2

The Eroding Artificial-Natural Distinction?

Some Consequences for Ecology and Economics

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2.1 Introduction

Since the publication of Thomas Kuhn's *The Structure of Scientific Revolutions* (1962), historians and philosophers of science have paid increasing attention to the implications of disciplinarity. For Kuhn, rigid disciplinary training was essential for progress within periods of "normal" science. "A commitment to a discipline," as Andrew Barry and colleagues put it, "is a way of ensuring that certain disciplinary methods and concepts are used rigorously and that undisciplined and undisciplinary objects, methods and concepts are ruled out" (Barry et al. 2008). This "ruling out" is valuable as it discourages intellectual wandering or false-starts, but it is also, and necessarily, normatively restrictive: the ideal of disciplinary purity—that each discipline is defined by a commitment to an appropriate, unique set of objects, methods, theories, and aims—has powerful implications for the structure and practices of many sciences, including life sciences, such as ecology, and social sciences, such as economics. This ideal has governed, and continues to govern, what ecologists and economists do, since it serves as a normative guidepost that establishes, in particular, which objects of study are appropriate and endogenous: pure ecologists are to study non-anthropogenic or "natural" factors, and economists are to study a specific set of anthropogenic causal factors, or an aspect of what might be termed the "artificial" realm.

While articulating the historical formation and full set of connections between the artificial-natural distinction and ideals of disciplinarity in ecology and economics is beyond the scope of this chapter, we provide evidence below to support the claims that (1) historically, the objects of ecology have been "natural" and those of the political economy "artificial" (in the sense that the causes are exclusively human) and that (2) this has been one important factor hindering interdisciplinary exchange (though, obviously, it hasn't made exchange impossible).[1] Attempting to sever the connection between the artificial-natural distinction and proper practice in these sciences, we argue that this distinction is conceptually and empirically problematic. We recognize that the artificial-natural distinction can be drawn in a number of ways, but argue that if this distinction is interpreted as dividing all phenomena into two sui generis categories—artificial versus natural objects of study—then the distinction, along with a commitment to it as defining appropriate objects of study, is problematic. In other words, this distinction should no longer serve as the mechanism that determines the objects of study for ecology and economics.

What, then, are the appropriate objects of study for ecology and economics? We claim that, in many cases, such objects are better viewed as a blend of the artificial and the natural, or what one might describe as *entangled phenomena*, rather than phenomena that are merely natural or merely artificial. Because entangled phenomena are a *shared* object of study, it remains an open question whether it is best for each science to operate independently or engage in interdisciplinary exchange. We argue that, if the goal of ecology and economics is to develop stronger predictions and explanations, and to develop better policy prescriptions, then the evidence suggests that interdisciplinary exchange is preferable, for epistemological and policy-oriented reasons, to these two sciences acting independently. Entangled phenomena, and the transdisciplinary questions they encourage, require interdisciplinary treatments.

The arguments of this chapter are divided into four additional sections. In Section 2 we draw selectively on a long philosophical tradition of analyzing the artificial-natural distinction in order to argue that the distinction is conceptually problematic, if understood as a binary; it should, instead, be understood as a continuum. In Section 3 we demonstrate that the objects of ecology have been "natural" and those of the political economy "artificial," and that this has been one factor hindering interdisciplinary exchange. In Section 4 we empirically problematize the artificial-natural distinction by providing recent case studies that highlight the benefits of treating objects of study as entangled phenomena. In the final section we draw out some of the implications of our analysis. Our main point is this: if disciplinary purity rests on the artificial-natural distinction, then it is in trouble for both conceptual and empirical reasons.

2.2 The Artificial-Natural Distinction?

The artificial-natural distinction is problematic in two senses that are relevant for the sciences of ecology and economics. We can think of these two senses as two separate questions: a conceptual question (can the distinction be clearly drawn?) and an empirical question (is it useful to draw the distinction?). These questions come apart insofar as it may be useful to draw the distinction *even if* it is ultimately conceptually problematic (e.g., it might be useful to categorize landscapes as artificial or natural for practical purposes, even if urban landscapes are also, in a widely accepted sense, natural). In this section we address the conceptual question and argue that relying on this distinction is a questionable strategy. This literature has become so vast that, in this section, we can only briefly consider some of the ways in which the distinction, particularly that originally developed by Aristotle and promulgated by John Stuart Mill ([1874] 2006), has been problematized.

For Aristotle, the concept of "nature" has several meanings (Kelsey 2003; DesRoches 2014). His most prominent concept denotes an inner principle of change that is characteristic of self-moving things. Unlike artificial objects, natural objects are involved in a process of growth, change, and flux. Nature, in this sense, is deeply intertwined with how things behave when left to themselves, free from intentional human agency.

In Book II of the *Physics*, Aristotle gives the famous example of a wooden bed. While the shape and structure of the bed has been fashioned by an intentional human agent, the carpenter, this formal cause is merely "human impositions on the unchanged matter that remains a natural product" (Bensaude-Vincent and Newman 2007: 5). If one were to plant the bed in the ground and that bed were to sprout anything at all, it would not generate beds, but trees. In this case, the inner principle of change or motion is independent of the form that is imposed on it by the carpenter and the nature of the object is associated with the unchanged matter. In this sense of "nature," the natural world would be one that owed its entire existence to natural causes and, therefore, would exclude all intentional human activity. This world would be one populated by objects, whether biotic or abiotic, without any forms imposed on them from without. It would be a world that was left entirely to itself, independent of human agency. With this concept of nature, one can easily imagine a contrary world, where there is no biotic or abiotic items that are left to be naturally expressed, where every last object and bit of material has been subject to the intentional activity of human agents.

There is perhaps no greater classical authority on the artificial-natural distinction than John Stuart Mill ([1874] 2006). In his *Three Essays on Religion*, Mill considers a variety of possible meanings of "nature," but eventually boils his analysis down to two distinct concepts, one of which is clearly inspired by the Aristotelian concept of nature described above. Mill states,

> In one sense, [nature] means all powers existing in either the outer or inner world and everything which takes place by means of those powers. In another sense, it means, not everything which happens, but only what takes place without the agency, or without the voluntary and intentional agency, of man. ([1874] 2006: 375)

In this quotation, Mill's first concept of nature denotes everything actual and everything possible, including human agents and their intentional activities. The second concept of nature, the one that Mill himself prefers, drives a wedge between intentional human agency and that realm of phenomena that has not yet been affected by human agency (Schabas 1995). On this account, human beings and their intentional activities are, in some sense, special. They are the only creatures in the universe that are beyond or "outside of" nature.

Our claim is that the idea of disciplinary purity exemplifies Mill's second concept of nature since it alone provides the resources for making a distinction between nature and nonhuman nature, as objects that are different *in kind*. Pure ecologists exclude human beings and their intentional activities from their models and theories, because they are not believed to be a part of nature (in Mill's second sense). The object of study for pure economic theory is a subset of intentional human activity or anthropogenic causal factors. Mill's second concept of nature has the resources to explain and justify why there would be such separate sciences: there are different objects of study to which each science applies. Mill's first concept of nature, by contrast, denies the possibility of this kind of exclusion since nothing, including human beings and their intentional activities, can be unnatural.

But, while this second Millian concept of nature may help to explain the idea of disciplinary purity as it applies to ecology and economics, the concept is far from unproblematic. For one, the extension of this concept appears to be empty. Strictly speaking, there is no longer any part of the earth's surface that remains completely unaffected by human technologies (Bensaude-Vincent and Newman 2007; Wapner 2010). In his *The End of Nature* Bill Mckibben states,

> An idea, a relationship, can go extinct just like an animal or a plant. The idea in this case is "nature," the separate and wild province, the world apart from man to which he has adapted, under whose rules he was born and died. In the past we have spoiled and polluted parts of that nature, inflicted environmental "damage" … We never thought we had wrecked nature. Deep down, we never really thought that we could: it was too big and too old. Its forces, the wind, the rain, the sun—were too strong, too elemental. But, quite by accident, it turned out that the carbon dioxide and other gases we were producing in pursuit of a better life—in pursuit of warm houses and eternal economic growth and agriculture so productive it would free most of us for other work—*could* alter the power of the sun, could increase its heat. And that increase could change the patterns of moisture and dryness, breed storms in new places, breed deserts. Those things may or may not have begun to happen, but it is too late to prevent them from happening. We have produced carbon dioxide—we have ended nature. We have not ended rainfall or sunlight … But the meaning of the wind, the sun, the rain—of nature—has already changed. (Mckibben 1990: 43–4)

Of course, Mckibben's (1990) claim that nature is dead is not meant to suggest that there is nothing left that is actual and possible—Mill's first concept of Nature—but simply that there is no longer any part of the earth's surface that can be truly described as completely detached from human agency.

Ever since McKibben's influential discussion of nature, the idea that humans play a dominant role in the world's ecosystems has been continuously and strongly reinforced. Moreover, we are, since 2000, increasingly inundated by a burgeoning literature on "the Anthropocene," which holds that human presence in the natural world is so pervasive it marks a new geological epoch (Bensaude-Vincent and Newman 2007; Steffen et al. 2011; Sarkar 2012; Church and Regis 2012; Kaebnick 2014; Vogel 2015). It is now estimated that 75 percent of ice-free land on Earth has been transformed by humans, changing ecosystem patterns and processes across the terrestrial biosphere (Vitousek et al. 1997; Ellis and Ramankutty 2008; Martin et al. 2012; Ellis et al. 2013). Paul Wapner (2010), in his recent book *Living through the End of Nature*, writes, "the wildness of nature has indeed largely disappeared as humans have placed their signature on all the earth's ecosystems" (2010: 19). He continues, a growing human population, unparalleled technological prowess, increasing economic might, and an insatiable consumptive desire are propelling us to reach further across, dig deeper into, and more intensively exploit the earth's resources, sinks, and ecosystem services … the cumulative force of our numbers, power, and technological mastery has swept humans across and deeply into all ecosystems to the point where one can no longer easily draw a clean distinction between the human and nonhuman realms. (Wapner 2010: 4)

Beyond planet Earth, the technology of our species is now so vast that it has extended past the sublunar region to include the Cydonia (the region of Mars) (Bensaude-Vincent and Newman 2007).

It would appear that the claim that there is some pure realm of phenomena on Earth that remains unaffected by human agency is false since there is nothing left on Earth that remains unaffected by human agency. Mill's two concepts of nature appear to present us with a dilemma. Accepting Mill's second concept of nature appears to explain the idea of disciplinary purity, but it also requires us to recognize the claim that everything on Earth is, in some sense, artificial because the whole planet has been, directly or indirectly, affected by human activity. Mill's first concept of nature, on the other hand, presumes that all humans and their intentional activities are part of nature, but it is not capable of selecting the supposedly distinct objects of study for the sciences of ecology and economics, respectively.

Fortunately, this dilemma is more apparent than real. The way out of this rabbit hole is to concede that while everything, metaphysically, is natural (i.e., naturalism is true), we can still operationalize the concept of "nature" for our purposes by insisting that those items that remain *relatively detached* from human agency, those items that do not possess *significant* features caused by intentional human agents, are natural. What counts as "relatively detached" and "significant" will depend on specific research contexts. In taking this pragmatic approach, we are following Sahotra Sarkar when he states,

Even if humans are conceptualized as part of nature, we can coherently distinguish between humans and the rest of nature. There is at least an operational distinction; that is, one that we can straightforwardly make in practical contexts. We can distinguish between anthropogenic features (those largely brought about by human action) and non-anthropogenic ones. (2012: 19)

By making this operational distinction, Mill's two concepts of nature are treated as compatible since one does not necessarily preclude the other. The first concept is more fundamental since even the most artificial of objects, such as atomic bombs, personal computers, and jumbo jets, are judged to be natural. On the other hand, for practical purposes, these same items are deemed artificial since they were intentionally built by human agents and they possess a variety of anthropogenic features. Be that as it may, in light of the claim that characterizes the Anthropocene—that no phenomena is completely insulated from human agency—it is always a question about the *relative* *detachment* that the objects of study (for ecology and economics) have in relation to human agency.

On this view, the natural and artificial can be positioned along a continuum, in which the most natural objects are those that remain relatively detached from human agency, and the most artificial objects being those that have been built and constructed by intentional human agents. It should be clear that, on this account, there is no sui generis difference between artificial and natural objects since the difference is always a matter of degree. In other words, there is a blending of the natural and the artificial, which we describe as *entangled objects*. This approach to the natural-artificial distinction has the virtue of preserving the practically significant distinction between, for example, intentionally modified environments, such as city centers, and environments that have been subject to relatively little human agency, such as remote uninhabited islands that were recently generated by natural causes in the Pacific Ocean. The point is that the artificial-natural distinction is conceptually problematic if understood as a binary, and so we advise understanding this distinction as a continuum.

2.3 Disciplinary Purity and the Artificial-Natural Distinction

In this section we show that ideas of disciplinary purity have been underwritten by the artificial-natural distinction. We argue that the objects of ecology have been "natural" and those of the political economy, "artificial," and that this has been one factor hindering interdisciplinary exchange. Although this distinction is conceptually problematic if understood as a binary, it, as we showed in Section 2.2, might still be empirically useful to draw the distinction in some contexts. We turn to this question in Section 2.4.

2.3.1 Ecology and Nonhuman Nature

Ecologists have, in general, largely ignored anthropogenic factors and discounted human activity as external to ecosystems (O'Neill and Kahn 2000; Martin et al. 2012; Worm and Paine 2016; Inkpen 2017, forthcoming). Ecologist James Collins and colleagues write, for example, that "from the perspective of a field ecologist examining a natural ecosystem, people are an exogenous, perturbing force" (Collins et al. 2000: 416). Boris Worm and Robert Paine agree that "humans have historically been treated as an externality, as if their effects belong in a separate category compared to other species and their interactions" (2016: 604).

We can divide the claim that humans are an externality into three categories, which we will characterize as *empirical*, *explanatory*, and *methodological* claims. As an *empirical* claim, one might hold that humans are not a major (causal) influence in ecological systems at most levels of ecological organization. Of course, they have a large influence over the structure of urban and agricultural spaces, but these spaces are inconsequential when compared to the diversity of other systems that ecologists study. This reason seems hardest to accept today, given the recent widespread agreement that humans are pervasive (considered below), but was influential in the early twentieth century.

As an *explanatory* claim, one might hold that human-disturbed nature is simply not worth studying or trying to explain. For example, the nineteenth-century biologist Thomas Henry Huxley argued that because man was a living creature, he and "all his ways" should properly be considered under the province of biology; yet, biologists, he felt, are a "self-sacrificing" bunch, for whom nonhuman nature is sufficient disciplinary territory (Huxley [1876] 1897: 270–1). Other ecologists have viewed human-disturbed nature as oxymoronic, as not really nature, and thus as not worth studying. Ecologist Mark McDonnell explains that, "for much of the twentieth century the discipline of ecology contributed relatively little information to our understanding of the ecology of human settlements … some biological researchers viewed cities as 'anti-life' (i.e., without nature) for they supported few plants and animals" (McDonnell 2011: 7).

As a *methodological* claim, one might hold that not including humans in one's models is the best starting place for an analysis of any ecosystem. This methodological claim has been held for a number of reasons. Some ecologists have felt that not including anthropogenic factors is the simplest, and thus best initial, step to take when trying to understand the dynamics of an ecological system. As one ecologist recently remarked, "Our understanding of even the basic characteristics of major areas, like the Congo Basin, are missing … Adding direct human impacts to studies requires a certain initial understanding first" (Corbyn 2010). In other words, to begin by including humans is experimentally and computationally intractable. In the future, one might aim to build models that do include humans, but for now there is a pragmatic justification (of complexity) for not including them. Other ecologists have felt that human activity is too unpredictable, contingent, or whimsical to be captured by ecological theory. For example, the urban ecologist Herbert Sukopp wrote that, for a long time "it was assumed that few plants or animals could survive in an urban setting and that urban animal and plant communities were products of coincidence. Attempts to discover patterns or reasons for such patterns were regarded as futile" (Sukopp 1998: 3–4).

Regardless of the reasons justifying it, the disciplinary practice of treating anthropogenic factors as externalities has given rise to two patterns of restriction in the science of ecology. A restriction on the choice of research site (that is, where ecologists conduct their research) and a restriction on the sorts of systems that are considered relevant for theory development and application.

With regard to the choice of research site, a recent meta-analysis of the ecological literature, attempting to quantify such trends in current ecology, showed a strong bias in favor of studies performed in "protected" areas (Martin et al. 2012). The authors argue that this trend is partly the result of an implicit bias among ecologists that nonhuman environments "better represent ecological and evolutionary processes and are therefore better objects of study" (Martin et al. 2012: 198). In other words, ecologists favor nonhuman environments because they take them to be the target of their analyses. Hobbs et al. (2006) provide anecdotal evidence of this trend when they report that a reviewer of their article about "novel ecosystems"—assemblages of species not previously occurring and often created through human-induced environmental changes—"indicated a lack of willingness to accept such ecosystems as a legitimate target for ecological thought or management action" (2006: 5).

The way that theory is developed and applied also shows the effects of treating anthropogenic factors as externalities. A good example of this is model-building and application in the renewed interdisciplinary field of urban ecology, which we further discuss below (Cittadino 1993; McDonnell 2011; Cadenasso and Pickett 2013). These ecologists are confronted by the challenge of building into their models the decision-making capacities of humans, which partly govern and determine the "shape" of urban ecosystems (Grimm et al. 2000; Marzluff et al. 2008; Pickett et al. 2001, 2008, 2011; Swan et al. 2011). They often lament the fact that many classical ecological models are poorly suited to their needs because such models were not developed to account for human-disturbed systems, like an urban center or its surrounding agricultural land, or anthropogenic factors at all (Collins et al. 2000; Alberti et al. 2003).

Traditional models of biological community formation and development, for example, include biotic variables such as the foraging and dispersal strategies of the species involved. These strategies are predictable enough that ecological community development follows a gradual and somewhat predictable series of changes known as succession. Humans, however, make this succession much less predictable from a traditional ecological standpoint, since their actions are often governed by individual whim or social forces—whether cultural, political, or economical—that are on a different disciplinary and explanatory level from what we commonly think of as ecological variables. In a seminal paper, Collins et al. (2000) write,

> An abandoned home site may begin to fill with plant growth—vegetative succession, to an ecologist—but redevelopment typically truncates the process that might otherwise fill the patch with trees and animals. Such redevelopment is an example of the single most important force of landscape change in urban areas: land conversion, driven by institutional decisions, population growth and economic forces. . . . Both the temporal and spatial scales of patterns in human-dominated ecosystems are likely to emerge from social forces far removed from foraging and dispersal strategies. (2000: 421, 423)

To ecologists, systems involving humans can appear unpredictable from an ecological standpoint because the variables that explain the dynamics of such systems are not a part of—not endogenous in—traditional ecological models. As with the choice of research site, it is well documented that restrictions on which target systems are considered relevant for theory development and application are underwritten by the artificial-natural distinction.

2.3.2 Economics and Human Society

Compared to ecologists, economists have frequently made an inverse restriction on which systems and factors are considered relevant, especially when it comes to the development of *pure* economic theory. In the Millian tradition, at least, the object of study was limited to a specific set of human or anthropogenic causal factors. Among most classical political economists, natural factors were invariably treated as "fixed" and, throughout much of the mid-twentieth century as well, such factors were, for a number of different reasons, omitted from aggregate production functions.

In *On the Principles of Political Economy and Taxation* ([1817] 1951), David Ricardo famously treated nonhuman factors as fixed and exogenously determined; in doing so, he arguably inaugurated the trend to discount the significance of nonanthropogenic factors in economic models and theories. In this work, Ricardo depicted "land" or "Nature" as an original and indestructible factor of production that was incapable of depreciation, and, unlike manufactured capital, did not require a period of production. His "corn model" in particular showed that scarcity in an economy was due partly to the diminishing returns to land, which he assumed was, although improvable with the requisite technology, both permanent and fixed in supply.

When John Stuart Mill ([1848] 2006) endorsed the Ricardian view of land in his *Principles of Political Economy*, the most influential text in political economy during the nineteenth century, he went a step further than Ricardo, driving a wedge between the social and natural realms by repositioning the entire core of phenomena studied by economists such that human agency is the proximate cause (Schabas 2005). Prior to Mill, political economists, such as Ricardo and Adam Smith, had characterized land in ways that made it a distinctive factor of production, but they still regarded the target phenomena of political economy to be part of the same natural world that was to be studied by natural philosophers (Davis 1989).

Mill's ([1848] 2006) basic analytical model was profoundly Ricardian. The science of political economy was to trace the laws of certain phenomena of society, which arose from the combined operations of human beings in the production of wealth. Pure political economy was to be inexact and separate, and its theorists were to employ the a priori method, or reasoning from assumed hypotheses. Because human beings were to be treated as creatures that solely desire to possess wealth, political economy abstracted from every other human passion and motivation, except for those perpetually antagonizing principles to the desire of wealth, such as the aversion to labor and the desire of the present enjoyment of costly indulgences. The reason for excluding natural factors, such as nature's "spontaneous productions," was mostly empirical. Toward the beginning of his *Principles of Political Economy*, Mill cites a variety of nature's products generated by purely natural causes, including the bees that produce honey and some caves that would be used by people for shelter. He states,

> It is to be remarked, that some objects exist or grow up spontaneously, of a kind suited to the supply of human wants. There are caves and hollow trees capable of affording shelter; fruit, roots, wild honey, and other natural products, on which human life can be supported; but even here a considerable quantity of labour is generally required, not for the purpose of creating, but of finding and appropriating them. In all but these few and (except in the very commencement of human society) unimportant cases, the objects supplied by nature are only instrumental to human wants. ([1848] 2006: 25)

While Mill was certainly familiar with the existence of original or natural objects, and how they grew up spontaneously, he believed that they were, on the whole, scant and relatively unimportant for the pure science of political economy. Nature's productions almost always require a significant amount of human labor to not only locate but also prepare and process for human consumption. On this account, aside from accommodating Ricardian land, other natural causes were not sufficiently efficacious to be considered a proper object of study. Pure social scientists were to account for a subset of intentional human activity, to the exclusion of every other social and natural factor. As Margaret Schabas (2005) has argued, Mill's economic theorizing rendered explicit the role of intentional human agency as the framework for standard economic analysis, a central feature of economic theorizing that was then perpetuated well beyond the neoclassical revolution.[2]

Not only have natural factors been generally excluded from *pure* economic theory in the Millian tradition, but it is well-known that during the latter half of the twentieth century, the majority of aggregate production functions and economic growth models posited two and only two factors of production: capital and labor (Solow 1957). This convention was enshrined in the Cobb–Douglas production function where the formula $Y = K^{\alpha}L^{\beta}$ represents total aggregate production (Y) that depends on capital (K) and labor (L). As for the status of land, a mainstay of classical political economy, this factor was eliminated from such formulations under the implicit assumption that manufactured capital could always serve as a substitute for any such natural factors. As the economists Klaus Hubacek and Jeroen van der Bergh observe, "by the second half of the twentieth century land or more generally environmental resources, completely disappeared from the production function and the shift from land to other natural inputs to capital and labour alone" (2006: 15). In fact, it had been tacitly assumed by economic theorists that reproducible capital was a near-perfect substitute for land (Nordhaus and Tobin 1972). Because capital is universally viewed as a factor of production that human agents produce, such formulations portrayed the production of all economically valuable goods and services as emerging from human agency alone. Even after the aggregate variable "resources" was introduced to such production functions after the oil shock of 1973, when economists were genuinely concerned with the question of sustained economic production in the face of a declining stock of fixed resources, this variable was largely taken to represent a conglomerate of inert materials that were capable of producing only when conjoined with the other two factors of production, capital and labor (Solow 1974; Dasgupta and Heal 1979).

To be clear, our claim is not that every economist has always wished to exclude natural factors from their theories or models. In fact, today, many have begun to wrestle with their Ricardian inheritance. The Cambridge resource economist, Partha Dasgupta, states that economists can no longer afford to assume that "Nature" is an "indestructible factor of production" (2010: 6). Moreover, there is growing transdisciplinary field of research known as "ecological economics" that has always emphasized the significance of including social and ecological factors in coupled or ecological-economic models for the purpose of prescribing public policy (Christensen 1989; Costanza 1989; van den Bergh 2001; Røpke 2005; Martinez-Alier and Røpke 2008). Rather, our claim is that pure economics, like pure ecology, has been defined around an appropriate set of objects of study, and that this seemingly innocuous decision concerning disciplinary boundaries has powerful implications for the structure and practices of the social science. Pure economics in the Millian tradition has chiefly focused on a specific set of human or artificial causes, but in Section 2 it was shown that the very distinction upon which the boundaries of this set depends—the artificial-natural binary—is problematic, both empirically and conceptually. Without this distinction, it remains an open question whether the objects of study traditionally analyzed by economists should remain limited to human factors alone.

2.3.3 Bringing Things Together

A useful exercise at this point is to imagine a world in which ecology and economics get on quite well without one another. This is a world that is made up of relatively independent human and natural systems: one set of systems, the object of ecology, consists of nonanthropogenic or "natural" factors; another set, the object of economics, consists of anthropogenic or human factors. In such a tidy world, these sciences, when operating effectively, make successful predictions and prescribe policy interventions without the need for interdisciplinary exchange.

Throughout much of the twentieth century, this imaginary world seems to have been implicitly assumed, as we have just shown. As ecologist Robert O'Neill and economist James Kahn wrote in 2000,

> The current paradigm in ecology considers humans not as a keystone species [a dominant species on which other species within an ecosystem depend] but as an external disturbance on the "natural" ecosystem… . The problem with this approach is that human beings are, in fact, another biotic species within the ecosystem and not an external influence.
>
> But the artificial isolation of humans from their ecosystem is not due only to the ecologists' paradigm. In the economic paradigm as well, human society, with all of its self-organization and self-regulatory activity, is represented as a separate "system." The ecosystem is viewed as external to society, providing goods and services, unoccupied territory in which to expand, and assimilative capacity to handle by-products… . The ecological paradigm isolates human activity in a box labeled "disturbances." The economic paradigm, in turn, isolates ecosystem dynamics in a box labeled "externalities." (O'Neill and Kahn 2000: 333)

Of course, this imaginary world is just that, a fiction. The real world is messy. Strictly speaking, there is no longer any part of the earth's surface that remains completely detached from human technologies, as we said above. The world today is a blend of anthropogenic and nonanthropogenic factors and prima facie this seems like a world in which exchange between ecology and economics would be a prerequisite to successful science.

2.4 Case Studies of Entangled Phenomena

Let's take stock. We've argued that, as a binary, the artificial-natural distinction is conceptually problematic, and so it must be understood as a continuum—that is, the world is populated by entangled phenomena that are more or less natural. We've argued that the distinction might still be worth drawing if doing so is empirically useful in particular research contexts. In the last section, we argued that ecologists and economists do, in fact, draw this distinction when choosing objects to study that are relevant for theory development and application and we've suggested that this has hindered interdisciplinary exchange. In this section we aim to undermine the usefulness of drawing this distinction. Because entangled phenomena are a *shared* object of study, it remains an open question whether it is best for each science to operate independently or engage in interdisciplinary exchange. Here, we argue that, if the goal of ecology and economics is to develop stronger predictions and explanations, and to develop better policy prescriptions, then the evidence suggests that interdisciplinary exchange is preferable, for epistemological and policy-oriented reasons, to these two sciences acting independently.

2.4.1 Biodiversity: Urban Ecology and Biogeography

Acknowledging the now pervasive influence of humans on the planet, many recent ecologists have begun to include human activity in their models. They want an ecology that applies to human-disturbed as well as undisturbed landscapes, but this forces them to take into account economic processes (Alberti 2008). We will consider two subfields where this interdisciplinary exchange is occurring: in urban ecology and in island biogeography.

A central problem for mid-to-late twentieth-century ecology has been biodiversity and its conservation. Understanding how to conserve biodiversity in urban areas is now recognized as a pertinent but complex problem. As a recent issue of *Science* dedicated to urban systems highlights, human urban populations are expanding in many of the world's richest biodiversity hotspots at an increasing rate (*Science*, May 20, 2016). Urban ecologists aim to mitigate the loss of native biodiversity by attempting to determine the conditions under which it could continue to flourish in human environments.

In "pristine" environments—and thus also in traditional ecological theory—spatial variation in plant diversity is often a product of heterogeneity in resource availability, importantly water and other nutrients (Hope et al. 2003). In arid landscapes, like Arizona, these resources are strongly influenced by geomorphic controls, like elevation. But in cities, such controls would seem to be much less powerful, since resource availability reflects social, cultural, and economic influences on urban land use (particularly as people create small "urban oases"). Ecologist Diane Hope and an interdisciplinary team at Arizona State University confirmed this prediction (Hope et al. 2003). Plant diversity throughout the greater Phoenix area was driven largely by socioeconomics.

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One interesting trend they discovered was what they dubbed: the "luxury effect." This was that median family income was highly predictive of variation in the biodiversity of plants in gardens. Plant diversity was on average double in neighborhoods with incomes above median compared to those neighborhoods with incomes below median. They also found that overall, such trends had increased total biodiversity for the region and increased diversity between sites (called "beta diversity"), but that this was largely due to native species being replaced by exotics introduced in urban areas.

Explaining trends in biodiversity in an urban setting—such as "the luxury effect"—requires building in anthropogenic factors, like the development and use of urban land. Without scientific representations that contain such factors, ecologists would not be able to explain these systems. But let us turn to a second example before drawing conclusions.

This example involves a less obviously human-disturbed system, and is important for this reason. The theory of island biogeography has long been the foundation for estimating extinction rates, predicting changes in biodiversity, and making policy recommendations (Diamond 1975; He and Hubbell 2011; Mendenhall et al. 2013; Thomas 2013). This theory explains and predicts the species richness (that is, number of species) that will be found on an island at equilibrium (that is, when rates of species immigration to the island and species extinction on the island balance out) (MacArthur and Wilson 1967; Diamond 1975).

The theory predicts that islands that are larger and nearer to the mainland will contain more species than islands smaller and further from the mainland. In a recent paper, Matt Helmus and colleagues (2014) tested the predictions of this theory for the distribution of Anolis lizard species among Caribbean islands. The theory predicts that a strong negative relationship will be found between species richness and geographic isolation: as a result of decreased inter-island immigration, more isolated islands will contain fewer species than less isolated ones.

This prediction is, however, false for Caribbean Anolis lizards because geographic isolation no longer solely determines immigration of new species. Instead, economic isolation mainly does so. Why? Because islands that receive more cargo shipments are more likely to contain lizard migrants from other islands, as the lizards move from island to island as stowaways on cargo ships. The result is that, for Caribbean lizards, geographic isolation is of less influence on biodiversity than economic isolation. Estimating economic isolation from global maritime shipping-traffic data, Helmus and colleagues found that when economic isolation was substituted for geographic isolation, the new biogeographic theory fit with their data: anole species richness was a negative function of economic isolation. They concluded that

> Unlike the island biogeography of the past that was determined by geographic area and isolation, in the Anthropocene … island biogeography is dominated by the economic isolation of human populations. [And] Just as for models of other Earth systems, biogeographic models must now include anthropogenic [variables] to understand, predict and mitigate the consequences of the new island biogeography of the Anthropocene. (Helmus et al. 2014: 543, 546)

Building anthropogenic factors into their biogeographic model also gives Helmus et al. a way to predict the effects of increasing economic traffic. If the goal is to protect exotic species from immigration of non-native species, then strategies for doing so will be ineffective unless they account for anthropogenic factors. Traditional theories of island biogeography alone do not provide helpful resources because the variables that make a difference are not included in the model. And models that account for anthropogenic factors are capable of novel predictions. For example, they predict that a removal of the US trade embargo on Cuba would result in the addition of one or two species of non-native lizards, a prediction that could not be made with traditional biogeographic theory.

What can we learn from these two examples? We don't think that the lesson is that ecologists should always take anthropogenic factors into account. Rather, that (1) there are cases in which not taking anthropogenic factors into account can be epistemically disadvantageous, it can diminish our ability to predict the dynamics of certain systems, and (2) that such cases are not limited to urban or agricultural settings, but range over cases of "pristine" ecology such as the distributions of Anolis lizards on Caribbean islands.

Many of the world's ecological systems are entangled phenomena, and to capture the features that are relevant to the processes and events we want to understand, and on which we want to be able to intervene, we have to represent the interaction between anthropogenic and non-anthropogenic factors. Although there will surely be cases in which excluding anthropogenic factors will be innocuous, entanglement implies that the question of whether anthropogenic factors should be included has to, at the very least, be asked.

2.4.2 Invasive Species in Yellowstone National Park

Nowhere are the epistemological and policy benefits of including ecological factors in economic models more evident than in the case of managing invasive species in Yellowstone National Park, Wyoming. When Yellowstone Lake was invaded by an exotic lake trout (*Salvelinus namaycush*), managers were worried that the growth of this species would significantly reduce the population level of the Yellowstone cutthroat trout (*Oncorhynchus clarkii bouvieri*), a native species that supports an inland fishery and a variety of nonhuman species, such as ospreys, pelicans, river otters, and grizzly bears. Chad Settle et al. (2002) specified a model for two separate systems: the economic system in Yellowstone National Park and the ecosystem in and around Yellowstone Lake. They asked whether their model, which combines details of an economic system and an ecosystem with explicit feedback links (economic and ecological factors are jointly determined) between them, yields significantly different results than a model that ignores those links. Their economic-ecological model, predicted that when ecosystems change, people will change their economic behavior, which in turn affects the ecosystem; correspondingly, any alterations in the ecosystem affects human economic behavior, including economic production possibilities.

Settle et al. (2002) ran three different scenarios with their model. The best-case scenario is a hypothetical one, when the lake trout are costlessly eliminated from

Yellowstone Lake. Under this optimistic scenario, the cutthroat trout would return to the lake as if the lake trout had never invaded in the first place. The worst-case scenario occurs if the lake trout are left to their own devices, which would have the effect of producing the smallest viable population of cutthroat trout. Their third policy scenario involved the National Park Service gillnetting the lake trout in order to reduce the risk to cutthroat trout populations.

Their results showed that a dynamic model that integrates ecological and economic systems *with feedback links between the two systems* yields significantly different results than when one that ignores these links. In every scenario they outline, cutthroat trout populations differ in both magnitude and survival rates once feedback is allowed between the two systems. For both the best-case and policy scenarios, Settle et al. (2002) predicted the steady-state population of cutthroat is lower without feedback than with feedback. Given the worst-case scenario, however, ignoring feedback leads to estimating a relatively high cutthroat population. Settle et al. concluded that "basing policy recommendations in Yellowstone Lake on data from models without feedback puts cutthroats at greater risk than would be true if feedback was explicitly considered" (2002: 309). In this case, the policy recommendations derived from a model without ecological factors would be worse than those derived from a model that connects the economic system to an ecological system with explicit feedback links.

As with the case of Helmus et al., the "exchange gain" of including an ecological system with explicit feedback links in the cutthroat trout example can be purchased rather cheaply. The latter does not require the development of a completely new theory of bidirectional collaboration between economists and ecologists. Instead, the model of Settle et al. merely required the addition of feedback variables that link two jointly determined systems. In this case, the economic variable that constitute the economic system is not jettisoned or even supplanted by another variable. Rather, the traditional economic theory, in this case, is retained, but in supplementary form.

2.5 Conclusion

Ideas of disciplinary purity have long reinforced a divide between the natural and social sciences. In this chapter, we have argued that, when it comes to the objects of study in ecology and economics, ideas of disciplinary purity have been underwritten by the artificial-natural distinction. We have tried to problematize this distinction, and thus disciplinary purity, both conceptually and empirically.

If we accept that a central goal of ecology and economics is to develop stronger predictions and explanations, and to develop better policy prescriptions, then a commitment to disciplinarity purity—for the sake of purity—can be a bad thing. Our two case studies have shown that an inflexible commitment to purity can entail predictions that are worse than those provided by interdisciplinary science. There are at least some cases in which interdisciplinary exchange between ecology and economics is preferable, for epistemological and practical reasons, to these two sciences operating independently. Our hypothesis is that, in a growing number of cases, entangled phenomena will require an interdisciplinary treatment.

To be clear, our aim has not been to argue that we should revolutionize the divisions of science, but to simply urge that they do not always reflect the evidence we have about our current world, and thus that the divisions themselves should not structure or determine interactions across disciplines. As Banu Subramaniam has recently written, disciplinarity tends to "obfuscate the inconvenient, avoid the uncomfortable, and promote ignorance about the profoundly powerful insights of interdisciplinary thinking" (Subramaniam 2014: 225). We agree with ecologists Boris Worm and Robert Paine that "the recognition of a novel geological epoch might also provide a new focus for ecology and the study of humans as a primary and dominant component of contemporary ecosystems," but we'd add that this will require interaction with social scientists, including economists (Worm and Paine 2016: 601). And, the reverse is true as well: it is to be expected that, in a growing number of cases, economics will need ecology, too. Indeed, in the age of the Anthropocene, without interdisciplinary exchange it is to be expected that ecology and economics would relinquish global relevance because the distinct and separate systems to which each pure science applies will only diminish over time.

Notes

1 Our modest claim is that the artificial-natural distinction makes interdisciplinary exchange less likely.

2 The Millian view of classical political economy has not only been significant for the trajectory of contemporary neo-classical economics but it has also been central to the works of leading contemporary philosophers of economics, such as Dan Hausman (1981) and Nancy Cartwright (see Hartmann et al. 2008) as well.

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Commentary: Toward a Philosophy and Methodology for Interdisciplinary Research

Michiru Nagatsu

1. Introduction

As DesRoches, Inkpen, and Green (DIG hereafter) point out, philosophers of science have been thinking about the nature and functions of scientific disciplines since Kuhn, but it is only recently that they started to systematically study how disciplines interact with each other. Not only historical but also other empirical methods such as qualitative case studies and quantitative methods (e.g., bibliometrics) have been utilized for this. However, few studies focus specifically on interdisciplinary interactions between economics and ecology, the practical relevance of which should be obvious to those who consider sustainability as a major challenge of our time. This chapter by DIG is thus a timely contribution both theoretically and practically. Since the chapter is well organized and its gist clear, in this commentary I will focus mainly on complementing and challenging its arguments.

In this commentary I will do two things: first, since interdisciplinarity is a relatively new topic in philosophy of social science, I will relate it to the existing debates in the field to guide the reader. I will argue that DIG's artificial-natural ontological distinction cannot be operationalized based on the "distance from human cause." Rather, specific ways in which human cause is special need to be unpacked. Second, I will highlight some ambivalence in their methodology of interdisciplinary research and suggest a more explicit (and perhaps conservative) methodology of interdisciplinary model-building, drawing on an ongoing study of interdisciplinary environmental sciences I have been working on with Miles MacLeod.

2. The Artificial-Natural Distinction

Disciplinary purity of economics and ecology that DIG highlight is closely related to the general problem of the social-natural distinction that philosophers of social science have been debating over many decades. Does social science have to be somehow different from natural science because of the ontological peculiarities of the human and the social, vis-a-vis the natural? As DIG state (p. 7), Mill's first concept of nature suggests a negative answer by noting that everything is natural including human agents and their intentional activities. This position corresponds to the underlying metaphysics of naturalistic philosophy of social science, which rejects any ontological divide between the natural and the social, and as a corollary, rejects any methodological divide between the two sciences. In contrast, Mill's second concept of nature isolates human agents and their activities as belonging to a specifically agential domain, thereby allowing the nonnaturalists to argue for a distinct methodology to study them, such as introspection and hermeneutic interpretation. I consider the standard strategy to ease this methodological tension is to say that the general standards of scientific reasoning such as rigorous causal inferences and severe testing of hypotheses apply to all sciences (since everything they study is natural in Mill's first sense), while at the same time accepting different methods to be epistemically fruitful in different domains, depending on their specific features. In short, the strategy is to erase the dichotomous artificial-natural distinction and redraw more fine-grained distinctions between different domains of science (see Mitchell 2009). This strategy is reflected in the current practice of many philosophers specializing in particular sciences (philosophy of biology, philosophy of economics, philosophy of physics, etc.), who work more or less independently from each other.

In philosophical and methodological discussions of interdisciplinarity, however, it becomes necessary to explicitly compare the domains of different sciences in question and their methodological ramifications. The challenge here, however, is not to demarcate scientific disciplines from unscientific ones or to rank them in terms of scientific rigor (as assumed in the naturalism vs. anti-naturalism debate in philosophy of social science) but rather to figure out epistemically gainful ways of linking distinct but closely related sciences. DIG's approach to this theoretical-methodological problem is pragmatic: they take the natural-artificial distinction to be a matter of degree, defined by "the *relative detachment* that the objects of study … have in relation to human agency" (p. 8, original emphasis). That is, the more remote the object is from human agency, the more natural and less artificial it is, and can be studied accordingly.

This pragmatic approach will probably suffice in most contexts, but it misses an important question of why the relative detachment from human agency matters in the first place. Take, for example, atomic bombs, one of the examples DIG give. The authors claim that atomic bombs are "deemed artificial since they were intentionally built by human agents and they possess a variety of anthropogenic features"(p. 8). In this sense atomic bombs are very close to human agency, but this proximity to human agency does not imply that the mechanisms of those bombs have to be studied differently from those of naturally occurring objects. Laws of physics should apply to both (in fact, first atomic bombs were invented by physicists such as von Neumann). Using proximity to human agency as a degree of artificiality is misleading if the natural-artificial distinction (or continuum, as suggested by DIG) is supposed to imply different methodological practices. One should ask why agency is special as a cause in the first place.

In this regard, philosophy of social science has several conceptual resources to offer. Take, for example, the distinction between interactive and indifferent kinds made by

Hacking (1999). Indifferent kinds include tigers and gold, which are traditionally called natural kinds, and called as indifferent because the categorized objects do not intentionally respond to the categorization. In contrast, interactive kinds include homosexuals and housewives, which, while having somewhat stable clustering properties, can change their behavior in response to the very categorization either by conforming to or deviating from it, thereby affecting the validity of the original categorization. More generally, the reactivity of human agency to scientific theorizing and manipulation, and its methodological implications, have been discussed in the literature (Cooper 2004; MacKenzie, Muniesa, and Siu 2007; Jimenez-Buedo and Guala 2016).

Game theory may also offer a useful perspective to understand the natural-artificial distinction. In game theory, an interaction between agents is modeled as a game consisting of players, their alternative actions, and outcomes as combinations of these actions. The crucial insight is that the outcomes for one player is affected not only by what she chooses but also by what others do, and vice versa. In contrast, Nature is used as a metaphorical figure whose actions are stochastically selected, independent of what other players think or do. In this framework, the fact that human agency affects Nature means that human players change Nature's available "actions" by modifying her conditions, as well as the probability distribution of which options she "chooses" in the future. But still her course of choices will not be affected by other players' preferences over outcomes and expectations about her choice because she has no beliefs nor preferences. As a result, human-Nature interactions are not fully analyzable by game theoretic concepts such as Nash equilibrium. But to the extent that human interactions mediated by such changes in Nature are the focus, game-theoretic analysis may be useful (cf. Bailey, Sumaila, and Lindroos 2010, review the use of game theory in fisheries management). In fact, human interactions mediated by technological changes (such as the invention of atomic bombs) have been intensively studied by game theorists. In this regard, it is probably not a coincidence that Thomas Schelling, the prominent game theorist who worked on the Cold War conflicts, was in his later career interested in how the geo-engineering technologies affect the politics of climate change.

The point of these examples is not that reactivity or intentionality crucially demarcates the artificial from the natural. Rather, the point is that there are different mechanisms through which agency affects the object of study, and that it is these specific mechanisms that pose specific methodological challenges. Reactivity or performativity of agents to social (scientific) categorization is one such mechanism.

In considering methodological and empirical implications of the Anthropocene, one should thus carefully consider the ways in which human agency makes a difference in the natural domains, and what kind of repercussions that difference has in the artificial or social domains. The Anthropocene might require natural scientists to pay more attention to human agency and how it affects their theorizing about the nature. Conceptual resources provided by the philosophy of the social sciences in the debates over the social-natural divide may be useful in this new challenge.

Another important dimension along which to distinguish the natural from the artificial is the relevance of values. In social sciences, values—what they are, how people try to achieve them, and whether a given means to realize them is optimal—have been central objects of inquiry. In contrast, in natural sciences human values have been always in the background of their modeling practice. Of course, philosophers of science have discussed value neutrality of natural science (Nagel 1979), more recently in the context of inductive risks and related discussions of epistemic roles of non-epistemic values (Douglas 2000). However, in these contexts values are categorized as external constraints on objective epistemic activities, rather than the main object to study and control. Can we maintain this division of labor in the Anthropocene?

Let's consider the case of ecology and economics. Can ecologists focus on empirical issues of how ecosystems operate while economists handle value-related issues of how to govern them? The current controversy around ecological economics seem to challenge this division of scientific labor. Economists have been criticized for treating natural capital as infinite and perfectly substitutable with capital and labor, following Ricardo, as DIG point out. Note, however, that the issue here is strictly empirical, that is, that natural capital is not infinite. And judging whether or not this is the case seems to belong to the expertise of ecologists, not economists, and therefore this critical interactions between ecologists and economists do not violate the abovementioned division of ecology and economics along the no value-value line.

A more substantial challenge to the division is found in ecological economists' work on the value of ecosystem services, which has been criticized by economists for being conceptually confused (Costanza et al. 1997, 2014). In this controversy, ecologists (or ecological economists) have directly dealt with the measurement of values (specifically, the dollar value of the planet Earth), which has invited economists' criticism about how they go about in this business. Generally speaking, economics has well-articulated and systematized models and methods to calculate and evaluate (and sometimes optimize) anthropocentric values as preference satisfaction from actual as well as potential use of the environment, while ecology does not have such an articulate system of valuation. Ecologists might even have an implicit notion of valuation that is at odds with economists' anthropocentric approach. And yet ecologists are increasingly aware of the need of more systematic ways of modeling values and trade-offs of competing values, in particular, in the context of natural resource management (Stephenson et al. 2017).

Why is the clear division of labor between natural and social scientists—the former study natural systems while the latter study value systems—being challenged? Perhaps, this is where the natural-artificial continuum helps us clarify what is going on. That is, since the environment is becoming more and more artificial in the sense of being affected and modified by human agency, it becomes necessary to explicitly evaluate its values in relation to what we can do to it. Even the most untouched wilderness needs to be valued in this way because our alternative actions make a difference to its destiny. If, in contrast, something is really natural in the sense that it is beyond human agencial cause, then there is no need for valuing it because its destiny is beyond our control. We now know that nothing is that natural on Earth.

To sum up this section, I discussed two implications of DIG's natural-artificial distinction-continuum. In terms of empirical methodological repercussions, this distinction does not seem to have much bite. It is more important to look at exactly how human agency is special as a natural cause, rather than how far things are from it. And debates in the philosophy of social science may provide useful empirical and conceptual resources. In contrast, the distinction-continuum is more directly relevant to social scientists' traditional monopoly of value-related investigations. As the environment becomes more artificial, the impact of human agency on it looms larger. Accordingly, natural scientists may need to explicitly evaluate consequences of human actions, or social scientists may need to incorporate unanticipated consequences of human actions down the causal chain in their valuation of alternative actions. Alternatively, they can engage in interdisciplinary research in which their expertise complements each other. I will discuss the methodology for this in the next section.

3. Methodology of Interdisciplinary Science

The main theses of DIG are that the current divisions of science, in particular between economics and ecology, "do not always reflect the evidence we have about our current world [the Anthropocene], and thus that the division themselves should not structure or determine interactions across disciplines" (p. 25). This is a variation of prevalent calls for interdisciplinary research. According to the widely cited definition, interdisciplinary research is

> a mode of research by teams or individuals that integrates information, data, techniques, tools, perspectives, concepts, and/or theories from two or more disciplines or bodies of specialized knowledge to advance fundamental understanding or to solve problems whose solutions are beyond the scope of a single discipline or field of research practice. (National Academy of Sciences 2005: 26)

According to this definition, two key rationales for interdisciplinary research are epistemic and practical. The epistemic rationale, simply put, is that since the world is not chopped up in a disciplinary way, fundamental understanding necessitates knowledge integration across disciplines. The practical rationale, in a similar fashion, states that since the real-world problems and concerns do not correspond to the academic disciplinary structure, real solutions need to come from research that cut across the disciplines.

These lines of reasoning can easily lead to a radical recommendation to dissolve disciplinary structure altogether, which has been criticized by sociologists of science such as Jacobs (2013). DIG clearly do not endorse such a radical position, but there is some ambivalence in their chapter. On the one hand, they propose a new ontology—the natural-artificial entanglement—as the basis for criticism of the status quo. And they favorably cite ecological economics as a "transdisciplinary field"—the term associated with radical knowledge integration that sits at the top of epistemic hierarchy, above inter-, multi-, and cross-disciplinarity in interdisciplinarity studies' parlance. On the other hand, DIG's actual examples of ecology-economics interdisciplinary exchange seems less radical. What do they have in mind when they say existing disciplinary divisions of labor should not structure or determine interactions between disciplines? What should substitute them?

In this regard, DIG's examples of methodologically "cheap" exchanges are suggestive. Despite their potentially radical call for interdisciplinarity, their cases that show tangible exchange gains are cases in which methodological costs of bilateral interactions are kept low. This suggests how difficult it is in practice to come up with completely new interdisciplinary models. In general, cognitive obstacles arising from ontological, conceptual, and methodological mismatches between disciplines make interdisciplinary research difficult (MacLeod 2018). Given such obstacles, using cheap exchanges to gain tangible epistemic and practical benefits is an effective and legitimate interdisciplinary model-building strategy.

MacLeod and Nagatsu (2018) call this strategy substitutive model-coupling. Substitutive model-coupling occurs when two fields share model templates of roughly similar structure for solving given classes of problems but use simplified methods and representations for components of those templates, which another field can handle with much more sophistication. MacLeod and Nagatsu (2016) have closely studied interactions between ecologists and economists addressing renewable natural resource management problems. Concerning modeling of values, ecologists use what economists would consider unreflective optimization criteria, with a history of relying on maximum sustained yield (MSY), which omits crucial economic considerations such as discounting of future value. Economists instead use as a management goal the maximum economic yield (MEY), that is, net present value of a flow of a given stock over an infinite time-horizon, taking more economic variables into account. Concerning modeling of tree growth, economists traditionally use crude biomass models for representing tree growth, which can then be replaced with more realistic process-based models with more variables and structure.

We find that substitutive model-coupling (including DIG's cases of variable substitution) can be a very effective interdisciplinary platform if it leverages off preexisting relations between variables to join components from different disciplines. Since, for the most part, the components are already in spatial and temporal alignment within the existing frameworks, scale problems do not arise. Furthermore, model construction tasks largely remain within the domain of each discipline, and thus under governance of their own disciplinary methods and standards. Since the modeling components are well integrated and relationships clear, feedback between components can be used to help better design components using information from across disciplinary boundaries.

There are a couple of differences between our case and that of DIG. First, our case involved coupling of models, not just substitution of variables. This means that the seemingly simple borrowing involved substantial mutual adjustments of details of models from ecologists and economists. In this sense, substitutive model-coupling is not as cheap as it seems. Second, and as a result of the first point, our case demonstrates tangible mutual epistemic gains, while the cases of DIG seem more like those of unilateral knowledge transfer than interdisciplinary exchange. We observed that economists' optimization became more realistic, and biologists' tree-growth model, which replaced economists' simpler biomass model, has also received corrective feedback from the economic model as a result of model integration. These differences in detail notwithstanding, our case and that of DIG both suggest that interdisciplinary exchange (or collaboration in our case) can yield tangible epistemic benefits with relatively low costs of model adjustments while maintaining the existing division of cognitive labor under favorable conditions such as relative similarity in modeling scales and concepts between disciplines.

Now, coming back to the other side of the ambivalence, namely DIG's radical proposal to go beyond the current disciplinary structure in interdisciplinary research, it seems that their cases do not motivate such a radical move. MacLeod and Nagatsu (2018) identify other types of integrative model-building strategies (modular model-coupling, integral modeling, and data-driven modeling), with their own affordances and limitations, but none of them support such a radical proposal, either. More generally, we argue that the convergence of interdisciplinary model-building strategies into these four types reflects a disciplinary way of effectively organizing problem-solving activities around a limited number of model-templates. We need to take such cognitive functions of disciplinarity more seriously.

I would like to add a final observation, which goes beyond the considerations of cognitive obstacles in interdisciplinary exchange. As I mentioned briefly in Section 2, economics and ecology may be different not only in epistemic standards but also in their value orientations. The latter kind of differences might, in distinct ways, make it difficult for the two disciplines to use common models. For example, MSY, which seems an unsophisticated management goal to economists, is still commonly used in fisheries science and in policy. Although there are probably cognitive (e.g., inertia) and many other contingent factors, differences in value orientations (e.g., economists' commitment to anthropocentrism and/or ecologists implicit rejection of it) may be one of the factors that need to be addressed. Although philosophers of science have studied the plurality of epistemic values, they have not systematically studied the plurality of non-epistemic values and how they affect epistemic activities, disciplinary or interdisciplinary. The interactions between economics and ecology provide fascinating cases to be explored for this purpose.

4. Conclusion

DesRoches, Inkpen, and Green's chapter is a fresh contribution to the growing field of philosophy of interdisciplinarity. Its virtues include (1) its explicit discussion of the history of the natural-artificial distinction and (2) its hands-on focus on concrete cases of interdisciplinary exchange between two practically relevant fields, economics, and ecology. Although much of conceptual issues—what interdisciplinarity is, how it is different from cross-, multi-, and transdisciplinarity (Huutoniemi et al. 2010), as well as what interdisciplinary exchange is and what this metaphor implies—are in the background and not explicitly discussed (see Grüne-Yanoff and Mäki 2014), DIG's focus on concrete and practical cases is informative. I see the future of philosophy of interdisciplinarity in this type of work, and in particular I suggest differences in value orientations across disciplines and their epistemic implications as an important topic to be explored in the future.

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