by appropriate processing of the data. This presumably meant modeling of the vibrations. If that modeling was not quite correct, it might systematically affect the final result.

If we take together the high quality of the original data of Kündig, the possibility that the calibration curve that he published was not the one used and the fact that a number of different authors also got $k = \frac{1}{2}$ in different setups, it does not seem unconceivable that this *is* the correct result.

However, I agree that the experiment should be repeated more carefully to see whether the deviation of k towards higher values does persist. I understand that most experimentalists are reluctant to do so. It is difficult to gain reputation from this kind of experiment – if the result from fifty years back is reinforced, it is not new, moreover there are more accurate experiments than the Mössbauer method that give $k = \frac{1}{2}$ and it is difficult to beat their accuracy with Mössbauer. Of course, it would be important, if $k > \frac{1}{2}$ turned out to be true. Apparently, not many experimentalists believe this will happen (and make them famous).

References

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