The Ostrogradskian Instability of 
Lagrangians with Nonlocality of Finite Extent

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ABSTRACT

I reply to the objections recently raised by J. Llosa to my constructive proof 
that Lagrangians with nonlocality of finite extent inherit the full Ostrograd-
skian instability.

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1 Introduction

Consider a nonlocal Lagrangian \( L[q](t) \) whose dynamical variable is \( q(s) \). I define the nonlocality to be of finite extent \( \Delta t \) if \( L[q](t) \) definitely depends upon (and mixes) \( q(t) \) and \( q(t + \Delta t) \), and potentially depends as well upon \( q(t') \) for \( t < t' < t + \Delta t \). Two years ago I constructed a Hamiltonian formalism for such systems [1]. (One should note the very similar earlier work by Llosa and Vives [2].) I also showed that this Hamiltonian could be obtained as the infinite derivative limit of the well-known construction of Ostrogradski [3] for nondegenerate higher derivative Lagrangians.

Ostrogradski’s construction [3] exhibits a virulent instability which grows worse as the number of higher derivatives increases. If the Lagrangian depends nondegenerately upon \( N \) time derivatives then there must be \( N \) canonical coordinates and \( N \) conjugate momenta, and the Hamiltonian is linear in \( N - 1 \) of the momenta. Such a Hamiltonian can never be bounded below. This is what I mean by the “Ostrogradskian instability”. The result is completely nonperturbative. Further, it should not be altered by quantum effects because the problem derives from a huge volume of the classical phase space. The fact that Lagrangians with nonlocality of finite extent can be viewed as the infinite derivative limit of higher derivative Lagrangians means that one can forget about any phenomenological applications for them. I illustrated the problem by explicitly solving a simple free theory of this type and demonstrating the existence of an infinite number of runaway solutions.

A recent comment by Llosa [4] disputes my negative assessment. His three criticisms are laid out in sections II, III and IV of his comment. Because of the continuing interest in nonlocal Lagrangians [5, 6, 7, 8, 9, 10, 11, 12] these replies to his comments may be of interest. This work does not pretend to be self-contained. One should read it in the context of my paper [1] and Llosa’s comment [4]. Each of my sections deals with the corresponding section of Llosa’s comment. Because the equation numbers have changed between his first draft and the version subsequently accepted for publication I will parenthesize the new numbers and bracket the old ones.
2 Reply to the comments of Section II

Llosa’s first objection mentions “defects . . . concerning the application of the variational principle”. This is a curious criticism because my paper did not state, nor did it apply any variational principle. I began with the nonlocal Euler-Lagrange equation and worked from there.

Of course there is a variational principle behind my equation of motion. It is not based on the action given in Llosa’s equation (4) \{4\} but rather upon the form which comes from integrating the Lagrangian between arbitrary fixed times as usual,

\[ S[q] = \int_{t_1}^{t_2} ds L[q](s). \]

The equation of motion is the variational derivative of the action at any time \( t \) outside the influence of the initial and final surfaces, that is,

\[ t_1 + \Delta t < t < t_2 - \Delta t. \]

Consider the nonlocal harmonic oscillator discussed in my paper,

\[ L[q](t) = \frac{1}{2} m \dot{q}^2(t + \Delta t/2) - \frac{1}{2} m \omega^2 q(t)q(t + \Delta t). \]

For \( t \) in the range \( \mathbb{P} \) one does not encounter surface terms from partial integration and the action’s functional derivative gives,

\[ \frac{\delta S[q]}{\delta q(t)} = m \int_{t_1}^{t_2} ds \left\{ \dot{q}(s + \Delta t/2)\delta'(s + \Delta t/2 - t) - \frac{1}{2} \omega^2 \delta(s - t)q(s + \Delta t) \right\} , \]

\[ = -m \left\{ \dot{q}(t) + \frac{1}{2} \omega^2 q(t + \Delta t) + \frac{1}{2} \omega^2 q(t - \Delta t) \right\}. \]

The general result is the right hand side of equation (2) in my paper \[\mathbb{P}\]. It derives from the fact that an arbitrary Lagrangian \( L[q](s) \) with nonlocality of finite extent \( \Delta t \) is defined to depend upon \( q(t) \) only for \( s \leq t \leq s + \Delta t \). The result is therefore unchanged if we restrict \( q(t) \) only for \( s \leq t \leq s + \Delta t \).

Now make the change of variables \( r = t - s \),

\[ \frac{\delta S[q]}{\delta q(t)} \left. \right|_{t_1}^{t_2} = \int_{t_1}^{t_2} ds \frac{\delta L[q](s)}{\delta q(t)} = \int_0^{\Delta t} dr \frac{\delta L[q](t - r)}{\delta q(t)} = 0. \]
In the original version of his comment Llosa maintained that the equation of motion should be defined as the full variational derivative of the action, including surface terms. He has since altered this to conform to the usual condition, for first order Lagrangians, of demanding that the variation vanish at the initial and final times. However, it is instructive to see what goes wrong when the original condition is applied. Consider the action integral for the simple harmonic oscillator,

\[
S[q] = \frac{1}{2} m \int_{t_1}^{t_2} ds \left\{ \dot{q}^2(s) - \omega^2 q^2(s) \right\} .
\]  

(7)

Its full variational derivative is,

\[
\frac{\delta S[q]}{\delta q(t)} = -m \{ \ddot{q}(t) + \omega^2 q(t) \} + m\dot{q}(t_2)\delta(t_2 - t) - m\dot{q}(t_1)\delta(t_1 - t) = 0 .
\]  

(8)

The surface variations enforce the conditions \( \dot{q}(t_1) = 0 = \dot{q}(t_2) \). Unless \( \omega(t_2 - t_1) \) happens to be an integer multiple of \( \pi \) the only solution is \( q(t) = 0 \).

Most physicists regard the harmonic oscillator’s solution space as two-dimensional, characterized by the initial position and velocity. If one instead demands the full variational derivative to vanish then the solution space is either zero or one dimensional, depending upon whether or not \( \omega(t_2 - t_1) \) is an integer multiple of \( \pi \). The distinction between zero and one does not derive from any property of the system’s dynamics but rather from the completely arbitrary choice of the range of temporal integration in what we call the action. Further, it is difficult to reconcile either dimensionality with one’s experience — with swings or masses on springs — that an oscillator can be released with a very wide range of initial positions and initial velocities. Hence we conclude that action principles should be formulated so as to determine evolution from an arbitrary initial condition, not to fix initial and final conditions.

The standard procedure for Lagrangians involving the variable and its first derivative is to demand stationarity with respect to variations which vanish at the initial and final point. This is not done because there is anything special about the initial and final times, or about the zeroth derivative of the variation, but rather because it suffices to null the surface terms coming from partial integration. Achieving the same end — the absence of surface contributions — requires more restricted variations as one adds higher derivatives. For each higher derivative in the Lagrangian one more derivative of
the variation is required to vanish. Extending these considerations leads one to demand stationarity with respect to variations which vanish outside range \( \mathcal{E} \). Thus my action principle is not "nonstandard" but rather a straightforward generalization of the usual procedure for freeing what one calls, "the Euler-Lagrange equations" from surface terms.

Of course one can always attempt to constrain an unstable theory so as to achieve a stable one, and Llosa's initial and final conditions can be viewed in this light. The big problem then is to make the constraints consistent with time evolution. This is trivial for theories with linear equations of motion but it is very difficult when nonlinearities are present, as they must be present for any sector of the universe which we can observe. In field theory one must also check that the constraints preserve causality and Poincaré invariance. It is so difficult to reconcile these three conditions for interacting field theories which suffer the Ostrogradskian instability that no one has ever succeeded in doing it. In any case I do not assert that no such constraints exist, only that the unconstrained theory is unstable.

Finally, one should note that the final argument in the revised version of Llosa's Section II is incorrect. The argument purports to show that there is a potential problem of consistency in specifying \( q(t) \) for the ranges \( t_1 \leq t < t_1 + \Delta t \) and \( t_2 - \Delta t < t \leq t_2 \). The argument consists of using the equation of motion (without surface terms) to determine \( q(t) \) in the latter region in terms of \( q(t) \) in the former region. This is false. The equations of motion actually determine \( q(t) \) in terms of its values in the region \( t_1 - \Delta t \leq t < t_1 + \Delta t \), not \( t_1 \leq t < t_1 + \Delta t \). Hence the range of times is \( 2\Delta t \), with either initial and final conditions or purely initial conditions. In this regard the nonlocal system is analogous to that of a first order Lagrangian for which one can specify either \( q(t_1) \) and \( q(t_2) \) or else \( q(t_1) \) and \( \dot{q}(t_1) \). Of course the fully initial value formulation is vastly preferable, both because it avoids the possibility of ambiguous situations such as \( \omega(t_2 - t_1) = \pi N \) in the harmonic oscillator, and because it corresponds to our sense of being able to prepare systems and then observe their time evolution. This is one more reason for not including surface variations in the action principle.
3 Reply to the comments of Section III

Section III purports to demonstrate that the nonlocalized harmonic oscillator (3) is actually stable. The problem with Llosa’s argument is that it assumes the conclusion. The general solution consists, as he states, of a superposition of the form,

$$q(t) = \sum_\ell \left( A_\ell e^{ik_\ell t} + A_\ell^* e^{-ik_\ell t} \right),$$

(9)

where the $k_\ell$’s obey the equation,

$$k^2 = \omega^2 \cos(k\Delta t).$$

(10)

As was proved in my paper [1], there is a countably infinite class of conjugate pairs of solutions with nonzero imaginary parts. This proves the existence of exponentially growing modes, which are a manifestation (although not an essential one) of instability. Llosa discards these solutions by imposing the condition that $q(t)$ and its first two derivatives are bounded functions of $t$. (By the way, why not more derivatives, or fewer?) It is not valid to check stability by first ruling out the existence of exponentially growing modes.

Suppose this same restriction were applied to the Lagrangian of a point particle moving on a concave parabolic hill of infinite extent,

$$L = \frac{1}{2}m\dot{q}^2 + \frac{1}{2}m\omega^2 q^2(t).$$

(11)

The equation of motion is,

$$\ddot{q}(t) - \omega^2 q(t) = 0$$

(12)

and the general initial value solution is,

$$q(t) = q_0 \cosh(\omega t) + \frac{1}{\omega} \dot{q}_0 \sinh(\omega t).$$

(13)

Most physicists regard this system as unstable, and the exponentially growing solutions as proof of the instability. But Llosa’s criterion of boundedness would require $q_0 = 0 = \dot{q}_0$, and his argument could be invoked to pronounce the system “stable”.

Another problem with Section III, which recurs in Section IV.A, is Llosa’s definition of stability. The Ostrogradskian instability is that the energy functional is unbounded below over essentially half of the classical phase space.
Again, I believe most physicists would regard this as evidence of instability. Llosa instead defines stability as the boundedness of deviations under variation of the initial conditions. For a conventionally interacting field theory the two definitions typically agree. However, when interactions are turned off it is possible to have negative energy modes which do not violate Llosa’s criterion.

The simplest example is obtained by reversing the sign of the harmonic oscillator Lagrangian,

\[ L = -\frac{1}{2}m\dot{q}^2 + \frac{1}{2}m\omega^2q^2. \]  

(14)

Of course this does not alter the general initial value solution,

\[ q(t) = q_0\cos(\omega t) + \frac{1}{\omega}\dot{q}_0\sin(\omega t), \]

(15)

so this system is “stable” in Llosa’s sense. It is also completely unobservable because it fails to interact with anything else in the universe. Suppose we couple it to electromagnetism. What happens is obvious: the oscillator jumps down to its first excited state, and the energy difference comes off as a photon. This process is overwhelmingly likely to go because there is only one state with the oscillator unexcited and no photons, versus an infinite number of directions for the photon to take. Nor do things stop there. It is just as favorable for the oscillator to jump to its second excited state and emit another photon. In fact the oscillator will continue jumping to lower and lower energies, without bound. That is why it makes sense to use the energy definition of instability, and why the application of Llosa’s criterion to a noninteracting system gives a misleading answer.

4 Reply to the comments of Section IV

Llosa’s Lagrangian (25) \{16\}, given in Section IV.B, is an example of non-locality of finite extent. However, the associated equation of motion is not Llosa’s equation (28) \{19\} but rather,

\[
\int_0^T dr \delta L[q](t - r) \frac{\delta L[q]}{\delta q(t)} = -\ddot{q}(t) - \omega^2 q(t) + \frac{1}{2}\omega^4 \int_0^T dt' G(t, t') q(t') \\
+ \frac{1}{2}\omega^4 \int_{t-T}^t ds G(s, t) q(s) = 0. \]

(16)
Note that acting \((d/dt)^2 + \omega^2\) does not result in a local equation on account of the final term, which is absent in Llosa’s equation of motion. Hence Llosa’s further comments concerning his equation of motion are not relevant to the stability of systems with nonlocality of finite extent.

Although Llosa’s equation (28) \{19\} is not the variation (in the standard sense) of his Lagrangian (25) \{16\}, it does have an interesting interpretation. It is what comes from integrating out the variable \(x(t)\) in the following local, first derivative system,

\[
\ddot{q}(t) + \omega^2 q(t) = \omega^2 x(t), \quad (17)
\]
\[
\ddot{x}(t) + \omega^2 x(t) = \omega^2 q(t), \quad (18)
\]

subject to the conditions \(x(0) = 0 = x(T)\). It is these two conditions which result in the original 4-dimensional space of solutions degenerating to the 2-dimensional space that Llosa finds. Of course there is no problem with the stability of this system, nor does my paper assert otherwise. In fact the first page of my paper states, \textit{... the higher derivative representation is certainly not valid for the inverse differential operators which result from integrating out a local field variable.}

5 Discussion

Most of Llosa’s comments have little to do with my work on nonlocality. What they challenge instead is the view that the Ostrogradskian instability is a problem for nondegenerate higher derivative Lagrangians. The propensity to raise such a challenge deserves comment in its own right.

It has long seemed to me that the Ostrogradskian instability is the most powerful, and the least recognized, fundamental restriction upon Lagrangian field theory. It rules out far more candidate Lagrangians than any symmetry principle. Theoretical physicists dislike being told they cannot do something and such a bald no-go theorem provokes them to envisage tortuous evasions. I do not believe Llosa has discovered any way around the problem, but it is impossible to demonstrate that none exists. I do urge the application of common sense. The Ostrogradskian instability should not seem surprising. It explains why every single system we have so far observed seems to be described, on the fundamental level, by a local Lagrangian containing no
higher than first time derivatives. The bizarre and incredible thing would be if this fact was simply an accident.

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