Discussion of Recent Articles on Pendulum Support

Pendulum Support Loss has been a subject of experimentation and theory for some time[1234567]. Even so reexamining it in this forum is appropriate, as questions remain unanswered and some maybe unfamiliar with past results. My interest stems from questions[8] asked by a member over the past several years. Finally answering the most recent one required my becoming a member. I have had no previous horological interest except as observing the classroom construction of a quartz clock in 1958. Reading the recent Newsletter articles impels me to contribute by correcting what I think are errors and adding my perspective as having been an experimental physicist and instructor.

Most practical is to use reverse chronological order. In the most recent article,[9] the author suggests his pendulum's wall support resonates when driven by the pendulum. This is highly unlikely as the frequency of a periodic driving force must differ less than 10% from the frequency of the driven oscillator for the steady state amplitude to be, as found, about three times that of the driver. Given the elastic moduli and masses of the casting and wall I think this is highly unlikely. I may have an explanation for the author's paradox, which I hope to discuss with my comments on his static measurement of wall movement.

A minor point: Rawlings evidently uses small angle approximations in his formulae for pendulum tension components. This is unnecessary as the exact formulae are simple[10].

One method of elucidating the paradox would be to measure the movement dynamically using the force gauge pulling on it quasi-sinusoidally. Timing could be done using the pendulum w/ a very light bob or a metronome. Another possible method would be to mount the heavy bob much closer to the suspension. At half a meter the period would be increased ~ 40%. I would be very surprised if this significantly changed the effect since the tension in the rod is independent its period. Still another method would be to video the movement of the LASER's spot from the time of releasing the pendulum. If it were a resonance phenomenon, then one would record an increase in amplitude w/ time. Incidentally, the coupling between oscillators must of necessity oscillate, but not necessarily be an oscillator, i.e. if impulsed will not ring, and if an oscillator, it is merely an artifact

---

1 . Sir George Gabriel Stokes, "On the effect of the internal friction of fluids on the motion of pendulums", Trans. of the Cambridge Phil Soc. Vol. IX (1850). Reference: Peters, Model of Internal Friction Damping in Solids

2 Sci. Am. July and August 1960

3 The Science of C. & W. 3rd ed. pp. 40 & 100

4 Accurate Clock Pendulums (Matthys) pp. 211 ff.

5 The not-so-simple harmonic oscillator, Am. J. Phys. **65** p. 1067

6 The pendulum—Rich physics from a simple system, Am. J. Phys. **54**, p.112

7 The preceding is only a small fraction of articles. They also include additional referred articles.

8 What effect will the moon have on the period of a pendulum? What effect will changing a pendulum's length have while oscillating? What effect will immersing a pendulum's bob in water have on its period? "Do you remember an experiment to measure the motion of a wall with a mirror and a laser beam? The goal is to measure the amount of wall movement as a pendulum swings. I can't remember where this is described." How does one know the amplitude based on its speed at the center of its oscillation?"

9 HSN 2007-2 pp. 20 ff. (Heldman)

10 I will be pleased to e-mail my derivations of the formulae I mention to those requesting and also my methods of obtaining various values. [bernardcleyet@redshift.com]

necessarily having a different period than the oscillators (pendulua). Examples of non-oscillatory couplings are mutual inductance of two coils, a string connecting spring or pendulum oscillators, a board on rollers (coupling metronomes[11]), and magnets on pendulua (torsion and plane).

Separate cases will easily eliminate the double pendulum atmospheric coupling.

When first apprised of the support problem by Mr. Mumford several months ago I suspected the impulsing was not constant and inquired as to the method of initiation. He explained it was the position of the rod. Even so, I thought the transfer of the gravity arm's momentum to the rod would vary depending on its motion. Less than a moment's thought confirms that intuition. The gravity arm must catch up to the pallet; otherwise, there is no contact! Desiring to quantify this effect, and being rather mathematically challenged, I modeled the escapement, as if it were a particle colliding with another. The resulting equation is, of course, only an estimate of the effect. Notice that this effect is a negative feedback tending to keep the amplitude constant. It, therefore, reinforces Mr. Heldman's primary thesis. Recently I found a very similar problem in a standard text[12]. A wedge whose face is concave (radius R) sits on a frictionless surface. A mass (m) is placed on the surface and sliding down causes the wedge (mass M) to move horizontally in the opposite direction. Find the equations of motion of the masses and the reaction (force) of the wedge on m. The authors' answer for the contact force is extremely complicated, and, therefore, I haven't taken the time to compare with a clock. I may if requested.

I decided: Beginning all trials with the same amplitude does not correct for a varying impulse. The equilibrium amplitude is determined by the dissipation and the impulse energy. The amplitude for which the dissipation equals the rate of energy delivered by the escapement is the equilibrium amplitude. Power in must equal the power out. However, the time to reach equilibrium will vary with the mass.

I agree a rod under more tension is more rigid; therefore, the hysteretic or viscoelastic loss will be less. However, there is another contrary effect [13]. A pendulum rod may be modeled as if a beam under three point loading with the ends poorly fixed. Both ends become more rigid with increased bob mass. The spring end because of the increased tension and the bob end by inertia, thereby, a greater portion of the impulse bends the rod initially instead of moving it. Unfortunately, this effect adds to the other losses confusing the "issue". It may, however, be approximately quantified by replacing the rod with one considerably more rigid. [14]

The difference in impulse ("beat rate" 2 parts per thousand) between the extreme pendulum masses is much too large to ascribe to circular error. The first order respective corrections[15] are, for the tungsten and iron / aluminum pendulua, 1.0 E-4 and 6.9 E-5. In addition, the corrections for the

---

[11] Pantaleone, Synchronization of metronomes, Am. J. Phys. **70**, pp. 992

[12] Marian and Thornton, Classical Dynamics 5th ed. Problem 7-34. It first appears in the third ed., but the answer given assumes the wedge is not free to move. [Text used at UCSC]

[13] "It will be reasonable to suppose (though needing experimental support)
that the flexure's loss is also proportional to the pendulum's weight.
This is certainly what an engineer would assume, prima facie." I thank Brian Whatcott PHYS-L (list)

[14] Using a large diameter tube will satisfy the required mass constancy while increasing the rigidity

[15] ([sin (A/2)]^2) / 4 Given in any advanced mechanics text or mathematical methods for physicists, and Baker and Blackburn, The Pendulum.

damping shift[16] are too small: order of magnitude 1 E-10! Pendulum length is an obvious explanation. A two mm length change in a one-meter long simple pendulum will result in the measured change. I will further discuss this in Mr. Emerson's article.

I think the author is correct in criticizing Mr. Emerson's analyses with regard to the other sources of dissipation. In addition suspension loss may increase as the radius of curvature of the spring is decreased with heavier bobs. This is a subject requiring more study. I suspect the effect, though, is negligible.

A minor, but niggling point: Mass has two attributes: inertia, as described by Newton's second, and the distortion of space we know as gravity. I suggest the phrase should be inertial mass, or just plain "mass".

The author's intuition is correct. If the dissipation is independent of the mass, the amplitude is also independent. I will argue this later. Curiously, one would think this had been verified experimentally. However, an hour's searching the Am. J. of Physics and googling yielded only one reference, a retired teacher's laboratory exercise. Unfortunately, he had not saved any data. I intend to construct a pendulum for this and other purposes and hope to submit an article.

Even if Mr. Emerson's model [17] were true, the measured loss and it are so inconsistent that his using slightly different manufactured values are irrelevant. I now discuss his response:

Mr. Emerson usefully points out the Synchronome pendulum is a physical one requiring, when accuracy necessitates, the addition of the bob's and rod's momenta of inertia. However, as I wrote above, this correction is irrelevant, as is using the first order correction term in the circular error. It amounts in the extreme example to less that 0.005 %. Its use is unnecessarily confusing. Curiously, He used the small angle approximation to simplify 1-cos(A). This error at five degrees is ~ 0.07%, which is greater than the circular error! Much more important is his conclusion that, no matter the nature of the pendulum loss, "changing the density of the bob while the drive is kept constant, will change the amplitude at which the pendulum stabilizes." This is manifestly at odds with the energy principle, if the energy dissipation is independent of the bob's mass. Then the amplitude of the pendulum is independent of the mass. As explained in Mr. Heldman's first article, the energy stored in a pendulum is a positive function of the maximum amplitude. Initially[18] energy input is divided between the dissipation and the stored energy of the pendulum, whose amplitude is a measure of that energy. The stored energy and the amplitude increases until the dissipation equals the energy supplied by the escapement. Then the amplitude is at the steady state amplitude. This requires, for constant energy input, that the dissipation increases with the amplitude. Otherwise, the amplitude would increase without end. Can this paradox be resolved? On page 26, Mr. Emerson skipped the step of differentiating energy with respect to the amplitude. If one instead writes it as a finite differential, its meaning is more clear: delta E = (MgLA) delta A. What this means is a positive change in energy results in a positive change in the amplitude, but increasingly less so with

---

[16] [k/2m]^2 T = 2Pi/ (g/l – k^2 / 4m^2)^0.5 for example Phillips "Differential Equations" and Baker and Blackburn, op. cit.

[17] HSN 2007-1

[18] the description of a clock's pendulum beginning at an amplitude less than its steady state amplitude. For the other initial condition, the pendulum's behavior is self-evident.

increase in the mass, g, length of the pendulum, [19] and the amplitude. Nothing yet that a mass increase increases the amplitude. The author then finds the energy rate as a function of the amplitude rate by substitution using the chain rule. i.e. E dot (dE/dt – the rate of change of energy) = (MgLA) A dot Again, what this means is a positive rate of energy results in a positive rate of amplitude, but reduced by an increase in any of the factors, MgL and A. Nothing wrong here, and is intuitive. The next step, I think, is not valid. That is to claim the equation defines the steady state amplitude. It only describes the change in amplitude as a function of the energy and the constant factors. The author then moves on to the solution of the differential equation for the model of the linearized, and linearly damped[20] pendulum. He apparently uses the torques method for his derivation. i.e., from N2, the sum of the torques equals the moment of inertia times the angular acceleration. However, he did not give the resistance (damping) in torque form. Here is the correct equation:

I * theta double dot + L*L*k theta dot [21]+ mgL * theta.

Where: I is the moment of inertia; theta the angle measured customarily anticlockwise from the vertical; theta double dot [d (d theta/dt)/dt] is the angular acceleration; L, the length (to centre of mass); etc. and the usual definitions.

Rearranging:

theta double dot + k (L^2) (theta dot)/ I + mgL theta/I

The amplitude part of the solution is

A = A (zero) * exp [-k*(l^2) t/(2 I)]. Not, exp [(-k/I) t].

Note: if the pendulum is further simplified by assuming all the mass is concentrated in a point bob, the moment of inertia, I, is m* L^2. The equation then becomes the one familiar in all the introductory physics texts, i.e. the resistance term's constant factor is k/m, the gravity term's g/L, and the exponent in the amplitude solution's term is k/2m. This is, of course, all irrelevant, because not only is the substitution of the amplitude portion of the differential equation model into equation 2 invalid for the reason I gave above, but also, the model itself is invalid. It is too simple to even roughly predict, or describe the behavior of the usual very stable long period clock pendulum. Finally, the author's doorknob comment. If one assumes the extreme value for the change in pivot point with mass of one cm, the amplitude would change < two minutes, likely not detectable by Mr. Heldman's method.

To be continued.

bc; who again invites critique at bernardcleyet@redshift.com

---

[19] Assuming a simple pendulum, otherwise it is the distance between the center of mass and the pivot.
[20] The damping is proportional to the first power of the angular speed.
[21] One L converts the linear speed (e.g. x dot) to angular speed (theta dot). The force then is k L theta dot. This is the tangential force. The additional L converts it to the torque.