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Commentary

What is theory?

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Abstract

Theory is vital in science, including evolution and ecology, yet is seldom defined. Theory is often amorphously described as anything abstract or mathematical, or is simply defined as the opposite of empirical work. By contrast, I explicitly define theory as the formation of testable hypotheses, while defining empirical work as hypothesis testing. This pair of definitions highlights the false dichotomy between theory and empirical work insofar as models, mathematics, and methods do not fall in either category, but instead provide the operational link between theory and empirical work. While multiple hypotheses and auxiliary assumptions may be too intertwined to discern which one is actually falsified by a given data set, hypothesis formation and hypothesis testing are still stalwarts of science. Finally, I discuss the odd structure of theoretical papers and proposals within a tripartite parsing of science composed of theory, empirical work, and math/models/methods.

Key Words: Theory and theoretical, philosophy and philosophical, mathematics and mathematical, hypothesis and hypotheses

What is theory? Is there some overarching definition of theory encompassing all sciences, including ecology and evolution? Popper (1959 [1934]) created a demarcation between science and non-science, i.e. falsification. Does a similar demarcation exist between theory and non-theory? There is no best definition. At best, we can measure quality of a definition by its utility and by its consistency of meaning and connotation across many contexts (Wagner 2010), including all natural and social sciences. I propose that *science* is comprised of a mélange of *theory* (hypothesis formation), *empirical work* (hypothesis testing), and *methods*, including mathematics, statistics, and models (tools for operationalising hypothesis testing). It should come as no surprise that most evolutionary biologists and ecologists do all three of these things, albeit in widely varying proportions.

Existing definitions of theory

Most practitioners of theory never ask what theory is. They simply have some gestalt for what does and does not constitute theory. Most journals containing the words 'theory' or 'theoretical' in their titles do not define theory in their 'aims and scope' nor in their 'instructions to authors'. Because of the paucity of theoretical evolution and ecology journals, I cast my net a bit wider and list 'aims and scopes' across many disciplines, including natural sciences and social sciences (Table 1), in which theory is associated with mathematics, abstractness, interdisciplinary study, and philosophy. A robust definition of theory should be applicable across as many disciplines as possible, just as the term science should be applicable to social sciences.

Non-theorists typically have an even vaguer notion of what theory is than do theorists. "Many [people] assume that theory is esoteric and concerns matters that are removed from daily life. As a consequence, they regard something as properly theoretical only if it is very abstract and they don't understand it very well—implying that it can't be real theory unless it is 'too hard' for them to grasp." (Bunch 1979: 14-15). Associating theory with abstractness is too slippery and amorphous a definition.

Table 1. Circumscriptions of Theory (all are quotes from 'Aims and Scopes' of journals)

A. Examples of associating theory with mathematics and abstractness

Theoretical Computer Science is mathematical and abstract in spirit, but it derives its motivation from practical and everyday computation.

The purpose of *Economic Theory* is to provide an outlet for research in all areas of economics based on rigorous theoretical reasoning, and on specific topics in mathematics which is motivated by the analysis of economic problems.

The *Journal of Economic Theory* publishes original research on economic theory and emphasizes the theoretical analysis of economic models, including the study of related mathematical techniques.

The journal [*Theoretical Physics* section of the *Annales de l'Institut Henri Poincaré*] covers a field encompassing all the aspects of theoretical and mathematical physics, likewise the aspects of pure and applied mathematics in direct relation to physics.

Many of the papers [in the *Journal of Theoretical Biology*] make use of mathematics, and an effort is made to make the papers intelligible to biologists as a whole.

The *Journal of Theoretical Medicine* seeks to…foster and encourage the application of mathematics to problems arising from the biomedical sciences.

Work that uses mathematical, statistical, computational, or conceptual approaches is all welcomed [in *Theoretical Ecology*], provided that the goal is to increase ecological understanding.

B. Examples of associating theory with interdisciplinary study and philosophy

Political Theory publishes articles on political philosophy from every philosophical, ideological and methodological perspective.

Theoretical Criminology is a major interdisciplinary and international journal for the advancement of the theoretical aspects of criminological knowledge. The journal is committed to renewing general theoretical debate, exploring the interrelation of theory and data in empirical research and advancing the links between criminological analysis and general social and political theory.

Fashion Theory provides an international and interdisciplinary forum for the rigorous analysis of cultural phenomena.

Feminist Theory is genuinely interdisciplinary and reflects the diversity of feminism, incorporating perspectives from across the broad spectrum of the humanities and social sciences and the full range of feminist political and theoretical stances.

Journals of political theory tend mainly to deal with the history of political thought. But the *Journal of Theoretical Politics* is different. It is concerned with thinking about problems in a systematic way which makes it unique in and important to the profession. We should constantly remind ourselves that a science without theory is a poor science.

Theoretical Medicine and Bioethics provides a forum for interdisciplinary studies in the philosophy and methodology of medical practice and research.

The *Journal of Theoretical Medicine* seeks to promote genuine interdisciplinary collaboration between those interested in the theoretical and clinical aspects of medicine.

Work that bridges disciplinary boundaries, such as the intersection between quantitative social sciences and ecology, or physical influences on ecological processes, will also be particularly welcome [in *Theoretical Ecology*].

Theory in any discipline is often associated with mathematical methods and models (Table 1a). This is especially true in the sciences (Gorelick 2007). For example, the *Journal of Theoretical Biology* has matured into a fine publication, albeit a journal of mathematical biology, often without formation of biological hypotheses. By contrast, *Biological Theory* contains hardly any mathematics, but contains a wealth of new biological hypotheses. In science, there should exist a distinction between theory and mathematics. Charles Darwin was the quintessential theoretical evolutionary biologist, but could hardly tackle any mathematics beyond balancing his accounts (Browne 2002). And Darwin was not a mere anachronism; evolutionary biologists are still producing superb non-mathematical theory (Godfrey-Smith 2006, McShea and Brandon 2010). An anthology of classical papers in ecology (Real and Brown 1991) contains a section titled "Theoretical advances: the role of theory in the rise of modern ecology" in which all but one paper seems to be mainly on mathematical ecology. And the one exception was not entirely an exception insofar as it was G. Evelyn Hutchinson's concluding remarks for the written proceedings of a symposium on quantitative biology. Even though many of the finest contemporary journals and books on evolutionary and ecological theory have associated theory with mathematics and abstractness (Tables 1 and 2), I implore that a more philosophically satisfying definition of theory is needed.

Unlike natural scientists, social scientists typically do not associate theory with mathematics, but instead usually associate theory with philosophical or inter-disciplinary studies (Table 1b). 'Interdisciplinarity' is too amorphous a definition of 'theory' to be operational. Ecologists who use geographic information systems (GIS) are considered interdisciplinary. Geochemists and especially biogeochemists are considered interdisciplinary. Oncologists using mathematical models are considered interdisciplinary. The new journal *Theoretical Ecology* considers interdisciplinary applications in ecology to be theoretical. There is certainly something synthetic about theory. However, defining something as theory should be more than a matter of counting how many narrow sub-disciplines are amalgamated into a research program.

Van Valen created and produced the journal *Evolutionary Theory*. Although he never stated what theory entails, he hinted that evolutionary theory was anything that did not deal with real organisms—"I thank the National Science Foundation for regularly rejecting my (honest) grant applications for work on real organisms, thus forcing me into theoretical work." (Van Valen 1973: 21)

Table 2. Examples of evolutionary and ecological theorists associating theory with mathematics, mathematical models, and abstractness

"In this book, I have provided a description of the structure of evolutionary theory in which the theory is viewed as a family of related mathematical models." (Lloyd 1988: 165)

"[E]volutionary theory is not just a collection of separately constructed models, but is a unified subject in which all of the major results are related to a few basic biological and mathematical principles." (Rice 2004: viii)

"[T]heoretical ecology—much of which is ineluctably mathematical…" (McGlade 1999: ix)

"Theory development - abstract general properties of system by ignoring certain components and emphasizing others (selective ignorance)." (Gross 2000)

Defining theory as hypothesis formation

In order to properly define 'theory', we must first delimit its domain. In other words, what is meant by 'science', possibly including 'social science'? I follow Popper's orthodox notion that falsification provides the demarcation of science (Popper 1959 [1934]). Physics, chemistry, biology, etc all clearly fall under the rubric of science. So do most social sciences, such as anthropology, psychology, economics, and sociology (the 'positive' parts of these disciplines are falsifiable; the 'normative' parts are not; Mas-Colell et al. 1995). More surprisingly, any assertions in law, education, and women's studies that can be falsified also constitute science. Although circumscribing science via falsification is naïve, the alternatives seem both unduly complicated and with too many substantive problems (Feyerabend 1975, Shapere 1989). Therefore, as a practicing organismal biologist with little training in philosophy, the naïve approach seems most productive, although I will discuss more sophisticated approaches to philosophy of science in subsequent sections.

The antonym of 'theoretical' is usually considered to be 'empirical'. In fact, this is the way that most scientists and social scientists appear to distinguish theory from other aspects of their discipline (e.g. Tirole 1988). Empirical work is usually defined to be studies in which data are acquired and subsequently analyzed, while theoretical work is left to be defined as any aspects of the science that are not empirical. Yet, it would be more satisfying to have a constructive definition of 'theory', one that does more than simply provide a contrast with the nominal case of empirical work.

By contrast with most sciences, in political theory and many social sciences, the antonym of 'theory' is usually considered to be 'practice'. Occasionally, 'theory' and 'practice' are defined. For example, in some radical feminist studies, 'theory' has been defined as "the development of ideas", while 'practice' is defined as "the actions of the movement" (hooks 2000 [1984]: 113). In many ways, this definition of theory is closer to the one I propose herein than to definitions of 'theory' as something mathematical or abstract, as given in many so-called hard sciences (Table 1).

Lloyd (1988: 2), a philosopher of evolutionary biology, stated that, "Under a general hypothetico-deductive view of theories, a theory is understood as offering hypotheses from which, in combination with empirical assumptions, deductions can be made regarding empirical results." Similarly Nagel (1971: 22), a philosopher of science, stated that, "the word 'theoretical' is frequently used more or less interchange-ably with 'conjectural,' as when a tentative hypothesis as to how a robbery was committed is said to be a theoretical account of the event." I therefore define 'theory' to be formation of testable hypotheses (testable so that they fall within the realm of science). While I do not insist that hypotheses be testable solely using data that currently exist, I implore my students to devise hypotheses that could probably be tested with data generated within the upcoming decade. While it is difficult to predict the future, such a benchmark keeps us honest. By contrast, I define 'empirical work' to be hypothesis testing. These definitions of 'theory' and 'empirical work' are commensurate with the philosoph-ical frameworks of Popper (1959 [1934]) and Lakatos (1970) and with a wide range of disciplines, including biology, psychology, education, and law (Table 3).

Should there be pluralism in defining 'theory' amongst different disciplines and researchers? The evolutionary game theorist Pollock (2001: 8) argues against pluralism:

> A work on [theory], placed into our hands, is apt to be experienced as a bullet—either to be dodged, or loaded and fired at unbelievers…. One is, of course, thereby safe and sound when confronted by an enemy's projectile; and pluralism produces a surfeit of enemies. But the price of safety is impotence. [Theory] is most likely to influence others when it forces its proponent to accept conclusions found personally distasteful. By limiting my autonomy, binding myself to conclusions I dislike, I am less dangerous to others—and, perhaps, more likely to find common ground with these others elsewhere.

Table 3. Examples of associating theory with hypothesis formation

BIOLOGY: "[The] most obviously fruitful role [of theory] is in providing explicit direction for research. From theory we can deduce conclusions not previously reached and that are occasionally counterintuitive…. If fortune smiles on the theorist, the theoretically derived conclusion will be readily testable by observation." (Williams 1988: 297)
BIOLOGY: "The development of a theory requires the introduction of as yet unobserved but observable entities connected to previous observation by lines of causality and of explanation. That is, we demand both prediction and explanation from a theory and especially prediction of new phenomena, not simply 'more of the same'…. The content of a theory, especially its predictive aspect, must render the theory falsifiable or verifiable as a result of new accumulations of experience." (Lewontin 1963: 223)
BIOLOGY: "No scientific theory is worth anything unless it enables us to predict something which is actually going on. Until that is done, theories are a mere game of words, and not such a good game as poetry." (Haldane 1937; as quoted in Grant and Grant 1995: 241)
PSYCHOLOGY: "[Theory is] a system of logically interrelated, specifically non-contradictory, statements, ideas, and concepts relating to an area of reality, formulated in such a way that testable hypotheses can be derived from them." (de Groot 1969: 40)
FEMINIST STUDIES and EDUCATION: "Theory is not just a body of facts or a set of personal opinions. It involves explanations and hypotheses that are based on available knowledge and experience. It is also dependent on conjecture and insight about how to interpret those fact and experiences and their significance." (Bunch 1979: 8)
LAW: "In law, we use theory as an explanatory set of principles that can help explain past behavior and predict future behavior." (Ruth Colker, pers. comm., 8 Jan. 2005)

Especially as our research programs become increasingly interdisciplinary, by abandoning pluralism and explicitly defining 'theory' to be hypothesis formation, we should be able to better communicate and work amongst disparate fields.

Equating 'theory' with hypothesis formation is an anachronism in evolution and ecology. Table 4 lists titles of a few older evolution books and papers with theory in their titles, works that are mostly hypothesis formation and completely devoid of mathematics.

Defining 'theory' as hypothesis formation is something I have only explicitly seen in feminist theory (Table 3). Theorists in a few other fields have been more circumspect. The theoretical psychologist de Groot (1969) came closest in defining 'theory' to be a logically consistent set of axioms from which testable hypotheses can be formed, although this definition comes perilously close to devolving into mathematics. The evolutionary theorist Williams implied that 'theory' entails hypothesis formation, but did not seem to have problems with untestable hypotheses ("If fortune smiles on the theorist, the theoretically derived conclusion will be readily testable by observation…. Theory can also be useful, if less directly, if it merely clarifies relations among concepts, even without explicit reference to testable hypotheses." (Williams 1988: 297). Scheiner and Willig (2008) go even further in stating unequivocally that, "[a] general theory does not make specific predictions." Philosophically, there does not seem to be any consistent way to distinguish hypotheses from predictions, so I consider these two terms synonymous. Haldane (1937) and Lewontin (1963) are the only two evolutionary biologists that I know of who seem to unequivocally assume that 'theory' must necessarily entail formation of at least one falsifiable hypothesis.

A tripartite parsing of science

By defining 'theory' to be hypothesis formation and 'empirical work' to be hypothesis testing, we necessarily imply that theoretical and empirical works do not form the entire universe of what encompasses science. There exist intermediate methodological constructs that allow us to test hypotheses, such as mathematical models and methods for exploratory data analysis (e.g. Tukey 1977). These methods do not fall into either hypothesis formation or testing. Thus, mathematical, statistical and computational methods should not be considered theory, despite a litany of unpersuasive arguments to the contrary. Instead, I propose a tripartite parsing of 'science' consisting of:

1. Theory, which is hypothesis formation
2. Empirical work, which is hypothesis testing, i.e. with data
3. Methods, which provide a bridge between hypothesis formation and testing

Neither natural history nor mathematics is 'science' per Popper's demarcation of falsifiability. As Lewontin (1963: 223), using the term 'models' as a catchall for mathematical methods, asserted, "successful use of models demands a preexistent theory and in biology, at least, it has been only those disciplines with a well-developed theoretical structure, which have had any notable success with models." However, both mathematical models and natural history can be construed as vital facets of science within this tripartite parsing.

Even with sophisticated falsificationism, natural history is not science, but contributes to maturation of scientific theories

Popper's naïve falsificationism was challenged and largely supplanted by sophisticated falsificationism (Lakatos 1970, Chalmers 1999 [1976]). Scientific theories are sufficiently large and complex that it is usually impossible to know which one is falsifying: one of the core hypothesis (usually inviolate) or one of the 'protective belt' of auxiliary assumptions (Duhem 1954 [1906], Quine 1980 [1951], Lakatos 1970, Loehle 1988). This has resulted in research programs where researchers compare and contrast variants, theories, or paradigms.[1] Nonetheless, there are still activities that support science, even within this more nuanced framework, but that do not by themselves qualify as science. Data in search of a question are often compiled by biologists, such as most of natural history and bioinformatics, and can be incredibly valuable for testing hypotheses. Yet, natural history and bioinformatics do not qualify as science because there is no hypothesis formation (theory), nor hypothesis testing (empirical work). Similarly, the synthesizing of data without hypotheses formation can be incredibly valuable, but does not qualify as science, also for wont of hypothesis formation or testing.

There is nothing pejorative about saying that a stand-alone activity does not qualify as science because that activity is neither hypothesis formation, hypothesis testing, nor method. I spend substantial time doing and publishing natural history, without any regrets and without any hypothesis formation or testing (e.g. Gorelick and Doan 2005; Gorelick and Bertram 2009). The U.S. Long-Term Ecological Research (LTER)

[1] The first three sentences of this paragraph were added in response to helpful comments by Göran Ågren and Mark Colyvan.

Table 4. Titles of old evolutionary works in which theory refers to hypothesis formation

The theory of the gene	Morgan 1917
Early theories of sexual generation	Cole 1930
The origin of land flora: a theory based upon the facts of alternation	Bower 1935
Position effect and the theory of corpuscular gene	Goldschmidt 1946
The strategy of the genes: a discussion of some aspects of theoretical biology	Waddington 1957

program has a mandate to collect huge amounts of baseline data, with the hope that someday the data can be used to track ecological changes. For the LTERs that I worked with, there were no apparent hypotheses that drove data collection, but simply the aim to collect as much ecological data as possible from as many sites as possible within a given geographic boundary, provided constraints of money and time. The beautify of this approach is that the data can be used for many different and unexpected research programs, such as to test erstwhile ideas about ecological succession, somewhat more recent ideas about competition in community ecology, or more modern null models. Biologists collect huge amounts of gene sequence and expression data for similar reasons, hoping that it will eventually be useful for testing hypotheses in many different research programs. As a final example, the landscape ecology originally practiced by Forman and Godron (1986) was a major contribution, but was mostly descriptive and—by the criterion herein—therefore not science.

Scientific theories mature (Bunge 1968, Loehle 1988). Unstated or implicit assumptions—including the protective belt of auxiliary assumptions—eventually become explicit, which can drastically alter predictions (Loehle 1988). As theories mature, not only do the hypotheses become internally more consistent and more readily testable, but data get pulled off the shelf to test predictions, something that often could not be done without legions of published natural history data, including classical collections of species ranges and abundances, as well as more modern molecular natural history data, such as from genomics and proteomics. Having such data at hand allows for rapid refinement and maturation of theory.

Some modelers disparage the role of data and hypothesis testing in building models and theory. For example, Peter Taylor (1989: 125) asserted, "My diagnosis is that ecological theorising can be stifled by an emphasis on hypothesis testing." I have even been known to facetiously quip that, "data clouds the issue." While mathematical modeling can take on a life of its own, it would be intellectually satisfying if such models were commensurate with existing data or suggested where to look for data. Most researchers in ecology and evolution entered the field because of allure of the natural world. Observations of the natural world are often what motivate us to modify hypotheses or theories and possibly generate new ones. It would be a shame if we did not let natural history guide our scientific theories and mathematical models.

Writing theory—papers and proposals

Like all other scientists, theorists must write publishable papers and fundable grant proposals to be successful. What constitutes a successful theoretical paper or proposal?

Most scientists conduct both theory and empirical work simultaneously. Most papers are largely testing of existing hypotheses, but often the authors modify hypotheses, which here constitutes a modicum of theory underlying a fundamentally empirical paper. On the flip side, most papers that supposedly are pure hypothesis formation are based on some empirical facts, hence constitutes a modicum of empirical work underlying a fundamentally theoretical paper. Many of the best theoretical papers not only lay out new hypotheses, but also provide some corroborative empirical support, a.k.a. meta-analysis.

Some theoretical papers contain no real empirical support, but rather are examples of meta-synthesis. These papers attempt to unify, simplify, and amalgamate an unwieldy morass of existing hypotheses. This is the sort of theoretical work that Williams (1988: 297) said, "clarifies relations among concepts". There are few evolution and ecology papers of this genre, probably because organismal biologists are too inspired by real organisms.

Because theoretical papers entail hypothesis formation, there are no obvious methods or results, although many journals require papers—and even some abstracts—to be shoe-horned into the standard introduction-methods-results-discussion format. In such instances, I have begged editors to use a more appropriate format. When that has failed, I have written one-word methods and results sections, with the one-word being "none", in order to be honest.

"Specific proposals are inappropriate for much theoretical work; when one knows just what one will do, it is done…. The heart of science is new ideas, and their development is discriminated against." (Van Valen 1976: 2). I am not as dispirited as Van Valen, in large part because of the Natural Sciences and Engineering Research Council (NSERC) of Canada's recognition of the importance of discovery in their discovery grants program—especially as compared with the U.S. National Science Foundation's (NSF) system—and the Canadian emphasis on smaller-budget science.

What should a theoretical proposal look like? How can somebody describe the process by which they form hypotheses? For me, the answer is to read as much as possible, think critically, observe nature, and let my mind wander. "People always ask how [theorists] come up with ideas, and the answer is so boring that we're usually tempted to make up something sarcastic. The truth is, we hold a blank sheet of paper, stare into space, and let our minds wander. To the laymen, this looks remarkably like goofing off." (Watterson 1995: 19). This is pretty much all there is to doing theory, however, I cannot imagine saying that on a grant proposal or having a graduate student say that on their research proposal. However, note that Watterson (1995: 19) rightly leaned heavily on natural history, "I like to sit outside when I write, partly because there are bugs and birds and rocks around that suggest an idea." Meta-analysis should usually be added to a theoretical proposal, especially since most theoretical organismal biologists do some meta-analysis. A theoretical proposal should carefully lay out how meta-analyses will be done, possibly with details on data sources, statistics, phylogenetic comparative methods, or how cherry-picking will be avoided. It may be appropriate to make reference to exploratory data analysis (Tukey 1977, Tufte 1983). I find it increasingly useful to describe meta-synthesis in proposals. While this may take valuable space in the proposal, it is probably worthwhile given that most reviewers largely conduct empirical work.

Concluding Remarks

Maybe I am naïve, but I disagree with Suppes (1967: 63) when he said, "If someone asks, 'What is a scientific theory?' it seems to me that there is no simple response to be given." Hypothesis formation, where a hypothesis is a simple declarative statement, seems to be an unambiguous definition of 'theory'.

Ironically, this paper is <u>not</u> itself theoretical, but is instead methodological, providing a common language to facilitate interdisciplinary and cross-disciplinary work. By separating the synthetic and conjectural portions of science (theory) from developing methodologies, including mathematical models (methods), and from data collection, data analysis, and hypothesis testing (empirical work), I hope to highlight commonalities across disciplines. I also hope to encourage more researchers to spend a greater amount of their time working with theory. Theory is neither mathematical nor abstract. Theory is the creative, inductive, and synthetic discipline of forming hypotheses, and hence forms one of the three necessary components of doing science.

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Referees

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Response to referee

For the most part I agree with Mark Colyvan (2011), genuinely appreciate his criticisms, and am truly learning from him. I disagree with him on only a few details, but please pardon me (or laugh at me) while I try expanding on and debating details with a philosopher of science on his home turf.

I completely concur with Mark Colyvan (and Göran Ågren) that theoretical work requires explanation, not just formation of testable hypotheses. Parsimonious syntheses of complex data are what I find to be the most fun and challenging portion of constructing theory. This is what I meant by theory being synthetic. For me, explanation is a necessary but not sufficient component of theory because hypothesis formation may be lacking.

I also agree that systematising and organising data can and often does lead to testable hypotheses. However, I do not believe that synthesis, systematisation and organisation without testable hypotheses should be considered theory because such work does not lead to progress in our overall understanding of the world. Thus I disagree with the evolutionary theorist George Williams, the ecological theorist Sam Scheiner (quoted above in 'defining theory as hypothesis formation'), and with Walter Dodd's (2009: 67) definition of 'theory' as "a plausible or scientifically acceptable principle or body of principles offered to explain phenomena"—Williams, Scheiner, and Dodds believed that explanation and synthesis *without* hypotheses could constitute theory. Instead, I subscribe to J.B.S. Haldane's subversive streak (1937; quoted in Grant and Grant 1995: 241), "No scientific theory is worth anything unless it enables us to predict something which is actually going on. Until that is done, theories are a mere game of words, and not such a good game as poetry." Likewise, some philosophers of ecology believe that non-testable explanations are merely re-descriptions, not *bona fide* theory (Loehle 1988).

Mark Colyvan illustrates his point about theory sometimes merely being about systematising and organising data with Johannes Kepler's synthesis of Tycho Brahé's data. But this begs questions about lack of acceptance of Nicholas Copernicus's astronomical theories, which provided a simplification and new organisation of Ptolemy's (and some of Copernicus's) data on motion of the planets: a heliocentric theory that remained without acceptance for most of a century until Galileo Galilei started making predictions and testing them.

My *ad hoc* benchmark is to create theory that is testable within the next decade. Either too much or too little time will not suffice. If hypotheses arising from theories are immediately testable, then why not test them before publishing the theory? The decade limit means that I will probably see my ideas tested—and probably falsified—while I am still an active researcher and can appreciate the results.

Colyvan and Ginzburg (2010) briefly alluded to the role of analogy in devising theory, something that Mark Colyvan and I probably should have better emphasized. Reasoning by analogy provides crucial stepping-stones in mentally organising any tangled morass of concepts, hypotheses and data. This closely resembles Elizabeth Peters' (2000: 200) insight that, "Speculation is never a waste of time. It clears away the deadwood in the thickets of deduction." Theory is probably often associated with interdisciplinary work because of the power of analogising and borrowing tools across disciplines.

Unlike Colyvan (2011) and Godfrey-Smith (2006), but in accord with Lewontin (1963), I relegate models to methods, not to theory. Popper's (1959 [1934]) definition of science clearly has problems. For instance, there are issues with hypotheses not only being falsifiable (fortune cookies are often falsifiable), but also not yet falsified. Popper's notions did not allow for progress in science, as did Kuhn (1962) and Lakatos (1970). But there are problems with these two alternatives (Feyerabend 1975). Kuhn's (1962) notion of science as revolutions with paradigm shifts seems sensible, at least until he declined to define paradigms and incommensurability (Shapere 1989). Lakatos' (1970) notion of science as progressing research programs seems sensible, at least until he declined to demarcate core versus auxiliary assumptions/ hypotheses. Lakatos believed that, "The main indication of the merit of a research program is the extent to which it leads to novel predictions that are confirmed" (Chalmers 1999 [1976]: 135). While I agree that science progresses by confirming novel predictions, with the proviso that it is nigh impossible to define 'novel', the important commonality between Popper and Lakatos is that predictions be testable/falsifiable, even if the scientist cannot know which of the linked hypotheses or assumptions has been falsified. That is the kernel of the work of Popper and Lakatos that I use to define 'theory' and to thereby partition science into theory, empirical work, and methods. The fundamental disagreement that Mark Colyvan and I have seems to be that he amalgamates theory with models—I classify models as methods—whereas I keep methods/models logically distinct from theory. In a peculiar turn of phrase, I am against method including theory.

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