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Dear Pierre:

Thank you very much for your letters dated January 4 (addressed to me) and January 7 (to Prof Vitale, copy to me). It is a pity that, although they have been posted on January 15 (the stamp at Châtillon has this date), after you had received in the meantime our revised manuscript on "*Radiometer effect in the μSCOPE space mission*", they contain no reference to it. This letter is organized as follows: 1. *Accelerometer for systematic checks*; 2. *Thermal disturbances and thermal isolation*; 3. *Temperature gradients at the signal frequency*; 4. *Spin frequency of the spacecraft*; 5. *Configuration of the μSCOPE accelerometer*; 6. *Mis-centering of the test masses*; 7. *Conclusions*. Since the topics may be of interest to other scientists in our community, I shall post the correspondence (and our revised manuscript) on the website of the GG project at the address *http://eotvos.dm.unipi.it/nobili/opendiscussion*

1. ACCELEROMETER FOR SYSTEMATIC CHECKS

Will an accelerometer whose test cylinders are made of the same material and have the same density (no internal holes), be sensitive to the radiometer effect in differential mode? The answer is no. The radiometer acceleration is proportional to the area–to–mass ratio of the test cylinder *times* the temperature difference between its two faces. For a given temperature gradient the latter is proportional to the length of the cylinder, and therefore only its density does matter. If the density is the same there is no differential acceleration due to the radiometer effect. And no differential acceleration either due to an EP (Equivalence Principle) violation, because the cylinders have the same composition. Therefore, this accelerometer does not provide a check for the radiometer effect.

2. THERMAL DISTURBANCES AND THERMAL ISOLATION

The spectral distribution of temperature fluctuations along the test cylinders has a component at the signal frequency, with a corresponding radiometer effect at this frequency. This is noise and decreases with the inverse square root of the integration time. In addition to this noise there is a *systematic radiometer effect* due to the fact that the temperature gradients of the test cylinders follow their exposure to the infrared radiation of the Earth at the rotation frequency of the spacecraft with respect to it, i.e. at the signal frequency. The systematic radiometer effect is the most relevant because it cannot be reduced by taking more data. Temperature differences inside the spacecraft can be attenuated starting from those produced on its surface. As long as passive thermal isolation is considered, its effect is that of a low pass filter, the threshold frequency being the inverse of the time scale of thermal inertia. It is the analog of an RC filter. Therefore, the heat signal getting through is attenuated, but it has the *same frequency* as the heat flux impinging on the surface of the spacecraft. As a matter of fact, there is more than a single stage filter (multilayers, vacuum...), and from the combination of the various stages depends the phase of the filtered heat signal that will reach the test masses. Thermal isolation cannot be pushed at will. As I wrote to you during our discussion of last March over our PRD paper "....There is a delicate trade off between two competing needs: on one hand the need to provide thermal isolation, on the other hand the need to radiate away the heat which is necessarily produced in the experimental chamber around the test masses." One way to eliminate heat flux at a given frequency inside the spacecraft would be to have a careful distribution of sensors (thermometers) and actuators (heaters; plus an additional set of thermometers for verification) to actively control temperature gradients at the frequency of the impinging flux. However, active thermal control (more so at the frequency of the signal) is known to be a risky choice in small force experiments, and it is the reason why LISA relies on passive isolation (so does GG).

From points 1&2 above it follows that *the systematic radiometer effect competes directly with the signal*. If you do not agree on these points, I would like to read your arguments. The actual computation of the radiometer acceleration and the evaluation of the physical parameters it depends upon, is a separate issue.

3. TEMPERATURE GRADIENTS AT THE SIGNAL FREQUENCY

We agree on the formula of the radiometer acceleration. Once the density of the test cylinders has been fixed (and they have no internal holes, as in your case) the only relevant physical parameters are: i) *the residual gas pressure*; ii) *the temperature gradient* along the axes of the cylinders at the signal frequency (about 10^{-3} Hz). The larger the pressure and the temperature gradient, the bigger the radiometer acceleration. For the pressure, you quote a measured value of 10^{-5} Pa (we assumed 7 times less in our PRD paper). For the temperature gradient you give a "*specification*" (at the level of the sensor mechanics –hence to be further reduced by the factor 36.5 given by the thermal inertia that you have measured) of $3{\cdot}10^{-3}$ K/m, which would result in a radiometer acceleration on the test masses 30 times smaller than the target signal. When you mentioned that to me last March (you quoted 10^{-3} K/m), I wrote back: "...So, your value of 10^{-3} K/m would be ok." (I apologize for the error in the November manuscript on this point; it has been corrected in the revised version). In the same message to you I added: "As for temperature gradients, I do not quite understand your statement that the values given *'...have been specified and accepted by the CNES micro-satellite team'*. In my opinion it would be important to know the following. The value of 10^{-3} K/m is: a) a measured value; b) a value estimated in the framework of a given thermal model; c) a requirement? In cases b) or c) it would also be important to know whether all possible internal heat sources have been taken into account. In all cases, you should give a reference to some published paper or internal report for people to understand." You did not answer these questions, and that is why our PRD paper (then in press) was not modified. To my knowledge there is nothing wrong in that paper, but if you think otherwise, you can bring your arguments to the attention of the journal. Your claim that the "*specified*" value of temperature gradients will be reached needs to be demonstrated. The comparison with LISA as performed in Sec. II of our revised manuscript is now quantitative. We argue that, because of being in orbit around the Earth, thermal disturbances are much larger and it is not easy to reduce them (at the level of the test masses) as much as it is required. Even a thermal isolation as good as in LISA would not be sufficient. Please, take these considerations seriously. It may be possible that μSCOPE can be isolated better than LISA from thermal disturbances at *mHz* frequency, but because of points 1&2 this is a very important issue, and a *specification* cannot be taken for granted.

4. SPIN FREQUENCY OF THE SPACECRAFT

You can convince yourself that if the spin rate of μSCOPE becomes larger than the natural frequency of a test cylinder, this will become highly unstable (no equilibrium position exists and the force required to counteract the instability is proportional to the rotation angular velocity squared; e.g., see Ch. 6 of the old textbook by Den Hartog on "*Mechanical vibrations*"). For STEP they have recently published the values of the natural frequencies of the test cylinders, both in common mode and in differential mode (Worden et al., Class. Quantum Grav. 18, p. 2543, 2001) and we were able to check that the spin frequency is just slightly below the limit which would make them unstable. You can ask the STEP scientists to check that this not by chance. You say that in μSCOPE "*there is no motion of the mass neither with respect to the instrument cage, not with respect to the second mass*". If this were true, why not clamping each test mass to the cage? Because with no motion there would be no EP test. So, you must let the masses move, if you want to control them back and have an output signal which may contain a violation of the universality of free fall, hence of the equivalence principle. Everybody agrees that a higher spin rate would be better. I strongly confirm (it is very easy to demonstrate) that the limitation to fast rotation in your case (as in STEP) is that the test masses are constrained to move in one direction only. It has nothing to do with the FEEP thrusters, and your statement that: "*the satellite rotation is only limited because of the range of the FEEP thrusters*" is wrong. The force that the FEEP are required to apply depends on the drag to be compensated, and that is not much different (<100 μN) in GG, STEP, μSCOPE or even LISA; in the latter case it must compensate radiation pressure. Its value has nothing to do with the rotation frequency of the spacecraft because the non gravitational force to be compensated acts on its center of mass, mostly at its (slow) orbital frequency around the Earth. If the spacecraft spins (like GG, STEP and μSCOPE), the thrust simply needs to be *modulated* at the rotation frequency. In GG the modulation frequency is 2 Hz, and that is no problem whatsoever, neither for the propulsion system nor for the electronics of the FEEP (GG Phase A Report, Chs. 4 and 6). I wish this point to be absolutely clear. Any gossip that GG is unfit for drag free control with FEEP thrusters is simply unacceptable. On the issue of the spacecraft spin rate, the reaction of many people is that fast spin should not be used in small force experiments because of the inevitable noise associated with it. While this statement is true in a ground laboratory (we can see it in the GGG experiment), it is definitely not true in space, and the reason is that once the spacecraft has been accelerated to the desired spin rate, the conservation of angular momentum will maintain its rotation. No motor is needed to keep it spinning, and if there is *no motor*, there is *no noise* associated with it. I'll give you a simple example: because of the diurnal rotation of the Earth, we are moving (at the latitude of Pisa) at 1200 km/hr. But nobody feels it, because we all rotate together with the Earth and the Earth does not have a motor to keep itself spinning. The only noise at the 2 Hz spin frequency of GG will be due to the FEEP thrusters firing (modulated) at this frequency, and that is why we have *an intermediate passive isolation stage* between the spacecraft and the test masses (the PGB) which reduces this noise by several orders of magnitude. Thruster noise at the rotation frequency of the spacecraft is inevitable also at slow spin, but in this case passive isolation cannot help to attenuate it. Moreover, since the energy of spin in GG is very large, even the largest disturbing torque (the one due to solar radiation pressure) is not a problem. The spin energy of μSCOPE is smaller than that of GG by almost 8 orders of magnitude, and therefore the disturbing torques must be taken more seriously. μSCOPE needs magneto-torquers and reaction wheels, which are known sources of vibration noise also at low frequencies; GG needs neither of them.

5. CONFIGURATION OF THE μSCOPE ACCELEROMETER

In my letter of last October to Prof Vitale I expressed a concern on the fact that in the μSCOPE accelerometer *the test cylinders are not coupled*, neither by the electrostatic suspension nor by the capacitance read out. Although they are concentric, each one has its own read out and forms a separate accelerometer. The target signal being differential, the measurement of interest must be obtained by taking the difference of the individual measurements of each accelerometer. Thus, a very small signal must be extracted from the difference of two much larger ones. The same is done in classical mass dropping experiments. However, since the work of Eötvös at the end of the 19th century, the best tests of the equivalence principle have been obtained with the test masses arranged on a torsion balance, not by dropping individual masses. The reason is that the torsion balance is sensitive only to differential effects, and is therefore best suited to test the equivalence principle: *no differential effect, no output signal*. Although in reality this is not exactly true, common mode effects are effectively rejected by a torsion balance and this is clearly an advantage when aiming to detect extremely small differential effects. In your letter to Prof Vitale (copy to me), you confirm that in μSCOPE the measurement of interest is obtained by taking the difference of the individual measurements of the two concentric accelerometers. Of course you could take this difference directly from the instrument, and of course you prefer to have the individual outputs instead. But this is not the point: if each test cylinder is suspended independently, and has its own capacitance read out, it also has its own servo loop to keep itself "motionless". Since the voltage applied by the servo loop is the output, you inevitably have two individual outputs, no matter at what stage you take the difference. This is why I would not call this instrument a differential accelerometer. In GG (and in GGG) the test cylinders are coupled to form a balance and have one single capacitance read out to read the differential motion directly, in full analogy with the torsion balance. The STEP accelerometer is differential too. If you compare the μSCOPE test with mass dropping tests on the ground, of course the dynamic range you need is much smaller, and you may have enough bits for the difference of the two outputs to contain some significant signal. But this also means that you are wasting most of the bits of the output voltage of the capacitance system simply because the apparatus is not differential.

6. MIS-CENTERING OF THE TEST MASSES

In the recent Announcement of Opportunity by CNES/ESA for contributions to μSCOPE, it is stated that the centers of mass (CM) of the test cylinders will be centered in orbit to $\Delta r \simeq 10^{-7}\, m$ before the actual experiment begins. A difference Δr in the orbital distance r from the center of the Earth can be interpreted as an "equivalent" *classical* "EP violation" with $\eta_{equiv} = 3\Delta r / r$ (the centrifugal force –proportional to the inertial mass– increases by a fraction $\Delta r / r$ while the gravitational force –proportional to the gravitational mass– decrease by $2\Delta r / r$). The tidal acceleration ($a_{tide} \simeq 3 \cdot GM_{\oplus} / r^3 \cdot \Delta r$) due to Δr turns out to be 42 times larger than the target EP violation signal. A way to separate it from the signal is if it appears at twice its frequency (i.e. at $2\nu_{spin}$, with ν_{spin} the rotation frequency of the spacecraft with respect to the Earth, $\simeq 1/1200 s$ in μSCOPE), although a ratio 42:1 is going to be a limitation anyway. Will it really be so? For the tidal force to appear at $2\nu_{spin}$ the relative position vector $\Delta\vec{r}$ along the sensitive/symmetry axis of the accelerometer *must not change sign* over half period ($\simeq 600 s$); if so, after half period the tidal force changes sign (with respect to the Earth) while the EP violation force does not, and they can hopefully be separated. More generally, all parts rotating with the spacecraft are subject to a tidal force at $2\nu_{spin}$; instead, any mass which were free to move with respect to it would feel a tidal force at ν_{spin}. For freely orbiting test masses initially separated by Δr there is therefore a tidal effect "equivalent" to an EP violation at the level of $\eta_{equiv} = 3\Delta r / r$ (see also Blaser, Class. Quantum Grav. 18, 2509, 2001). This is why in STEP a freely moving (frictionless) He bubble gives a disturbance at ν_{spin}, and not at $2\nu_{spin}$. This subtle issue became clear when STEP was studied for the M2 competition of ESA. In μSCOPE the test masses are not freely moving, each one being controlled by a feedback voltage. This voltage is much larger than the target signal because of the residual drag, by 4 orders of magnitude according to the dynamic range that you quote. At frequencies higher than the orbital one (e.g. from 500 to 1000 s), drag disturbances due to air granularities directly affect each test cylinder. Although at these frequencies drag is smaller than its component along track, it is likely to be outside the frequency range of the drag free control. As a result, each test cylinder will undergo significant disturbances –although not at the signal frequency– which require a correspondingly high feedback voltage. Each cylinder being controlled independently, how can you make sure that drag noise acting over 600 s will not affect the relative position vector $\Delta\vec{r}$ so as to change its sign? If so, the tidal effect (42 times larger than the signal) will no longer appear at twice the signal frequency. As Blaser recalls in the paper quoted above, an EP test in space requires the test bodies to be *confined* and *very accurately centered* to avoid that classical tidal effects be indistinguishable from an EP violation. The point with the μSCOPE (and the STEP) design is that the test masses must be confined actively because the required centering is more demanding (by far) than what could be achieved *by physical laws* at rotation speeds below the natural frequencies. And with no position of *physical equilibrium* to go back to, the relative position vector which ideally should be subject only to the tiny effect of an EP violation, is in fact at the mercy of all (much larger) disturbances. In GG, with an offset of 10 μm by construction, the test masses are confined *by physical laws* (in super-critical rotation) to 9 picometers; the resulting tidal effect is 0.4 of the target signal, and it is at $2\nu_{spin}$. In his paper Blaser recognizes that: "*In the clever concept of GG, the CM of the two masses confined by super-critical rotation actually exactly follow the orbits of figure 1*" (Note: In figure 1 Blaser shows the orbits followed in the presence of EP violation only, and no relative position error at initial time). Then Blaser argues that noise due to the fast spin would limit the measurement in GG. As we have seen in point 4 above, this is not the case in space because there is no motor. We have a paper in preparation on "*Constraints from Celestial Mechanics on experiments to test the equivalence principle in space*" dealing with the issue of mass centering and how to distinguish an EP violation signal from an "*equivalent*" purely classical tidal effect.

7. CONCLUSIONS

Systematic radiometer effect at the signal frequency is a serious limitation to the μSCOPE experiment. A quantitative comparison with LISA shows that the level of thermal isolation required is a challenge. The Pt-Pt (same composition) accelerometer would not check for this systematic disturbance, which could therefore be misinterpreted as an equivalence principle violation; a result that would invalidate General Relativity. Until experimental evidence is provided for an adequate thermal isolation, the current target of an equivalence principle test to 10^{-15} would have to be revised. The accelerometer is not differential in its design, and although it clearly shows the expertise achieved at ONERA on capacitance sensing, it also shows little attention to the history of equivalence principle tests in the laboratory.

I wish to thank you very much for accepting to discuss these issues with me, and apologize for taking so much of your time. I am sure it will be fruitful. All the best for a good and peaceful 2002. Yours,

Anna
(Anna M. Nobili, PI of the "GALILEO GALILEI (GG)" small mission project)