9.5 Evaluation of “Galileo Galilei” proposal

9.5.1 Requirements

a. Completeness of proposal
   [Is the information of sufficient quality to allow for an informed opinion?]

Partly. The description of the scientific payload is not enough to decide if the proposed test of the Weak Equivalence Principle (WEP) is feasible.

b. Mission scientific value
   [Is the mission of the right scientific calibre to be considered in the frame of the present call?]

The SARP believes the science goals are of sufficient potential and importance to be relevant for the M4 call. However, the realisation and analysis of the measurement process and instrumentation is not considered mature enough for M4.

c. Need for access to space
   [Does the proposed science need to be performed from a space facility?]

Yes. Since the existing terrestrial experiments are not expected to bridge the gap between their current limits (of order 10^{-13}) and those needed to test the WEP at the levels proposed here. Furthermore there are no known plans for new terrestrial experiments that would reach the sensitivity levels needed for a test at the 1 part in 10^{17} level.

9.5.2 Science value

How valuable is the science return from the proposed mission?

Strengths:

The scientific goal presented in the proposal is of fundamental importance. Experimentally verifying whether or not the motion of all neutral matter in a purely gravitational environment (free fall) is independent of its material composition follows a well-established tradition. A non-null result that could be reliably attributed to a breakdown at some scale of any accepted tenets that lead to General Relativity would herald a revolution in scientific thought.

Achieving tests of the WEP at the level of 10^{-17} (and potentially, 10^{-18} with an advanced Galileo Galilei mission (GG)) is an ambitious goal but if realised would improve on the best ground-based measurements by at least 4 orders of magnitude, thus imposing important constraints on fundamental physics.

The proposal discusses in some detail the historical background leading to this proposal and adequately reviews the state of the art of comparable ground and space-based experiments.

Weaknesses:

One significant weakness in the potential science return is that the experimental concept does not include a control measurement that can be used to validate any non-null result that might be claimed.

The proposal makes reference to the literature on tests of Lorentz symmetry violation. The theoretical emphasis of this notion has changed significantly over the last few decades. It is felt that the statement “gravitational Standard-Model-Extension provides a broad and general framework for searching for Lorentz violation” isn’t well founded and indeed it is not clear if this has any direct relevance to the proposed experiment.

Conclusions:

Assuming feasibility, and that the test could indeed be performed at the quoted level, and had a control measurement been part of the experiment, then the potential science returns would be high.

9.5.3 Scientific feasibility

a. Can the proposed science be achieved with the proposed mission?

The proposed mission uses macroscopic objects to test the Weak Equivalence Principle. This is a well-trodden path, and one expects, for example, results at the level of 10^{-15} in the coming years with the Microscope mission.

The instrumentation presented in the proposal promises to deliver a sensitivity to differential acceleration high enough to be able to detect a violation of the WEP at a level of 10^{-17}. If this could be realized within the constraints of the M4 call, it would represent an improvement over existing measurements of 2 orders of magnitude (assuming Microscope is successful and achieves its target sensitivity). Both Microscope and GG use rotation of the spacecraft to up-convert the violation signal from the orbital frequency of around 0.2mHz to a higher frequency. In the case of GG, this up-conversion puts the signal closer to 1Hz where the instrumentation challenges are significantly less, and likelihood of achieving the required sensitivity is correspondingly higher. This is an interesting approach and could yield increased sensitivity over previous methodologies.

While the proposal adequately summarises the current state of such measurements, it does not develop or present the GG experimental technique in a way that makes it possible to judge whether the scientific payload can achieve the proposed test of the WEP. Indeed, it is actually difficult to form a reasonable understanding of how the instrument works in detail, what the limiting noise sources and effects are expected to be, and how these drive the instrument performance. Testing the WEP at the level of 10^{-17} is not clearly linked to the instrument performance. Hence, the proposed level of the test is not well founded. Therefore, it is not possible to assess the potential impact that different instrumental and environmental noise sources could have on the final test of the WEP. The lack of details on the relevant known noise sources and dissipation processes, together with a believable mathematical description of the dynamics in 3 dimensions is a serious shortcoming.

The description of the instrument design is very vague. In particular, the schematic in Figure 3 is not of a quality high enough to understand exactly how the construction is made.

The proposers claim that their experiment could yield the target level of test within 1 day, and with subsequent validation during one year of mission operations. However, this raises serious questions about potential biases in the analysis.

The optical readout is based on heterodyne interferometry but using a masking technique to probe both cylinders with a single interfering beam. On the one hand, the performance of this laser gauge (sensitivity of 1 pm/sqrt(Hz) at 1Hz) is stated to be of crucial importance, on the other hand, it is not possible to see this from the error budget. However, the statement was also made that the optical readout is not the limiting factor in the overall instrument performance. Given the stated importance of the laser gauge, there is little detail given about the various effects that could affect the readout performance.

In addition, the proposal lacks sufficient description to convince the SARP that there is a well defined programme to develop this technology for space on the time-scales relevant for M4. If the gain in sensitivity of GG over other missions relies on this readout technology, then more detailed information is needed to show feasibility on the required time-scales.

b. Are there any issues not mentioned in the proposal that could hamper the proposed scientific return?

There are a number of issues discussed above, mostly relating to the experimental concept and physical description, which seriously hamper the science return.

The breakdown of the WEP is sought in the framework of the response of test matter to terrestrial Newtonian gravitation. The source of terrestrial Newtonian gravitation is independent of the Earth's (non-uniform) rotation. Furthermore, the test cylinders in the proposed experiment are spinning. In General Relativity the gravitational field of a spinning source depends on its spin. Also the mass centroid motion of extended spinning test matter in an external gravitational field may depend on its spin and still be geodesic (independent of inertial mass) when its spin is zero. The estimates, based on General Relativity, of the effect of the Earth's rotation on the motion of each spinning cylinder or the laser interferometer and their relevance to the interpretation of any non-null signal at the expected level of accuracy have not been sufficiently explained to the satisfaction of the SARP.

Axially rotating heavy masses are prone to whirling motions in some circumstances. Mention is made in Table 6 of a whirl control, presumably to inhibit such motions in the experiment. The details of how this is achieved are sparse. It is known that in some circumstances whirling motions can become chaotic. After the Question & Answer (Q&A) session, the little additional information that was given did not significantly reduce the concerns of the SARP.

The error budget in Section 3.3 of the proposal is critical for the expected accuracy claimed for the experiment. The figures appear to be based on ground-based GG tests and an end-to-end simulator involving electromagnetics and orbit dynamics. However it is difficult to ascertain from Section 3.2.1 precise details of the modelling assumptions behind many of the calculations shown. It is also not clear that all sources of noise have been quantitatively accounted for, even for those that are claimed to be negligible or irrelevant.

9.5.4 Timeliness of mission

Is the M4 time frame compelling for this mission? Why?

The knowledge gained from the Microscope mission may have important consequences for subsequent experiments and their design, however, it is not clear if this would be available in time to influence or impact on the design of GG. This raises doubt on the question of the timeliness for M4.

9.5.5 Competitiveness and complementarity with other projects

a. Are there other space- or ground-based facilities addressing similar science goals?

Ground-based instruments testing the WEP reach levels of 10^{-13}.

Microscope is addressing precisely the same goal, but it has a lower sensitivity and anticipates tests of the WEP at the level of 10^{-15}. GG claims to be able to detect a WEP violation 2 orders of magnitude smaller than Microscope. The SARP is currently not aware of any ground-based experiments that can promise such levels of precision. Hence it appears that going to space may be necessary to push the tests of the WEP to increasingly significant levels.

Though improvements continue to be made in a ground-based demonstration of GG (called GGG), the GGG target sensitivity has not been reached. It is not clear from the literature what limits this performance, and whether or not it can be further improved.

b. If so, how does the proposed mission compare with them or complement them?

This mission proposes to go one step beyond those currently planned or running by gaining 2 orders of precision. But again, this claim is not substantiated, and it is far from clear how this leap in sensitivity will be achieved with the proposed instrument.

c. Is the science output of the mission self-contained or does it require complementary data from other missions or from ground-based observations?

As a test of the WEP, the proposed mission is self-contained and doesn't rely on any other mission to be carried out nor to interpret the results.

d. What is the expected impact of the proposed mission in the relevant scientific field(s)?

Strengths:

There is no doubt that a measured violation of the Weak Equivalence Principle would be extremely valuable for modern physics, and would push forward alternative theories of gravity, which may eventually lead to the unification of the standard model with a theory of gravity. There are two possible outcomes from this mission. If a violation is detected, then the science return is obvious and vast. If no violation is detected, then the field is motivated to push the test to new levels.

Weaknesses:

The lack of a proper justification of systematic and quantitative error analysis leaves the proposal open to severe criticism.

Conclusions:

As proposed, the expected impact lacks credibility.

9.5.6 Collaborative environment

Is the proposed scientific collaboration scheme likely to produce the promised results? Why?

Strengths:

The proposal clearly states which groups will participate in formulating the instrument requirements, which groups will support the development phase, and which groups will focus on the analysis of the experimental data, including the development of the required algorithms and analysis tools. In addition, the PI team will be responsible for the development of the mission operations and the dedicated sequences needed to operate during the science phase.

The need for an international science team is identified, and the high-level roles and responsibilities are elucidated.

Weaknesses:

The proposal states that a detailed science plan will be developed after mission selection, casting doubt on whether the current science plan is sufficiently detailed to inform the selection process.

It is not clear from the proposal whether or not the manpower needed to support the design, implementation, commissioning, and eventual operation of this complex instrument is sufficient. In addition, the SARP cannot judge from the information given whether the effort needed to properly analyse the full data set is available from the participating institutions.

Conclusions:

Despite the identification of many institutions and their roles in the project, there is no specific discussion of individual commitments. In addition, the level of commitment of these participating institutions is unclear, particularly with regards to the level of available manpower.

Without a more detailed management plan the SARP cannot assess if the proposed level of participation is sufficient to implement the programme.

9.5.7 Overall assessment of the proposal

In the opinion of the SARP, the proposal, Q&A material, and interview did not provide convincing evidence that the mission is feasible within the M4 call boundary conditions. In particular, it is far from clear whether the experiment can achieve the promised test of the WEP.

The mission is considered risky since both the experimental design and modelling assumptions that underpin their error budget analysis lack sufficient detail to allow an informed opinion. As a result, there is significant doubt that the science objectives could be met to the level promised in the proposal.