Greetings John,

Since I've already given you a preliminary response to the draft papers you sent, I'll not belabor things in great detail. But there are some further remarks that should prove helpful should you and your colleagues decide to proceed with publication of the papers, for almost certainly any referee going through them with care will have the same sort of concerns if s/he is familiar with the matters at issue. I'll do "Revised theory of transient mass fluctuations" first.

1.

The statements in sections 2 and 3 about the cancellation of the average force on the devices in the presence of mass fluctuations are wrong. I've already pointed out that in the instantaneous frame of rest of the part of the device where the mass fluctuations occur that the $v(dm/dt)$ term in Newton's second law vanishes, but ma does not, so the stationary term in ma leads to the net force presented in the various papers that I and more recently Tom and I have written on this subject. Since these remarks seem to have fallen on deaf ears, let me put the argument in the context of a simple rocket equation argument (to be found in basic texts on the subject).

In the case of a rocket accelerated by the ejection of propellant, no external force acts on the system. On may therefore choose the inertial frame of the rocket before propellant ignition as one's inertial frame of reference and ask what the forces in the system are after ignition. Since the external force on the rocket/propellant system is equal to zero before propellant ignition, it follows that $ma = - v(dm/dt)$ after ignition where ma is the force experienced by the rocket and $v(dm/dt)$ is the thrust in the propellant. That is, the force acting to accelerate the rocket is equal to the thrust produced by the exhaust plume.

This example carries over without modification to the devices in question if the reaction mass is identified with the rocket and the element in which the mass fluctuation takes place is considered to be the "propellant". Note, in particular, that there are no "external" forces acting on the devices, as is the case for the simple rocket (assuming that gravitational coupling of the parts of the system to other objects is ignored). The ma that acts on the reaction mass, thus, is just equal to $- v(dm/dt)$, exactly as in the rocket case. And if there are no mass fluctuations, no accelerations driven by the [massless] actuator can produce a stationary force on any part of the system – as would be expected given the conservation of momentum. But, as both your team and John Cramer have shown, if mass fluctuations occur, then $v(dm/dt)$ is exactly equal to the ma computed in the instantaneous rest frame of the mass varying element. So, what John Whealton has done is show that in the initial rest frame of the reaction mass the thrust produced by these devices is predicted to be exactly the same as the alternate calculation. That is, he has confirmed that thrust should occur – he has not shown that a cancellation occurs.

2.

In section four you make some assertions that are not correct. ϕ in the various equations we have written is not some "non-local" gravitational potential. It is the **total** gravitational potential. It is the result of the appropriate integration over all matter sources, both near and far, in the causally connected part of the universe. Thus the assertion that "vastly larger" forces were ignored is simply wrong. Indeed, the forces involved in the effect are inertial reaction forces, which by Newton's third law are identical to (with opposite sign) the "vastly larger" forces that you allude to here. The fact that the spatial gradient of the potential due to a uniform distribution of distant matter vanishes does not mean that that matter exerts no, or negligible, forces on local objects. But it only exerts forces on them when they are accelerated by other "external" forces – indeed the distant matter exerts the inertial reaction forces they experience when they are accelerated by "external" forces. The effect of local objects in this is entirely negligible – exactly the opposite of what you assert. The problem here seems to be that you have neglected to take into account that the field in question (like the electric field in electrodynamics) consists of two parts: the spatial gradient of the potential **and** the time rate of change of the vector potential. It's the time derivative of the vector potential that is responsible for the inertial reaction force. And, as Sciama shows, the vector potential turns out to be proportional to the scalar potential in this case.

The consequence of the above is that your comments in section five are not right. Local gravity, with a contribution to the total potential that is about ten orders of magnitude smaller than ϕ (which is roughly equal to the square of the vacuum speed of light) does not produce effects 26 orders of magnitude larger than the predicted effects. So the measurements to a part in 10^{26} accuracy or exact cancellations to this accuracy that you claim are not in fact needed to see the predicted effect. Moreover, as just mentioned above, forces do not arise simply as the consequence of spatial gradients of potentials as you suggest in the last paragraph of section five. Matters are a bit more subtle than that.

3.

Tom has already told you of his calculations in the matter of thermal expansion of the devices due to their heating when power is applied. Those calculations show that the effect that does follow from heating is much too small to account for the effects we see with the devices that we have run (and running). Nonetheless, I should make at least one comment on your discussion of thermal effects should you decide to pursue this. In the third paragraph of section six on page 5 of the draft you talk about a "momentum jerk" produced by the initiation of heating of the PZTs. Conservation of momentum *requires* that no such thing occur. After all, if it did, mass fluctuations would not be needed to get propellantless propulsion. We could just use a series of thermally induced momentum jerks to do the job. In fact, ignoring the effect of reaction of the beam/suspension to motion of the suspension point of the devices produced by thermal expansion of the devices, the center of mass of the devices must stay fixed in space to conserve momentum.

4.

As far as your thermal expansion paper is concerned, aside from the fact that the effect is too small to account for the motion we see, there are a couple of statements of fact that I should correct.

The heating is not nearly as pronounced as you have surmised (chiefly from Tom's thesis it seems). In fact, driven at a power of 50 watts per device (100 watts total) for a period of 3 to 4 seconds, pulsing the devices at intervals of 3.5 to 5 minutes produced a temperature rise of about 15 to 20 degrees farenheit. This is not nearly as large as you assume

While it is true that Tom could only resolve 0.5 mm with his thesis system, the system that we have had running for several months now (described in the STAIF pdf file I sent you) can resolve displacements of 0.01 mm if needed. That is, the resolution of the system is more than two orders of magnitude better than the displacements actually observed. Part of the observed displacements, as recounted in the paper, is due to thermal effects. But the phase modulated part of the displacements (which do **not** track the power as they should for thermal effects) are still two orders of magnitude above the displacement resolution limit.