

REVIEWS ON THE MANUSCRIPT [15]

Reviewer 1:

The research methods are appropriate, and are explained clearly. The conclusions are adequate and are supported by the content.

Specific comments:

1. The third term in equation (1) requires reference to relevant literature.

Added as a citation

2. In section 4.2, and further, the term "Trajectory" apparently refers to an oscillogram.

The term 'trajectory' refers to the position of the magnet as a function of time which trajectory really is. When plotted as such, for oscillatory behaviour, it is an oscillogram. So, the pictures obtained through the trajectory measurements are oscillograms. I have amended that in places where I refer to the picture obtained experimentally but I keep the term where it refers to the trajectory as such.

Spelling issues:

page 1, line 15: "There are two physicaly important". "physically"?

page 1, line 28: "he volumne of the magnet". "volume"?

page 2, line 5: "two extremns are analyzed". "extremes"?

page 2, line 13: "can be aproximated". "approximated"?

page 2, line 14: "the magnet beahve". "behave"?

page 3, line 25: "theoretical approch". "approach"?

page 4, line 1: "check the cosistency". "consistency"?

page 4, line 6: "begins to dissagree for large initial hights, where obviously friction needs to come ino account". "begins to disagree for large initial heights, where obviously friction needs to come into account"?

page 4, line 10: "during the oscilations". "oscillations"?

page 5, line 2: "Thus,parabolas". "Thus, parabolas"?

page 5, line 12: "positin on mass". "position"?

page 5, line 18: "prediciton form the period". "prediction"?

The spelling issues have been solved.

Recommendation:

The presented manuscript can be published, but after making the appropriate changes as specified in this review.

Reviewer 2:

General Impression

The authors' solution is well reasoned and logical. It gives a consistent and accurate model of the behavior of the "magnetic spring" and also provides adequate empirical proof in support of his model.

The only weakness is the neglect of energy losses due to friction and/or eddy currents.

From his experimental investigation it is evident that there is a damping term present that depends linearly on the velocity. However the author acknowledges this fact and therefore only makes predictions for short time periods (eg. only one period and then taking the next oscillation as a new initial condition) or slow velocities (eg. small oscillations near the equilibrium position).

Therefore I completely recommend his solution for publication.

Theoretical Model

First I would like to commend the author for his good qualitative explanation of the phenomenon, where he looks at two different regimes of the moving magnet. First the free-fall trajectory far away from the lower magnet (where the B-field is low due to the scaling law z^4) and then close to the lower magnet where he observes an elastic collision.

Then he introduces the magnetic and gravitational potentials and derives an expression for the Period of the oscillations using energy conservation. He does omit some steps in his calculations but this is forgiven since they are quite tedious as I found out trying to re do them.

However all the dimensions are consistent and I do trust his arithmetic. The only theoretical assumption that I could find that might be questionable is the modeling of the bar magnet as a microscopic dipole.

However he does address this later experimentally by investigating the equilibrium positions dependence on mass and shows that this assumption is quite reasonable in his setup. (the magnets never come very close to each other.)

Finally I would like to remark that I don't believe eddy currents to be a major source of energy dissipation. I believe the energy loss can mostly be attributed to air resistance and then secondly to friction with the walls of the pipe. (The reason for this statement is a similar experiment inside a vacuum chamber where a dramatic decrease of the damping was observed.)

I have added air resistance and friction as sources of energy loss to the manuscript, section 3.2. where the eddy currents were mentioned. The friction was neglected because of the frictionless approximation of the theory. The eddy currents are highlighted here because of the comparison with the bouncing ball model. Air resistance exists in both so it may have passed neglected. I agree that the air resistance still has effect, though it has been lessened by detaching the tube from the housing.

Experimental Setup and Results

The author's experimental Setup is quite ingenious! Using a cart and a long exposure photograph to capture the trajectory is a very good idea as it captures the entire phenomenon easily and without any interference with the oscillation process. Also using an Induction coil for the period measurement is a good idea provided the coil is relatively small and the voltmeter used to measure the voltage has a high internal resistance.

Both coil size and high internal resistance of the voltmeter demands were indeed met in the experiment. The voltmeter information was added to the manuscript while the coil size may be seen in Figure 2.

The results for the dependence of the equilibrium position and the z_{min} to z_{max} dependence show a good agreement with his theoretical model and therefore validate his assumptions. However some kind of estimation of the measurement error would have been nice!

Estimated error bars have been added to the graph in Figure 5a, while in Figure 5b the error estimate is given with the point size (added to caption).

The two period dependencies on mass and z_{max} show a larger deviation from the predicted results, but this is expected since the both assume energy conservation. However especially in the dependence of the period on the mass error bars should have been included since I believe that the result might not be very statistically

significant (assuming the same measurement error for the period as in the graph next to it).

Estimated error bars have been added to the graph in Figure 6b. I agree with the reviewer's concerns but this is partially due to the dependence being almost constant for this mass range.

Remarks

The v^2 in equation (1) is not rendered properly in the PDF.

The problematic term has been changed to v_z and should be legible now even in PDF.

Reviewer 3:

The presented paper is very interesting. Both theoretical and experimental approaches are presented and results are in a good agreement. The comments are following:

1. Quality of the labels to the axis of the histograms (and graphics) is low, and sometimes not readable. Units in the Fig. 1 are not presented. I think a legend in the Figures, which shows a comparison of theoretical calculation and experimental measurement is necessary.

All figures have been changed to provide better quality.

Figure 1 is an entirely qualitative representation of the given dependence given to illustrate the behaviour and the energy approach of the theory, the constants do not match the real ones and so units can not be stated. I have removed the numbers from the axes to minimize the confusion.

Explanations of the symbols and lines in the figures are given in the captions. I believe that additional legends on the graphs themselves would be redundant and would make the Figures overcrowded and that the comparison of theoretical curves and the measured values is best visible from the graphs themselves. If this is not what the reviewer is referring to, please clarify what kind of legend is deemed necessary.

2. Uncertainties of the measurement are presented only in Fig. 6. Precision of the measurement is not discussed.

Uncertainties have been added to other graphs as well. Precision of the measurement may be read from the error bars added, and is deemed satisfactory considering the agreement of with the theoretical curves.

3. The amplitude of the oscillation is not discussed.

The amplitude of the oscillation is set with the initial height from which we release the magnet into motion. From there a parabolic amplitude modulation may be observed as seen in Figure 7 and discussed in the text.

4. It is not clear how the Eq. 3 appears. Probably a few more intermediate equations are necessary in order to make calculation clear.

Eq. 3 is an artefact of algebraic manipulations made to simplify the integral. All the key steps have been given, and what is left is a long series of algebraic steps that are purely mathematical and are of no relevance to the physical view. There are no steps among them important enough to single out, and the entire procedure is too long to be a part of the text.

5. How the theoretical calculation was actually done? (entirely analytically, via a numerical calculation, or with a simplification model)? Which equation was used as the final formula for theoretical calculation?

The theoretical calculation amounts to solving the integral given in (2) with varying the parameters. I have added this clarification to the captions of figures with theoretical

prediction lines. The integral can not always be solved analytically so numerical integration was used, but with no further simplifications of the expression. Hence the final formula for the theoretical calculation is (2) using (3) as a formula by which the parameters of the system transfer to the integral in (2). I have clarified this at the end of section 3.1.

6. There are two references at the end of paper, but there is no citation in text.

The citations have been added to text.

Editorial request:

Figure 2: consider adding a scale bar.

Scale bar added.

References: Clarify what parts of the text cite or rely on the references [1] and [2]. What particular information is used from these two references?

Citations have been added to text.