# Marc Davis

PROFESSOR of ASTROPHYSICS

## University of California, Berkeley

*Email Correspondence*

**December 31, 1995 – January 2, 1996**

## PREFACE

Davis suggests two analogies to support his guess that the standard prediction for Galileo's experiment is correct. He vehemently denies that it ***is*** a guess, because, as he proclaims: We "must believe [Newton's Laws]…[They] tell us absolutely how the ball falling in the earth will behave."

One of Davis' analogies involves *circular motion*. The other one involves *electricity*.

Circular motion obeys a cosine curve, as does the linear oscillation prediction for the Small Low-Energy Non-Collider experiment. In circular oscillation the distance remains constant, as does the force. By contrast, in linear oscillation, both the distance and the force dramatically *change* over the length of the path. Still, the analogy may be true. Why don't we test it?

Electricity is not gravity. Electricity is bi-polar (+)(–) (attractive and repulsive). Whereas gravity is monopolar (unidirectional). Both sets of phenomena exhibit behavior characterized by an inverse-square law. Therefore the analogy may be true. Why don't we test it (by doing the experiment with gravity)?

After Davis seemingly exhausts his plea to simply *surrender to untested predictions*, my next (and last) reply is to share some astronomical evidence that seems to support my model, or at least suggests the need to question Newton. In the intervening 23 years, my strategy has changed. So I interrupt the correspondence at this late point to give a more poignant response to Davis' final volley, and to put my astronomical arguments in perspective.

Those arguments culminated in my first published paper (in 2007) which includes an analysis of data gathered from observations of gravity-induced motion of stars in globular clusters. As noted in my "interruption," I still try to keep up with many developments in astronomy. But such observations—being of remote and complicated systems—lack the directness of the more accessible and purposeful Small Low-Energy Non-Collider, which is the singularly most potent method to provide the long-awaited unequivocally convincing physical evidence.

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**NOTE 1:** The format of these email messages is different from more recent ones. Available technology and my archiving skills have evolved since 1996, when this exchange took place. The content is nevertheless clear enough. Davis is satisfied with guesswork and faith in human authority. He is not impressed with my insistent appeal to the authority of Nature.

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**NOTE 2:** My final reply to Davis involves astronomical research that requires some context to appreciate. I have therefore provided this context as a "Hindsight (2019) Reply..." just prior to the actual chronological (1996) reply. In these two pages I also criticize Davis' last response for its logical fallacy (of misplaced concreteness).

To: marc@bkyast.berkeley.edu
From: rbenish@continet.com (Richard Benish)
Subject: Gravity-induced oscillation

Dear Professor Davis

I have a question concerning a gravity experiment which, to my knowledge, has never been done. It's the one often posed in elementary physics texts: Given a uniformly dense spherical mass with an evacuated hole through a diameter, show that a test object dropped into the hole harmonically oscillates.

This is easy enough to show theoretically, but is there any empirical evidence? I know of examples where the idea has been proposed to used the oscillation as a clock—whose frequency would give a measure of Newton's G (satellite experiment). And I've heard it said that stars can oscillate through the centers of star clusters. But I've never found any data to substantiate it.

This strikes me as curious. So I am asking you, if you can, to please tell me where the predicted oscillation has been physically demonstrated.

If you don't know of any evidence, perhaps this would be a worthwhile experiment to do. (Because it would replace an extrapolation for a concrete fact.) Using an apparatus resembling a Cavendish balance, but having the attracting masses sculpted so as to permit movement of the bobs through the center, I think it would not be too difficult, at least to demonstrate the oscillation as a first approximation.

I thank you very much for any comments or information.

Sincerely,

Richard Benish

Date: Sun, 31 Dec 1995 13:34:21 -0800
From: marc@coma.berkeley.edu (marc davis)
To: rbenish@continet.com
Subject: Re: Gravity-induced oscillation

Richard,
I agree that nobody has ever built an oscillator similar to what you describe. But a Cavendish experiment of this sort will be very difficult to build and isolate from other fields. Oscillations of stars in a cluster take much longer than a human lifetime.

I don't agree that this type of oscillation is "only theoretical". The x or y coordinate of a satellite orbiting the earth is undergoing oscillatory motion in a very similar manner as if the satellite were on a radial orbit. There will be no new physical principle involved for a satellite to actually move in a radial orbit, if that were possible.

It might be fun to try such an experiment, but there is no great theoretical interest in doing such an experiment.

Marc Davis

To: marc@coma.berkeley.edu
From: rbenish@continet.com (Richard Benish)
Subject: constant, angular vs. changing radial

Dear Professor Davis,

I am grateful for your quick yet thoughtful response.

I understand that in the case of a circular satellite orbit the motion projected on a given line in the same plane is simple harmonic, as is the prediction for radial motion through a uniformly dense sphere. So there is a close similarity, as you state. But qualitatively, the motions are clearly much different: the one is ANGULAR under CONSTANT gravitational potential, while the other one is RADIAL under widely CHANGING gravitational potential.

In Cavendish's original experiment, the first torsion filament he tried (a silvered copper wire) was not stiff enough to prevent the small masses from colliding with the wooden enclosure. They sensibly moved, due only to gravity. In an arrangement that allowed movement through the center, I guess air convection would be the biggest problem. But since great precision is not the goal, 19th century technology should still suffice.

"There is no great theoretical interest in doing such an experiment" because we have great confidence that our concept of gravitational attraction can be reliably extended to circumstances we have not yet explored. I just think it would be nice to actually explore the unexplored, even if there are no surprises. Then all those text book holey sphere thought experiments could be backed up by empirical evidence.

Thanks again.

Sincerely,

Richard Benish

Date: Sun, 31 Dec 1995 14:57:10 -0800
From: marc@coma.berkeley.edu (marc davis)
To: rbenish@continet.com
Subject: Re: constant, angular vs. changing radial

Richard,

If you want to try this experiment, it would probably be much easier if you did an analogous experiment with electrostatics. Consider a set of conncentric conducting spheres, all with a some charge on them. Then take another object with opposite charge, a small ball, for example. it should be possible to build metal concentric spheres all with a slot cut into them, which would allow a small ball to be suspended from a thin string. With no charge, the ball would oscillate like a typical pendulum. But then you could oppositely charge up the spheres and the ball, and the oscillation frequency should increase. The more concentric spheres you can build, the closer the experiment will be to a solid, uniformly charged volume.

Good luck.

Marc Davis

$\frac{1}{r^2}$

To: marc@coma.berkeley.edu
From: rbenish@continet.com (Richard Benish)
Subject: dipole vs. monopole

Dear Professor Davis,

Again I thank you for your quick reply.

The experiment you describe, with a pendulum swinging through concentric charged spheres must, of course, demonstrate something about electricity. The inverse square law that gravity has in common with electricity suggests, I suppose, that doing the experiment with electricity is the same as doing it with gravity.

But you are surely more aware than I am of the many differences between electricity and gravity.

I understand that, especially in astronomy, we often have no choice but to assume that our laws are applicable in circumstances in which we have never directly tested them. The kinds of experiments that have been done with gravity in recent years are vastly more difficult than the modified Cavendish experiment that I have proposed.

The only reason I can think of not to do it is that we are satisfied with the guess that we know the result. I myself am not satisfied with a guess.

Sincerely,

Richard Benish

Date: Tue, 2 Jan 1996 10:49:29 -0800
From: marc@coma.berkeley.edu (marc davis)
To: rbenish@continet.com
Subject: Re: dipole vs. monopole
X-Sun-Charset: US-ASCII

Richard,

You may not be satisfied, but in fact we are not "guessing" when we say the pendulum will oscillate with harmonic motion. If that were not so, Newtonian gravity would be inconsistent at the weak field limit, * and there is NO evidence that it is inconsistent. Checking for consistency is critical in physical science, but it is pointless to fuss overly much on experiments that cannot be executed, when there are alternative tests that can check the same physical principles. These tests have shown with great precision that Newton was correct, that the equations as he wrote them are consistent with most tests (barring the very small effects of General relativity, but that is another story).
Given that statement, the equations tell us absolutely how the ball falling in the earth will behave. You must believe them, because your life depends upon them every time you cross a bridge, or fly in an airplane.

Marc Davis

* There is no evidence that Newtonian gravity is inconsistent in the limited domains where we have looked. But why do we *REFUSE* to gather more evidence from places—even *HUGE* places—where we have not yet looked? Let's just stop looking and pretend to know what we'd find if we did. Is this science?

## *Hindsight (2019) Reply and Context for Understanding the Reply Given in 1996 (which follows)*

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Being abundantly confident of the correctness of his stance ("we are not guessing") Davis makes a valient effort to convince his amateur correspondent (me). I admit that the experiment—when it is at last carried out—may support the standard prediction. But until Nature stamps this prediction with her approval, the scientific thing to do is to put forth and execute a plan to expedite the day of reckoning (do the experiment)—not entrench oneself in the beliefs of human authorities.

Instead of doing the scientific thing, Davis commits a serious logical blunder: "You must believe [Newton's laws] because your life depends upon them every time you cross a bridge or fly in an airplane." Many a serviceable, life preserving bridge had been built and many a bird has flown for eons prior to Newton. Surely, lives depend on the *structural properties* of stone, aluminum, steel, air and feathers—not on the *abstract laws* that formalize quantification of these properties. More importantly, it is surely the responsibility of a physicist, perhaps even as a matter of life-and-death, to *test Newton's laws* in those accessible extreme regions where they have not yet been tested. That is, *inside matter*.

By failing to consider the possibility that his *assumptions* (predictions, equations, expectations, extrapolations) may be wrong, by instead asserting his FAITH in the validity of these untested abstractions, Davis thus commits the *fallacy of misplaced concreteness*. Unfortunately, this practice has become perniciously acceptable in modern physics.

The *prediction* for the result of the experiment counts for almost *nothing*. According to the ideals of science predictions have no enduring status until they are backed up by the concrete, empirical facts of physical reality. We can argue till we're blue in the face. Why not just shut up and arrange to hear what Nature has to say? *Why is it* that PhD "scientists" will not see this as the proper course of action? The answer lies in psychology and sociology, not physics.

As for my last reply, given in 1996, it reflects my research into astronomical evidence that seemed to bear on the question. Before having convinced myself that the data—though perhaps indicative—were ultimately too indirect to have the needed force to convince, I pursued the strategy and presented the results in my first published paper (*Laboratory Test of a Class of Gravity Models*, enclosed). In that paper I present my analysis of "proper" versus "radial" motion of stars in globular clusters. The evidence suggests an unsolved and mostly unrecognized anomaly in these marvelous swarms of gravitating bodies.

The most dramatic statement which acknowledges the problem without offering a clue as to how to resolve it, concerns Globular Cluster NGC 6752. The "measured" quantity serving as the key datum in this kind of analysis is the *distance*—sometimes referred to as the *kinematic* or *dynamic* distance. My analysis reveals a trend that violates expectations in a way that favors my gravity model. Astronomers have devised various methods for measuring distance, most of which, for a given object, are consistent with one another and so serve as ways of checking any stand-outs. NGC 6752 stood out so much as to evoke the authors' comment:

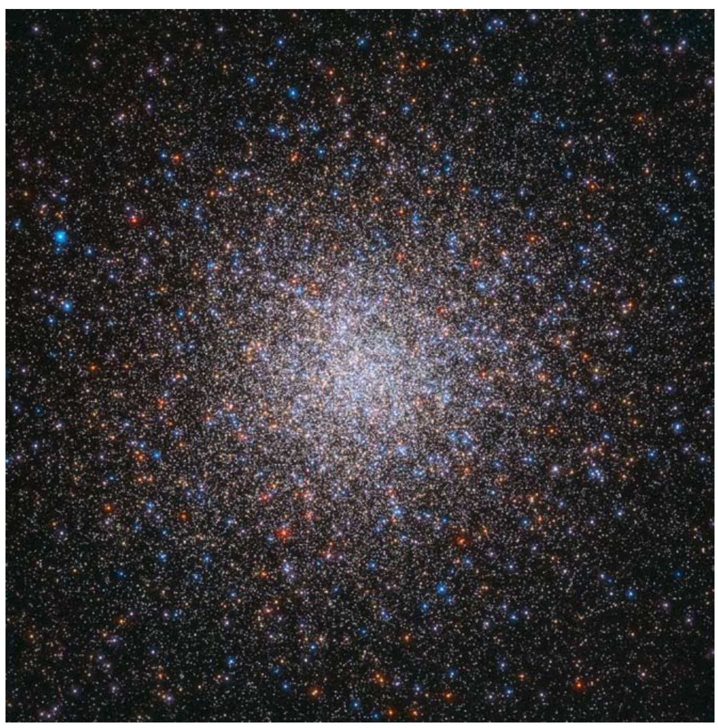
> While there is some uncertainty in the distance to NGC 6752, it is certainly known to better than the factor of roughly two which would be required to bring the two measurements [radial vs proper-motion velocity dispersions] into agreement… a most peculiar situation.

When I replied to Davis in 1996 I had not yet delved into the evidence from globular cluster analysis, but I was on the trail leading to it. Even while pursuing this line of thought, I recognized its possible long-term futility, because astronomers have way too much faith in Newton to be swayed by the resulting indirect and mostly obscure evidence. Even the "most peculiar situation" of NGC 6752 would fade as against all the apparent successes of their go-to theory of gravity. Therefore, although I continue paying attention to evidence from astronomy, my concentrated effort remains to urge building and operating humanity's very first Small Low-Energy Non-Collider.

If the result agrees with my prediction, there would be no way to save Newton's and Einstein's theories. They would have been proven to be ill-founded models whose usefulness in *exterior* fields would never diminish—within limits—but whose essential cores would be exposed as utter failures for making grossly incorrect predictions for test-object motion through the *interiors* of massive bodies.

Whereas if the result agrees with the standard prediction, then the astronomical anomalies will someday receive their proper Newton/Einstein-consistent explanations, and I would surrender to the revelation that my new gravity hypothesis is wrong.

Independent of any competing gravity model, however (I'll say it again) *we owe it to the spirit of Galileo to build and operate humanity's very first Small Low-Energy Non-Collider.* The sooner the better.



**Globular Cluster Messier 2.**

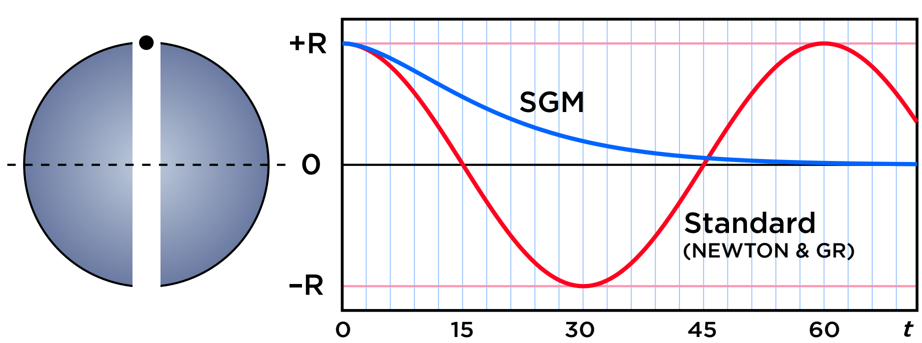



**Figure 2.** Schematic of Galileo's experiment with graph of competing predictions: The standard textbook answer is that the test object executes simple harmonic motion (red curve). But in none of the many textbooks, papers, and classrooms where this prediction is given do we ever find empirical evidence to back it up. Even without a competing model, therefore, doing the experiment is a valuable contribution to science. For our immediate purpose, the SGM's drastically different prediction (blue curve) would be unequivocally supported or refuted. The 60 minute oscillation period corresponds to a sphere whose density is about that of lead.

To: marc@coma.berkeley.edu
From: rbenish@continet.com (Richard Benish)
Subject: Interior vs. Exterior

Dear Professor Davis,

I thank you for your reply (1/2/96) and for your patience with me.

I am well aware that the evidence in support of Newton's and Einstein's gravity theories is abundant and seemingly quite compelling. Considering the better of the two (General Relativity) notice, however, that this evidence is entirely (or nearly so) in support of Schwarzschild's EXTERIOR solution. The Schwarzschild INTERIOR solution—which of course includes as a first approximation Newton's radial oscillation prediction for a uniformly dense sphere—has not been tested. (Taylor, 1961)

The various successes of the exterior solution instill great confidence that the interior solution should also be supported by physical evidence. The faintest hint of suspicion may perhaps be aroused, however, by the following anomalous astrophysical data:

(1) In an analysis of the dynamics of the Virgo cluster of galaxies (Shaya, 1986) in the section titled, 'The Dilemma of the Velocity Dispersion,' the author remains puzzled as to why the maximum apparent infall velocity is found well beyond the cluster's center; why are the higher velocities expected near the center not found?

(2) Similar results followed from an analysis which included 14 rich clusters (Cowie and Hu, 1986). Each of the 14 samples displayed the same effect: "The major point to note from Figure 1a is the 'swarming' of many of the galaxies in a low velocity population near the origin."

(3) On a smaller scale, an analysis of the motion of stars in the disk of our own galaxy (Lacey, 1984) indicated that the component of motion perpendicular to the plane of the disk was 10%-30% smaller than expected.

(4) Cooling Flows. "Perhaps the most surprising result in recent years is the discovery that in many clusters, 10 to 1000 M(sun) per year of hot gas is cooling and condensing out in the central cluster galaxy." (Helfand, 1995) Why should a "flow of material into the central regions" cool and condense? (Mushotsky, 1993). The question is reminiscent of the whole problem of star formation (especially low-mass star formation). The phenomenon responsible (gravity) for the condensation of a gas cloud into regions of high density is also supposed to produce high velocities in that region. How is this dispersive effect overridden by the condensing effect?

Because of the remoteness of these objects of study, this evidence must be considered indirect and inconclusive. But each case may be regarded as an "interior problem," whose problematic aspect would be lessened if we had any reason to EXPECT lower velocities in the central regions. Surely, it would be good to know, with absolute certainty, that we are justified in extending our model of gravity to the central regions.

I am grateful to Newton for our visit to the Moon, and I am grateful to Einstein for the knowledge that clocks in the attic tick faster than clocks in the basement. But it is not obvious to me that I "must believe" their interior solutions to be correct.

Assuming that extraneous forces can be sufficiently minimized, if the large spheres in the modified Cavendish experiment were made of lead, the torsion pendulum would have a period of less than an hour. Wouldn't it be nice to see the predicted oscillation actually happen?

I am truly grateful for your time.

Sincerely,

Richard Benish

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# Emeriti Faculty


## Marc Davis

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### Specialty Areas

Large-scale structure, cosmology, evolution of galaxies

### Research Interests

My field is the study of large-scale structure. In the past this has taken the form of large redshift surveys combined with careful analysis of nbody simulations. We have used the Keck telescope to obtain the world's largest sample of galaxies at redshift one, which is extremely useful for study of the evolution of galaxies and the evolution of large-scale structure.

### Biography

Marc Davis received his SB degree from MIT and his PhD in physics from Princeton. He continued at Princeton as a postdoctoral fellow and then went to Harvard as an assistant professor of astronomy. in 1981 he joined the Astronomy and Physics departments of UC Berkeley. He has received a number of honors for his work over the years, including a Sloan Fellowship, the Pierce prize, a Miller research professor, a member of the National Academy of Sciences, the American Academy of Arts and Sciences, the Heineman prize, an honorary PhD from the University of Chicago, and the Gruber prize.

The work was seriously hampered when he suffered a major stroke in June, 1993. A detailed summary of his life from high school to the present is given HERE.

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Statement on Collegiality




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