Response to 2/21/00 draft of “**An Explanation for Anomalous Oscillations in Terms of Thermal Expansion**”, by M.A. Akerman, J.W. McKeever, J.H. Whealton and J.B. Andriulli

by Tom Mahood
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The essence of the draft paper is that observed oscillations were likely caused by a thermal effect, rather that anything based upon transient mass shifts. The authors propose that thermal expansion of the test units, originating due to the power applied to the piezo stacks, creates a sort of a thermal “acceleratory wave”, which spreads through the test devices and creates a small, but measurable force.

It is always valuable to have outside parties review one’s work, as fresh eyes often see things in new ways. Certainly considerable thought has been put into this review, the modeling for it quite detailed and the effort certainly appreciated, as is the chance to review a draft copy. Unfortunately, this focus on detail may have resulted in the oversight of some extremely important points. In other words, the forest may have been missed for the trees.

Prior to getting into that issue, a couple of comments on the first paragraph are in order. First, it states the thesis reported an “... experimental determination of average forces due to a transient mass fluctuation.” Perhaps it’s a fine point, but that statement attributes far more certainty to the conclusion and findings than was intended. The thesis states that small amounts of motion were observed, consistent with a force being generated. It also stated the experiments strongly supported the existence of a small “thrust-like” force. Given the possibility that alternative, mundane explanations may yet exist, the reference to the thesis findings might be more accurately labeled “...an <u>apparent</u> transient mass fluctuation.”

The second item regarding the first paragraph has to do with the mention of three alternate classical interpretations of the data considered by the authors. It states two of the explanations could not be ruled out, that of “time varying thermal oscillations”, (which is the thrust of the draft paper), and “low frequency oscillations to the piezo stack from the oscillator/amplifier near the pendulum resonance.” While the latter explanation is suggested, the details of precisely how it could produce the observed behavior are unfortunately not mentioned. The mechanisms of this explanation become all the more important, as it will be shown that time varying thermal oscillations are <u>not</u> a viable explanation for the observed motions.

To avoid getting distracted by details, the following examples used in this discussion will be very simplistic, worse case idealizations. As a prelude to getting into specifics, it would be useful to review the behavior of an object and its center of mass (CoM) floating freely in an inertial frame. As an example, consider the device in Figure 1. It consists of two blocks of the same density, floating in space, with the block on the left twice the length (and therefore twice the mass) of the block on the right. The CoMs of the two individual pieces are shown, as well as the CoM of the entire assembly. These locations are obvious and need no discussion.

Now assume an ever-popular massless spring, which had been cleverly inserted between the two blocks, is sprung, resulting in a final separation between the two blocks of L. Note that the CoM of the system doesn't move. It remains fixed in space unless molested by some external force. No matter how the device extends, expands (including thermally) or modifies its shape, the CoM will remain fixed at the same point in space. This is simply conservation of momentum at work.

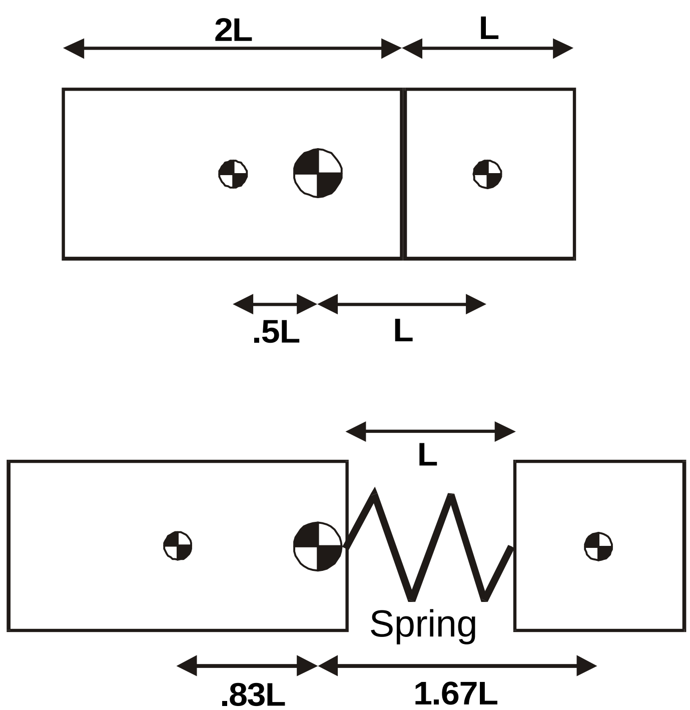

Figure 1.

Of course, the location of the CoM may change in relation to certain points on the device, as things shift between the before and after conditions. This is the pertinent item to keep in mind from this simple example. If there is a connection from points on the device to an external reference inertial to the CoM, an offset could be created. For example, the faces of the left, larger block shift 0.33L to the left in relation to the CoM. The important question is, could this be occurring with a test unit under the normal conditions of operation?

Resonant units

To address that question, we'll examine a simplification of one of the resonant test units, with dimensions as shown in Figure 2. These dimensions are room temperature dimensions, and using densities of 7.5 gm/cm^3 and 2.7 gm/cm^3 for EC-65 PZT material and aluminum, respectively, the CoM of the room temperature device may be calculated by subdividing the overall unit into four subvolumes, A through D. Note that the mass contributions of the cap retaining screws have been ignored for simplicity. The room temperature combined CoM turns out to be located 0.714 cm from the face of the attachment flange. This matches closely with an actual unit and its balance point under the torsion arm, however the point of attachment is to the flange.

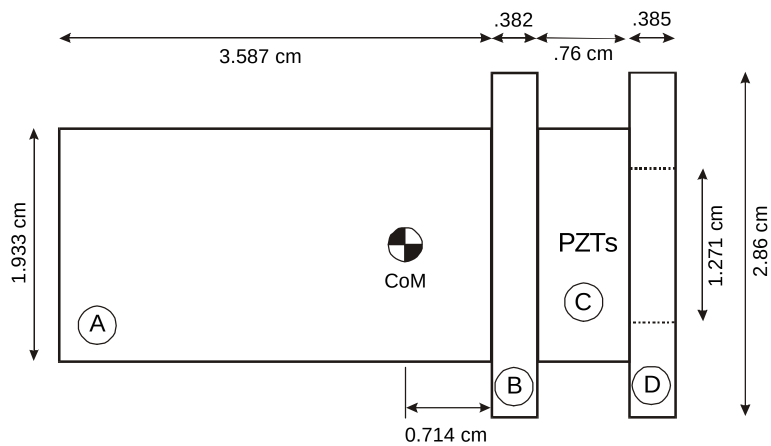

Figure 2.

We'll now assume the PZT stack has been heated by 3-five second pulses of 81 watts. As calculated in the draft paper, each pulse should result in a temperature gain of about 43° C, for a total temperature increase of about 130° C. For simplification, we'll assume the temperature increase occurs almost instantly, and is confined to the PZT stack (taking into account actual temperature dispersion throughout the device considerably complicates matters and doesn't have a substantive impact on the outcome). Given those parameters, the only changes over the previous CoM calculations will be a change in length of the PZT stack, and a change in position of the retaining aluminum collar. As an aside, it should be noted that in actual operation the tension of the six retaining screws strongly act against any expansion of the PZT stack. However this is a worse case idealization.

Using the coefficient of thermal expansion value for PZT material contained in the draft paper, 4×10^{-6} °C^{-1}, it is found the PZT stack will expand 0.0004 cm. The CoM of the PZT stack will move in relation to the original combined CoM half that value, and the CoM of the annular end cap will move that full value in respect to the overall CoM.

Remembering that the combined CoM will tend to stay at the same point in space, it can easily be computed that the attachment flange will move 0.00014 cm toward the combined CoM. The critical point here is that there is <u>no</u> net movement of the device. There is only a shift of some of its points in relationship to the stationary CoM. This is illustrated in Figure 3.

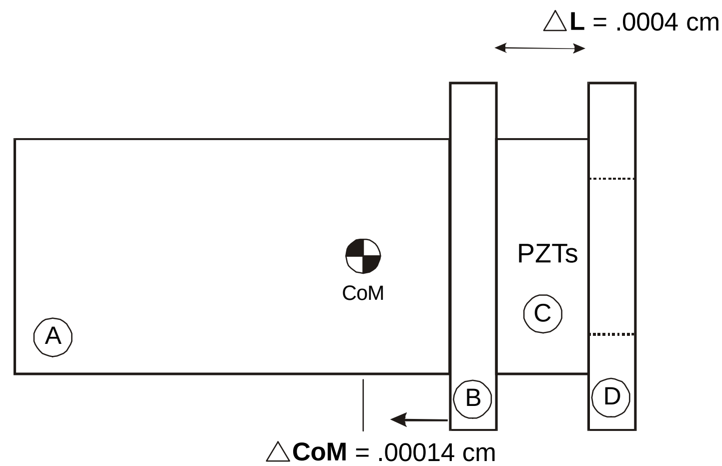

Figure 3.

The torsion arm is connected to the devices via a vibration isolation arrangement attached at the flange. Given that the combined CoM will try and stay in the same spot in space, the resulting effect on the torsion arm will be to create a displacement equal to the relative change in position of the CoM, or 0.00014 cm. This displacement would result in an overall oscillation of the torsion arm with an amplitude of twice the displacement, or 0.00028 cm.

What would this motion equate to on the indicator scale? Given a torsion arm radius of 0.075 meters, a distance to the indicator scale of 0.457 meters, a multiplication factor of 2 for the laser indicator system, we find:

$$2 \times .075 \times .457 \times 0.0000028 = 2 \times 10^{-7}\ \text{m} = .0002\ \text{mm}$$

A value of 0.0002 is far below what was measurable on the indicator scale for these tests, which had a resolution of about 0.5 mm. This in itself would strongly suggest thermal expansion of the resonant test units is unlikely the cause of the observed motion, but there are a number of other considerations.

Non-resonant units

Similar calculations were performed for the non-resonant units. In the testing of these devices (for purposes of the thesis), a number of different pulse lengths were tried, as well as energizing either one or two units at a time. Of all these tests, the worst case from a thermal standpoint would appear to be a sequence of 5-five second pulses, applied to a single unit. This would cause a temperature increase of about 175° C. The resulting shift in the CoM was found to be 0.005 mm, or about four times the amount calculated for the resonant units. However even with a much larger CoM shift, the indicator would only be affected by 0.001 mm, again, far below its resolution.

It also should be mentioned that the method of attachment of both the resonant and non-resonant units to the torsion is an important factor. Of the 32 test runs tabulated in the thesis only the first 7 had a semi-rigid connection to the torsion arm. The semi-rigid connection had nylon and polyethylene spacers to damp vibration, but flexibility was not great. The latter 25 test runs all were made with the inclusion of neoprene strips from which the test units were suspended. These were highly flexible and would permit a test unit to imperceptibly droop or pivot in the event of a CoM shift, keeping the new CoM under the torsion arm, thus negating any CoM shift.

Other considerations

So far it has been shown that any thermal displacement effect should be so small as to be immeasurable. However there are additional points which also rule out a thermally driven effect, for a least a significant portion of observed motion. It is acknowledged that with some suspensions and test arrangements, a thermal component to some motion may be discerned.

First there is the direction of motion. Of the tests the resonant test units were subjected to, about 1/3 resulted in a clockwise (CW) direction, and about 2/3 resulted in a counterclockwise (CCW) direction of rotation. Interestingly, noting the orientation of the test units, as pictured in Figure 9 of the thesis, the result of the previously described thermal shift of CoM, if measurable, would be an overall rotation of the torsion arm in the CW direction, the minority direction. Were the motion the effect of some thermal activity, the motion should be consistent in one direction.

To explain this variation in direction, the draft paper states in the discussion section:

> "Further, one could expect that inevitable differences in construction details such as relative efficiency, epoxy thickness or uniformity, torque on bolts, would make the two units on opposite sides of the pendulum behave differently."

This suggests the authors believe the units were operated simultaneously, and that differences between the units led to rotation direction variations. If this is the intent of that statement, it is mistaken. As was clearly mentioned at the beginning of the "Results and Analysis" section of the thesis, only one of the resonant units was operated at any one time. Indeed, it was this very concern that the different units had differing operating characteristics, which led to the decision to energize only one unit at a time.

Another consideration is that if the source of the observed motion (and thus any assumed force) were the result of a thermal phenomenon, one could expect the motion to scale with the power applied to the devices. Experimental data does not show this to be the case.

The graph in Figure 4 is a consolidation of two graphs contained in the STAIF-2000 paper, specifically Figures 18 and 20, and represents many runs of detailed testing of the non-resonant units. The phase angle is the phase difference between the base and doubled frequencies applied to the units. As is evident, different phase relationships result in differing power levels. However the oscillations resulting from those power levels do <u>not</u> track the power. If the motion were thermally generated, its amplitude would follow the input power. Interestingly, the periodicity of the power and oscillation (and implied force) is consistent with the terms of the mass shift derivation equation. The STAIF-2000 paper contains detailed discussion of this aspect.

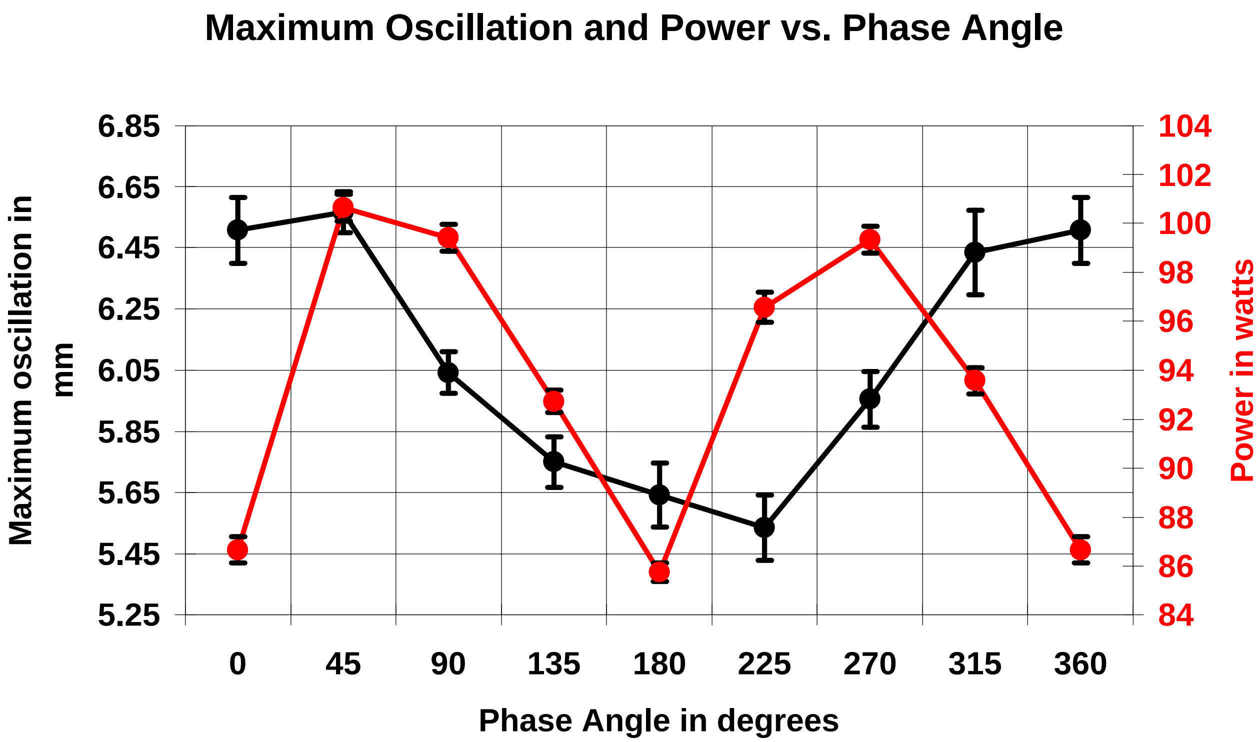

Figure4

Another issue is the promptness of response of the test devices. They typically exhibit a very rapid onset of motion, within a tenth of a second. Thermal effects have time constants on the order of several to many seconds.

The graph below (Figure 5) is Figure 25 from the STAIF-2000 paper for the non-resonant units. At the 50 Hz sampling rate, it shows strong, measurable deflection beginning only about a tenth of a second after the devices reach full power. Given the lags associated with the entire torsion arm setup, thermal causation can't account for such rapid onset. The remainder of the plot does show clear evidence of a thermal effect building, most likely in the multi-strand suspension. Notwithstanding that, the initial, underlying effect is not masked.

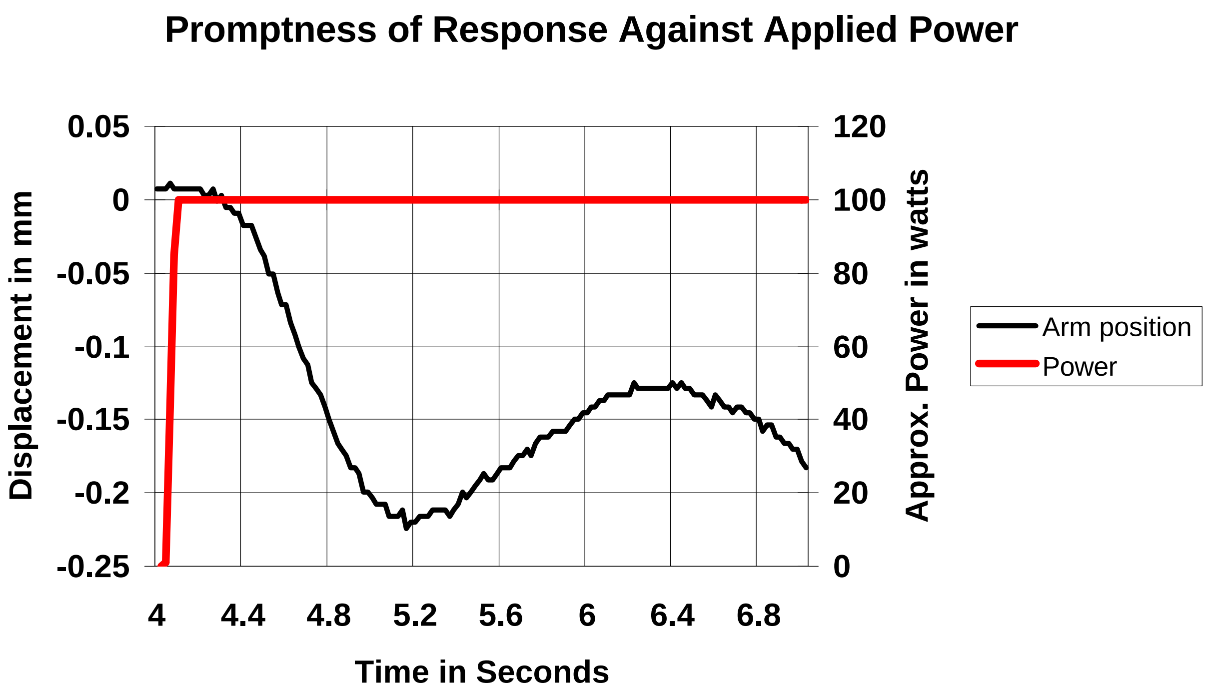

Figure 5

Finally, there is the behavior of the non-resonant units as they warm up. The extensive testing of the non-resonant units for the STAIF-2000 paper revealed a very distinctive characteristic. The bulk of this testing consisted of a single 3-4 second pulse, with subsequent runs spaced at 5-minute intervals. The units produced their maximum oscillation only after becoming fully warm. Initial, room temperature results tended to be rather small. It was only after the units became fully heat-soaked, did they produce their reliable, sizeable results. This would be the opposite of what would be expected were this a thermal effect.

Conclusions

There may well be an as-yet-undiscovered mundane cause of the motion observed in the test units. Certainly a number of other possibilities have been carefully considered and tested for. However the behavior of the test units have been well characterized, with half a dozen or more different unit designs in three different test cells. The only explanation to date that appears to fit all the data is the creation of an anomalous force by the test units.

A thermally induced oscillation may be excluded for the following reasons:

- Conservation of momentum prohibits any net force or motion from a thermal expansion effect. However thermal expansion can (and likely does) create a motion of the torsion arm. However the amplitude of the oscillation can be shown to be far below the resolution of the indicator system.

- The direction of motion for the resonant units was not consistent from test to test, as would be expected from a thermal origin.

- If the observed motion is thermally produced, it should track with variations in the power applied to the test devices. It doesn't. The amplitude and power values produce their own unique curves, consistent with the order of the terms contained in the theoretical derivation.

- The units start their motion a very short time after power is applied. A thermal effect has a much longer time constant that what has been observed.

- The units do not exhibit their maximum performance when cool, but rather when warmed. This is the opposite of what would be expected from a thermal effect.

References

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