Letters

to the Editor

Work Reworked Problem

I was quite pleased when I saw the *TPT* article[1] "Work Reworked" since teaching about the concepts of work and potential energy can be quite challenging and tedious, especially when teaching these concepts to advanced physics students who never miss a beat. Unfortunately the article gets bogged down at Eq. (1), which is incorrect. The rest of the derivation, in turn, is rendered pointless. The net force on the book should be $F - mg$, where F is the force applied by the person lifting the book.

1. R.G. Jordan, "Work reworked," *Phys. Teach.* **40**, 526–527 (Dec. 2002).

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Work and Potential Energy

Regarding the note "Work Reworked," by R.G. Jordan, which appeared in the December 2002 issue of *TPT* (pp. 526–527):

- Potential energy is a state function; its change does *not* depend on how. Work is not a state function; its value depends on how.
- Jordan's methods of an external agent lifting a mass m through a vertical distance h and then determining the work done by the agent are OK. They do provide insight into the physics of it. However, there are other ways the external agent could lift a mass through a vertical distance. For example, the agent could exert a constant upward force resulting in a constant upward acceleration a and then the work done by the agent in lifting the mass through a vertical distance h is:

  $$W_{\text{ext agent}} = m(g + a)h,$$

  where g is the constant acceleration due to gravity. In this example the initial and final velocities need not be specified. However, the vertical distance is the same as Jordan's, and the work done by the external agent is greater. The initial and final velocities are related by the work-energy theorem:

  $$W_{\text{ext agent}} - W_{\text{gravity}}$$

  = 1/2mv2f – 1/2mv2i,

  where v_f and v_i are the final and initial speeds, respectively.

The comment in Jordan's note that "the work done is $W = mgh$ and it is independent of the acceleration, etc." is misleading. It is true, in his method, that the work done by the external agent is mgh. However, the "etc." suggests that it is true in general and that is the misleading part; "work depends on how!"

I agree with Jordan about the difficulty of doing this example, of lifting a mass against gravity, early on in an introductory physics course. However, teachers of physics must remember the total body of physics. When we get to thermodynamics, the conclusion presented in Jordan's paper has to be argued away.

I am comfortable in defining potential energy right after defining work. I prefer doing it the way I was taught; namely, make the external force just a bit bigger than mg, then the work it does is just about mgh and that is the potential energy also.

A physical system is "given" its potential energy by an external agent who does work on it. However, the definition of change in potential energy is that it is the negative of the work done by the field in a displacement from one point to another in the field, not necessarily the work done by the external agent.

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Work Reworked, Author Response

I thank all those people who have written to me directly for their interest in and comments regarding my paper.[1] In reply to Jeffrey Wetherhold, let me say he is both right and wrong! He is correct in pointing out that Eq. (1) is not the net force, but it is the force one applies to raise the book. I'm afraid the error was due to my inadequate reading of a revised manuscript. In the original version I had written, "So, at any instant the net force on the book is $F - mg = ma_y$, [Eq. (1)], where $F = m(g + a_y)$ is the force applied to the book. Therefore, the incremental work done is …." To reduce the length of the manuscript, I deleted the first equation and renumbered the remaining equations accordingly, but I failed to modify *all* of the associated text! However, in spite of this slip on my part, the rest of the derivation is sound since I calculate the work done in lifting the book, which is determined by the force applied to the book.

In answering Richard Mancuso's comments, I would simply point out that I was dealing with a *specific* problem that some students have difficulty accepting, at least initially, and before the introduction of energy and the work-energy theorem. Of course, the work-energy theorem produces the answer — and it might be the preferred approach by many instructors — but the question was posed to me *before* we had talked about energy at all. As a result, any reference to potential energy is moot although some of the points he raised are valid. Again, my statement about being "independent of the acceleration, etc." is specific to the work done in lifting the book, and it follows closely where I stated that the initial and final velocities are both zero. In that context, the work done does not depend on the acceleration or on the path taken, and that's why I wrote "acceleration, etc." I did not show the latter in the paper as I was trying to be brief, but it can be demonstrated easily. For example, let us generalize the problem to three dimensions and take the floor as the *x-z* plane and the *y*-direction vertical. Then we can write the force on the book as $\vec{F} = m(\vec{g} + \vec{a})$, with $\vec{g} = (0, g_y, 0)$ and the instantaneous acceleration $\vec{a} = (a_x, a_y, a_z)$, so the incremental work done is $dW = \vec{F}(d\vec{s})$, where $d\vec{s} = (dx, dy, dz)$. Following the approach shown in the paper, and using the properties of the scalar product, one can see that the total work done is $W = mgh$, since the (three) integrals involving the components of the velocity $\vec{v} = (v_x, v_y, v_z)$ [see Eqs. (2) and (3)] are all zero if the book starts and finishes at rest, i.e., if $\vec{v}_f = \vec{v}_o = (0, 0, 0)$. So, the result, $W = mgh$, is independent of the acceleration and the path taken; it depends only on the vertical height the book is raised. I do agree with Mancuso that this approach does provide insight into the problem, but I would add that as the problem is specific and the conditions are stated clearly, I'm not sure there's anything that's misleading and needs "arguing away."

1. R.G. Jordan, "Work reworked," *Phys. Teach.* **40**, 526–527 (Dec. 2002).

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Bridge Oscillations Reference

It is most interesting to read the continuing articles about the collapse of the Tacoma Narrows Bridge.[1] Although the bibliography was not intended to be comprehensive, I do wish to call attention to an interesting article that is rarely cited in discussions of the bridge collapse.[2] The author of this article was one of the major designers of large suspension bridges, and in it he gives a brief history of other bridges that have had aerodynamic problems: "Some twenty known bridges completed since 1930 have been subject to disturbing or dangerous aerodynamic oscillations, and some of them have required the application of corrective measures to make them safe." He describes his own aerodynamic studies and the prescriptions he made to stiffen suspension bridges that were experiencing dangerous oscillations. I feel this article should be a part of any bibliography about the Tacoma Narrows Bridge.

1. Bernard J. Feldman, "What to say about the Tacoma Narrows Bridge to your introductory physics class," *Phys. Teach.* **41**, 92–96 (Feb. 2003).
2. D.B. Steinman, "Suspension bridges: The aerodynamic problem and its solution," *Am. Sci.* **42**, 397–438 (July 1954).

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Drag Forces

The authors of a recent article[1] report on using MBL equipment to monitor the damped oscillations of a mass hanging from a spring and fitting the data to a theoretical curve based on the assumption that the drag force is linearly proportional to the velocity. Figure 3 in the article shows their best-fit result. Although the authors do not comment on it, the plot reveals the existence of significant systematic residuals that cast doubt on the validity of the fitting function and, as a result, its underlying assumptions. Specifically, the data show that the observed oscillation amplitude decays *more quickly* at high amplitudes (and, by the same token, *less* quickly at low amplitudes) than can be accounted for under the assumption of linearly damped harmonic motion.

The observed trends in the residuals *are* reasonably consistent, however, with the operation of a velocity-dependent drag force that has both linear *and* quadratic components. Such a drag force more properly

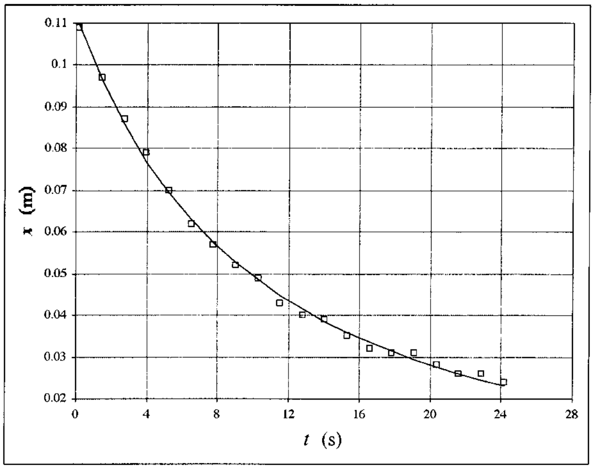

Fig. 1. Plot of data from Ref. 1 along with a best fit obtained numerically using a drag force with both linear and quadratic components.

models the behavior of an object with speeds that vary from low values (at which the air flow is laminar and a viscous interaction dominates) to higher values (at which the flow becomes turbulent and dynamic effects dominate).[2,3]

It is not terribly difficult to use a spreadsheet to numerically integrate the equations of motion and to manually adjust the coefficients to fit the observed data. I have done this for the authors' data[4] and obtain the significantly improved fit shown in Fig. 1.[5] As indicated by that figure, I find that the authors' data are extremely well fit using the force law $F = -kx - bv - c|v|v$ with $k = 8.21$ N/m, $b = 0.020$ N/(m/s), and $c = 0.109$ N/(m/s)^2. Further consideration of the best-fit results suggest that the quadratic drag component is about three times as large as the linear drag component at the highest velocity amplitudes (~53 cm/s) and that it still accounts for more than one-third of the total drag force even at the lowest observed velocity amplitudes (~11 cm/s).

This case may serve as a cautionary tale: Although linear drag forces are appealing candidates for modeling velocity-dependent drag due to the existence of analytical solutions, they are often inadequate models of reality.

1. Michael C. LoPresto and Paul R. Holody, "Measuring the damping constant for underdamped harmonic motion," *Phys. Teach.* **41**, 22–24 (Jan. 2003).
2. James A. Lock, "The physics of air resistance," *Phys. Teach.* **20**, 158–160 (March 1982).
3. G. Fowles and G. Cassiday, *Analytical Mechanics*, 6th ed. (Saunders, Fort Worth, 1990), p. 56.
4. For my analysis I estimated values for the authors' data using Fig. 3 in Ref. 1. Those estimates are also plotted in my Fig. 1, which is formatted to facilitate a direct comparison.
5 The theoretical curve is the result of interpolating between successive oscillation maxima obtained via numerical integration.

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Drag Forces, Authors' Response

We are pleased that A. John Mallinckrodt found our note worthy of follow-up investigation. Our decision not to include a quadratic damping term was a conscious one, and we still feel that since we were able to obtain two matching values for a linear damping constant, we present a legitimate introductory experiment.

Rather than integrating a second-order differential equation numerically, we investigated the components of the damping force by integrating energy equations, which we were able to solve in closed form. A better fit than we had previously came from the solution in which both the linear and quadratic terms were used. The value of b was comparable to Mallinckrodt's, but our value of c was, although still greater than b, not as large as his. We are grateful for his interest and input.

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Correction: "The History and Fate of the Universe: A guide to accompany the Contemporary Physics Education Cosmology Chart" [*Phys. Teach.* 41 (3), 146–155 (2003)]

An error was introduced during the processing of this paper. The number of galaxies contained in the visible universe is 4 x 10^{11}.

Note: The Distinguished Service Citation winners announced in the April issue should have been listed as the 2003 awardees.

Author update:

Ariel R. Libertun, author of January's "Warning! Objects in mirror are closer than they appear," is now a research associate at JILA–University of Colorado at Boulder. He can be contacted at arl@colorado.edu.