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Are humans disturbing conditions in ecology?

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Abstract In this paper I argue, first, that ecologists have routinely treated humans—or more specifically, anthropogenic causal factors—as disturbing conditions. I define disturbing conditions as exogenous variables, variables "outside" a model, that when present in a target system, inhibit the applicability or accuracy of the model. This treatment is surprising given that (1) humans play a dominant role in many ecosystems and (2) definitions of ecology contain no fundamental distinction between human and natural. Second, I argue that the treatment of humans as disturbing conditions is an idealization: since it is, and has long been, known that humans are pervasive, this treatment amounts to an intentionally introduced theoretical distortion. Finally, characterizing this treatment as idealization forces us to confront the question of its justification, and so, drawing on three different kinds of idealization, I evaluate how this treatment may be justified.

Keywords Disturbing conditions · Ecology · Anthropogenic factors · Nature · Idealization

Despite Darwin, we are not, in our hearts, part of the natural process.
Lynn White Jr, The Historical Roots of Our Ecological Crisis (1967)

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Introduction

In this paper I argue, first, that ecologists[1] have routinely treated humans—or more specifically, anthropogenic causal factors—as disturbing conditions. I define disturbing conditions as exogenous variables, variables "outside" a model, that when present in a target system, inhibit the applicability or accuracy of the model.[2] This definition employs terminology from the philosophical literature on modelling, which draws a distinction between the model (an epistemic stand-in) and the target (i.e., real-world) system. Second, I argue that the treatment of humans as disturbing conditions is an idealization: since it is, and has long been, known that humans play a dominant role in many target ecosystems, this treatment amounts in many cases to an intentionally introduced theoretical distortion.[3] Finally, characterizing this treatment as idealization forces us to confront the question of its justification, and so, drawing on three different kinds of idealization, I end with a discussion of how this treatment may be justified.

Before I turn to these arguments, consider two reasons why the practice of treating humans as disturbing conditions is surprising. First, today we are continually reminded that humans play a dominant role in the world's ecosystems. We are inundated by a burgeoning literature on the Anthropocene, which holds that human presence in the natural world is so pervasive it ought to mark a new geological era; environmental philosophy is increasingly concerned with the difficulties of distinguishing anthropogenic from non-anthropogenic features of the world; and issues from climate change to synthetic biology to legal patenting all raise questions about the divide between the human and the natural (Bensaude-Vincent and Newman 2007; Steffen et al. 2011; Sarkar 2012; Church and Regis 2012; Kaebnick 2014; Vogel 2015). It is estimated that 75 % 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). In other words, it seems unlikely that one could find a square inch on this "used planet" that is not in some way affected by human activities (Ellis et al. 2013).[4]

[1] And biologists in aligned disciplines, like evolutionary biology, natural history, biogeography, conservation biology, and so on. Throughout I will simply use "ecologists" for ease of diction.

[2] I rely here on a distinction between exogenous factors—variables that are "outside" the model and whose effects are not explained by the model—and endogenous factors—variables "inside" the model and whose effects are capable of being explained by the model.

[3] See O'Neill and Kahn (2000) for a similar argument: they characterize the treatment of humans as "external" in ecology as an *abstraction*. Following Jones (2005) it has been common to draw a distinction between *idealization* and *abstraction*, the former being the assertion of a falsehood, the latter being merely an omission. In this paper, I follow Weisberg's (2007) pluralist account and treat abstraction as a form of minimalist idealization, as I explain below.

[4] Although our current intellectual climate perhaps more fully embraces the presence of humans, historian Matthew Chew has shown that ecologists have long accepted that humans are pervasive (Chew 2009).

Given this state of affairs, it is striking that ecologists have routinely acted as if humans are outside nature (Shrader-Frechette and McCoy 1995; O'Neill and Kahn 2000; Kingsland 2005). Human-disturbed systems have been deemed uninteresting, pathological, and misrepresentative of, or different in kind from, natural systems *sans* humans. "From the perspective of a field ecologist examining a natural ecosystem," biologist James Collins and colleagues write, "people are an exogenous, perturbing force" (Collins et al. 2000, p. 416). Ecologists 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" (Worm and Paine 2016, 604). Some dub this the "'human-free' ecosystem paradigm" (Alberti et al. 2003, p. 1173), and a meta-analysis of recent literature demonstrated a clear bias among ecologists for field sites in which human presence was minimal (Martin et al. 2012). The assumption that human activities are exogenous is both long-standing and well-documented, and yet it is strictly false for much of the terrestrial globe.

The second reason why treating humans as disturbing conditions is surprising is, as Mark Sagoff has recently argued, that definitions of ecology, and aligned disciplines, do not contain any reference to drawing a fundamental distinction between human and natural (Sagoff, under review). The Oxford English Dictionary defines ecology broadly as "The branch of biology that deals with the relationships between living organisms and their environment" (OED Online 2016). The Ecological Society of America defines it as "the scientific discipline that is concerned with the relationships between organisms and their past, present, and future environments. These relationships include physiological responses of individuals, structure and dynamics of populations, interactions among species, organization of biological communities, and processing of energy and matter in ecosystems" (ESA 2016). And philosopher Gregory Cooper, in a book length treatment to defend an adequate definition of ecology, defines it as "the science that studies the Darwinian struggle for existence" (2003). None of the various definitions of ecology explicitly couple ecology with the "natural" world and exclude the world of humans, and yet in practice that distinction has seemed at times almost fundamental.[5]

My aim in this paper is to use resources from philosophy of science to describe and evaluate the practice of treating humans as disturbing conditions. This is not to say that humans are *always* treated as disturbing conditions, nor that, when they are, it is *always* disadvantageous.[6] Instead, I aim to provide an account of how humans have been treated in ecology and how this treatment may be justified. This paper is

[5] Three examples: the distinction plays a well-known and contentious role in invasion biology, since human dispersal seems to render a species "invasive" or "alien"—for critical recent accounts, see Chew and Hamilton (2011) and Pearce (2015); Mark Sagoff has shown how the distinction lurks behind the absence of treating domestic, agricultural, and zoo animals as objects of study, and determines the correct application of major concepts in ecology like "habitat," "species richness," and "niche" (Sagoff, under review); urban biologists often lament their fight to get human-dominated ecosystems taken as seriously as "natural" ecosystems (Alberti et al. 2003).

[6] In fact, very recent articles in ecology suggest a growing trend to stop treating humans as disturbing conditions (see for example, Worm and Paine 2016). The current paper thus also speaks to this recent literature and aims to provide one way of discussing the issues involved cogently and productively.

divided into three further sections. In the first section, I define "disturbing conditions," and argue that this concept captures the ways that some ecologists have treated humans. In the second section, I provide wide-ranging examples supporting this claim; I aim to show how widespread the effect of treating humans as disturbing conditions has been. Although it has been fairly routine to treat humans as disturbing conditions, I argue in the final section that this treatment is an idealization, and building on one recent case study, I offer a partial framework for evaluating how this treatment might be justified.

In the end, the impetus for this paper isn't that the practice of treating humans as disturbing conditions is never justified, rather it is that because the practice is fairly routine, its justification has been taken for granted.[7] I propose that the justification of this idealization be evaluated, rather than taken for granted, and this paper provides a partial framework for doing so.

Variable choice and disturbing conditions

Two related senses of "disturbing conditions"

When I claim that humans have been treated as disturbing conditions I mean this in a particular sense invoked by philosophers of science, or rather, as I will suggest, two related senses. Before going further, however, let me forestall a potential confusion: there is a distinction between (1) *humans as sources of disturbance in the ecological sense* and (2) *humans as disturbing conditions in the philosophical sense*.[8] In ecology, the term "disturbance" has a technical meaning: "a relatively abrupt loss of biomass or alteration of ecosystem structure or ecosystem function" (Walker 2012, p. 1). In this sense, disturbances can be natural—caused by fire, flood, or earthquake—or anthropogenic—caused by oil spills or urban sprawl. Ecologists do sometimes theorize anthropogenic causes of ecological disturbance, and when they do so, they are *not* treating humans as disturbing conditions, since they are treating them as endogenous variables.[9] In this paper, it is the practice of treating humans as disturbing conditions—as exogenous variables—in the philosophical sense described below, and not as sources of ecological disturbance, that I will be directly evaluating.

[7] Ecologist Robert O'Neill, winner of the 1999 Robert MacArthur award, also raised this worry in 2000 (see O'Neill and Khan 2000).

[8] Thanks to an anonymous reviewer for pointing out this potential confusion.

[9] I am not claiming that treating humans as ecological disturbances is immune from criticism, only that it is not my target in this paper. One reason to be critical of treatments of humans as ecological disturbance is that humans are not external to ecosystems in the same way that earthquakes are. As O'Neill and Kahn write, "The problem with this approach is that human beings are, in fact, another biotic species within the ecosystem and not an external influence" like most other sources of ecological disturbance (O'Neill and Kahn 2000, p. 333). Why, they ask, are humans the *only* species exempt from being integrated into a "self-organized, self-regulated entity called the ecosystem"?

Experimental disturbing conditions

In philosophy, the phrase "disturbing conditions," sometimes "disturbing factors," is invoked in two ways (see Reutlinger et al. 2014). First, it is a common presumption among scientists and philosophers of science that an experiment only counts as a proper test of a generalization, model, mechanism, or hypothesis when so-called "disturbing conditions" are controlled for or absent (Hempel 1988, p. 154; Morgan 2012, p. 278). Rudolf Carnap perhaps put it best: a "test procedure […] should not be taken as absolutely reliable, but only with the tacit understanding 'unless there are disturbing factors' or 'provided the environment is in a normal state'" (Carnap 1956, p. 69). Disturbing conditions, in this sense, are confounding variables: variables correlated with both dependent and independent experimental variables that can mislead researchers with regard to which variables are difference-makers (that is, which variables play which causal roles).

Let's say, for example, an ecologist hypothesizes that one large island will contain more species than several small islands equaling the same total area. When she sets up an experiment to test her hypothesis, she will want to control for disturbing conditions such as distance from a mainland shore, since this variable is also known to have an influence on species richness (the number of species found on an island). Without controlling for, or "shielding from," this disturbing condition, her test procedure would be unreliable in the sense that a difference in species richness which she would attribute to a difference in areal subdivision, may in fact be the result of a difference in distance from the mainland. Experiments are thus often explicitly designed in such a way as to eliminate known disturbing conditions (Cartwright 1999, 82*ff*; Morgan 2012).[10] To avoid ambiguity, I will label this first sense: *experimental* disturbing condition.

Theoretical disturbing conditions

A slightly different sense of "disturbing condition" arises in theoretical discussions, frequently with reference to laws of nature, less-invariant generalizations, models, mechanisms, etc., and whether they contain or require *ceteris paribus* clauses (Hausman 1992; Earman and Roberts 1999; Lange 2002; Craver and Darden 2013).[11] Christopher Eliot provides an apt characterization of this sense of "disturbing condition": those "factors which interfere with the applicability of a model (or model system) to the members of the designated set of [targets] to which a model can be, is, or is intended to be, applied" (Eliot 2004, p. 3). In this sense,

[10] Put in a different way, one might likewise say that according to those who consider humans to be disturbing conditions, anthropogenic forces are treated as giving rise to experimental artifacts (i.e., phenomena observed during an experiment that are not considered to be "naturally" occurring, but instead a result of the experimental procedure itself).

[11] Craver and Darden (2013) provide the most detailed account of this sense of disturbing condition with regard to mechanisms. What I call, by the traditional name, "disturbing condition," would by their account fall under two other categories: "inhibiting conditions" and "nonstandard conditions." For the present essay, I will stick with the simpler, common vocabulary of "disturbing condition.".

laws or models are said to "apply to" target systems only in the absence of disturbing conditions.[12]

For example, the species-area model in biogeography ($S = CA^z$; where S is the number of species of a given taxon on an island, A is the area of the island, and where C and z are parameters), may *not* accurately describe a set of real islands when these islands differ, not only in size, but also in their habitat heterogeneity (MacArthur and Wilson 1967; Whittaker 1998). Habitat heterogeneity can thus be a disturbing condition relative to the species-area model. I will label this second sense: *theoretical* disturbing condition.

The philosophical motivation behind introducing either of these senses of disturbing conditions is typically a straightforward point about modelling (whether theoretical or experimental) and variable choice. Carnap conveniently drew attention to this point in the quotation above. In investigating and attempting to explain a particular natural system or phenomenon (what I am calling the "target" of the model), scientists must make routine boundary decisions: they must decide what counts as a "normal" "part" of the system or phenomenon under study, and what counts as "outside" such a system or phenomenon.[13] We call the normal parts endogenous factors or variables, and the outside parts exogenous factors or variables. Typically, when we refer to disturbing conditions, we are drawing attention to a class of exogenous factors that, when present, inhibit the applicability or accuracy of a model, mechanism, theory, etc.[14] And when we say that a particular model, theory, or mechanism, etc., applies to a natural (or experimental) system, we mean to say that no disturbing conditions are present.

Humans and their effects as disturbing conditions

All experimental and theoretical work, of course, invokes disturbing conditions, whether explicitly stated or not. What seems distinctive about ecology is the practice of treating humans in particular as disturbing conditions. Humans may be disturbing conditions in multiple ways as specific anthropogenic causal factors can have effects at local (such as land conversion driven by urban or rural development) to continental (such as the transformation of the western plains of the United States from tall grass prairie to agricultural farmland) to global (such as climate change) scales.

Let me start with a simple example. Beginning with experimental disturbing conditions, think of an ecologist who is attempting to experimentally test the species-area model. A target system composed of a set of islands that are highly heterogeneous with respect to habitat would not be the correct place to test this model because it simply doesn't apply when this disturbing condition is present. Likewise for human interference. Think of another set of islands, some of which, irrespective of size, are settled by humans. These humans spray the islands upon which they live with insecticides. This set of islands would again not be the correct

[12] For a detailed discussion of the "applies to" relation, see Weisberg (2013).

[13] See Craver and Darden (2013) and Peschard and van Fraassen (2014) for recent discussions.

[14] We might alternatively put this in a related, causal vocabulary, and say that disturbing conditions are those conditions under which a generalization fails to be invariant (Woodward 2001).

place to test the species-area model because it does not apply when this disturbing condition is present. This is an extreme example, but the point is that human influence on an ecosystem, even in the much less extreme cases discussed below, is routinely assumed to be a disturbing condition in this sense.

Explaining how humans can be theoretical disturbing conditions is almost redundant given what was just said. We should not appeal to the species-area model to explain or predict the species richness of islands that have an obvious disturbing condition present. This disturbing condition may be in the form of habitat heterogeneity or in the form of humans interfering by spraying insecticides.

A final, general point about disturbing conditions. As I have said, phenomena that are treated as disturbing conditions in ecological models are often phenomena that are thought not to be a part of the normal or typical state of the system under study. It makes sense to count earthquakes as disturbing conditions—as an exogenous variable relative to a model used to explain and predict the dynamics of a target ecological system—with regard to ecological systems that rarely encounter earthquakes. Earthquakes, in this case, are not part of the *statistically* typical state of the system, and are thus considered a disturbing condition. If we predict—to use a simple, overworked example—that the future state of an ecological system will involve a much higher relative frequency of fast running zebras, but this turns out not to be the case because a rare earthquake kills all the fast-type zebras, we would not throw out the model that made this prediction. We would say that the model didn't apply in this atypical case because a disturbing condition was present.

Similarly, when humans are treated as disturbing conditions this is sometimes because anthropogenic causal factors, however operationalized, are not considered to be part of the, as Carnap put it, "normal" state of the system studied; they are, in other words, treated as rare or atypical and thus exogenous. It's worth pointing out that Carnap's notion of "normal" can be understood in two different senses: statistical and functional (Wachbroit 1994; Siipi 2008, pp. 88–89). Statistical normality is a descriptive notion and refers to the state of the system being statistically common or usual, or if it can be mathematically measured, close to the mean. Functional normality is a normative notion and refers to the system being in a state of proper, correct, or healthy functioning. Even if humans are widely acknowledged to be parts of statistically normal ecological systems, as I claimed above, they may still be denied the status of parts of *functionally* normal ecological systems, and thus treated as disturbing conditions for this reason. In "Anthropogenic factors and idealization" section I consider a disciplinary reason why humans are treated as disturbing conditions: that humans and their effects are properly studied by the social, not the ecological, sciences. This reason could be underwritten by defining humans as external to functionally normal ecological systems.

Ecology and anthropogenic activity

Let me turn now to a few wide-ranging illustrations of how ecologists have treated humans as disturbing conditions. These depict diverse aspects of what I take to be a pretty common picture, a fairly routine habit of thinking. I will split these

illustrations into four categories: (1) language, (2) choice of research site, (3) experimental practice, and (4) theory development and application.

Language

Ecologists and evolutionary biologists routinely invoke specific phrases that indicate whether a study was done in the absence of humans—phrases like "in the wild," "in nature," "protected"—and such phrases are widely, if contentiously, considered to be important qualifiers (Shrader-Frechette and McCoy 1995; Haila et al. 1997).[15] There is, of course, the justifiable question of how much experimental intervention is too much, but these phrases are doing more than simply keeping this concern at bay. The ubiquity of such phrases is a testament to the importance many biologists attach to things they take to be more natural, and as such they can be indications of treating humans as experimental and theoretical disturbing conditions. Such qualifiers are also noteworthy and striking in part because they are uncharacteristic of the so-called exact sciences—appending the phrase "in nature" to one's analysis might strike a physicist as tautological, or at least unnecessary, given that in a sense everything happens *in nature* (see Kroes 2003).[16]

On the flip-side, "artificial" and its synonyms can have pejorative undertones. When the geneticist Theodosius Dobzhansky referred to another biologist's work derisively as being based on merely "laboratory flies!," he was both describing the fact that these flies were bred in captivity rather than taken directly from the field *and* exploiting the negative connotations of the term "laboratory," a place of artificiality (Provine 1981).[17]

Choice of research site

It is well-documented that ecologists tend to favour research sites that are purportedly undisturbed by humans. 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. Those organisms

[15] For example, a www.scopus.com analysis of article titles containing "in nature" from 2005–2015 shows that of 579 articles, 77.9 % of those articles were in the "life sciences."

[16] This also suggests to me that nature for the physicist is different from nature for the biologist, but this discussion would bring us far afield from the present analysis (see Wachbroit 1994 and Dussault 2016). Relatedly, see Brandon (1994, 1996) on why experimental (evolutionary) biologists invoke the phrase "in nature," while physicists do not.

[17] Similarly, in their influential paper discussing the advantages of studying ecological processes in the laboratory using the flour beetle, Neyman et al. (1956) found it necessary to discuss the extent to which such experiments are artificial, since this was a common criticism. The criticism is unfounded, they argued, when "'artificial' is intended to imply triviality," rather than simply refer to the study as being unrealistic, which they said was nonetheless true (1956, p. 45). In other words, they worried about their research being characterized as "artificial" because this characterization often had a normative side: it implied that such research was unimportant, rather than just unrealistic.

> that did survive had distribution patterns that were merely coincidental and were thus considered undeserving of study (McDonnell 2011, p. 7)[18]

A recent meta-analysis of the ecological literature, attempting to quantify such trends in current ecology, showed a strong bias in favour of studies performed in "protected" areas—to be clear, the term refers to protection from humans (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, p. 198). This is a clear statement that some ecologists themselves worry that their colleagues treat humans as disturbing conditions. 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, p. 5). It "is hard to make lemonade out of these lemons," the reviewer wittily concluded (Hobbs 2006, p. 5).[19] Another ecologist responded that Hobbs et al.'s "novel ecosystems" were too artificial, the academic-ecological equivalent of "eating at McDonalds" (Marris 2009, p. 453).

Experimental practice

Disagreements over the usefulness of interventionist-experimental methods in biology have long been intertwined with a general, one might say inscrutable, skepticism that *human-created* biological systems are importantly different from *natural* biological systems. In this context, humans have sometimes been treated as experimental disturbing conditions. The ecologist Larry Slobodkin wrote,

> the distinction between theoretician, laboratory worker and field worker is that the theoretician deals with all conceivable worlds while the laboratory worker deals with all possible worlds and the field worker is confined to the real world. The laboratory ecologist must ask the theoretician if his possible world is an interesting one and must ask the field worker if it is at all related to the real one. (Slobodkin 1961, pp. 152–153)

Slobodkin's characterization of the relationship between styles of work in ecology showcases a long-standing undercurrent: that there is something "unreal" about the laboratory.[20] In ecology, laboratory experiments are often performed using

[18] Ecologist Jari Niemelä writes that "Traditionally, ecologists have ben reluctant to study urban ecosystems, because they have been regarded as inferior to less disturbed rural ones" (Niemelä 1999, p. 3). Niemelä doesn't specify further the reason for their inferiority.

[19] See similar anecdotes by biologists Massimo Pigliucci and Gretchen Daily in Pennisi (2000) and Voosen (2013), respectively, and in O'Neill and Kahn (2000). See also Leonelli (2007, p. 212).

[20] The history of biology is full of examples of where things *made* seem to lack the reality of things *found*. In the eighteenth-century, the naturalist Georges-Louis Leclerc, Comte de Buffon wrote, referring to domesticated organisms, that "it is the duty of the naturalist […] to separate artifice from Nature; and never to confound the animal with the slave, the beast of burden with the creature of God" (Inkpen 2014). Domesticated organisms were the results of degeneration—the sheep had been "denatured" by humans—and thus both opposed to Nature, but also lacking the reality of natural species.

microorganisms or small arthropods kept in bottles because this set-up is cheap, easy to maintain, and easy to control genetically and environmentally. Critics contend that these laboratory conditions go little way towards answering ecological questions (Crone and Molofsky 1998; Odenbaugh 2006). Ecologist Steven Carpenter writes, for example, "a molecular biologist who isolates ribosomes is working on ribosomes; an ecologist who isolates organisms in bottles may not be working on communities and ecosystems in any relevant sense" (Carpenter 1999, p. 678). According to Carpenter, although the objects of molecular study can and should be experimentally isolated, to do so with the objects of ecology is, in a sense, to sacrifice those objects altogether; they are altered from their "natural" or "normal" state so fundamentally that little worthy of ecological study remains.

Some, following Diamond (1986), argue that the most fruitful types of experiments are so-called "natural" experiments, in which biologists study the effects of perturbations (e.g., storms, droughts, fires, etc.), that are not human-induced. Such "natural" experiments are not merely descriptive exercises and should thus not be confused with natural history. Rather, they test specific hypotheses, involve significant manipulations—albeit "natural," rather than human-induced—and aim at determining the causal factors at play in a particular biological process (Dunning 2012). Their purported advantage is realism, by which is meant that the study's results have at least one natural application—even if only the study situation itself. In defence of natural experiments, one sometimes encounters the belief that "Any [human experimental] manipulation is also a disturbance," as one biologist bluntly put it, *not* referring to the ecological sense of disturbance (Miller 1986, p. 122). In other words, some ecologists may be justifiably skeptical of overly-manipulative experimental methods because they see human-created systems as problematically misrepresentative of the "natural" systems they take to be their target systems.

Theory development and application

The way that theory is developed and applied can show the effects of treating humans as disturbing conditions. A good example of this is model-building and application in the renewed interdisciplinary field of urban ecology (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).[21] They often lament the fact that many classical ecological models are poorly

[21] As an anonymous reviewer helpfully pointed out, I should acknowledge here that it is the job of urban biologists to build anthropogenic factors into their models and they have been doing so since the 1990 s. So, there is a contingent of biologists who never treat humans as disturbing conditions. But it should be noted that (1) this is a small, yet growing, minority of biologists, and (2) they are very interdisciplinary, and often see themselves as such (see McDonnell 2011), drawing on, and being employed in, ecology, urban planning, architecture, geography, economics, political science, sociology, anthropology, and so on. They are not, in other words, the "pure" ecologists I have in mind in this paper, and as such, they often provide a (sometimes needed) critical perspective.

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. (2000, p. 421)

> In ecosystems not dominated by people, individual plants, dispersal distances and the scale of disturbance can define the spatial scales over which processes are influenced and patterns emerge. 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, p. 423)

To ecologists, systems involving humans can appear unpredictable from a ecological standpoint because the variables that explain the dynamics of such systems are not a part of—not endogenous in—classic ecological community models. It is thus a primary goal of the field of urban biology to remedy this explanatory lacuna and integrate the social and the biological.[22]

I might go on multiplying examples, but I wish only to establish the fact that ecologists struggle with target systems that include anthropogenic factors, and fairly routinely treat humans as experimental and theoretical disturbing conditions. This can be seen in the language they invoke, the research practices they employ, and in the way theory is developed and applied.

Anthropogenic factors and idealization

Given that humans are dominant members of most ecosystems on earth, I argue that, in many cases, the practice of treating humans as disturbing conditions is an idealization: an intentional theoretical distortion that is in fact false of many target systems. The relevant questions for any idealization are (1) whether the factor that was omitted from, or distorted in, the model would substantially change the

[22] This is one of the goals set for the recently funded NSF project: Dynamics of coupled natural and human systems (or CHANS). See Liu et al (2007).

predictions or explanatory power of the model if it had been taken into account, and (2) if the predictions or explanatory power are substantially changed, what we should do about the idealization. In this section, I evaluate three different types of idealization that provide three possible avenues to justify the practice of treating humans as disturbing conditions. The first is *disciplinary* idealization, the second is *minimalist* idealization, and the third is *Galilean* idealization.[23] My evaluation of all three justifications builds on a recent scientific study—similar to the fictional biogeography examples I provided in "Variable choice and disturbing conditions" section—that sheds light on the specific, potential disadvantages of this idealization.

Reason 1: disciplinary boundaries

The first reason is the most straightforward: anthropogenic factors should be treated as disturbing conditions because non-human nature is, and should be, our target system. Treating humans as disturbing conditions is thus, what we might call, a *disciplinary idealization*: ecologists study ecological variables, which do not include anthropogenic factors, and leave anthropogenic variables to the social sciences, such as economics. One difficulty that makes this potential reasoning hard to accept is that there is little left that we could uncontroversially call non-human nature; many systems on earth are human-natural coupled systems (O'Neill and Kahn 2000; Liu et al. 2007). Thus, if these ecological studies are supposed to be a foundation for target-directed modelling and experimenting, they provide analyses whose targets may not actually exist or may be severely limited. And some ecologists recognize this: Martin et al. (2012) write, "confining ecology to the non-human world sharply curtails its global relevance, because there are few, if any, places on Earth that have not been impacted by human activity" (2012, p. 200).

Within ecology itself this sort of reasoning manifests itself in the form of the "pure" versus "applied" divide. Biogeographer James Brown diagnoses the problem as follows:

> The natural sciences, in their search for rigor and objectivity, have tended to view everything influenced by humans as "artificial" and everything else as "natural." Nowhere is this more apparent than in the recent tradition of ecology. Most ecologists study wild organisms in "natural areas" where human influence is minimal. The study of humans and their interrelationships with the rest of the natural world has been left to the "social" and the "applied" sciences, both of which have been viewed with disdain by many of those who practice "pure" ecology. (Brown 1995, p. 205)

Brown recounts what he sees as a dilemma that exists in ecology: either one is a "pure" ecologist or one works on human-disturbed or "artificial" objects. Brown suggests that the bias for "natural areas" has roots in this dilemma, which he sees as fallacious and detrimental.

[23] "Disciplinary idealization" is my own term. Minimalist and Galilean types of idealization have become standard in the modelling literature following Weisberg (2007, 2013).

To flesh out this discussion, consider an example: E. O. Wilson exemplified this reasoning in the 1970s when describing himself and ecologist Robert MacArthur as "hard ecologists" interested in "simplifying theory as much as possible" (Chisholm 1972, pp. 181–182). He contrasted their school of ecology with that of "complex theory ecologists," like Paul Ehrlich, C. S. Holling, and Kenneth Watt. These latter ecologists had a "political agenda" and accused him and his colleagues of "playing with ideas and hiding out in our ivory towers." They believe, Wilson said, that "ecologists should harness themselves to the movement of applied ecology, plotting the management of the world's fisheries, re-routing water systems, managing the world's forests, and so on." To Wilson, "these are social engineering problems, and there's not much ecology in them. There are basic ecological *principles* involved, but that's all." Where, we might ask, did these ecological principles come from? In fact, Wilson speculates, "it's mostly been the simple theorists who have made new discoveries about how nature actually works." Wilson's reflections demonstrate how this dichotomy between "pure" and "applied" ecology can delineate two schools of ecology, and be mobilized to defend the study of "natural areas" at the expense of the human interrelationship with nature, ultimately reinforcing the treatment of humans as disturbing conditions.

Wilson's contention that Ehrlich and his colleagues were not doing ecology depended on at least two assumptions that are worth highlighting. The first is disciplinary: pure ecology does not involve the study of humans as parts of ecosystems; these are social engineering problems. What Ehrlich and colleagues are doing is thus not purely ecology. The second is pragmatic: in fact, says Wilson, more new discoveries (of "basic ecological principles") have been made by pure ecologists and this proves its intellectual worth over applied ecology. In other words, what Ehrlich and colleagues are doing—whether ecology or not—hasn't proven itself as a useful science. (I will return to this pragmatic reason in "Reason 2: representing core causal factors" section.)

As a claim about utility, Wilson's argument is hard to adjudicate; from our current standpoint, forty years on, it's clear that both sets of ecologists have been extremely influential. More interesting for the present section is Wilson's point about disciplinary identity, that is, what sorts of biological systems *should* ecology attempt to explain? Again, this is in part also an issue of choosing what should count as the "normal" state of the target system under study and of delineating endogenous from exogenous variables. Humans may be statistically normal, and yet still not be a part of ecologically, functionally normal "natural areas." To repeat, Wilson's point is that ecology (or *pure* ecology) is concerned with "how nature actually works," whereby 'nature' is synonymous with 'non-human nature.'[24]

A recent biogeographical study nicely illustrates the problems with treating humans as disturbing conditions on the basis of delineating "pure" from "applied" biology: doing so renders inexplicable and unpredictable even those ecological systems that many would consider to be "undisturbed" or relatively undisturbed (Helmus et al. 2014). In other words, there are types of biological systems which

[24] There are of course many conceptual problems with deciding what should count as an area's state prior to human interaction which I have side-stepped in the present essay (Cronon 1995).

Wilson would describe as "pure" that cannot be understood without taking humans into consideration.

The theory of island biogeography, a seminal theory in ecology and conservation biology, 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).[25] Helmus et al. (2014) recently attempted to test the predictions of this theory for the distribution of *Anolis* lizard species among Caribbean islands. The theory predicts a strong negative relationship 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. What they found is that this prediction is no longer true for Caribbean *Anolis* lizards because geographic isolation no longer determines immigration of new species. Instead, it is *economic* isolation that does so: islands that receive more cargo shipments are more likely to also contain migrants from other islands, as lizards can move from island to island as stowaways on human cargo ships. That is, today, in the case of Caribbean lizards, geographic isolation is of less influence on biodiversity than economic isolation. Estimating the economic isolation of Caribbean islands from global maritime shipping-traffic data, Helmus et al. found that when economic isolation was substituted for geographic isolation, the new biogeographic theory fit with their data: anole richness was a negative function of economic isolation. The authors concluded that, in the Anthropocene, island biogeographic models must include anthropogenic factors, like economically-influenced trade, as well as the more traditional non-anthropogenic factors, like island area, to explain species richness.[26]

Building anthropogenic factors into their biogeographic model also gives them a way to predict—with the ultimate aim of mitigating—the effects of decreasing economic isolation. As economic isolation decreases, for example, we must increase our efforts to protect exotic species from the immigration of non-native species (if that is the conservation strategy adopted).[27] Traditional theories of biogeography that do not include anthropogenic factors may provide few resources—or worse may actually suggest inapplicable, harmful strategies—when it comes to the conservation of these Caribbean lizards because the variables that make a difference are not included in the model. The only way to know whether economic isolation is going to increase or decrease is to follow economic trends. For example, the US embargo increases Cuban economic isolation, and a cessation of the embargo would decrease isolation and increase species richness: Helmus et al. predict that Cuba

[25] For further discussion of this and related biogeographic models, see Inkpen (2016).

[26] Another interesting finding, which I haven't discussed, is that species-area relationships were strengthened in the Caribbean. That is, although geographic isolation does not explain species richness, as the traditional model predicts, the relationship between species richness and island area is even stronger than the traditional model predicts. This is because larger islands have larger banks which have more people and thus more ports (Helmus et al. 2014, p. 546). Again, it is economically influenced trade that matters to Anole species richness.

[27] Island biogeography theory has long been used to inform environmental policy decisions about the size of nature preserves and biodiversity; see Diamond (1975) for a seminal paper.

would rapidly gain between 1 and 2 non-native anole species. Such a prediction could not be made with the traditional (non-anthropogenic) theory.

What the Helmus et al. case demonstrates is *not* that ecologists should always take anthropogenic factors into account in every case. What it shows is rather that (1) there are cases in which not taking anthropogenic factors into account can be epistemically disadvantageous, such as diminishing our ability to predict the dynamics of certain systems (that is, the practice of treating humans as disturbing conditions has undermined our ability to explain and predict the dynamics of some ecosystems), (2) that these cases are not limited to urban or agricultural settings, but range over cases of "pure" ecology—human presence leaves a trace even among the distributions of *Anolis* lizards on Caribbean islands—and (3) that whether anthropogenic factors should be included is an empirical matter, not something that should be routinely or a priori decided against.

Of course, one could insist that these sorts of cases are not, and should not be, under the purview of ecology, again, as a matter of disciplinary identity (perhaps referring again to the distinction between statistically and functionally normal ecosystems). One could argue that anole lizards in the Caribbean are actually part of a human-natural coupled system now, and thus pure biogeographers should not worry about them. Or that the theory does not apply to anole lizards in the Caribbean because a disturbing condition was present, that is, humans. While this bite-the-bullet approach is available, it seems to me to be too extreme, as it judges reasons of disciplinary identity to be much more significant than epistemic reasons, such as predictability or explanatory power.

Reason 2: representing core causal factors

Another way in which the practice of treating humans as disturbing conditions may be justified is as a form of minimalist idealization. This is the practice of building models (whether experimental or theoretical) that only include the core causal factors that give rise to a particular phenomenon (that is, that "make a difference" to the occurrence of the phenomenon). Recall the theory of island biogeography invoked by Helmus et al. (and subsequently deemed inapplicable to the case of Caribbean anoles): one might justify building and using this model by claiming that the model represents what are thought to be the core causal features of a biogeographic system (or many such systems) that influence species richness—these are the factors that really matter to species richness, regardless of whether the target system contains other factors not contained in the model (that is, human influence may be considered causally innocuous, even for systems in which humans play a dominant role). Perhaps this explains why some ecologists have thought that nonhuman environments "better represent ecological and evolutionary processes and are therefore better objects of study" (Martin et al. 2012, p. 198).

We already know that this is not a justifiable reason when it comes to Caribbean anoles. Economic isolation, an anthropogenic variable, *was* a core causal feature that did determine the outcome of species richness, and was therefore not causally innocuous. The practice of treating humans as disturbing conditions would in that case undermine our ability to predict and explain the target system. Still, we can not

discount that there will be other cases in which ignoring anthropogenic variables is innocuous, even cases involving the theory of island biogeography. Regardless, as the Helmus et al. case demonstrates, these are empirical matters that should be decided on a case-by-case basis, and not assumed to be the case in advance, especially given the pervasiveness of human-natural coupled systems.

Reason 3: tractability

Finally, a third way in which the practice of treating humans as disturbing conditions may be justified is as a form of Galilean idealization. This type of idealization is the practice of introducing distortions into a model (again, whether theoretical or experimental) with the goal of simplifying them in order to make them cognitively and/or computationally tractable. This type of idealization is pragmatically justified (rather than epistemically, as in the case of minimalist idealization): we investigate simplified models because such simplifications make our models more tractable. In the end, we hope to de-idealize and create models that are more accurate (i.e., similar in the right sorts of respects and degrees) to our target systems.

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). We might read this quotation as promoting a Galilean idealization: for the time being, to include humans in our studies is experimentally and computationally intractable. In the future, we aim to build models that do include humans, but for now there is a pragmatic justification (of complexity) for not including them (and once again, we could frame this in terms of decisions about exogenous and endogenous variables). This is also sometimes put in the form of "baseline" or "benchmark" modelling: non-human nature provides a baseline or benchmark against which to measure the influence of anthropogenic factors, and should therefore have investigative priority.

Although it is beyond my current objective to evaluate the full conditions under which this strategy is justifiable, let me end this section with two considerations. First, the benchmark approach, as argued by O'Neill and Kahn, tends to characterize human activity within an ecosystem as a black box labelled "disturbance" (O'Neill and Kahn 2000). When human influence is minimal, this can be a harmless assumption, but when humans are more integral members of the community or ecosystem, this can also hide or obscure the kinds of complicated interactions that take place between humans and the rest of the community or ecosystem. And it is this latter information that is most helpful for making meaningful interventions in such systems.

Second, I should note that this form of idealization is at least contentious: some argue that human-disturbed systems are not altogether different from "natural areas," differing mostly in the relative importance and prevalence of certain patterns and processes, like habitat "patchiness" (Niemelä 1999); while others argue that human-disturbed systems are so different from the "natural areas" traditionally studied that they require the development of entirely new theory (Grimm et al. 2000; Kaye et al. 2006). In the latter camp, for example, Alberti et al. (2003, p. 1173)

"challenge the assumption that a "human-free" ecosystem paradigm can be productively applied to human-dominated ecosystems". Galilean idealizations, according to the latter position, are not pragmatically justified. In other words, the natural state of an area—when defined as that area's state prior to human interaction—does not provide an appropriate foundation in any meaningful sense.[28] That's not to say that studying the "undisturbed" state of a system is unimportant, just that it should not be assumed capable of providing an epistemically tractable foundation upon which the effects of humans can subsequently and straightforwardly be built.

In reality, it seems likely that cases will go both ways, or that they will run the whole gamut, and thus that there will be no singular answer to whether new theory is needed to explain human-natural coupled systems. In some cases, traditional ecological models which do not include anthropogenic variables, may need to be adjusted only slightly (or not at all) to apply to human-disturbed systems. In other cases, such as the Helmus et al. case, new anthropogenic variables may need to be substituted for traditional ecological variables. And in other cases still, new theory may have to be developed altogether. What this means, ultimately, is that treating humans as disturbing conditions for reasons of minimalist or Galilean idealization is likely to be a context-sensitive matter, rather than one decided, explicitly or implicitly, in advance.

Conclusion

I have argued that ecologists routinely treat humans as disturbing conditions, and that this manifests itself at the level of the language they invoke, the research practices they employ, and in the way that theory is developed and applied. I have also argued that the practice of treating humans as disturbing conditions is an idealization: since it is known that humans play a dominant role in many target ecosystems, this treatment amounts to an intentionally introduced theoretical distortion. Finally, thinking of this practice as idealization forces us to confront the question of its justification(s), and so, drawing on three different types of idealization, I have evaluated how this practice may be justified.

This analysis leads me to caution against taking for granted the practice of treating humans as disturbing conditions. This idealization, I believe, has been influential on past biological practice, but today must be recognized as untenable in many cases, perhaps an increasing number of cases. The problem is not that treating humans as disturbing conditions is never justified, but that it requires justification, which has by-and-large been lacking. Doing so requires addressing problems of variable choice and of idealization, and of confronting a world in which accepted theories and models struggle to do justice to the messy state of things. This will be a colossal undertaking. Here, my aim has been to offer a cogent way to discuss the issues surrounding the treatment of anthropogenic factors in ecology and to indicate how this treatment may be justified.

[28] Obviously this also presupposes some meaningful notion of the "natural state of an area."

One final consideration. A productive similarity exists between the reasons for treating humans as disturbing conditions and the different types of adaptationism philosophers draw attention to when debating the place of natural selection in evolutionary theory (Gould and Lewontin 1979; Godfrey-Smith 2001; Lewens 2009).[29] In the adaptationism literature, it has been helpful to distinguish between empirical, methodological, and disciplinary or explanatory claims. As an empirical claim, adaptationism states that natural selection is the only (or most) important factor in evolution; as a methodological claim, it states that it is most fruitful to begin with an adaptationist hypothesis; as a disciplinary claim, it states that adaptations are worth, or what we should be, investigating. We can likewise split up claims about humans as disturbing conditions. As an empirical claim, one might hold that humans are not a major (causal) influence in ecosystems. As a methodological claim, one might hold that not including humans in one's models—that is, treating anthropogenic factors as idealizations—is the best starting place for an analysis of any ecosystem. And as a disciplinary or explanatory claim, one might hold that human-disturbed nature is simply not worth studying or trying to explain. Only the methodological claim, I have suggested, has real merit. But, even if we grant that treating humans as disturbing conditions for methodological reasons has some merit, it still must be evaluated on a case-by-case basis.

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[29] Thanks to Adrian Currie for pointing out the similarity to me.

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