# Reply to "Perturbative forces in the proposed satellite energy exchange experiment"

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We present our reasons for believing that the accuracy of the proposed satellite energy exchange experiment for gravitational measurements will not be degraded by the possible sources of error discussed in the foregoing comment by Keyser.

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Professor Keyser discusses four potential problems in the satellite energy exchange (SEE) experiment [1] which he suggests might adversely affect accuracy. The essence of our reply is that his numbers presuppose *typical* circumstances or materials, while precision-measurement experiments are *not* typical. To wit, he cites large values for outgassing, magnetic susceptibility, optical-path errors, thermal-expansion coefficients, and also mass-density variations. We contend that much smaller values will be obtained, since great care is called for in choosing materials and methods in precision-measurement experiments. Moreover, our margin of safety is large enough in two instances that accuracy will not be degraded even if we have substantially underestimated these effects.

For example, Keyser may be correct that the outgassing-induced effective pressure will be $\sim 10^{-9}$ to $10^{-6}$ Torr, far above the hard vacuum of space ($10^{-13}$ to $10^{-12}$ Torr at 1500 km altitude). The reason for his concern is less clear, however, since the pressure is well into the free-molecular-flow regime in any case and will therefore produce negligible drag and convection effects. We also do not anticipate a problem due to persistent outgassing gas jets, because the long duration of the SEE experiment is compatible with thorough prelaunch outgassing and a substantial post-launch "shake-down" period. The pertinent literature cited above (his Refs. [3] and [4]) entailed outgassing periods of only 50 or 100 h, and moreover, these sources found that outgassing rates continue to fall off nearly inversely with outgassing time, and would therefore fall 2 more orders of magnitude by $t=1$ yr. We remain concerned, however, about possible mass loss from the shepherd due to outgassing (Ref. [1], pp. 498–499).

Next, we believe that the problem of magnetic force on the test bodies due to induced magnetism in the compensator masses is overstated. Nevertheless, we are grateful to Keyser for pointing out that stainless steel is unsuitable for this purpose because of the very high magnetic susceptibility $\chi$ of some stainless steels (we might add that $\chi$ in stainless steel may be critically sensitive to welding, cold-working, and other stresses). A number of satisfactory alternatives exist, using either sandwiches or alloys with $|\chi| \sim 10^{-5}$ or less. Examples of low-$\chi$ metals and alloys include ($\chi$ is given in parentheses in SI units, with $10^{-5}$ understood): OFHC Cu ($-0.98$), Pb ($-1.63$), and a number of Al alloys (1.35 to 2.27) [2]; and Sn (0.24) and Mg (1.18) [3]. Moreover, zero susceptibility can be achieved by alloying various paramagnetic and diamagnetic metals; among these alloys are 50/50 Cu-Pd [4], 96/4 Cu-Ni [5], and various bronzes. Finally, we must reiterate that any residual magnetic force on the SEE test bodies will average to nearly zero every orbit (Ref. [1], p. 501) and, if detectable at all, could be isolated by its periodic variation, thus allowing a satisfactory correction.

Likewise, we are also grateful to Keyser for calling our attention to Peck's thorough analysis of the corner-cube interferometer (his Ref. [13]). However, he overlooks the fact that, for an *open*-octant reflector, $n=1$. Therefore, the second term in the spurious path-length change, $D(n-1)/n$, vanishes identically, and hence the angle required to make $\Delta L' \cong 1\ \mu$m is $\theta = 32$ mrad, not 20 mrad. More importantly, the suggested applications of such path-length analysis to the SEE experiment appear to us as correction terms, not errors. We see no reason to treat the incident angles $\theta$ as unknown and the resulting $\Delta L$ as an error.

Although the foregoing discussion of solar-heating effects on position error in the optical system is stimulating, it is perhaps misleading because it blurs three critical distinctions.

(1) *The "favorable six months" versus the "other solstice."* It is essential to distinguish among the various seasonal phases of the sun-synchronous orbit. Throughout the favorable six months, the *maximum* amplitude of the (twice-per-orbit) solar-heating oscillation is only 1% of the incident radiation. In contrast, at the "other solstice" the amplitude is ten times as large, assuming the capsule is tumbled about its orbital angular momentum (alternative tumbling protocols can reduce this amplitude, albeit with some loss of experimental time) (Ref. [1], pp. 497 and 499).

(2) *The capsule per se versus the outer insulating layers.* This is important, both because the capsule temperature will fluctuate much less than that of the outer layers and because the outer layers will be segmented and mounted so that their thermal expansion will not force a corresponding proportional expansion of the capsule *per se.* The length of the capsule will be governed by its own temperature, which will fluctuate at least one order of magnitude less than the insulating layers (Ref. [1], p. 500).

 

(3) *Materials with small coefficients of thermal expansion (CTE's) versus those with "typical" values of CTE.* We see no reason to use typical materials and have identified several materials with low CTE. An attractive candidate is graphite epoxy with "quasi-isotropic" winding, which has $\mathrm{CTE} \leq 1 \times 10^{-6}$ and will also provide a high degree of electrical conductivity.

Taking account of the above distinctions, we must stand behind our original estimates that the temperature of the capsule *per se* will oscillate less than 0.05 K and that its length will oscillate less than 1 $\mu$m during the "favorable six months."

Moreover, recall that the size and shape of the capsule will be continuously monitored interferometrically (Ref. [1], p. 500), so we can in fact tolerate much more than a 1-$\mu$m oscillation in capsule length. In short, we remain confident that the SEE apparatus will have distance resolution to spare.

Finally, Keyser's remark concerning possible relative density variations in the shepherd suggests the figure of $10^{-4}$. This is too high by an order of magnitude [6]. In fact, a salient strength of the SEE method is its relative immunity to density variations because of (1) the large separation between the test bodies and (2) SEE's capability to analyze density variations *in situ* by "geodesy" (Ref. [1], pp. 499 and 502).

We appreciate Keyser's helpful comments on the SEE experiment, and we anticipate that it will also be strengthened by the scrutiny of others. We are indebted to G.T. Gillies, D.N. Mashburn, and J.R. Thompson for useful discussions.

[1] Alvin J. Sanders and W. Edward Deeds, Phys. Rev. D **46**, 489 (1992).

[2] Paul T. Keyser and Steven R. Jefferts, Rev. Sci. Instrum. **60**, 2711 (1989).

[3] Calculated from *Handbook of Chemistry and Physics,* 71st ed. (Chemical Rubber, Boca Raton, FL, 1990), various tables.

[4] A. H. Wilson, *The Theory of Metals* (Oxford University, New York, 1958), p. 158.

[5] E. W. Pugh and F. M. Ryan, Phys. Rev. **111**, 1038 (1958); J. R. Thompson (private communication, 1992). For dilute solutions of Ni in Cu, $\chi$ varies nearly linearly with Ni concentration. The precise value for the crossover from diamagnetic to paramagnetic depends weakly on temperature and trace impurities, but in no case precludes attaining $\chi \ll 1 \times 10^{-5}$.

[6] J. H. Nash, A. C. Neeley, and P. J. Steeger, Atomic Energy Commission Research and Development Report No. Y-1654, Oak Ridge Y-12 Plant, Union Carbide Company Nuclear Division, 1968 (unpublished).