Response to Oscillations in Population Sizes – From Ecology to History

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Abstract

Natalia Komarova brings up a very important issue in modeling dynamical phenomena—what mathematical framework to use. I respond to her critique.

I wish to thank Natalia Komarova for bringing up a very important issue in modeling dynamical phenomena—what mathematical framework to use. Komarova argues that a delayed logistic model provides a better framework for modeling historical dynamics than the ordinary differential equations used in my paper.

Before addressing the main point of Komarova's critique, however, I need to clarify one other aspect of my paper—the role of internal warfare in the theory of secular cycles. Komarova wonders how "warfare … can become an independent entity, take a life of its own and exert prey-like pressure" on population. First, I want to stress that my model of interaction between population dynamics and socio-political instability in no way is based on an analogy between secular cycles and predator-prey cycles. These are phenomena from completely different disciplines, and I find any cross-parallels between them completely unhelpful, and even potentially misleading (which is probably the source of my critic's puzzlement). True, mathematical models are somewhat similar, but that's the power of mathematics—that formally the same equations can be applied to completely different fields of science by interpreting the variables in appropriate ways. A model for a planet hurtling around the Sun is another example of the same mathematical approach applied to substantively very different phenomena.

Second, "warfare," as used in the model, is not an actor but a description of the state of the social system. It can be measured by the death rate (proportion of people killed per unit of time) due to collective violence (killing where at least one side of a conflict involves groups rather than individuals; this is to distinguish it from homicide). At the micro-level we do not observe "warfare"—we see groups of people fighting and killing each other. Intensity of warfare is meaningful only when we combine statistics for the whole society. This situation is akin to thermodynamics, where the concepts of "temperature" or "pressure" are meaningless when we observe individual molecules bouncing off each other. Temperature and pressure emerge only when we consider a whole ensemble of molecules, such as "ideal gas." This analogy is further developed in my recent book *War and Peace and War* (Turchin 2006). Incidentally, lest I be again misunderstood, I am not implying that people are molecules; the point is that any phenomenon can be viewed at both micro- and macro-levels. We will conceptualize different variables at different levels of organization. Finally, there is nothing particularly startling in putting macro-level variables, such as warfare intensity or gas temperature, into equations. In fact, later in her critique, Komarova herself discusses various processes that could increase or decrease warfare intensity. Other mechanisms that affect the dynamics of warfare intensity—competition for scarce resources, revenge cycles, and war fatigue are discussed in Turchin and Korotayev (2006) and Turchin (2006).

Turning now to the main issue, what is the appropriate mathematical framework for investigating dynamical processes? Here I would like to avoid dogmatism, so I will state from the outset that different frameworks may be used in different circumstances. Dynamicists have used ordinary and partial differential equations, continuous and discrete models, delayed, and stochastic differential equations. Each framework has advantages and drawbacks, and the best approach depends both on the nature of the system and the types of questions that we want to ask (Turchin 2003a).

In my opinion, however, the specific equation that is proposed by Komarova, first used by the ecologist G. Evelyn Hutchinson, is not a particularly helpful model for secular cycles. All models have to strike the balance between enough detail to capture the essential dynamics, but at the same time be simple enough so that we don't become bogged down in unnecessary complexity. As Einstein famously said, models should be as simple as possible, but no simpler.

The dynamics of the delayed logistic model are well understood (Turchin 2003a). The qualitative type of dynamics is controlled by a single parameter combination, $r\tau$. The model has a monotonically damped stable point for $0 < r\tau < e^{-1}$, an oscillatory damped stable point for $e^{-1} < r\tau < \pi/2$, and a stable limit cycle for $r\tau > \pi/2$. Increasing $r\tau$ beyond this point simultaneously lengthens the cycle period and increases the amplitude. In other words, there is a rigid connection between the cycle period and amplitude—an artifact of the model structure, which is not observed in real-world dynamics. Another unpleasant feature of the delayed logistic is that the amplitude of cycles depends very sensitively on $r\tau$. For example, changing $r\tau$ from 2.4 to 2.5 increases the amplitude from approximately 1,000-fold to 3,000 fold.

The main problem with the Hutchinson model, however, is not its mathematical peculiarities, but its vacuousness. All the model says is that if there are lags in regulation then cycles are likely. But we already know this from the theory of dynamical systems, i.e., this is the general mechanism by which oscillations arise in a variety of mathematical frameworks. The question that I pursued in my paper was, what is the social mechanism that causes population oscillations in agrarian societies? In other words, what are the key variable, or variables, that interact with population dynamics to drive cycles? Is it real wage? Disease? Internal warfare? The Hutchinson model sweeps such issues under the rug. It simply says that there is some factor that acts with delay and drives the cycles. It's vacuous, if our purpose is to investigate the mechanistic causes of oscillations. At best, it could provide a phenomenological description of dynamics (and it may fail even at that due to its anomalous mathematical structure).

In general, delayed differential equations should be used where the nature of lags is clearly understood and noncontroversial, such as in population ecology when we deal with a population of organisms characterized by a fixed time period between birth and sexual maturation (not gestation, as stated by Komarova). A delayed differential equation provides a simple framework to investigate the effect of such fixed time delays on dynamics (there is also a more complex variant, involving distributed delays). When our purpose is to understand the nature of time delays responsible for cycles, a delayed differential equations framework is unhelpful because it sweeps too much relevant detail under the rug.

In closing, I also want to address a terminological issue. Komarova insists on calling warfare as an "exogenous" variable. This is not standard terminology (see Turchin 2003b). *Exogenous* variables are those that are not part of feedback loops: they affect other variables but are not affected by them in turn. *Endogenous* variables are part of feedback loops: they both affect other endogenous variables, and are in turn affected by them. As conceptualized in my model, internal warfare is an endogenous variable: its rate of change depends on population, and it in turn affects the rate of population change.

Literature cited

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