Commentary

The multiple roles of theory: a reply to Gorelick

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In a recent paper, Gorelick (2011) equated theory with hypothesis formation and prediction. I will not attempt to replicate the well-formulated rebuttal of Colyvan (2011). However, Gorelick quotes me as stating that theory does not make predictions. That quote was taken out of context. I will not replicate the entire three paragraphs, but here are the key sentences:

“Confusion exists because the term “theory” is used in reference to a variety of kinds of systems at different levels of specificity. Moreover, the nature of theory differs at each level. In this context, we recognize three levels of theory....A general theory does not make specific predictions. Rather, it provides the scaffolding on which the components of more specific, constituent theories are assembled and integrated....At the intermediate level are constituent theories, which set boundaries and identify parameters for particular interest, guiding model development....At the most specific level, models are instantiations of a constituent theory. At this level, predictions are formalized and causal understanding motivates the process.” (Scheiner and Willig 2008)

Gorelick quoted only the fourth of those sentences. As can be seen from the entire context, I very much agree that theories lead to hypothesis formation and the making of predictions. We even highlight that role by giving those types of theories a specific name: models. But that is just one role of theories.

Hypotheses have to come from somewhere. Gorelick would have hypotheses come from data. But something has to organize those data into a coherent framework.

Observations are never theory free. Take a simple example: counting the abundances of organisms in a community and then make a statement about species diversity. The concepts of “species,” “individual” and “diversity” are complex, theoretical entities that exist only in reference to other theoretical frameworks (e.g., the theory of evolution).

Theories give us the questions that the hypotheses are designed to answer. They tell us how to formulate the hypotheses, and how to relate the various measurements to each other. They tell us how to build our models. They tell us the assumptions that are hiding in our models. Models, hypotheses and predictions are just part of this entire edifice, and require the rest of that edifice for their efficacy. Those other components are just as much theories, or parts of theories, as are the models embedded within them.

Gorelick’s equating of theory with just narrowly-focused models perpetuates the belief that ecology has no general theories (e.g., Roughgarden 2009) or laws (e.g., Lawton 1999). In fact, as our recent book shows (Scheiner and Willig 2011), ecology is rife with theories at all levels, as is the rest of biology (Scheiner 2010). Ecology is as rich in laws as any other science (Colyvan and Ginzburg 2003).

Laws are simply models with four or fewer variables. Or, to put it another way, if we consider Levin’s (1968) model trade-offs among generality, simplicity, and accuracy, laws are those models that are general and simple. Such laws precede the founding of the discipline (Velhurst 1838), were part of its early quantification (Lotka 1925, Volterra 1926), motivated the founding of evolutionary ecology (Odenbaugh 2006), and are part of current efforts at model consolidation (Scheiner and Willig 2011). Equating theories with predictions places

laws outside of those theories, or at least makes them seem like very poor theories, because their predictive accuracy has been traded off for their generality and simplicity.

Gorelick's limiting of theory to predictive models perpetuates the predilection within ecology, and all of biology, to act as if larger theoretical constructs do not exist or are unimportant. Such blinders are at best not helpful and at worst harmful. Science is at its most efficient when its efforts are motivated by a clear conceptual framework, its research advances broad theoretical understanding, and its disparate pieces are interwoven. It is only by recognizing the overarching theories of ecology and biology (Scheiner 2010) that these tasks can be accomplished.

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Referees

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Response to referee

Clearly, I need to work on my “pithy and ironic” skills. The objections raised by both Root Gorelick and Mark Colyvan to my statement about laws being models with no more than four variables was meant as a sideways comment about the debate over laws in ecology, which is part of a much larger debate about laws in all of science. In part, the debate hinges on whether laws refer only to universals, or whether they can include regular patterns that have exceptions. I am inclined to include the latter. If so, what we recognize as a law will typically be those models that are simple and general, i.e., those with few variables.

However, Gorelick (2012) raises more substantive issues. First is an objection to my goal of efficiency in the scientific enterprise. He contrasts that with a goal of creativity. Not only are those goals not in opposition, efficiency allows for greater creativity by preventing wasted effort chasing down blind alleys, or avoiding years of debate over hidden assumptions. One purpose of the conceptual framework that I proposed is to make explicit the links between underlying assumptions and specific models, or between different models within a single constitutive theory. See Scheiner and Willig (2005) and Fox et al. (2011) for an example of how such a framework cut through an enormous amount of confusion over theories about ecological diversity gradients.

Gorelick (2012) claims that hypotheses need not arise from theory. Hypotheses always arise from theory, but sometimes those theories are implicit. In such instances, the hypotheses may be very inefficient tests of those theories. While monkeys typing randomly may create statements that resemble hypotheses, those statements have meaning only within a theoretical framework. More important, theory tells us which of the myriad plausible hypotheses are worth pursuing. How can we recognize “truly innovative hypotheses”

(Gorelick 2012) unless we have a standard against which they can be compared?

Finally, Gorelick (2012) objects to my claim that he equated theory with models. His definition of theory is “the formation of testable hypotheses” (Gorelick 2011). That sounds like model making to me. Gorelick wants models to “include mathematical and statistical methods” (Gorelick 2012). But if a statistical or mathematical model is simply a translation of words into symbols, then the theory is just as much a model when it is still words. It may be that Gorelick agrees with my formulation of constitutive theories with their embedded models. That would be consistent with his statement that models are the link between theory and data. Where we part ways is that I see models as just one type of theory and that I recognize yet other types of theories that have different sorts of relationships with data.

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