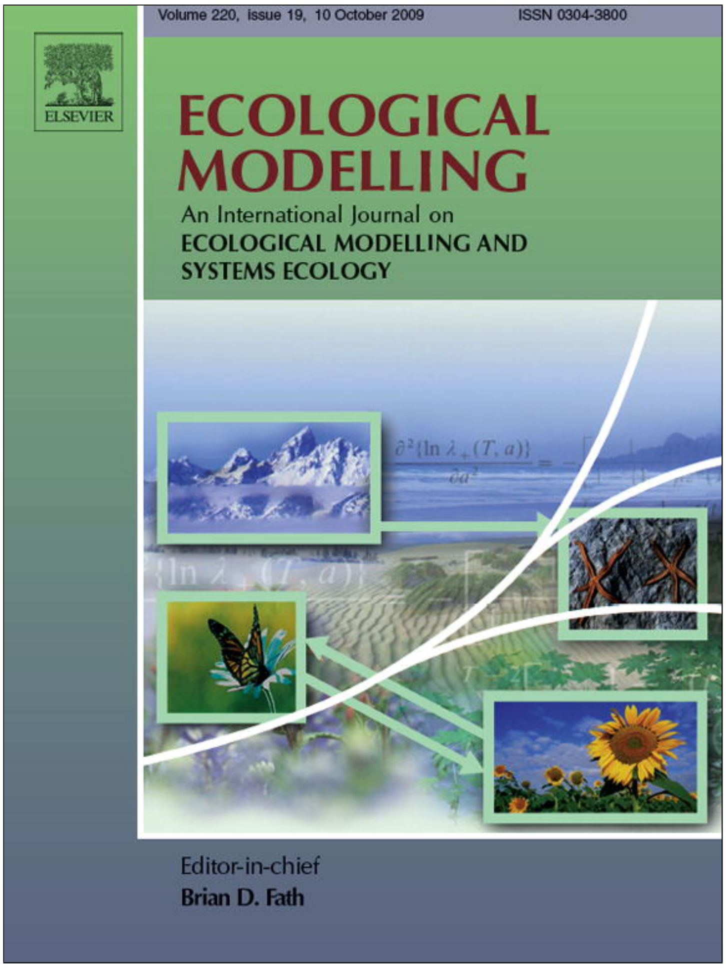
Volume 220, issue 19, 10 October 2009
ISSN 0304-3800
ELSEVIER
ECOLOGICAL MODELLING
An International Journal on
ECOLOGICAL MODELLING AND
SYSTEMS ECOLOGY
Editor-in-chief
Brian D. Fath

Ecological Modelling 220 (2009) 2600–2601

Contents lists available at ScienceDirect

Ecological Modelling

journal homepage: www.elsevier.com/locate/ecolmodel

Reply to Letter to the Editor

Can discrete modellers work without the TIP?

This is a reply to Dr. L.V. Nedorezov's letter to the Editor regarding my paper on the Time-Invariance Principle (TIP) and discrete modeling (Deng, 2008). My comments below are organized not in the order of Dr. Nedorezov's points made but in the order of their priority I ranked.

One objection I often encountered the first was the claim that the iterative composition identity $F^m(F^n(\cdot)) = F^{m+n}(\cdot)$ satisfies my definition of TIP-conformity because it has the same functional form, $F^k(\cdot)$, for all iterates and as a result my argument against discrete modeling is self-contradicting on the outset. Coupled with the simple fact that the identity is taught to most first year graduate students as the group property characterizing all dynamical systems, my failure of acknowledging the obvious must be a sign of a poor training or a forgetful student, further reinforcing a skeptic's immediate impression. Although Dr. Nedorezov only pointed this out as his second objection to my conclusion, I choose to comment it first since I suspect it is the root to most other doubts and counterarguments. The point I want to make is this, the seemingly TIP-conformity of the iterative composition is only superficial—the conformity does not hold for all maps.

The iterative composition relation is a group property just as TIP-conformity is. The question is, must be the former always a subcase of the latter which requires the composing exponents m and n to represent times? The answer is no. They are thought to represent times in two cases. In one case, such a map F is defined as the Poincare return map of a flow with a fixed time increment τ as in $F(x) = \varphi(\tau,x)$ with $\varphi(\tau,x)$ being the solution to a differential equation with the initial state x. In this case, the composing integers do represent times which progress uniformly and regularly at the fixed but arbitrarily chosen time increment τ. There is little disagreement here. The other case is the point of contention when a skeptic believes the same even for TIP-nonconforming maps. To make my point, I will use the logistic map again below in a thought experiment.

For the argument sake, let us assume the logistic map model a process for which there are two independent observers. Observer 1 derived the degree-2 polynomial to be his model by using every 1 unit time (say 1 day) as his observing interval in time. Independently, Observer 2 deduced the degree-4 polynomial using every 2 days as her time increment. Each believed in their models until they met. Observer 1 said to Observer 2 that if she halved her 2-day time interval her law would be the same as his, to which Observer 2 asked Observer 1 to double his time interval to fit her particular point of view. At this point both realized that there was a problem. Does the process theoretically prevent them from observing it every half day? Not unless they are studying a microscopical process which progresses at the shortest time jump in the fraction of trillionth of trillionth of trillionth of a second. The question is can they use their modeling methodology to build a model that progresses at every half day so that in two iterates it becomes the logistic map? Will such a model be a degree-1 polynomial or a combination of monomials in x^a? The mathematical question is can any map always admit any fraction of integer compositions? Namely, can F^k be extended to all real and positive number k? If not, the iterative composing group is only a superficial conformity to TIP.

The moral of this thought experiment is that if the logistic map is a model for a macroscopic natural process, then its iterative composition must not represent regular time progression. But there are two loop-holes allowed by the TIP. The first is when the iteration represents the occurrence of an event rather than time. For example, for the three-trophic food-chain model studied in Deng (2001, 2006), an event Poincare return map is defined whenever the prey population hits a local maximum. It is a 2-dimensional map and for some parameter values it approximates a unimodal 1-dimensional map, behaving qualitatively the same as the logistic map. For such event return maps, however, the time intervals between two adjacent events are not fixed. For instance, the prey's population may take 1 year to hit the next local maximum or 1.8 years for the next, and so on. So, in order for the map to have a predictive power, we must first know when the defining event (in local maxima of the prey) occurs in time which the map neither encodes nor predicts. The second loop-hole, which was already alluded to above, is the case that the iteration of the logistic map represents the absolute physical limit in time jump which cannot be subdivided further. At this absolute limit, TIP does not rule out the map to be a law, which on the other hand is not the providence of men. The conclusion is: the iteration of a TIP-nonconforming map does not represent time, and if it does, the map must be TIP-conforming in order for it to be a model candidate for some macroscopic process.

To justify the Leslie methodology, Dr. Nedorezov went on to repeat a typical derivation of the Leslie model that one can find in most textbooks. The derivation seems reasonable enough just as a typical derivation of the logistic map is. But it is the-means-justifies-the-end type of argument, habitually ignoring the most wanted justification—the consistency problem of the methodology. Denote by L_n the matrix model for n developmental stages. To be consistent, a practitioner of the art must use the same method to construct, for instance, a L_{2n} model using $2n$ developmental stages with each stage of the L_n model halved into 2 more substages. Can one think of an age-structured macroscopical process for which an independent observer cannot take census at finer incremental ages? The question now becomes, will the two models predict the same outcome? The answer is troubling either ways. If they do, then they will behave like the L_1 model, which is a long-discredited exponential growth, or decay model, which violates the One-Life Rule (Deng, 2008). If not, will L_n converge in some sense as n tends to infinity so that L_n is consistent for all sufficiently large n? Where is

DOI of original article: 10.1016/j.ecolmodel.2009.06.009.

0304-3800/$ – see front matter
doi:10.1016/j.ecolmodel.2009.06.049

such a consistency theorem? The methodology has no foundation without it, and I do not believe in its existence precisely because the Leslie matrixes are not TIP-conforming. The correct alternative has been in place long ago. It is the McKendrick–von Foerster's PDE model (McKendrick, 1926; von Foerster, 1959; Kot, 2001), which is automatically TIP-conforming for being a differential equation model. Dealing with the PDE is as simple as one can hope for to seriously study age-structured processes. Anything simpler is simplistic. The answer to Dr. Nedorezov's questions is: yes, the Leslie matrix is worse than bad, and yes there is a better and mechanistically correct alternative.

This is not to say all discrete models are always TIP-nonconforming. In fact, any process modeled by a Markovian probability matrix is perfectly TIP-conforming since all compositions of a Markovian are themselves Markovian. But an uncritical generalization of the Markovian matrix to the Leslie matrix cannot be justified in the framework of TIP.

I cannot pin point when using this form, $x_{t+n+1} = F(x_{t+n}) = F^n(x_t)$, to represent discrete models was introduced to the literature. To argue that t represents the continuous time is misleading. If it is, why not just write it as $x_t = \phi(t,x_0)$ so that x_0 is the initial state we can all agree upon? The form is an illusion to trick the unsuspected into believing that all time increments are allowed by the model. For the lack of a redeemable alternative I used the misused convention in my paper without pointing out the misusage which I thought should be obvious to most readers.

I now come to Dr. Nedorezov's first point regarding ODEs with impulses. The stand-alone logistic map is a pathological model. It cannot be justified by any means that derives it. If the outcome of a derivation is pathological, then the method is either pathological or does not permit the stand-alone interpretation of the outcome.

Let us first take a look at the interpretations. If indeed the logistic map is derived from an ODE with impulses, then the map can only be an event map, and therefore the iteration does not represent an independent progression in time. Here is why. Using impulses is an *ad hoc* way to model a multi-timescaled system—with the ODE part modeling a slow variable or a slow subprocess and the impulse part modeling a fast variable or a fast subprocess. Such processes can be properly modeled by singularly perturbed differential equations (see Deng, 2006), which are always TIP-conforming. Impulses are usually the result of assuming an instantaneous transition of the fast and the slow subprocesses. However, the slow dynamics does not always take a fixed time to reach the interphase of the transition. If the interphase is defined to be the event when the slow variable reaches some special values, then the resulting map is the so-called outer approximation of the fast-slow system, which is an event return map. At the best, it serves only as a means to study the structure of such fast-slow systems, or a 'model' of a model. For example, the logistic-like unimodal return map mentioned above for the three-trophic food-chain model in Deng (2001) was used as a prop to demonstrate the existence of chaos for the food-chain. The parameter that drives the map through a period-doubling cascade to chaos has exactly the opposite interpretation against the intrinsic-growth-rate interpretation of the logistic map—it corresponds to the death rate of the top-predator so that the higher it is the more chaotic the system becomes, perfectly consistent with the efficiency stabilization principle (Deng, 2006). So, in this case the derivation does not permit the stand-alone parameter interpretation of the logistic map as the intrinsic growth rate.

Next, let us take a look at a pathological derivation in which a modeler arbitrarily initiates the impulse after a predetermined time interval for the slow subprocess, say at a fixed month of every year when all babies are born (rather than at the conception of the offspring, or half way into the gestation), and then proceeds to derive the map, with the iterative reset precisely timed at that specially chosen moment—an event map nonetheless. In any case, there are better alternatives to modeling by impulses. More specifically, beside the multi-timescale modeling approach mentioned above, the elemental biomass transfer from the mothers to their newborn can also be modeled by either a continuous stoichiometric model or by an age-structured PDE model. There is no known consistency theorem for the equivalence of all alternative approaches. In the very likely event that the impulse discrete model will not produce the same results as the alternatives, we must decide which model to choose. Should it be the more mechanistically constructed alternatives based on the mass balance laws which always satisfy TIP, or the event-predicated impulse model which may or may not be TIP-conforming? From a purely theoretical stand, the choice is obvious.

Dr. Nedorezov's characterizations about the Abstract and Introduction of my paper are too abstract to rebut. I can only reply in kind by restating that their logical foundation is perfectly sound.

I want to close my reply with a few comments which were not made in my previous paper. In field ecology, students are taught to randomize their samples against unintended biases. This protocol is diligently followed in all aspects but less so regarding time series. There is nothing against collecting data on a regular time interval, say the first month of every spring. But to mitigate biases, it is more important to collect data at random moments in time. Hence a basic requirement for a theoretical model is to fit or at least attempt to fit data collected at all arbitrary times. This consideration alone points to continuous models as the qualified candidates to model most if not all ecological processes. The second comment, because TIP is beyond reproach, the burden of proof lies with the advocates of discrete modeling to show why we should adopt discrete models when for every one of which there is always a continuous alternative which is not only TIP-conforming but also without time gap for data fitting. In anticipating a tired counterargument that linear interpolation can fill such time gaps for discrete models, I want to point out that the theoretical foundation for this practice lies in a proof, which I cannot imagine its existence, that a discrete modeler's choice in discrete times will not miss any local maxima or local minima of the modeled process. Any misses will render the interpolation meaningless. The third, it is not an opinion but rather a mathematical proof found in my paper that a TIP-nonconforming map cannot be verified by independent experiments to be a law or a model for any macroscopic natural process. Finally, part of theoretical ecology has been on the wrong track of discrete modeling, and TIP can now put a stop to it, notwithstanding Dr. Nedorezov's legacy argument that no one has suspected anything wrong at its foundation since the time of Fibonacci.

References

Deng, B., 2001. Food chain chaos due to junction-fold point. Chaos 11, 514–525.

Deng, B., 2006. Equilibriumizing all food chain chaos through reproductive efficiency. Chaos 16, 043125–043125-7.

Deng, B., 2008. The Time Invariance Principle, the absence of ecological chaos, and a fundamental pitfall of discrete modeling. Ecological Modelling 215, 287–292.

Kot, M., 2001. Elements of Mathematical Ecology. Cambridge University Press.

McKendrick, A.G., 1926. Applications of mathematics to medical problems. Proceedings of the Edinburgh Mathematical Society 44, 98–130.

von Foerster, H., 1959. Some remarks on changing populations. In: Stohlman, F. (Ed.), The Kinetics of Cell Proliferation. Grune and Stratton, New York, pp. 382–407.

Bo Deng

Department of Mathematics, University of Nebraska-Lincoln, Lincoln, NE 68588, United States

E-mail address: bdeng@math.unl.edu

3 June 2009

Available online 6 August 2009