Assignment 1: Functions
Some solutions

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Problem 1 (modeling a hanging slinky—step zero) Determine/identify some (interesting) quantity associated with a hanging slinky which you think can be measured and modeled by, i.e., compared to, a real valued function

$$f : (a, b) \to \mathbb{R}$$

of one variable on an interval (a, b), or possibly on a closed interval $[a, b] = \{x \in \mathbb{R} : a \le x \le b\}$. You should identify the the real numbers a and b with $a < b$ determining the **interval of definition** though measurements may be needed to give them actual numerical values, and identify the quantity or measurement to which **the values** of the function f should be compared.

When you get done, you should have an idea of exactly what you want to measure and how.

You may wish to change the name of the function f. For example, if you want to compare the values of f to a linear density, then you may want to call the function ρ, λ, or δ, which are more traditional symbols used to denote a linear density. Hint: Do not let f correspond to/model a linear density but rather some quantity which is easier to measure and from which a linear density may be derived.

Let's call your function a **model measurement function**.

Solution: The model measurement function I find most interesting and natural for modeling the slinky is what I call the **extension**. I think the extension can most simply be modeled by a real valued function of one variable $u : [0, L_0] \to [0, L]$ where the domain variable $x \in [0, L_0]$ models the vertical distance from the end of the slinky attached to the bracket to a point on the slinky in its **completely compressed**

configuration which I would take/assume to be an equilibrium in zero gravity, and the function value $u(x) \in [0, L]$ models the vertical distance from the same point of attachment on the bracket to the height of the same "material point" on the slinky in its **gravity induced extended configuration**.

The identification/explanation I have given above may be essentially adequate (for you) to understand the extension function I would like to use to model the extension of the slinky. The values of the extension function should correspond rather directly to the measurements taken using the marks on the slinky and the measuring stick to which it is attached—though it may be convenient to change the units from inches to, for example, meters. Additional explanation and discussion may add additional clarity. The value L_0 can be obtained by measuring the slinky on a table. The last time I checked, that compressed[1] slinky was 55.4 mm tall and had (approximately) 84 coils, i.e., single full circular helices (the nature of which we can discuss in more detail if necessary).

The codomain I have given in the statement of the problem may be adopted so that $u : [0, L_0] \to \mathbb{R}$. One expects, however, that the extension as defined above will be an increasing function of x reaching a maximum value L corresponding to the lowest point of the hanging slinky. For more details and further discussion of this point, see my solution of Problem 10 below. The (full) model on the one hand may be assumed to predict this length. On the other hand, one can obtain comparison value for L by measuring the full length of the extended slinky. The value I have on hand is 52.5 inches or 1.3335 m.

One can relate the value of any extension to a more detailed mathematical model of the **slinky/spring geometry**, and it may be worthwhile to discuss this process in order to more clearly appreciate the measurements taken using the measuring stick. A uniform helix (extending vertically from $z = 0$ with radius $r > 0$ and winding counter-clockwise downward at "pitch" $c > 0$ when viewed from above) may be parameterized by

$$\gamma_0(\theta) = (r \cos \theta, r \sin \theta, -c\theta). \tag{1}$$

If we take $0 \leq \theta \leq 168\pi = 2(84)\pi$, then we obtain a helical curve making precisely

[1] Incidentally, I just purchased two more Slinkys on sale for about 3.50 wfrn (worthless federal reserve notes). The manufacture of these Slinkys seems slightly inferior to the one I purchased 3 or so years ago. The tie ferrules on the older one securing the ends are not present, and the ending cuts are not aligned. But overall, these new Slinkys are probably serviceable for my purposes. The height on the one I have in hand measures about 50 mm in height compressed and extends about 5 mm when relaxed on a table in a horizontal position. I read that the Slinky brand changed hands in 2020, and perhaps this accounts for some change in manufacturing.

84 full cycles. It seems reasonable to assume this curve lies at the center or "core" of the spring steel helix of the slinky in equilibrium position so we have the "pitch" c given (approximately) by

$$c = \frac{L_0}{168\pi}.$$

The equilibrium radius r may also be measured. When the slinky extends under its own weight in gravity, one expects the helical geometry to become somewhat more complicated. It should be the case, however, that each **material point**, i.e., each point $\gamma_0(\theta)$ corresponding to a physical position of the unextended slinky, should determine a corresponding extended (model) height $-u(c\theta)$. It is to certain of these points, for example,

$$u(0) = 0, u(4c\pi), u(8c\pi), u(12c\pi), \ldots, u(168c\pi) = L$$

to which we may compare measurement data. The geometry of the slinky model in the completely compressed position and the use of the parameterization (1) for the core curve in particular is illustrated on the left in Figure 1 with a close-up of the point top front point at $(r, 0, 0)$.

It seems to me there are several advantages of using the extension function u. A first advantage has to do with regularity as discussed more fully in my solution to Problem 10 below. In short, the extension function u should be expected to be naturally **smooth** in the sense of **differentiable**. Another advantage is that this (kind of) function can be used naturally to represent a large collection of different extensions other than the model extension corresponding to the hanging equilibrium induced by gravity. Most obviously, the completely compressed equilibrium corresponding to the zero gravity case may be modeled by the identity function

$$u_0(x) \equiv x.$$

Finally, the relation of other extensions to the identity extension u_0 is natural with respect to a peculiar qualitative property of the slinky considered as a spring as described in more detail in my solution to Problem 10 below. As a (very) preliminary observation in that regard, notice that the model extension u associated with essentially **every** physical extension should be expected to satisfy

$$u(x) \geq u_0(x) = x.$$

One simple geometric assumption concerning the **extension geometry** is that the core curve of the extended slinky, analogous to the core curve of the equilibrium

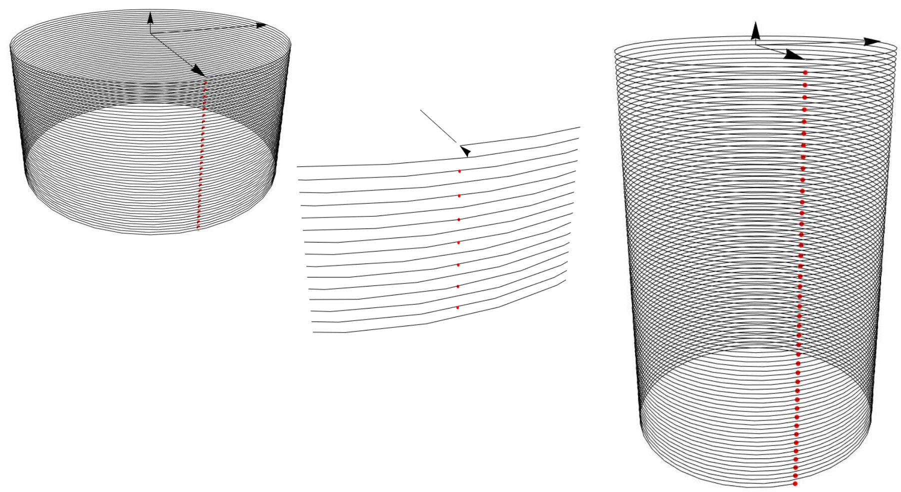

Figure 1: A model core curve for the compressed slinky (left with a close-up in the middle). On the right there is a plot of a "core" curve with increased pitch corresponding to an evidently non-physical extension. This may be considered a very crude or primitive mathematical model of the extension of the slinky, but producing such graphics is a good exercise to see the basics of a model are understood and behaving as expected.

configuration, has a parameterization of the form

$$\gamma(\theta) = (r(\theta)\cos\psi(\theta), r(\theta)\sin\psi(\theta), -u(c\theta)) \tag{2}$$

where $r = r(\theta) : [0, 168\pi] \to \mathbb{R}$ and $\psi = \psi(\theta) : [0, 168\pi] \to \mathbb{R}$ are geometric model functions auxiliary to the basic modeling built around the function u. Notice that (possibly) an **angular offset** $\psi = \psi(\theta)$ and a **deformed radius** $r = r(\theta)$ may also conceivably be modeled and compared to (additional) measurements. Notice also the relation $x = c\theta$, so that extension geometry functions ψ and r may also be naturally considered as functions of $x \in [0, L]$. The basic assumption here is that there is a preserved **vertical centerline**. This assumption could presumably also be checked by measurement. I do not have, at this point, any reasonable geometric model functions r and u to substitute in (2), but just to illustrate how such a parameterization for a deformed core curve works in principle, we can keep the radius $r = r(\theta)$ constant, say the same constant r, and angular offset $\psi = \psi_0(\theta) \equiv \theta$ used in (1) and on the left in

Figure 1 but replace the extension function $u_0(c\theta) = c\theta$ by a function $u(c\theta) = (c+\epsilon)\theta$ for some $\epsilon > 0$. The result is illustrated on the right in Figure 1. This may be considered a primitive or crude model for the extension of the slinky in gravity, but it is obviously very inaccurate and does not say anything particularly interesting.

Let me emphasize that the discussion of the spring geometry here is secondary and mentioned primarily to illustrate the meaning of the extension function u, which I think should be (or something rather like it should be) the basic model measurement function.

Exercise 1 One obvious problem with the model extension $u(c\theta) = (c + \epsilon)\theta$ used to produce the model extension on the right in Figure 1 is that the resulting core curve is longer than the original core curve (and one does not expect/anticipate significant stretching along the length of the coils/wire of the slinky). In particular, the length of the model core curve on the right in Figure 1 between each pair of consecutive red marks is longer than the corresponding distance along the core curve on the left in Figure 1. Modify the parameterization in two different ways to correct this inaccuracy/discrepancy and create graphics for the corresponding "improved" core curve.

(a) Take the radius $r = r(\theta)$ to have a smaller value, so that the spirals of the model slinky lie in a thinner cylinder (so that corresponding lengths are correct). In this case, all the red marks will/should still line up in a vertical line down the front of the model slinky.

(b) Keep the same radius r, but scale the angle in the parameterization so that corresponding lengths are correct with the red marks no longer falling along a vertical line but falling along a helix with reverse chirality/twist.

Which of these modifications is "better?"

Further Comments on Problem 1

At least a couple students went further in their solution of this problem, and many students made some comment about Hooke's law and Hooke's constant. These topics are taken up in a later assignment (Assignment 9 Problems 1-4), and strictly speaking I did not intend for students to address the actual modeling of the system in this problem. With a view toward offering some constructive criticism and suggesting some alternative viewpoints, I think it may be helpful to address some of the best student efforts in this direction. I think Lila Bernhardt gave the clearest and most detailed exposition I saw, so I will pretty much stick to quotations from her solution.

I will present Lila's work in an indented (quotation) environment with some minor modifications (hopefully) for clarity:

> A slinky is a precompressed helical spring toy and, as such, Hooke's law of spring elongation $F = k\eta$, can be used in modeling where k is the spring constant and η is the elongation.

Let me note that there is a fundamental problem at this point. The problem is perhaps, first of all, that the "elongation" is not defined. This, it turns out, is quite important, though the importance may not be immediately obvious. Let me try to fill in some detail:

The Hooke's constant is a constant associated with a particular specific spring. The application of Hooke's constant, strictly speaking, only applies to the homogeneous extension (or possibly compression) of that spring. It is an important point which I attempt to bring out in Assignment 9 that the Hooke's constant is not a "material constant," or a constant that depends fundamentally on the (infinitesimal) physical properties of the spring. In particular, Hooke's constant is not something that depends on the elastic properties of the spring material (alone) but also on the specific length of the spring: If you cut a certain "kind" of spring in half, or you look at any small piece of a spring having certain homogeneous elastic (material) properties, then the smaller piece(s) do not have the same Hooke's constant as the original spring. As a consequence, it is not physically reasonable to apply Hooke's law to (small pieces of) a non-homogeneous deformation of a spring.

Thus, I am disputing directly Lila's assertion above that Hooke's law can be used to model a slinky, at least when non-homogeneous deformations like those observed in the hanging slinky are considered. Of course, what I am saying is not quite correct, as at least a couple students (including Lila) have indeed "used" Hooke's law to model the hanging slinky. I am arguing here, however, that the "use" is not physically reasonable. Many such, shall we say, instances of modeling that do not make good physical sense may be found in the literature—especially in the "engineering" literature. I hope to make a convincing argument that there is good reason to stick to physically reasonable modeling. Either way, what you decide to do has consequences. You can "use" Hooke's law, but there is a consequence, a quite undesirable consequence, which I will try to explain clearly below. For now, however, let me simply continue with my explanation of Hooke's law, which is physically reasonable when applied within the proper context, namely homogeneous deformations of a specific "macroscopically modeled" spring.

The **elongation** is defined to be the displacement of one end of a homogeneously deformed spring from the position taken by that end in equilibrium, assuming the other end remains fixed. This is a proper definition of the term elongation, which may be considered as a kind of increment $x - x_0$ where x is the position of the (moving/extended/compressed) end and x_0 is the position of that end in equilibrium. With this definition in hand, **Hooke's law** gives an expression for the force, in effect Hooke's law defines the force by

$$F = -k(x - x_0).$$

Presumably the sign convention here is clearly understood: If the end of the spring is extended beyond the equilibrium position x_0 to a position x, then $x - x_0 > 0$ and the force modeled is that exerted (at x) by the spring in the opposite direction of the extension. This may also be taken as the tension force along the entire length of the homogeneous deformation. Specifically, if the spring is cut and a tensiometer (of negligible displacement) is placed between the two resulting pieces, then the force registered will be the same as the force at the end position x.

Note carefully, that Hooke's law effectively defines the force resulting from the spring. It is a constitutive law in this sense. If you know what a displacement/elongation is, and you have one, then you can calculate the force.

Let me modify the discussion slightly to fit with my deformation function $u : [0, L_0] \to [0, L]$. For a homogeneous deformation $u_h(x) = Lx/L_0$, Hooke's law reads

$$F = -k(u_h(L_0) - L_0) = -k(L - L_0).$$

Here is a next extract from Lila's presentation:

> Using Newton's second law, $F = ma$ with $a = g = 9.8$ m/s^2 as the acceleration due to gravity, we can calculate the tension force in the slinky at each point:
>
> $$F(x) = m(x)g = (L_0 - x)\,\frac{M}{L_0}\,g$$

where M is the total mass of the slinky and $m(x)$ is the mass of the portion hanging below the distance $u(x)$ from the fixed end.

This is a very nice observation. It is quite clear and, one might even say, correct. The linear density of the slinky here is modeled by

$$\frac{M}{L_0}$$

and $L_0 - x$ is the length in the equilibrium/compressed configuration assumed to extend beyond $u(x)$. It may be noted that I did not mention the mass of the slinky above,[2] but it is an important quantity, and in fact I see in my notes a previously measured value of 212 g. This is, I think, a solid element in the modeling of the hanging slinky. Thus, Lila obtains an alternative expression for the force. Unfortunately, Hooke's law as it is "used" in this instance, does not give an expression for the force, but let's see what comes next:

> Let the function $u_1 = u_1(x)$ describe the elongation of the slinky. This elongation value can be found by using Hooke's law.

It will be noted first that the quantity here called the "elongation," namely $u_1(x)$, is different from the elongation $u_h(L_0) - L_0 = L - L_0$ where $u_h(x) = Lx/L_0$ in Hooke's law. Thus, I am skeptical about the assertion that, presumably, $u_1(x)$ can be found using Hooke's law. Nevertheless, here is the explanation:

> The relation
> $$\eta(x) = \frac{F(x)}{k} \tag{3}$$
> describes the elongation at each point x as a function of the force applied at each point x. To find the total elongation of a point x along the slinky,

[2] I was not undertaking the actual modeling.

an integral must be taken:

$$\begin{aligned} u_1(x) &= \int_0^x \eta(\xi)\, d\xi \\ &= \int_0^x \frac{F(\xi)}{k}\, d\xi \\ &= \int_0^x \frac{(L_0 - \xi)\ Mg/L_0}{k}\, d\xi \\ &= \frac{Mg}{L_0 k}\left(L_0 \xi - \frac{\xi^2}{2}\right)\Big|_{\xi=0}^{x} \\ &= \frac{Mg}{L_0 k}\left(L_0 x - \frac{x^2}{2}\right). \end{aligned}$$

Letting L denote the total length of the hanging slinky, the relation $u_1(L_0) = L$ implies

$$\frac{Mg}{L_0 k}\ \frac{L_0^2}{2} = \frac{MgL_0}{2k} = L \qquad \text{or} \qquad \frac{Mg}{k} = \frac{2L}{L_0}. \tag{4}$$

Substituting this value into the expression for u_1 one gets

$$u_1(x) = \frac{L}{L_0^2}(2L_0\, x - x^2). \tag{5}$$

My first observation is that the **elongation at a point** $\eta(x)$, which was never properly defined, does not have a physical meaning. At this point, one has left mathematical modeling, and is engaging in what might be called "symbol manipulation." Of course, some kind of definition is implicit in the relation (3). According to that implicit definition the **elongation at a point** $\eta(x)$ is simply a quantity proportional to the force. There are two basic problems with this: The first is that the same symbol η is being used here and for the "elongation" $u_h(L_0) - L_0 = L - L_0$ in Hooke's law, but these are clearly two different things. The other problem, is that now, unlike when Hooke's law was introduced, this is not a constitutive relation. The force has no meaning on its own, and the elongation at a point is defined in terms of the force. This is very very different from defining the force according to Hooke's law where the elongation $\eta = u_h(L_0) - L_0 = L - L_0$ is well-defined (only in the context of a homogeneous deformation).

Nevertheless, the force resulting from gravitational acceleration is well-defined, and the substitution is made resulting in a deformation function u_1 quadratic in x and having coefficients with some relation. Notice that the Hooke's constant cancels out in this procedure of symbol manipulation and really plays no role at all. One is actually not using Hooke's law, but something else. One can see further that the integral relation

$$u_1(x) = \int_0^x \eta(\xi)\, d\xi$$

is questionable by differentiating:

$$u_1'(x) = \eta(x).$$

The derivative $u_1'(x)$ is a good modeling quantity with solid physical meaning, but here it is essentially related to the force (or a not-so-properly defined quantity $\eta(x)$ proportional to the force) by a proportionality constant $1/k$ which ultimately plays no role whatsoever.

Incidentally, even before one gets started with the integration, another quantity/term is introduced with no explanation. That is the **total elongation** at x. This seems to be another name for the value of the displacement function u_1, but why another name? In fact, this quantity does not seem to be any kind of reasonable "elongation" but rather it's derivative looks like something that is loosely identified with a kind (some kind) of elongation coming from the dubious relation (3).

Having made all these objections, let me be quick to point out that the result is okay, or even "pretty good." There is a good solid route involving mathematical modeling which leads to precisely the same conclusion, namely, that the model measurement function u_1 for the slinky should be quadratic, and I think of exactly the same form. I will discuss this presently. Unfortunately, that conclusion ought to be questioned, as I will explain further below. And when it is questioned, as it should be, then the very serious weakness of the "symbol manipulation" approach becomes painfully manifest.

Returning to Hooke's law, properly applied, we have

$$F = -k(u_h(L_0) - L_0) = -k(L - L_0) = -kL_0 \left(\frac{L}{L_0} - 1 \right)$$

where

$$u_h(x) = \frac{L}{L_0}\, x.$$

Notice that $u_h' = L/L_0$ is constant, and this constant appears in Hooke's law. Reflection suggests an alternative constitutive relation with the force in the spring proportional to the quantity $u'(x) - 1$ for any deformation. Thus, an initial modeling assumption for the modeling of an inhomogeneous spring made of material with a **modulus of elasticity** or elasticity constant (as opposed to a Hooke's constant) is

$$F = -\epsilon(u'(x) - 1).$$

Consideration of various examples, especially homogeneous deformations of a spring or particular pieces of spring made of the same material, indicates that the modulus of elasticity ϵ should be a material constant. Now then we can either apply Newton's first law (in equilibrium) suggesting that the spring force is opposite and equal to the force due to the mass or Newton's second law with the same conclusion:

$$\epsilon(u' - 1) = (L_0 - x)\rho g = Mg(1 - x/L_0)$$

where $\rho = M/L_0$ is the linear density of the spring. Thus, we obtain a differential equation for u:

$$u' = -\frac{Mg}{\epsilon L_0}x + 1 + \frac{Mg}{\epsilon}.$$

The solution is quadratic:

$$u(x) = -\frac{Mg}{2\epsilon L_0}x^2 + \left(1 + \frac{Mg}{\epsilon}\right)x.$$

The condition $u(L_0) = L$ implies

$$-\frac{MgL_0}{2\epsilon} + L_0 + \frac{MgL_0}{\epsilon} = L \qquad \text{or} \qquad \frac{Mg}{\epsilon} = 2\frac{L - L_0}{L_0}. \tag{6}$$

That is,

$$u(x) = -\frac{L - L_0}{L_0^2}x^2 + \frac{2L - L_0}{L_0}x. \tag{7}$$

So we see then that what I said was partially correct: We do obtain a quadratic expression for the displacement u, but the quadratic function we obtain has a slightly different form from the function

$$u_1(x) = -\frac{L}{L_0^2}x^2 + \frac{2L}{L_0}x$$

given in (5). It is not precisely the same, but it does bear some resemblance.

I should be quick to point out that the disappearance of the elasticity constant which occurs in (6) looks strikingly like the disappearance of Hooke's constant in (4) which I criticized as an indication that Hooke's law was not actually being used but rather something else. I am also using something else, and the constant ϵ does seem to disappear, but I think the situation is somewhat different. Perhaps my criticism above was somewhat misplaced or at the very least poorly explained. Let me see if I can explain the situation more clearly. One has a presumed modeling constant, like Hooke's constant or the modulus of elasticity. One does not know a value for that constant apriori from an independent source as one "knows" for example constants like the mass of the spring, which I obtained by placing the spring on a scale or the acceleration constant $g = 9.8\,\mathrm{m/s}^2$ which was obtained from other experiments. Notice that in both (4) and (6) what is happening can be interpreted as "finding a value for the unknown physical constant," based on the use of a single data point, in this instance the total length L of the hanging slinky. In each case, one can go back and find a value for the constant in question:

$$k = \frac{MgL_0}{2L} \qquad \text{and} \qquad \epsilon = \frac{MgL_0}{2(L - L_0)}.$$

I guess (what I am claiming) the crucial difference is that the modulus of elasticity has a more meaningful modeling/physical interpretation: The force is proportional to the dimensionless quantity $u' - 1$, and the constant of proportionality is ϵ with natural physical dimensions

$$[\epsilon] = [\text{force}] = \frac{ML}{T^2}.$$

Again, I will try to indicate the significant difference this makes below. It may be, and surely is, that the generalized Hooke's constant satisfying $F(x) = k\eta(x)$ can be given units presumably

$$[k] = \frac{[\text{force}]}{L}$$

where $\eta = \eta(x)$ is the "elongation" at x. Is the "elongation at x" a length? What physical length is it? If it is not a length, then the dimensions of the new Hooke's constant are not M/T^2. what are the dimensions of the new Hooke's constant? These questions are difficult to answer when $\eta(x)$ does not have a solid model description.

I would like to now compare the predictions of the two models with the measured data (and with each other).

At about this point in her solution Lila does something that is very nice. She includes what she calls a "sanity check" substituting various values for x in her formula and checking to see that the predictions she gets seem reasonable compared to her expectations. This is always a good idea for several reasons, and in this instance the calculations she makes mostly amount to checking that the integration and algebra were done correctly—which is one of those reasons.

Here is roughly how Lila's "sanity check" goes:

$$u_1(0) = 0 \qquad \text{and} \qquad u_1(L_0) = L.$$

She adds $u_1(L_0/2) = 3L/4$ which "looks OK." Let me start my matching Lila:

$$u(0) = 0, \qquad u(L_0) = L, \qquad \text{and} \qquad u(L_0/2) = \frac{3}{4}L - \frac{1}{4}L_0.$$

This is very interesting, no? It strikes me that, were we careful, the difference in our predictions for the deformation position corresponding to $x = L_0/2$ could give a way to see if one model is better than the other. One could, in particular, make a comparison to the data. I'll try to comment on this further below.

But let me also raise you one: I believe that according to both of our models, we should have $u'(L_0) = 1$. The reason is that there is no slinky mass below the point the point corresponding to $u(L_0) = L$. This is certainly true of my model. It is not entirely clear for Lila's model, though I guess she would/should agree this should be the case. In point of fact:

$$u'(L_0) = 1 \qquad \text{but} \qquad u_1'(L_0) = 0.$$

I claim it is a natural physical constraint that any model measurement (deformation) function must always satisfy $u' \geq 1$. The condition $u' = 1$ is what you get if there is complete compression. The condition $u' < 1$ is not physically possible.

I am not confident that I (or more properly "we" in the sense of Sabrina, Olu, and I) have a perfect data set. Lila may very well have a better data set associated with her slinky, but I do not know of any others who have a better set. In any case, I will use my/our data set (as I have compiled it) simply because I have it at hand. Applying Lila's model and my model, I obtain Figure 2. Initially it appears both models give values roughly comparable to the measured values: Both give increasing concave functions of x. Various analyses could be made of how well the models fit the data, but two observations stand out:

1. Generally, it looks like $u(x) < u_1(x)$ with both models underestimating the measured values, so that Lila's model gives a substantially better fit.

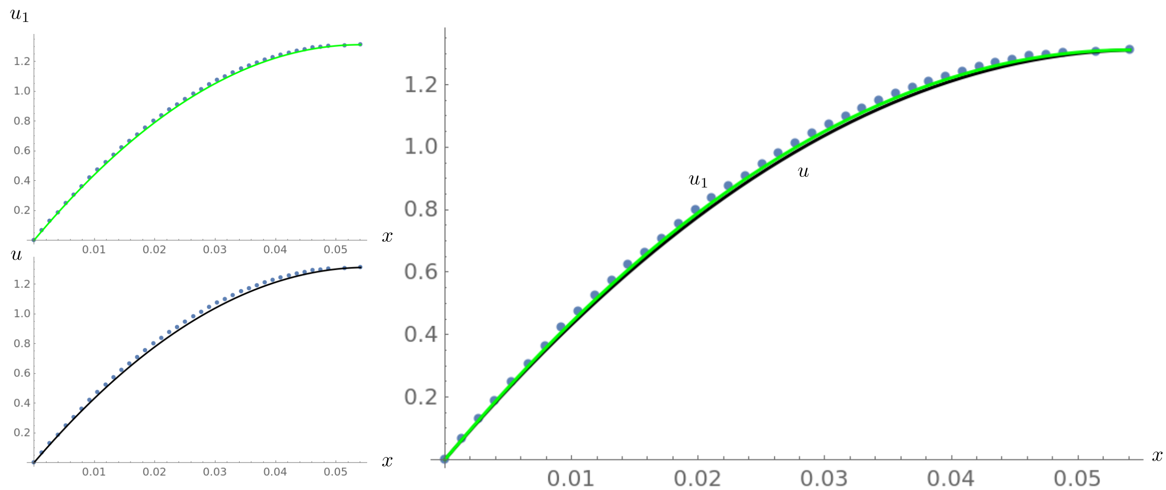

Figure 2: Two initial models of the hanging slinky deformation: Lila's "Hooke's law" (top left) and my constant elasticity model (bottom left)

2. Both models give measurements systematically and consistently lower than the measured values.

The last point should indicate to us that we are missing something fundamental concerning the physical (slinky) system in our models: The basic shape we are producing is **incorrect**. As we should see later, all mathematical modeling should be viewed as "suspect" or, put another way, almost certainly incorrect, though it may even be the case that we cannot discern any noticeable difference between the prediction and the measured data—or practically speaking no difference between the prediction and the measured data that is not within the known tolerance for the accuracy of our measurements. It is a well-defined and natural goal to look for a model capable of this level of predictive accuracy.[3] One very good way to see one has not achieved this

[3]Given that what I am suggesting is correct, then conversely I would add further that if one has not achieved this kind of accuracy of prediction, then a sober, yet basically valid, conclusion is that the physical system is fundamentally not adequately understood; there is still work for the scientist (engineer, applied mathematician, physicist, biologist,. . .) to do. Maybe one understands "something" about the system, but at the very least there is something fundamental about the system one clearly does not understand. In such a case, one can perhaps say (further) that the level of "mathematical modeling" has not been achieved; to model something mathematically means the behavior of the system can be predicted to within the accuracy of measurement. This still does not mean the system is fully understood, but it means that deeper understanding is likely to only

level of predictive accuracy is if there is a region of systematically low or systematically high prediction. Here both models give systematically low prediction (at least evident over certain ranges).

Now, let us look more closely. If I zoom in near the top of the slinky, corresponding to $x = 0$, then we see the illustration on the left of Figure 3. This is consistent with

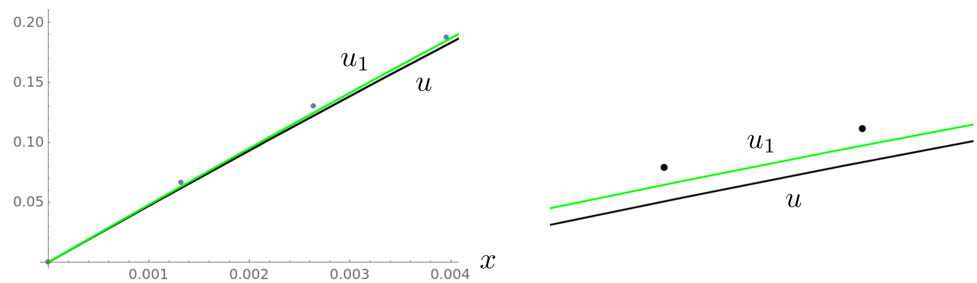

Figure 3: Model behavior closeup: Near the top $x = 0$ (left) and near $x = L_0/2$ (right). The two data points shown on the right are the twenty first and twenty second.

the two main observations above. Similarly, if we consider the model values around $(L_0/2, u(L_0/2))$, then we see behavior consistent with our two main observations. Something interesting happens, however, near the end corresponding to $x = L_0$ as indicated in Figure 4. All the prediction is not low. Both of us predict a value higher than the second to the last data point with corresponds roughly to the first coil before the end coils "bunch" at the bottom. If I have interpreted the data correctly, this corresponds to the thirty-ninth (nominally) measured coil or the seventy-eighth coil out of eight-two coils. In my interpretation of the data, we did not get a measurement for the (nominally) measured thirty-eighth coil either, but this thirty-eighth coil is the first coil counting from the bottom for which there is a noticeable visible gap/space below it and above the next coil, meaning the slope u' should be greater than $u' = 1$. I have plotted the tangent line at the bottom $u(x) = L$ having slope $u' = 1$ in the lower left illustration of Figure 4, and we will discuss what is happening here in greater

come when better measurement can be achieved. It is the remarkable assertion of eighteenth century mathematical modeling that, within the prescribed context of their applicability Laplace's equation, the heat equation and the wave equation appear to be valid mathematical models in this sense. For example, the application of the theory of integration, along with the law of specific heat and Fourier's law of heat conduction give us a model for heat conduction with which there seems to be, at the moment, little fundamental conflict with measurement.

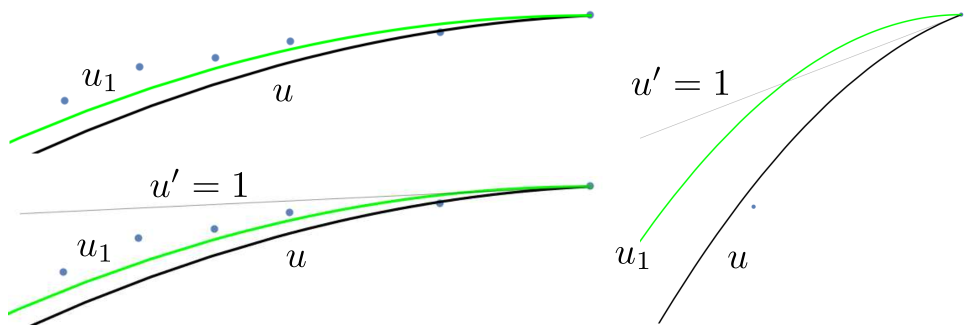

Figure 4: Model behavior closeup near the bottom $x = L_0$.

detail below. For the moment, let me make two observations about the illustration on the right of Figure 4.

(i) Every data point should appear below the tangent line and indeed the tangent line at the following point and (hence) the line of unit slope through the following point. It is clear that not all the measured data points satisfy this condition, but they do all lie below the final tangent line of slope $u' = 1$. The last data point could clearly be low, but notice there is not much possibility that Lila's green curve is closer to the (missing) fortieth data point, which must lie somewhere below the tangent line.

(ii) The green curve runs into a "non-physical" region at the end having slope smaller than $u' = 1$. I mentioned this above, but it is clearly illustrated here.

Happily Lila is not yet done.[4] She intends to both correct for her (high) non-physical region and for the systematic low prediction over the majority of the interval.

[4]It is possibly clear at this point, and I hope it is clear, that we have moved from the topic of Assignment 1 Problem 1 (proper) to the topic of Assignment 4 Problem 1 which is compiling a reasonable set of data. I'm not sure that I have really done that, but my data doesn't seem too bad. At least it seems to be working to illustrate what I think are important points. What I had in mind was simply the qualitative comparison with the expectations outlined in Problem 10 of this assignment below. Roughly speaking, my answer to Assignment 4 Problem 1 at this point would be: The measured data is increasing (expected) and mostly concave (expected) but perhaps not perfectly. The deviance from my expectations, however, is probably within the tolerance of the accuracy of my measurements. I am now going to quote from Lila's work on Problem 1 of Assignment 4, which I would suggest is more properly addressing the content of Assignment 9 Problems 1-4, at least in part.

The former correction, if I am interpreting her exposition correctly, is very interesting. Here is roughly[5] what she says:

> A slinky is a **precompressed** helical spring. **Precompression** implies an **internal compression** force, meaning that the tensile force induced by gravity and the mass of the coils must overcome the internal force, or else there will be no expansion. This is seen in the bottom six coils[6] where the mass of those coils is not great enough to overcome the internal compression force, and the coils remain in their initial unstretched state.

Yes! Great job. This is very clear, and is a solid modeling hypothesis. In fact, I've discussed it with my son Seth, and (for whatever it's worth) he agrees with you (Lila). I, on the other hand, do not think it is correct. Neither I nor Seth could determine a convincing way to either verify or conclusively disprove this hypothesis. Let me start by stating an alternative hypothesis, which I've stated above, and for the moment I am going to stick with. Here I quote myself:

> The fully compressed state of the slinky is a **strict equilibrium** in the sense that the internal compression force Lila postulates is not present. The slinky is specifically neutral with respect to expansion of the coils in the absence of other forces rather than one in which an internal tension holds the coils together.

Under this hypothesis, "precompressed" is taken to simply mean "fully compressed" in the sense that further compression beyond the physical impingement of the coils one upon another is not possible and this "fully compressed" state is the equilibrium state. As (an admittedly somewhat weak) justification for my hypothesis I offer that if you stand the slinky upright on a flat surface and push down, then you can induce anywhere between 0.5 and 1.5 mm of compression, depending on the slinky.

Seth's criticism of my justification is as follows: The fact that the slinky is not actually fully compressed (even under an internal compression force as suggested by Lila) is that there is a slight variation in the actual helical form of the coils so that they simply do not geometrically fit together and impinge upon one another completely and uniformly as you suggest. Even under precompression in the sense of Lila certain isolated points touch and stop the compression. When you push down and see compression, you are "straightening/flattening" the coils so they fit together

[5]Some of the boldface is mine in particular.

[6]In my slinky this apparent phenomenon is only evident for five coils at most, but still…it's an extremely nice observation and modeling hypothesis.

more uniformly. To exaggerate in order to make this explanation clear, imagine that one coil is bent slightly at one point and then bent back in the other direction at a subsequent point. The result is a (small) gap along a portion between two coils which can be compressed by applying a downward force.

This might be correct. On the other hand, were this correct, one might expect some lack of uniformity in the compression, noticeable at the discrete "bends." What one finds, however, is the (small downward) compression against a supporting horizontal surface seems uniform with greater compressibility at the top (as would be explained by gravity) and the phenomenon reverses if the slinky is turned upside down. I would not say this is conclusive either way, and Seth agrees.

Seth asserts/concludes the best evidence for Lila's hypothesis is the apparent bunching of the last few (five or six) coils. I would suggest that the separation may just be small, but not entirely zero. One indicator might be obtained by shining a light behind the last six coils and checking to see if there is a change of intensity of the light passing through the (very small) gaps. My initial attempt at that suggests I am correct and there is a noticeable increase in the light passing between coils four and five from the bottom as compared to between coils one and two.

At any rate, it is nice that Lila has stated her hypothesis clearly, so we/I know what to do with it. Again, if I am interpreting her hypothesis correctly, she is saying there should be an interval at the bottom, just to the left of $x = L_0$ corresponding to five or six coils where $u' \equiv 1$. This is indicated in Figure 5. If things stayed

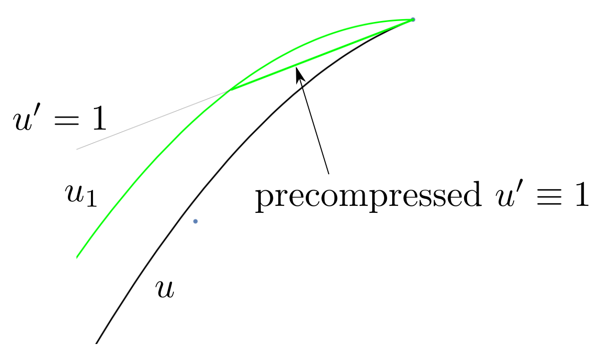

Figure 5: Lila's "precompressed" region to the left of $x = L_0$.

as in Figure 5 this would also take care of Lila's non-physical region in which her deformation function has $0 \le u' < 1$. Unfortunately, if I am reading correctly, she wishes to reintroduce this evident problem. She's going to renormalize the end value determining u_1 by replacing the relation $u_1(L_0) = L$ with $u_1(L_0 - \delta) = L_1$ for some appropriate $\delta > 0$ and $L_1 < L$ corresponding to a data point $(L_0 - \delta, L_1)$ at or near the transition to the precompressed region. I will try to obtain something like what Lila has submitted for Assignment 4 Problem 1. I think I'm pretty successful, and the overall result in illustrated in Figure 6.

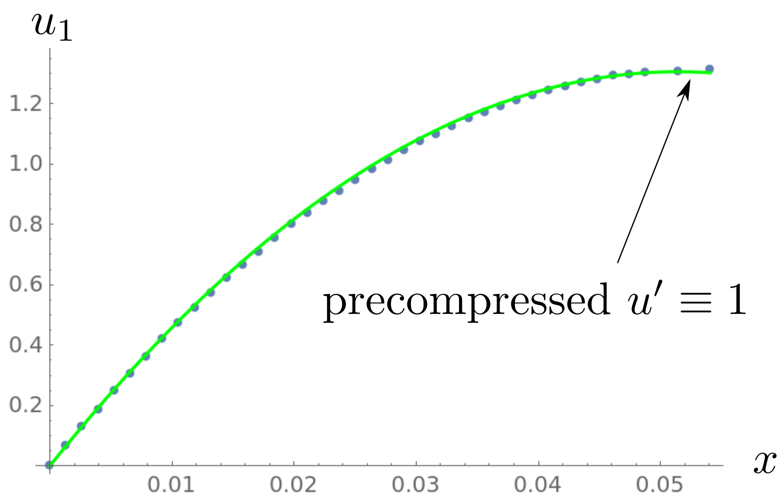

Figure 6: Lila's modified model based on her precompression hypothesis.

Finally, the intrepid mathematical modeler Lila concludes:

> Adoption of the threshold gives the graph above (Figure 6) which matches the measured data points dead on.

Remembering what I wrote above: A mathematical model can rarely if ever be "dead on."[7] The best one can hope for is to match the data as well as it can be measured—without evident systematic model failure. But I ask: Does Lila even have that here? Let's look closely: In Figures 7 and 8 I've illustrated what one sees zooming in, as usual, at the top, middle, and bottom of the deformation.

Rather than "dead on," I would make the following observations:

[7]The only time this happens is if you are modeling something discrete, and in that case, it is not clear you are doing mathematical modeling at all (at least in the sense of eighteenth/nineteenth century "classical" mathematical modeling in the tradition of Newton, Laplace, Fourier, and d'Alembert).

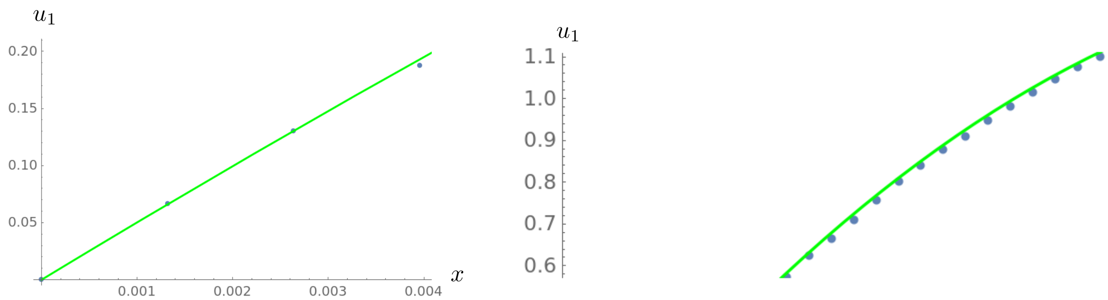

Figure 7: Closeup of Lila's modified model based on her precompression hypothesis near $x = 0$ (left) and in a large middle region (right).

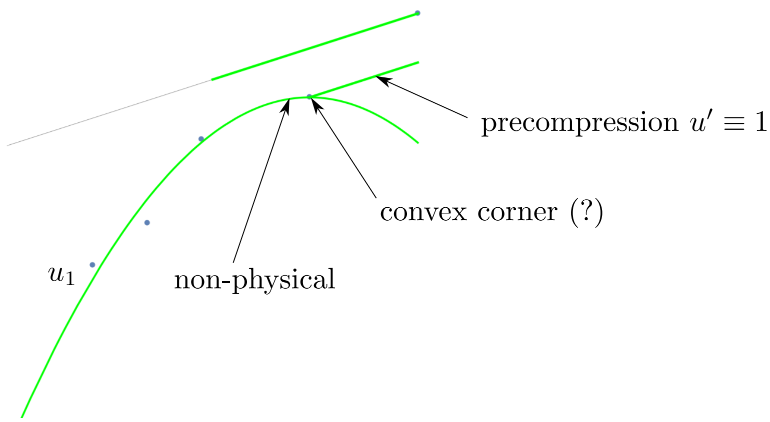

Figure 8: Closeup of Lila's modified model based on her precompression hypothesis near the end $x = L_0$.

1. Near the top end of the hanging slinky, the model deformation function seems to predict the position of the first few (two or three, maybe four) measured data points within the accuracy of the measurements.

2. The predicted values in the large middle region are systematically higher than the measured values indicating the model fails to capture the basic physical properties of the hanging slinky.

3. The predicted values near the end are quite interesting. It is not clear how Lila intends to "normalize" the precompression region or equivalently extend her

quadratic model function into the precompression region. One might guess it is natural for this function to be continuous, and there is a relatively identifiable "bunching" region to mark the transition with a measurement. I have drawn lines of slope one through the last data point and the preceding data point (which represents pretty well the transition position). Either way, it seems to me this model (likely) contains a small interval where $u_1' < 1$ which is non-physical. Also, one possibility for the extension leads to a continuous solution which is not concave, but has a convex corner at the transition. The model solution in this case is continuous but not differentiable, which is sort of interesting though I guess not fundamentally correct nor capturing anything fundamental about the physical system. Perhaps determining a point at which the slope satisfies $u_1 = 1$ and extending with $u' \equiv 1$ after that point eliminates the non-concavity and the non-physical region. Such a model solution would satisfy $u_1 \in C^1[0, L_0]\backslash C^2(0, L_0)$. This seems to be a more likely/acceptable modeling prediction though I think there is still a small problem in that this leads to a solution in which the precompression region extends noticeably outside the physically measured precompression region.

Note: An alternative would be to start with my elastic modulus model, which has final point with $u'(L_0) = 1$. Adapt that to Lila's precompression hypothesis and match the precompression region with the one physically measured. I'll guess this very well may lead to a model which gives predictive accuracy to the tolerance with which our measurements are currently made.

Final comments:

A I'll tell you a secret: Once upon a time, I had some pretty good slinky data and, though I did not have or use anything like Lila's precompression hypothesis, I fitted all the data to an arbitrary quadratic function using a least squares fit. When I did this, there was still observable systematic error in the model. I concluded that the shape of the deformation was fundamentally non-quadratic. I will admit that I may not have been able to make this conclusion with the employment of Lila's precompression hypothesis. It may depend to a certain extent on whether what I suggest in the note above is correct or not.

B The approach using the elastic modulus suggests an alternative modeling hypothesis (quite different from Lila's precompression hypothesis). This alternative hypothesis does lead to a deformation model which matches the measured data (at all points corresponding to the interval $0 \leq x \leq L_0$ with no precompression hypothesis) within the tolerance of the measurements of the slinky data

I had on hand. There were other physically measurable quantities that were consistent with, and to a certain extend "confirmed," this alternative modeling approach—which I have not presented to you yet with the hope that you might come up with it on your own. This model, in particular, gives a single ODE solution which does not have quadratic solutions.

C What I have just discussed in comment **B** above is what I consider the fundamental strength, or one fundamental strength among several others, of the elastic modulus approach to modeling the slinky. More precisely, when you finish with the "Hooke's law" approach to obtaining a quadratic model measurement function, you have (as far as I can tell) no clue about how to get a different ODE. Of course, you can invoke something like Lila's precompression hypothesis and maybe even come up with a solid mathematical model, though I don't think we are quite there yet. But with the elastic modulus approach a natural generalized hypothesis for the global constitutive relation is almost obvious.

Problem 2 (convexity) Find a convex function $f : (-2, 1) \to \mathbb{R}$ satisfying

$$\lim_{x \searrow -2} f(x) = \lim_{x \nearrow 1} f(x) = +\infty.$$

A function $f : (a, b) \to \mathbb{R}$ is **convex** if the inequality

$$f((1-t)x_1 + tx_2) \leq (1-t)f(x_1) + tf(x_2) \tag{8}$$

holds whenever $x_1, x_2 \in (a, b)$ and $0 \leq t \leq 1$.

Problem 3 Draw a picture of the **graph**

$$\{(x, f(x)) : x \in (a, b)\}$$

of a convex function $f : (a, b) \to \mathbb{R}$ illustrating the condition (8).

Problem 4 Is it possible to find an example of a convex function $f : (a, b) \to \mathbb{R}$ that is discontinuous?

Problem 5 Use your example from Problem 2 above to illustrate the value of the **difference quotient**

$$\frac{f(x+h) - f(x)}{h}$$

with $x = 0$ and increment $h = -1$. (Hint: Start your illustration by drawing the graph of f.)

Problem 6 Show the derivative of the **absolute value function** $g : (-\infty, \infty) \to \mathbb{R}$ by

$$g(x) = \begin{cases} -x, & x \leq 0 \\ x, & x \geq 0 \end{cases}$$

is not well-defined at $x = 0$. Hint: Show the limit of the difference quotient does not exist as follows:

(a) Assume by way of contradiction that there exists a limit $L \in \mathbb{R}$ for which

$$\lim_{h \to 0} \frac{g(0+h) - g(0)}{h} = \lim_{h \to 0} \frac{|h|}{h} = L.$$

(b) Conclude there is some $\delta > 0$ for which

$$\left| \frac{|h|}{h} - L \right| < 1 \qquad \text{when} \qquad |h| < \delta.$$

(c) Get a contradiction by finding increments h_1 and h_2 satisfying $|h_j| < \delta$ for $j = 1, 2$ and

$$\left| \frac{g(0+h_2) - g(0)}{h_2} - \frac{g(0+h_1) - g(0)}{h_1} \right| \geq 2.$$

Hint hint: Use the triangle inequality.

Problem 7 Compute the **left derivative** $\delta : (-\infty, \infty) \to \mathbb{R}$ given by

$$\delta(x) = \lim_{h \searrow 0} \frac{h(x+h) - h(x)}{h}$$

of the Heaviside function $h : (-\infty, \infty) \to \mathbb{R}$ by

$$h(x) = \begin{cases} 0, & x < 0 \\ 1, & x \geq 0. \end{cases}$$

Note, δ is not a real valued function but rather an **extended real valued function** taking values in the **extended real line**, that is $\delta : (-\infty, \infty) \to \mathbb{R} \cup \{\pm\infty\}$.

Problem 8 Recall (or note) the following two definitions: A function $f : (a, b) \to \mathbb{R}$ is **continuous at a point** $x \in (a, b)$ if for any $\epsilon > 0$, there is some $\delta > 0$ such that

$$|f(\xi) - f(x)| < \epsilon \qquad \text{whenever} \qquad |\xi - x| < \delta.$$

A function $f : (a, b) \to \mathbb{R}$ is said to be **continuous on the interval** (a, b) if f is continuous at every point $x \in (a, b)$. In this case we write $f \in C^0(a, b)$. ($C^0(a, b)$ is the set of all real valued functions which are continuous on the interval (a, b).)

The Heaviside function h is continuous at every point $x \in (-\infty, 0) \cup (0, \infty)$, so we could write $h \in C^0((-\infty, 0) \cup (0, \infty))$, but $h \notin C^0(\mathbb{R})$.

Draw the graph of $\sigma : (-\infty, \infty) \to \mathbb{R}$ by

$$\sigma(x) = \begin{cases} x \sin(1/x), & x \neq 0 \\ 0, & x = 0 \end{cases}$$

and show $\sigma \in C^0(\mathbb{R})$.

Problem 9 Recall the following three definitions: A function $f : (a, b) \to \mathbb{R}$ is **differentiable at a point** $x \in (a, b)$ if the limit

$$\lim_{h \to 0} \frac{f(x+h) - f(x)}{h}$$

exists (as a finite real number). A function $f : (a, b) \to \mathbb{R}$ is said to be **differentiable on the interval** (a, b) if f is differentiable at every point $x \in (a, b)$. In this case $f' : (a, b) \to \mathbb{R}$ by

$$f'(x) = \lim_{h \to 0} \frac{f(x + h) - f(x)}{h}$$

is a well-defined function.

Given a function $f : (a, b) \to \mathbb{R}$ which is differentiable on the interval (a, b), we say f is **continuously differentiable** if $f' \in C^0(a, b)$. In this case, we write $f \in C^1(a, b)$.

Find an example of a function $f : (a, b) \to \mathbb{R}$ which is differentiable on the interval (a, b), but is **not** continuously differentiable.

Problem 10 Recall (or note) the following two definitions: A function $f : (a, b) \to \mathbb{R}$ is **increasing** if

$$f(x_2) > f(x_1) \qquad \text{whenever} \qquad a < x_1 < x_2 < b.$$

A function $f : (a, b) \to \mathbb{R}$ is **decreasing** if

$$f(x_2) < f(x_1) \qquad \text{whenever} \qquad a < x_1 < x_2 < b.$$

A function which is increasing or decreasing is said to be (strictly) **monotone**.

Think about the function $f : (a, b) \to \mathbb{R}$ you suggested in Problem 1 above (proposed to be part of modeling a hanging slinky). We have a number of definitions in this assignment concerning continuity, differentiability, monotonicity, and convexity. If your function f turns out to have values which may be reasonably compared to measurements taken from the hanging slinky, what properties do you expect the model measurement function to have (in terms of continuity, differentiability, monotonicity, and convexity)? Also include a discussion of the expected **boundary values** $f(a)$ and $f(b)$. Tell me anything you think should be true about a reasonable model function, i.e., your model function, which you can assert (or think you can assert) without actually making any measurements.

Note: I am planning to give you my "solutions" to Problems 1 and 10 above in about three weeks. (Then we can compare notes carefully.) Of course, you are free to discuss your thoughts about these problems with me and each other in the mean time.

. . . three weeks later

A solution of Problem 10: My intuition says, first of all, that the value of the rate of change $u'(x)$ of the extension function u at a given value x should be an increasing function of the mass of the spring located below a certain distance x from the top. More mass should correspond to greater stretching and greater tension force in the spring/slinky. Since the mass below $-x$ (in equilibrium position) which is the same as the mass modeled as below $-u(x)$ in the extended configuration, should decrease with x, this means we should have u' decreasing or $u'' < 0$. That is $-u$ should be convex.

Backing up a bit, u itself should be monotone increasing with, as mentioned above $u(0) = 0$ and $u(L_0) = L >> L_0$. There appears to me no reason to believe u is not continuously differentiable (at least two or three times). In fact, I see no particular reason not to assume (or at least guess) $u \in C^\infty(0, L_0) \cap C^1[0, L_0]$. The regularity at the endpoints is not clear to me. Probably, in fact, $u \in C^\omega(0, L_0)$, though I anticipate that we do not need to assume more than four derivatives, $u \in C^4(0, L_0)$ for the modeling.

At this point it is natural to make some preliminary and elementary observations about the peculiar elastic properties of the physical slinky system considered as a spring and in comparison with the modeling of linear harmonic oscillators—generally considered as springs—in for example a course on ODEs. Modeling of linear harmonic oscillators usually assumes an equilibrium position for one end $x = 0$ of the spring/oscillator and a resulting force due to extension or compression with changing sign according to Hooke's law

$$F = -kx$$

where k is a positive constant (Hooke's constant) having dimensions force per length. It will be immediately observed that the force distribution associated with the slinky is very different. One can identify the equilibrium configuration (completely compressed) as having zero force (or at least being associated with zero force in some manner), but essentially no compression is possible in this configuration. The coils of the slinky contact one another leading to the interesting "constraint"

$$u(x) \geq x \tag{9}$$

for any model extension function $u = u(x)$ admitting a reasonable comparison to an observed physical configuration of the slinky. In fact, most springs have a fully compressed position with the coils physically contacting one another and beyond which the spring is naturally considered incompressible or at least insignificantly further compressible. However, this configuration first of all usually corresponds to a state of compression associated with a positive (compression) force and, second, is usually excluded from the modeling considerations associated with linear oscillators and Hooke's law in particular. For the modeling of the slinky, this extreme case is not only relatively nearby the configurations one might wish to model (in low gravity for example) but also corresponds itself to the zero gravity (zero force) equilibrium, which seems of evident interest. A more subtle and profound departure from Hooke's law will be encountered later, but immediately we can obtain something analytically meaningful and interesting by applying the basic constraint (9) on the small scale of a difference quotient. Notice that given a difference quotient

$$\frac{u(x+h)-u(x)}{h}$$

corresponding to a positive increment h, one has from (9)

$$u'(x) \geq \lim_{h \searrow 0} \frac{u(x+h)-u(x)}{h} \geq \frac{(x+h)-x}{h} = 1.$$

Further reflection strongly suggests that not only does every reasonable model extension $u : [0, L_0] \to [0, L]$ satisfy

$$u'(x) \geq 1$$

but that a model force in a given deformation of the slinky (associated with extension) should be monotonically related to the amount $u'(x)$ exceeds 1, that is the **local force** should be non-constant and given as a monotone function of the quantity $u'(x) - 1$. This is strikingly different from the modeling of springs as linear harmonic oscillators where the expansion or compression is assumed to be uniform corresponding to a constant tension or compression force along the length of the spring given by Hooke's law.

Having made these general observations, let me turn to the physical extension of the slinky under the influence of gravity and how I might expect the measurements and observations of that physical system to be reflected in a reasonable model using the extension function suggested in my solution of Problem 1. The maximum vertical deformation/stretching should occur at $x = 0$ corresponding to $u'(0) \geq u'(x)$ for $x \in [0, L_0]$ and satisfying $u'(0) > 1$. Again, assuming u' is an increasing function of the mass suspended below $-x$, we expect u' is decreasing in x and since no mass (presumably corresponding to no stretching) is below $z = -u(L_0) = -L$, I expect

$$u'(L_0) = 1.$$

More generally, I expect $u' : [0, L_0] \to [1, u'(0)]$ is a monotone decreasing function, and we return then to the initial expectation

$$u''(x) < 0 \qquad \text{at least for } 0 < x < L_0$$

as mentioned above. Also, in this regard, the **normalized extension function** $v = u - u_0$ may be of interest.

Exercise 2 Start with a function $u'' : [0, L_0] \to [\alpha, \beta]$ where α and β are simply some negative numbers you have chosen. Integrate up from u'' to obtain functions u' and u satisfying boundary conditions compatible with those suggested above. Plot the functions u and u' and the normalized extension function $v = u - u_0$ corresponding to your choice of u''. Plot corresponding geometric core curves corresponding to (2) and something like the illustrations in Figure 1 using the extension function u you have obtained.

I think the discussion above captures the main qualitative features I imagine the model function u to display, presumably in accord with the measurements. I mention in closing that the boundary values are interesting in that one $u(0) = 0$ is more or less a given, one $u'(L_0) = 1$ is, or can be, theoretically motivated apart from direct measurement while the other two $u'(0)$ and $u(L_0) = L$ should be expected to be predicted from the model, but have values that are (at least initially) entirely non-obvious without a full model and also of prime interest to compare to measurements. These ($u'(0)$ and $u(L_0) = L$) are, aside from the measurements of all the extension heights/lengths, the main quantitative model values of interest (to me, YMMV).