Conservation Biology

Essay

Fact and value in invasion biology

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Abstract: Some invasion biologists contend their science has reached a consensus on 4 facts: cost estimates of the effects of nonindigenous species provided in papers by Pimentel et al. are credible; invasive species generally, not just predators, pose significant extinction threats; characteristic biological differences distinguish novel from native species, ecosystems, communities, and processes; and ontological dualism, which distinguishes between natural and anthropogenic processes and influences, plays a useful role in biological inquiry. I contend there is no convincing empirical evidence for any of these propositions. Leading invasion biologists cite their agreement about these propositions as evidence for them and impugn the motives of critics who believe consensus should be based on evidence not the other way around.

Keywords: coevolution, dualism, invasive species, Pimentel estimates, science denialism

Hecho y Valor en la Biología de la Invasión

Resumen: Algunos biólogos de la invasión sostienen que su ciencia ha alcanzado un consenso en 4 hechos: las estimaciones de los costos de los efectos de las especies no nativas proporcionadas en artículos por Pimentel et al. son creíbles; las especies invasoras en general, no solo las depredadoras, representan amenazas de extinción significativas; las diferencias en las características biológicas distinguen a las especies, ecosistemas, comunidades y procesos nuevos de los nativos; y la dualidad ontológica, que distingue entre procesos e influencias naturales y antropogénicas, juega un papel útil en la investigación biológica. Sostengo que no hay evidencias empíricas convincentes para cualquiera de esas propuestas. Los principales biólogos de la invasión están de acuerdo acerca de estas propuestas como evidencia para ellos e impugnan los motivos de los críticos que consideran que el consenso debe basarse en evidencias, no al revés.

Palabras Clave: coevolución, dualidad, especies invasoras, estimaciones Pimentel, negación de la ciencia

摘要: 些入侵生物学家认为入侵生物学已在以下四个事实上达成共识: Pimentel 等人的论文中提出的对非原生种影响的成本估计是可靠的; 入侵物种, 不仅限于食肉动物, 通常会造成重大的灭绝威胁; 生物学特征上的差异可以区分出新出现的与原有的物种、生态系统、群落与过程; 划分自然和人为的过程及影响的二元本体论在生物学研究中发挥了有益的作用。我认为以上的主张都缺少令人信服的经验证据。一些主流入侵生物学家把对这些主张的认同当作证据来引用, 甚至质疑那些认为应该在证据的基础上建立共识, 而非以共识为证据的批评者的动机。**【翻译: 胡怡思; 审校: 聂永刚】**

关键词: 入侵物种, Pimentel 估计, 科学否定主义, 共演化, 二元论

Introduction

Recently in this journal, Munro et al. (2019) examined what they called "allegations of invasive species denialism." They argue that alleged "denialists" criticize the influence of values on the application and interpretation of invasion science but that "scientific facts are not disputed." Mainstream invasion biologists believe,

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***Article impact statement**: Contested invasion-biology issues are clarified so opposition groups can together achieve shared goals with evidence-based solutions.*

Paper submitted July 14, 2019; revised manuscript accepted October 25, 2019.

DOI: 10.1111/cobi.13440

on the contrary, that those they label *denialists* cast doubt on otherwise undisputed facts and engage in science denialism "by the systematic rejection of empirical evidence" (Ricciardi & Ryan 2018:2731). As to values, "the motivations behind invasive species denialism are diverse, involving actors with disparate ideologies (e.g., anti-pesticide activists, antiregulatory ideologues, postmodernist philosophers and other groups who distrust scientific institutions)" (Ricciardi & Ryan 2018:2732).

I examined 4 areas of agreed scientific knowledge in invasion biology. First, Pimentel et al. (2000, 2005) estimated the costs of nonindigenous species, and these papers have been cited as authorities on costs thousands of times. Their estimates may be too high or too low; that is beside the point. Those who criticize these estimates believe that are not backed by empirical evidence.

Second, invasion biologists generally agree that invasive species, including nonpredatory species, such as plants, represent the second leading cause of extinction after habitat change. Critics contend that introduced nonpredatory species, including plants, have not been the direct or principal cause of the extinction of any native species (Smith et al. 2006).

A third belief known to invasion biologists is that native communities differ from invaded ones—and native species from introduced ones—with respect to general or characteristic biological properties. Uninvaded ecosystems function normally, whereas invaded or novel systems do not. Ricciardi et al. (2011:312) point out that "biological invaders have profoundly changed the normal functioning of ecosystems by altering biological communities, physical habitats, nutrient cycling, primary production, or natural disturbance regimes." Critics argue that no general or so-called normal biological characteristics distinguish native from non-native species or pristine from invaded systems (Thompson & Davis 2011; Valéry et al. 2013). According to these critics, observers who do not know the history of a species or an environment have no way to know whether it is native or non-native, pristine or novel.

Fourth, invasion biologists draw an ontological divide between the natural and the artificial, the ecological and the cultural, and the wild and the human. Nature and humanity are treated as separate powers or systems that interact but possess independent kinds of agency. "We can distinguish between biological invasions by alien species, which occur after a species is transported by humans outside its native range, and natural invasions, which are better referred to as colonizations, whereby a species expands its range to a new location under its own powers of dispersal" (Russell & Blackburn 2017:312). According to Russell and Kaiser-Bunbury (2019), "the introduction by humans of species outside their native range is an unnatural process." All processes, however, are equally and in the same way natural, which is to say, actual or possible. The meaningful opposite of *natural* is *supernatural*.

Prominent invasion biologists label as contrarians and denialists those who argue that the Pimentel et al. estimates are not credible; nonpredatory species are not significant causes of extinction; invaded systems do not differ from pristine ones in any general or characteristic biological ways; and human–nature dualism is not an empirical and therefore not a scientific proposition. I considered these 4 kinds of denialist objections and whether, as prominent invasion biologists believe, ingenuous, perverse, venal, and ideological motivations account for them.

Costs of $120–$137 Billion per Year

Pimentel et al. (2000, 2005) estimated the economic costs of nonindigenous species at $120–$137 billion annually in the United States alone. According to Google Scholar, Pimentel et al. (2000) has been cited by 3675 scholarly articles. Pimentel (2005) has been cited in 4234 articles, of which 1810 were published after 2015; 726 use the $120 billion number. Approximately 350 articles published in *Biological Invasions* cite Pimentel et al. as an authority. Of these, 15 use the figure $120 billion. Nine refer $137 billion, and most use phrases such as "tremendous environmental damages and economic costs."

According to Meyerson et al. (2019:1919), "In 2019, in the US, biological invasions remain an unrelenting environmental and economic calamity ... at a cost estimated at more than $100 billion annually." Duenas et al. (2018: 3172) cite Pimentel for costs "amounting to $120 billion annually in the United States alone." Ricciardi et al. (2011:313) rely on Pimentel et al. to conclude that the "annual cost of biological invasions in six nations [is] . . . $314 billion." Simberloff (2013*b*:357) wrote that an "estimate by D. Pimentel and others suggests an annual global cost of invasive species greater than US $1.4 trillion."

Nine biologists wrote in *Biological Invasions*, "In the United States alone, invasive species are responsible for over $120 billion in damage and control costs each year" (Lewis et al. 2019). Similarly, Stigall (2019) refers to "the fact the invasive species impacts cause more than $120 billion in damages each year in the United States alone." Questioning these numbers is tantamount to questioning global warming (Simberloff 2005:604).

The Pimentel et al. articles are far more often cited than read. If one reads their variant versions, I think one would agree that no studies or data support their estimates. The articles rely largely on Pimentel self-citation (20 times in the 2005 version), citations that circle around to self-citations, and citations that do not support the text, are speculation, or prediction. Repetition, not evidence, corroborates these estimates.

The Pimentel articles use the costs involved in controlling or eliminating nonindigenous species as a measure of the damage they produce. Any species may cost a lot to eliminate, but this cost is not a measure of the losses it causes or would otherwise cause. If a government agency identifies a threat to persuade Congress to increase its budget to fight it—as the U.S. Forest Service did to prevent forest fires—one may suspect that it is the agency not the public that benefits.

Pimentel et al. (2000:56, 2005:277) used "potential control costs as a surrogate for losses." Relying on a paper titled, "Regulation of the Street Pigeon in Basel," these authors concluded "the control costs of pigeons are at least $9 per pigeon per year" in the United States. Pimentel et al. reasoned that pigeons therefore "cause $1.1 billion per year in damages." Pimentel et al. used similar logic to infer the costs of cats. For evidence of the number of feral cats in the United States, these authors cite an editorial in *Audubon* magazine (not peer reviewed), titled "Catfight," which reads, "Estimates suggest that the numbers of feral (semiwild) and abandoned cats in the United States run well into the millions – and may exceed 30 million." The article does not offer evidence or provide a citation. Pimentel et al. (2005: 276) reasoned:

> Assuming 8 birds killed per feral cat/year (McKay 1996), then 240 million birds are killed per year in the nation. Each adult bird is valued at $30 . . . . Therefore, the total damage to U.S. bird population is approximately $17 billion/year.

One might cavil that the product of multiplying 240 million birds by $30 each is $7.2 billion not $17 billion, but that is not the point. The McKay (1996) article has to do with feral cats in Australia, not in the United States. It is not a study of predation, but it does mention a study of predation by domestic cats in Australia, which argues that it varies with kinds of habitats. For a critique of Pimentel et al. (2005) on the costs of feral cats, see Goldstein (2013).

In a thorough study of the disconnect between the costs and benefits of the control of invasive species, Guiaşu (2016) showed that in Canada the only cost associated with purple loosestrife *(Lythrum salicaria*), a notorious invader, is the expense of the programs initiated to control it. "If there are no more control programs against purple loosestrife, then, this plant would no longer cost us anything, so the 'problem' of our own making would be solved" (p. 207).

Pimentel et al. published variants of the 2000 paper over the next 5 years; these are regularly cited by invasion biologists. Ricciardi et al. (2011:313) wrote that the "damage inflicted by invasions amounts to $1.4 trillion per year, which constitutes 5% of the global economy (Pimentel et al. 2001)." Pimentel et al. (2001) is "based on extrapolated data from the United States (Pimentel et al. 2000)," but includes more data on "microbe invasions" (included in the title of the article), including AIDS, influenza, and syphilis. Pyšek and Richardson (2010:37–38) wrote, "The pioneering study of Pimentel showed that costs incurred by biological invasions globally amounted to about 5% of the global gross domestic product (GDP)"; however, the cited edited volume provides no such estimate.

Of the nearly 10,000 articles that cite the Pimentel et al. estimates, I found only 1 that questions them. Kareiva et al. (2017) called them a mix of data leaps and expert guesses and contended that in some cases there was no way to get from one number to the next. "The absence of serious efforts to improve of Pimentel's cost estimate is revealing—it suggests a willingness to accept the science so long as it roughly aligns with what conservationists want to hear" (Kareiva et al. 2017:5).

Economic Costs of the Zebra Mussel

Pimentel et al. (2000, 2002, 2005) estimated the costs associated with the zebra mussel (*Dreissena polymorpha*) to be about $1 billion annually in the United States. According to the Pew Ocean Commission (Carlton 2001:5), this number "became an 'urban legend,' and remains the single most quoted figure to express the economic impact of an aquatic introduction in the United States. The number, however, was based upon no study and thus no data."

If anything is known to invasion biology, however, it is "that dreissenids have been estimated to cost the American economy billions of dollars annually" (Amberg et al. 2019:97). Simberloff (2013*a*:35) wrote that "zebra mussel (*Dreissena polymorpha*), and Asiatic clam (*Corbicula fluminea*) each impose damage and control costs of $1 billion annually." According to several invasion biologists writing in 2018, "the annual cost of the zebra mussel . . . amounts to $1 billion in the United States alone" (Harrison et al. 2018). Nothing will shake the scientific consensus that "damages caused by the mussel . . . are valued at around one billion USD per year" (Courtois et al. 2018:610).

A sure way to be typed as a denialist is to allude to the lack of evidence for the billion-a-year estimate. Ricciardi and Ryan (2018:2733) said, "Sagoff . . . has often used the zebra mussel, an iconic invader, to defend his argument that impacts of invasions are overblown" (see similarly Lodge and Shrader-Frechette [2003:35] and Frank [2019]). I referred to the "costly or disruptive" effects of the zebra mussel (Sagoff 1999:17). I wrote that the zebra mussel "clogs intake and distribution pipes and cooling systems, which in America – unlike in Europe – were not originally designed to keep them out" (Sagoff 2000).

I provoked outrage by citing empirical evidence (O'Neill 1997) that the costs were lower than $1 billion/

year. Connelly et al. (2007) gathered data from 81 electric and 321 water treatment facilities and then extrapolated these numbers to every identifiable such facility. These authors estimated the total (not annual) expenditure throughout the mussels' entire North American range at $267 million for the 15-year period from 1989 to 2004, mostly in the first years as managers retrofitted facilities and figured out what additional treatments were needed.

As far as I know, there have been no other surveys of facilities affected by the zebra mussel to determine the associated costs over the last 15 years. There are useful case studies. Chakraborti et al. (2016) published data from 10 heavily invaded water-intake facilities. They found that the capital expenditure and operations and management costs for mussel control varied from facility to facility, but in larger (>1 mgd) mussel-infested facilities costs approximated $40,000 per year. This fits roughly with the Connelly et al. (2007) estimates and suggests that the $1 billion/year estimate is off by 2–3 orders of magnitude.

Invasion and Extinction

That introduced organisms constitute the "second greatest threat" to species after habitat change may be the second most agreed on conviction in invasion biology after the Pimentel estimates (Chew 2015; Guiaşu 2016). While critics of invasion science all understand that introduced predators are extinction threats, they have drawn attention to the difference between predation and competition (Sax et al. 2007). They argue that nonpredatory organisms, such as plants, have rarely if ever been the sole cause of the extinction of any species. "There is not much documented evidence so far that competition from an invasive plant species can cause the extinction of a native plant species" (Duenas et al. 2018:3178).

Pearce (2016:18), an environmental journalist often cited as a denialist (e.g., Ricciardi & Ryan 2018), recognizes that invasive predators cause extinctions. He called the brown tree snake (*Boiga irregularis*) in Guam a crafty, hungry, carpetbagger. "Without the snake, there would undoubtedly be many more birds on Guam today." Ken Thompson, a botanist who has also been listed as a denier, laments the horrors of the brown tree snake and describes them in detail. Thompson (2014:80) concludes, "Guam's brown tree snake is a Bad Thing." I note that "exotic predators in enclosed environments can destroy endemic fish, birds, and other animals; predation is a real problem" (Sagoff 2009:84). I also acknowledge the power of "selected examples, such as the predations of the tree snake in Guam" (Sagoff 2005:225).

Yet invasion biologists often write that their critics deny that invasive species are significant causes of extinction. Frank (2019) referred to my ". . . often-repeated claim that few or no extinctions can be attributed to invasive species." Had I not heard about the Guam tree snake? Blackburn et al. (2019) wrote that "some researchers have argued that the impacts of alien species are exaggerated and contend that they are no more likely than native species to cause environmental damage, such as extinctions." The researchers Blackburn et al. mentioned argue that unlike predators, invasive competitors, including plants, are rarely significant factors in the extinction of species.

To refute the suggestion that competition is not a significant cause of extinction, Blackburn et al. (2019) described and displayed pictures of 3 invasive predators, including the Guam tree snake and the black rat (*Rattus rattus*), to represent invasive species generally. To answer critics who distinguish competition from predation in its extinction threat, these authors argued that nonnative plants have been implicated in some way in the loss of 39 species during the last 500 years. These authors did not find that any nonpredatory alien species was ever the sole or the principal cause of any extinction. Even if invasive plants have contributed somehow to the loss of 39 species in the past 500 years, this does not make them a significant factor in the sixth mass extinction event (Ceballos et al. 2017).

General Biological Differences Between Native and Non-native

A third pillar of the consensus in invasion biology—in addition to the authority of the Pimentel et al. estimates and the extinction threat posed by invasive species generally—lies in the shared assumption that characteristic biological differences distinguish noninvaded, pristine, or native systems (and species) from novel, invaded, and non-native systems (and species). Biological invasions disrupt and damage the structure and function of ecosystems (Levine et al. 2003; Traveset & Richardson 2006). Heirloom systems remain "intact" while invaded systems are often "degraded" (Pyšek & Richardson 2010:46).

If invasive species harm, degrade, or damage ecosystems, it should be possible for someone without historical information to observe which ecosystems are invaded and which are not by seeing or testing which are and are not degraded or shambolic. Likewise, such an observer should be able to tell from the biota which state of an ecosystem is the before and which is the after invasion. In the absence of historical information, how can this be done?

Poe and Latella (2018:2532) wrote that philosophers "have questioned whether nonnative species and assemblages are objectively, ahistorically identifiable as different entities relative to native species and assemblages . . . ." These authors (p. 2554) doubt there is an objective biological difference between native and non-native and argue that no one can tell which is which except by determining historically how much human influence it bears.

Pereyra (2019) argues that the native range of a species is a function of the role humans have played in its distribution; in the absence of human involvement, every species must be native to wherever it thrives. "Native plants are defined in terms of human activities and influences, not in terms of the plants themselves" (Head 2017:2).

According to Rejmanek and Simberloff (2017), "In their native range, resident species have coevolved with such native biota and thus have traits permitting their coexistence." The species found in novel or hodgepodge communities, dispersed however from wherever, have traits permitting their coexistence. What is the difference? How would one tell by observation which systems are coevolved and thus normal, healthy, and functional, and which are not?

That introduced species often cause communities and systems to change is what nobody denies. It is the core belief of invasion biology that these changes are or can be detrimental, disruptive, and disturbing to those communities. Invasion biologists, however, have failed to identify any general ahistorical biological trait that distinguishes the before from the after or a coevolved community from one in which species have dispersed from various places at various dates and have coexisted (or, if one prefers, fitted) together over time. Invasion biologists may agree that heirloom communities are more normal, coevolved, or integrated than hodgepodge communities. How would this proposition be tested?

How could coevolution in ecological communities have come about? Not by natural selection. Natural selection biases reproductive success in favor of heritable variations in fitness-relevant traits in an interbreeding population of a single species. Microevolution or descent with modification involves changes within a potentially continuous population. The unit of selection is the population or the species, the individual, or even the gene. How does one get from trait displacement or allelic substitution in a potentially continuous population to coevolution among many species that do not share a common phylogeny or descent?

One does not. Except in rare circumstances, described by Wilson and Sober (1989), ecological communities are not units of selection in Darwinian theory. Maynard-Smith (1964) and Williams (1966) wrote critiques of group selection that evolutionary biologists have found persuasive. Evolution is an intraspecific phenomenon. There is no way to scale pairwise phylogenetic arms–race interactions (Ehrlich & Raven 1964), supposing they occur, to the community level. If communities do not compete for survival, the idea of an evolutionary community is a nonstarter. If scientists did find general biological differences by which they could distinguish native from novel ecosystems, they could not explain these differences in evolutionary terms if evolution refers to natural selection and not, as it often does in the literature of community ecology, to a cosmic creative force (Sagoff 2019).

Ontological Dualism

Simberloff and 13 coauthors (2013*c*:58) defined the 2 key concepts of invasion science this way: "'Introduced population': population that arrives at a site with intentional or accidental human assistance. 'Invasive population': introduced population that spreads and maintains itself without human assistance" (citing Richardson 2011). These definitions assign to humans a power no other creature possesses or could possess, the power to make another species non-native, introduced, and possibly invasive. According to Essl et al. (2018:499–500), "clear evidence for direct human agency as the causal factor for species introduction is needed to fulfill the criterion of a taxon being deemed alien."

It is not the taxon that is alien but the human—the taxon becomes alien because it is affected by human activity or agency. Human beings are considered external to nature; the introduction by humans of a species outside its native range is therefore an unnatural process (Inkpen 2017*a*, 2017*b*). "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 & Paine 2016:604).

A century ago, ecologists grappled with the question whether all *Homo sapiens* are unnatural or only those of a certain race, gender, or level of education. Early ecologists speculated that before the Columbian encounter, tribes indigenous to the New World belonged to nature (Shelford 1913:13). According to Clements and Shelford (1939:24), there is "an important difference in the reactions and coactions exerted by man at various culture levels." In pastoral areas, "man perhaps is still to be reckoned as a constituent of the biome" (Clements & Shelford 1939). It is still not clear how human–nature dualism in ecology sorts out in terms of race, class, and gender.

To define its fundamental terms, invasion biology must divide human beings—or some of them—from the rest of nature as separate kinds of agencies. The concept of invasion would not otherwise differ from that of colonization or dispersion. How would one test as an empirical proposition that an introduced species is alien or that an invaded system is unnatural? One cannot. Invasion biology implies there is something wrong with humanity as it stands and therefore human activity taints or stains nature with this wrongness.

Robbins and Moore (2013) describe conservation science, restoration ecology, and invasion biology as "Edenic sciences." These sciences assess ecological relationships against an a priori baseline, "a condition before the Columbian encounter, or a time or place before human contact, or a place of expulsion or return – one Before the Fall" (p. 4). The ontological dualism between humanity and nature cannot be found in any biological science outside of ecology—for example, in genetics, neurobiology, anatomy, physiology, cytology, and so on. Among biological sciences, only the ecological greet human–nature dualism with a straight face.

Denialism

In an article titled "The Rise of Invasive Species Denialism," Russell and Blackburn (2017:3) allege that critics of invasion biology exploit the fact that science always involves uncertainty. Latombe et al. (2019) agree that scientific uncertainty "is exploited by denialists." This helps explain, they wrote, the exponential increase of "the number of articles denying or trivializing the impacts of alien species, and indeed the field of invasion biology."

Invasion scientists are certain—or appear to be certain—that the Pimentel estimates are reliable, that invasive species generally, not just notorious predators, are leading causes of extinction, that native and non-native differ not just historically but in characteristic and general biological ways, and that human beings are ontologically external to nature. Critics of invasion biology have not dwelled on scientific uncertainties; rather they have called attention to what invasion scientists do know with certainty but that is not so.

To defend their agreed-upon consensus, some invasion biologists impugn the motives of their critics. According to Ricciardi and Ryan (2018:551), "contrarians" may be publicity hounds or "free-market ideologues opposed to ecologists' calls for increased regulation." In this vein, Frank (2019) misquoted an article in which I examined the legal framework by which governments "properly exercise the police power to control or eradicate plant and animal nuisances and pests." There I defended "the extent to which science-based laws intended to control invasive species may [i.e., are permitted under the police power to] restrict personal liberties and property rights" Sagoff (2009:26). Frank restates this sentence as follows to reverse its meaning – "science-based laws intended to control invasive species . . . restrict personal liberties and property rights." Frank infers from this misquotation, "Sagoff's own ethical/political commitments may be incompatible with invasive species *regulation* . . . "

Frank accuses me of science denialism by attributing to me views I do not hold. In one article, I wrote, "Many ecologists believe that species richness increases the stability of ecological systems or communities and adds to the resilience of ecological processes" (Sagoff 2018:225). On this basis Frank attributes to me the argument others may make that species introductions increase local biodiversity and thus improve ecosystem function. He accuses me of "cherry picking" examples because I mention some beneficial effects of non-native species. To avoid cherry-picking, one must stick to snakes, rats, and negative effects. Several recent articles (e.g. Boltovskoy et al. 2018; Guiaşu & Tindale 2018; Guerin 2019) document other examples in which invasion biologists attribute to their critics views they do not hold, motives they do not possess, and arguments they do not make.

I welcome evidence that the zebra mussel imposes costs of $1 billion a year in the United States, that introduced plants are major extinction threats, that there are characteristic biological differences between heirloom and novel ecosystems, and that humanity is ontologically separate from nature. I do not think this evidence will be forthcoming. This may explain why invasion biologists make their case by impugning the motives of those who differ with them. I agree with Munro et al. (2019) who condemn invasion biologists for making baseless ad hominem allegations, which are "calumnious" and "must stop."

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