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The algorithmic turn in conservation biology: Characterizing progress in ethically-driven sciences

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ABSTRACT

As a discipline distinct from ecology, conservation biology emerged in the 1980s as a rigorous science focused on protecting biodiversity. Two algorithmic breakthroughs in information processing made this possible: place-prioritization algorithms and geographical information systems. They provided defensible, data-driven methods for designing reserves to conserve biodiversity that obviated the need for largely intuitive and highly problematic appeals to ecological theory at the time. But the scientific basis of these achievements and whether they constitute genuine scientific progress has been criticized. We counter by pointing out important inaccuracies about the science and rejecting the apparent theory-first focus. More broadly, the case study reveals significant limitations of the predominant epistemic-semantic conceptions of scientific progress and the considerable merits of pragmatic, practically-oriented accounts.

1. Introduction

Humanity faces existential environmental threats with dauntingly complex causes. Addressing these difficult problems is now the objective of several sciences. Among them, conservation biology supplies scientifically grounded means for protecting, preserving, and rehabilitating biodiversity. This ethical goal shapes the science—what subjects are studied, which investigative methods are developed and utilized, how hypotheses, potential plans, and theories are evaluated, etc.—and specifically how progress should be gauged. The ethically-driven nature of conservation biology and many other sciences demonstrates, we argue, that traditional conceptions of scientific progress need to be rethought.

The heart of our case is an unmitigated scientific success story. In the 1980s conservation biology emerged as a rigorous science distinct from ecology focused on protecting biodiversity. Before then, as Section 2 documents, reserve design had been based on intuitive and highly problematic appeals to ecological theory, island biogeography theory in particular. Moreover, and unsurprisingly, Section 3 explains why most reserves had been designated on unsystematic, *ad hoc* grounds and consequently poorly represented biodiversity. Demonstrating this convincingly was crucial to ensuring biodiversity would be adequately protected in future policymaking contexts. Section 4 argues that algorithmic breakthroughs in information processing made this possible: place-prioritization algorithms and geographical information systems. They provided a defensible, data-driven methodology for designing reserves to conserve biodiversity, and in turn supplied quantitative, critical assessments of existing reserves.

Despite these unquestionable advances, that they constitute scientific progress has been criticized. Ecological theory, it is claimed, is required for genuine progress about reserve design; algorithmic innovation in data processing is insufficient (Linquist, 2008). Place-prioritization algorithms are also supposedly “less scientifically grounded” and produce reserves that poorly protect biodiversity (ibid. 530). On all accounts this criticism is indefensible. As the history recounted in section 4 demonstrates, it involves numerous inaccuracies about the science, misconstrues the character of ethically-driven applied science,[1] and, most crucially, Section 5 also shows it relies on an untenable conception of progress for sciences with ethical objectives. Although ethically-driven sciences are unquestionably science and employ scientific methods, what constitutes progress within them should not always, and definitely not in this case, be judged by standards thought appropriate for classic descriptive sciences such as chemistry, evolutionary biology, and

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[1] By using the label ‘applied,’ we are not endorsing a sharp or philosophically significant pure-applied science distinction. Douglas' (2014) critique of such an aggrandized distinction is compelling.

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physics. The actual historical and scientific details (discussed in sections 2-4) matter: existing philosophical accounts of scientific progress that exclusively emphasize acquiring knowledge (Bird, 2007) or ontologically uncovering the “true nature” of reality (Niiniluoto, 2017) must be rethought and broadened to recognize the progressiveness of ethically-driven sciences.

2. A new ethically-driven science emerges

The newly-energized environmental movement of the 1960s thrust ecology into the social limelight (Nelkin, 1977).[2] The 1970 and 1971 presidential addresses of the Ecological Society of America reflected the sea change:

> “Ecology has been pulled out of the shadows and thrust upon the central stage’’ (Bormann, 1971, p. 4); “In the last three years [the discipline of ecology] has achieved a degree of fame (or notoriety) far exceeding our most extravagant hopes and dreams of a decade or more ago’’ (Auerbach, 1972, p. 205).

This exerted considerable pressure on ecologists to remedy the environmental problems being revealed to the public. Among them, species loss due to tropical rainforest deforestation was being heralded as a crisis in numerous high-profile scientific publications (e.g. Gómez-Pompa, Vázquez-Yanes, & Guevera, 1972; Ehrenfeld, 1976; Ehrlich & Ehrlich, 1981; Soulé, 1985). A strategic institutional response was called for, and a new discipline emerged: conservation biology.

2.1. Early reserve design: theory-heavy and data-light

Given the magnitude of destruction, ecologists felt uniquely obliged and qualified to evaluate the threat. The immediate imperative was creating reserves to protect portions of the diversity of biological entities and phenomena, or “biodiversity,” the rainforests contained. Ideally, reserves would maximize representation *and* persistence of biodiversity. The challenge was developing methods to identify such reserves. This was especially daunting since distributional data on most species did not exist. At the time, only a handful of detailed datasets had been developed. Myers (1988), for example, estimated only 0.5 million of 2.5–30 million species in tropical forests alone were identified; pinpointing areas of high species richness for protection therefore seemed hopeless. Haphazardly selecting areas for protection without adequate data about what they contained would be indefensible and pointless.

Furthermore, little was known about the ecology of the vast majority of species—environmental factors affecting plants or animal habitat requirements—especially for tropical species. Beyond a scattering of economically valuable species, or those that interested professional biologists, only a handful of charismatically appealing birds, butterflies, and large mammals were exceptions. Ecologists also knew little about interspecific interactions, specifically, dependency relationships between species. Hence, they could usually only speculate that protection of some species would ensure adequate protection of others. In the mid-1970s, the dearth of ecological information relevant to conservation planning prompted Ehrenfeld (1976, p. 652), a founder of the United States Society for Conservation Biology, to remark: “the population dynamics and management ecology of nearly all species are still largely unknown.”

There was, however, a new, mathematically sophisticated ecological theory that promised to fit the methodological bill: the equilibrium theory of island biogeography (MacArthur & Wilson, 1967). The scarce data difficulty prompted several prominent American ecologists to use this theory to champion general principles of reserve design (see Wilson and Willis 1975; Diamond, 1975, and reference therein). To make the theory applicable they assumed most species protected areas surrounded by degraded habitat—for example, rainforest patches protected from clear-cutting—were ecologically similar to islands. According to island biogeography theory (IBT), these ecological “islands” would hold more species the larger and closer together they were. Specifically, IBT predicts that extinction and immigration processes determine an equilibrium species richness; increases in island isolation decreasing immigration or decreases in island area increasing the extinction rate will produce equilibria with fewer species. That IBT may provide, “geometrical rules of design of natural reserves” (Wilson and Willis 1975, p. 528) was first proposed by Edwin Willis as early as 1971 (Willis, 1984) and independently by Edward Wilson around the same time (Wilson, 1992).

The most influential analysis to use IBT in reserve design was Diamond, 1975. Adapting a diagram from Wilson and Willis (1975), and choosing a somewhat weaker label of “principles” rather than “rules,” Diamond (1975, p. 143) proposed six design principles (Fig. 1), “derived from island biogeographic studies.”

Diamond (1975) justified each principle by appealing to two factors: minimization of population extinction rates, or maximization of immigration rates. For instance, closer proximity of protected areas with homogeneous habitat required by principle C arguably helps ensure greater immigration rates between populations. This, in turn, arguably helps ensure individuals will emigrate to areas with declining or extinct populations.

But despite its theoretical pedigree, only principle A proved uncontroversial. All the other principles (B–F) were highly contentious and on the face of it thoroughly misguided from the start. The obvious problem is insensitivity to context. Consider principle C again. It's a facile exercise

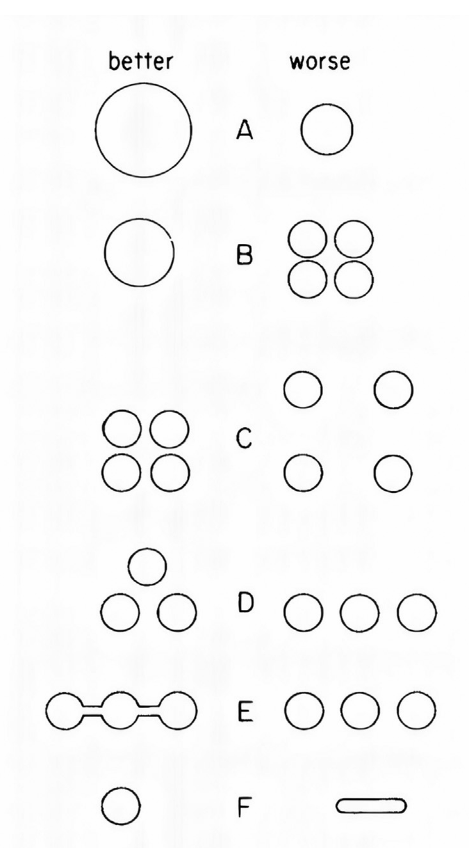

Fig. 1. Principles of CAN design. Principles B, D, E, and F are from Wilson and Willis (1975) (Adapted from Diamond [1975], p. 143.).

[2] It's hard to overstate the cultural significance and societal power of the environmental movement. The legislation the movement catalyzed, to highlight just one kind of example, was unprecedented. In the United States alone, it included the Clean Air Act (1963), Wilderness Act (1964), National Environmental Policy Act (1970), Endangered Species Act (1973), Forest and Rangeland Renewable Resources Planning Act (1974), and Clean Water Act (1977).

to image circumstances in which locating protected areas farther apart is far superior. Enhancing spatial disparity can spread risk and thereby decrease probabilities of disease outbreaks, widespread fires, and other catastrophic events precipitating local species extinctions.[3] The possible benefits of enhancing immigration have to be weighed against these risks, and might be swamped by them.

Most importantly, principle B initiated the vituperative controversy over whether, *in general*, a single large area or several small ones of equal total area would protect more species, first labeled the "SLOSS" debate by Simberloff and Abele (1982). Diamond (1975, p. 144), however, only qualified B by noting:

> Separate reserves in an inhomogeneous region may favor the survival of different groups of species; and that even in a homogeneous region, separate reserves may save more species of a set of vicariant similar species, one of which would ultimately exclude the others from a single reserve.[4]

Similarly, principle E initiated contentious debate about the conservation value of habitat corridors (Simberloff et al. 1992). Although Diamond (1975, p. 144) only claimed habitat corridors "may significantly improve the conservation function of [reserves]," there was little empirical support for this claim or appreciation of how difficult its acquisition would be (Nicholls & Margules, 1991).

Simberloff and Abele (1976), two prominent members of what was later memorialized as the 'Tallahassee Mafia' (see Dritschilo 2008), were the earliest and most visible critics of Wilson and Willis' (1975) rules and Diamond's (1975) principles. Against principle B they argued that "the" species-area curve that was central in IBT's development:

$$S = k \times A^z;$$

where S is species richness in area A, and k and z are constants, did not unequivocally support single large areas. Specifically, whether a large area or several small ones contain more species at equilibrium depends upon: (i) the proportion of species the latter share; and, (ii) the species-area curve's exact shape. Existing data, they claimed, showed the shape varies markedly across taxa. Accounts of the curve treating all taxa as equal, such as IBT, were indefensible. Depending on the region's taxa, therefore, a large area may contain much fewer species than several small ones. In support, Simberloff and Abele (1976) presented data from a red mangrove (*Rhizophora mangle*) archipelago and suggested this ecosystem was not atypical in contradicting principle B. They also pointed out, rather astoundingly, that IBT itself implies species gradients in dispersal and survival can *favor* several small over single large.

Subsequent studies reinforced these criticisms and added others (Gilbert, 1980; Margules, Higgs, & Rafe, 1982; Simberloff & Abele, 1982, and references therein). For example, whether non-degraded habitat "islands" are ecologically similar enough to oceanic islands to make IBT theory applicable depends on the species and degree of degradation (Margules et al., 1982). Often the dissimilarities were glaring and significant, and too often judging similarity was entirely unclear. By the mid-1980s, it was clear the initial promise of IBT had been seriously oversold. In an exhaustive review, for instance, Gilbert (1980) argued there was insufficient empirical support for IBT itself. Thus, Margules et al. (1982) stressed, design principles supposedly based on it were unjustified. The "scientific revolution" (Diamond, 1975, p. 131) catalyzed by MacArthur and Wilson proved unhelpful when, "For a variety of taxa, for a number of different habitat types, and for a wide range of sizes of biota as a fraction pool, either there is no clear best [reserve design] strategy, or several small sites are better than one large site" (Simberloff & Abele, 1982, p. 48).

Disagreement about the goal of reserve design also complicated the issue. Diamond's (1975) and Wilson and Willis' (1975) design criteria sought to protect maximum species richness when reserves reached equilibrium after their ambient area was degraded, not protection of the particular species they currently contain. An implication of IBT focused attention on the former: "although the *number* of species on an island may remain near an equilibrium value, the *identities* of the species need not remain constant, because new species are continually immigrating and other species are going extinct," (Diamond, 1975, pp. 134–135). From this perspective, an area's specific species composition is irrelevant since it would change over time, especially as reserves reach new equilibria after habitat destruction. Yet, Margules et al. (1982) criticized that attempting to maximize species richness may leave many species unprotected *now*, or not efficiently protect them in the minimum total area. And it betrayed a faith in IBT that far exceeded what the evidence warranted. Together with the magnitude and pace of habitat destruction, the dubious status of IBT convinced many conservation scientists the priority should be protecting species in the least area possible *now*.[5] Persistence, after all, *requires* representation: biodiversity must be adequately represented *before* its persistence can be ensured.

Despite these controversies, by the early 1980s one clear consensus had emerged: adequate reserve design required more data about species and ecosystems of conservation interest (see Simberloff & Abele, 1982, 1984; Willis, 1984). Few doubted large areas were important for some species, such as large carnivores, and small areas adequately protected many species. The problem was that neither approach was *generally* defensible and limited financial resources for conservation precluded following both. But no defensible alternative methodology to IBT-based reserve design that would adjudicate this issue existed before the advent of place-prioritization algorithms (see section 4).

Despite trenchant criticisms, the theory significantly influenced the conservation community well into the 1980s (Kingsland, 2002), especially in the United States. Although Diamond's (1975) principles were immediately criticized in high profile journals like *Science*, the International Union for Conservation of Nature and Natural Resources (IUCN 1980) adopted them wholesale when devising a global conservation strategy. As presented there, the few qualifications Diamond (1975) originally made, and the incisive criticisms the principles received, were strikingly absent (Margules et al., 1982).

Despite its serious flaws, the benefits IBT *would have* achieved should be appreciated. First, it would have bypassed the need to acquire area-specific data. The urgency and limited funds for protecting threatened areas (Simberloff & Abele, 1984), combined with the high cost of data acquisition, made applying IBT to reserve design appear attractive and efficacious. Second, as a widely publicized scientific theory, conservationists could invoke its authority to justify and convey an air of objectivity to their recommendations. This helped counter pro-development interests. Diamond (1976, p. 1027), for instance, emphasized that the lack of a "firm basis" for predicting extinctions (before IBT according to Diamond) had hindered, "convincing government planners faced with conflicting land-use pressures of the need for large refuges." He argued that since IBT might provide that basis, Simberloff and Abele's (1976) criticisms were troubling, because "those indifferent to biological conservation may seize on Simberloff and Abele's report as scientific evidence that large refuges are not needed" (Diamond, 1976, p. 1028). Terbourgh (1976, p. 1029) seconded this worry: "Simberloff and Abele, if accepted uncritically, could be detrimental to efforts to protect endangered wildlife." Without the apparent scientific authority of IBT the

[3] This sensible advice is endorsed by the IUCN Redlist of Ecosystems policy guidelines (Bland, Keith, Miller, Murray, & Rodríguez, 2017, p. 49). We thank an anonymous reviewer for calling our attention to this information.

[4] Diamond (1975) did not clarify the meaning of 'homogeneous' or consider problems involved in classifying different habitats. And *contra* Diamond's (1975) assumptions, most regions are not habitat homogeneous (Margules et al., 1982).

[5] MacArthur and Wilson's (1967, p. v) frank admission, "We do not seriously believe that the particular formulations advanced in the chapters to follow will fit for very long the exacting results of future empirical investigation," also supported this conviction.

conservationist agenda arguably lacked justification: "pro-conservation individuals and groups, in and out of governments, hardly have a leg to stand on when competing for land and resources" (Soulé & Simberloff, 1986, p. 44).[6]

Controversy about the role of IBT in reserve design concerned more than its insufficient empirical support or inapplicability. Disagreement about the proper function of intuitions, presumably biologically informed in some way, also catalyzed controversy. In response to Simberloff and Abele (1976), for instance, Diamond (1976) emphasized that IBT may justify biologists' intuition that many existing conservation areas were too small to protect their biota adequately. For Diamond, these intuitions likely track important truths and should thereby contribute to reserve design. If IBT didn't suffer from such empirical shortcomings, that counsel might be prudent. But it did, and Simberloff and Abele (1984, p. 399) worried that "theory is often seductive," perhaps because of the contaminative effect theory can have on intuition. Instead, they held that inconclusive and speculative arguments, which are obviously inadequate surrogates for field data, were nevertheless marshaling too much of the perceived support for large conservation areas. As section 4 details, place-prioritization algorithms provided a case-specific methodology that helped eliminate intuitive approaches to reserve design and helped transform it into a characteristically data-driven science. Rather than trading in questionable intuitions and supposed theoretical implications that don't actually follow from island biogeography theory, with the emergence of PPAs—and arguably at least two other technology-based methodological innovations[7]—conservation biology came into its own as a science.

3. Politically expedient "worthless land" reserves

Science is a human activity, and largely a collective one. As such it is supported by and subject to the same cultural, economic, institutional, and political forces as any other human activity. Even researchers in so-called pure sciences must navigate the contours of these forces to secure grant funding, necessary permits, conduct human testing, get access to sensitive data, move lab spaces, etc. But ethically-driven sciences in general, and certainly conservation biology in particular, are much more tightly tethered to those forces. This greater dependency has myriad consequences, more stringent scrutiny and regulation is an obvious one (e.g. strict safety standards for implantable medical devices and rigorous structural integrity testing for bridge designs). Another consequence affords an opportunity: connecting to broader societal concerns makes any advances in these sciences all the more potent. Besides developing novel methods, theories, and discovering new truths, these achievements sometimes help solve the crucial societal problems ethically-driven sciences are intended to address. Or, as the case described below shows, progress can be made by demonstrating that a problem previously thought solved, or goal previously thought achieved, was in fact not.[8]

The status of extant reserves and conservation value of parks was the issue. As an alternative to flawed theory-driven approaches to reserve design, Simberloff (1986) suggested returning to the intensive field research he thought characterized designation of national parks and reserves in the late 19th, early 20th century. His belief, unfortunately, reflected the exception, not the norm. As debate about IBT and reserve design emerged in the mid-1970s, the *ad hoc*, unsystematic past bases for protecting particular areas, and the obstacle this posed for successful biodiversity conservation, became clear.

The historian Alfred Runte first made this criticism, about the original motivations for creating the US parks. Borrowing California Senator John Conness' 1864 description of Yosemite Valley as "for all public purposes worthless" (quoted in Runte, 1979, p. 48), which featured in his argument it could have protected status, Runte (1972, pp. 4–7) proposed the "worthless lands" thesis. It claims that absence of certain kinds of economic value—primarily mineral and agricultural value—was the principal prerequisite for protecting areas. Scenic, recreational, or cultural values often constituted the publicized reason for protection, but the government only seriously considered them when mineral or agricultural value was minimal. The thesis obviously bears on conservation. Since there is no reason to expect conservation value and mineral or agricultural value to be inversely correlated, or the former to be positively correlated with high scenic or recreational value, areas protected for these reasons probably do not achieve conservation objectives to any meaningful degree. If correct, the worthless lands thesis suggests these areas may be worthless to conservation.

Runte (1972, 1977, 1979) chronicled the ideological factors behind the creation of Yosemite (1864), Yellowstone (1872), Mount Rainier (1899), Glacier (1910), Rocky Mountain (1910), Grand Canyon (1919), and other US national parks in support of the thesis. According to Runte, in the 19th century the US lacked an internationally reputed literary or artistic heritage, which invited criticisms from European intellectuals.[9] Runte argued one reason for creating national parks and (rightly) celebrating their importance as part of US heritage were feelings of cultural inferiority to Europe among US leaders. The natural wonders these parks protected were somehow taken to remedy this deficiency. For instance, when early explorer and surveyor of the Sierra Mountains Clarence King considered the Sequoia trees in 1864, he suggested that no, "fragment of human work, broken pillar or sand-worn image half lifted over pathetic desert—none of these link the past and to-day with anything like the power of these monuments of living antiquity" (quoted in Runte, 1977, p. 69).

Regardless of scenic or cultural value, however, the primary obstacle to protecting areas as national parks at the time (and continuing today) was farming, grazing, forestry, or mining potential. This was the US Congress' main contention in protecting Yellowstone, and it made very clear at the time that the Yellowstone Park Act (1872) would be repealed if commercial interests became apparent (Runte, 1977).[10] In Runte's estimation, the same non-commitment characterized much of the government's conservation agenda in the 1960s and 1970s. In 1968, for example, Congress failed to protect a major watershed within Redwood National Park from deleterious erosion caused by adjacent logging. Similarly, its ostensible prohibition in 1976 of mining in Death Valley National Monument and other national parks actually sanctioned much of the strip mining that had motivated the public to demand prohibitory legislation. Runte persuasively argued the historical precedent was manifest: "ecological needs have come in poor second because the nation has been extremely reluctant to forego any reasonable opportunity, either present or future, to develop the national parks for their natural resources" (1983, p. 138).

The history of protected areas in Australia tells a similarly dispiriting story. Like the US, Australian national parks and reserves were

[6] The ability to supply scientifically-compelling bases for conservation policy, which in turn provides cognitive ammunition in stakeholder negotiations involving politicians, is paramount to progress in ethically-driven sciences like conservation biology (see section 5).

[7] Those essential innovations were geographical information systems and population viability analysis. On the former, see the end of section 4 and Justus (2021). On the latter, see Beissinger and McCullough 2002, Ch. 1.

[8] For another interesting historical case study examining the impacts of political and public pressures, and personal values on perceptions of scientific integrity that bears on judgments of progress, see Steel & Whyte, 2012.

[9] For example, in 1820 English clergyman Sydney Smith queried, "In the four quarters of the globe, who reads an American book? or goes to an American play? or looks at an American picture or statue?" (quoted in Runte [1977], p. 67).

[10] And there is little doubt that the same fate would befall to other parks. But as tragic as it is clear, such reversion would undoubtedly not include restoring Native American claims to their homelands, lands that they were forcibly displaced from to create some national parks (Dowie, 2009).

established where forestry, agriculture, mining, or commercial development were not viable (Recher, 1976; Hall, 1988; Harris, 1977). For instance, the second legally designated national park after Yellowstone, Royal National Park (1879) outside of Sydney, Australia, "was largely rugged, dessicated sandstone plateau land, hopeless for agriculture and out of the way as far as Sydney Town of the 1870s was concerned" (Strom, 1979a, p. 46). Furthermore, its primary purpose was as a haven for recreation seeking Sydney residents (Hall, 1988). Other Australian protected areas, such as Flinders Chase National Park in western Kangaroo Island, were designated only after the government concluded they had little agricultural or mining potential (Harris, 1977).

Another problem with the predominantly economic focus was that protected areas were susceptible to declassification if judged economically valuable later. A favorable turn in wheat markets and coincident increase in the value of wool due to US purchases for the Korean War motivated the Australian government to delist reserves for agricultural and pastoral development. From 1954 to 1962 16,903 ha were delisted in Australia (Harris, 1977), a trend which continued well into the late 1980s (Hall, 1988). Similarly, under the Reagan administration, in 1986 the Fish and Wildlife Service recommended large portions of the Alaska Arctic National Wildlife Refuge be explored for oil and natural gas, despite predicted adverse effects on wildlife (Tobin, 1990).

But blatant disregard for conservation priorities was only part of the problem. The lack of a method was another. For example, even after the Australian Fauna Protection Act (1949) was enacted, absence of a systematic rationale for identifying areas to protect based on defensible conservation criteria prompted Strom (1979b, p. 68) to characterize the subsequent creation of parks and reserves as, "a scramble for whatever was offering." Unsurprisingly, the areas that had been protected by the mid-1970s poorly represented major vegetation types and fauna inhabiting them (Recher, 1976). In Tasmania, for instance, protected areas predominantly sampled low economic value alpine areas and buttongrass plains while other important ecosystems went unrepresented (Hall, 1988). The initially impressive increase in total protected area after the Fauna Protection Act—53 947 ha in 1937 to 249,260 ha in 1954—"must be tempered by the fact that in many cases the reasons for dedication had little or nothing to do with flora or fauna conservation" (Harris, 1977, p. 63). Instead, areas were protected because, economically, it made little difference. Although politically expedient—politicians could vaingloriously publicize the truly substantive area they had helped set aside for conservation, all the while without any real economic or political sacrifice—its cost was inadequate representation of Australia's species and ecosystems.

After the scope and severity of this problem was appreciated, conservationists still faced a serious challenge: demonstrating it beyond reasonable doubt. Doing so would preempt politicians from mollifying conservation demands by claiming existing, "worthless lands" protected areas achieved conservation goals. And, of course, conservationists also needed to identify, as precisely as possible, what areas should be protected for adequate representation of biodiversity. This would pressure politicians wishing to appear conservation-friendly to *actually* protect specific areas despite significant economic or political costs.

In combating powerful countervailing pressures, the lack of clear, quantitative assessments of the contribution (likely quite meager) extant protected areas made to conservation goals, and what other areas could contribute, hindered conservationists. Although such quantitative assessments do not guarantee governments would act appropriately, place-prioritization algorithms and geographical information systems produce these assessments,[11] and thereby provide clear information about the severe costs of forgoing conservation priorities. With these superior methods, which the increased speed and availability of microcomputers made much more readily accessible and useable, conservationists could employ this information to combat competing land-use interests more effectively.

4. An algorithmic turn in applied ecology: place prioritization algorithms

Understanding how place-prioritization algorithms (PPAs) helped overcome the obstacles described above—demonstrating the "wasted lands" thesis with scientific rigor (§3) and supplying a compelling reserve design methodology that IBT couldn't (§2)—requires recounting their emergence. The history is a case study of scientific progress.

Jamie Kirkpatrick, an Australian geographer with an interest in conservation, discovered the principle of complementarity at the core of PPAs in 1979 (Pressey, 2002). While attempting to prioritize Crown-owned lands in Tasmania, Kirkpatrick first tried simple scoring methods. These methods involve attributing quantitative scores to areas in a region based on the number and kinds of species it contains. With this methodology, Kirkpatrick noticed areas with several important species sometimes scored low, while several high scoring areas sometimes shared almost all the same species. Prioritizing protecting areas based on score would potentially inefficiently over-protect several species while under-protecting others. To rectify this problem, Kirkpatrick formulated an iterative, heuristic scoring procedure. After selecting the highest scoring area, scores of unselected areas were recalculated on the assumption the species in the highest scoring area were protected (i.e. complementarity). This ensured areas selected later complemented those selected earlier. Kirkpatrick's complementarity-based procedure was the first published PPA, first appearing in a somewhat obscure report (Kirkpatrick, Brown, & Moscal, 1980) and later in the only journal then devoted solely to conservation biology, *Biological Conservation* (Kirkpatrick, 1983).

In conservation contexts, complementarity was independently discovered three additional times: in the United Kingdom (Ackery & Vane-Wright, 1984), Australia again (Margules, Nicholls, & Pressey, 1988), and South Africa (Rebelo & Siegfried, 1990).[12] The first two complementarity-based PPAs were manually calculated, pencil and paper. By the late 1980s A. O. Nicholls utilized the increased availability and sophistication of microcomputers to program the first computerized PPA, presented in Margules et al. (1988). Besides heuristic PPAs, exact PPAs also originated in the 1980s in Australia. Cocks and Baird (1989) first utilized a commercial integer programming software package to identify optimally efficient reserves. They prioritized areas within the Australian Eyre Peninsula according to several representation goals. Advances in microcomputing throughout the 1980s made this computationally intensive analysis, virtually impossible a decade earlier, feasible.

[11] Before PPAs, other scientific approaches to reserve design made some progress filling the post-IBT methodological vacuum. For example, a GIS-based process labeled "gap analysis" was revealing the dramatic shortfalls of existing reserves and priorities for protecting new areas by the late 1980s (see Scott et al., 1993 and Justus, 2021, Ch. 6). The critical habitat designations the Endangered Species Act legally necessitated, as well as so-called biodiversity "hotspot" analysis (Myers, 1988) also merit mention. But although these methods were improvements over what came before, they were all dramatically inefficient—especially compared with PPAs—at maximizing biodiversity representation while minimizing the total area protected because they did not conform to the principle of complementarity (see Reid, 1998 and below). Even GAP analysis suffered from this deficiency until the relevant GIS was integrated with PPAs in the early 1990s.

[12] See Justus and Sarkar (2002) for a detailed history. For a comprehensive review of the impressive and legion advances that now encompass the wide-array of sophisticated methods used in contemporary place-prioritization, see Moilanen, Wilson, & Possingham, 2009. Some of the most important advances involved integrating insights about population viability, habitat suitability, resource selection and utilization, and others from ecological theory to more directly incorporate persistence and maintenance concerns into reserve design for biodiversity conservation.

From the outset, the importance of PPAs to reserve design was abundantly clear to its developers and most of the community of applied ecologists working on conservation issues.[13] This helped shift the goal from preserving maximum species richness at some hypothesized future equilibrium level as putatively counseled by IBT (see §2.1) to representing biodiversity surrogates now.[14] In methodological terms, it refocused attention from general theories to algorithmic procedures that require geographically explicit data. By producing complementary sets of areas achieving representation goals, PPAs ensure biodiversity is represented efficiently in the smallest total area possible, which is imperative given the limited monetary resources available for conservation. As Margules et al. (1988) emphasized about the SLOSS debate, conservation biologists had excessively focused on ecological processes within potential reserves without first adequately understanding how to represent biodiversity in the first place. PPAs rectified that deficiency.

In so doing, PPAs filled the methodological hole the failings of general reserve design principles had left. This transition was on clear display in a special issue of *Biological Conservation* a year after Margules et al. (1988) proposed the first computer-based PPA. The issue's overarching topic was the growing trend towards computer-based methods, led by Australian conservation scientists. In its introduction, Margules (1989a) indicated that none of the papers addressed the long-term adequacy of reserves, for two reasons. First, he emphasized processes leading to extinction were poorly understood. Despite claims made on behalf of IBT—claims contrary to its originally exploratory nature—long-term field studies required for a better understanding of extinction had yielded few helpful insights. The second reason constituted the conceptual basis for PPAs:

> knowledge of patterns of species distributions has priority over a knowledge of ecological processes in our efforts to maintain biological diversity. Techniques for managing reserve systems to prevent extinctions will not maintain diversity if the reserve systems being managed do not contain the full range of species in the first place (Margules, 1989a, p. 8).

As a data-driven methodology, PPAs supplied the scientific basis for reserve network design IBT could not. Its focus on geographically referenced data, not hypothesized equilibria derived from controversial theory, and its mechanistic application of explicit and relatively uncontroversial conservation criteria made results of place-prioritization compelling.

Based on his analysis of Crown lands in Tasmania, for example, policymakers acted on all of Kirkpatrick's (1983) seven recommendations for new conservation areas (Pressey, 2002, see Fig. 2), an unprecedented success.

This stood in stark contrast to Australian politicians' legacy of ignoring the counsel of informed conservation interests in and outside government (Harris, 1977). The explicitness of his algorithm helps explain its power. In an interview, Kirkpatrick added that its effectiveness was due to, "the desire of the forestry people to appear scientific in their conservation efforts … the logic of the process, and its minimalism, also appealed" (quoted in Pressey, 2002, p. 436). Correct perception of algorithms as repeatable, objective processes that harness the power of sound data made policymakers more receptive to the priorities they yielded, especially as a scientifically grounded alternative to more economically threatening agendas of other conservation groups in Tasmania (Kirkpatrick personal communication).

With such a different data-driven approach, it is unsurprising that prioritization results often conflicted with intuitions motivating earlier strategies supposedly inspired by IBT. In general, complementarity conflicted with Ehrenfeld's (1976, p. 653) assertion that, "The need to conserve a particular community or species must be judged independently of the need to conserve anything else," and the actual prioritizations PPAs produced revealed similar conflicts. Margules et al. (1988), for instance, prioritized areas within the Macleay Valley Floodplain of northern New South Wales with respect to two targets: (i) representing 98 native plant species; and, (ii) representing these species and nine wetland habitats. The two PPAs they implemented for each target selected 44.9% and 75.3% of the region analyzed, respectively. Most importantly, similar to Kirkpatrick's (1983) analysis, the pattern of selected areas achieving the targets did not fit *any* of Diamond's (1975) reserve design principles (Robert Pressey, personal communication). Unlike IBT, reserve design based on algorithmic prioritization depends essentially on the specific locations of biodiversity features, in this case species and habitats. Their analysis therefore confirmed with hard data the general suspicion that reserve design principles that ignore this kind of geographically explicit information would usually yield poorly representative reserves (see §2.1). Protecting a few large areas in the Macleay Valley Floodplain might protect many species and habitats, but not all 98 native species and nine wetland types, nor would they be protected in the smallest area possible (Chris Margules, personal communication).

Besides freeing reserve design from the grip of IBT, one essay in the special issue also demonstrated how PPAs could verify the "worthless lands" thesis, by revealing with quantitative precision how poorly existing reserves represented biodiversity.[15] With a modified version of an algorithm of Margules et al. (1988), they prioritized areas in western New South Wales for targets of one and five representations of 128 "land systems." Somewhat similar to habitat types, land systems were classified according to topography, soil, and vegetation type. Their analysis quantitatively confirmed what history strongly suggested (see §2.2): the 13 existing conservation areas in the study region poorly represented the 128 land systems. Pressey and Nicholls (1989) conducted four prioritizations: (i) two with the two targets in which the 13 existing conservation areas were assumed protected and the land systems they contained already protected; and, (ii) two with the same targets in which the 13 areas were not assumed protected. To achieve the first target, 11,503 km^2 was required if the 13 areas were assumed protected, compared to 7980 km^2 if they weren't. The five representation target required 30,065 km^2 if the 13 areas were assumed protected, 28,726 km^2 if they weren't. The additional area required for the one representation target was 44% of the total area required when the 13 conservation areas were not assumed protected, a dramatic demonstration of substantial inefficiency. This was especially problematic since the total area required for the one representation target, let alone the five, when the 13 areas were excluded exceeded all reasonable estimates of the financial resources available to protect areas. Every conservation dollar counts, and extant protected areas wasted them.[16]

[13] This was not true in the United States. For example, the first conservation biology textbooks, written by US conservation biologists, didn't discuss PPAs at all (Primack, 1993; Meffe and Carroll 1994). Much of their discussion of reserve design focused on Diamond's (1975) principles, whose epistemic credentials had aged poorly over almost two decades. Meffe and Carroll (1994), for instance, incredibly claimed the species-area relationship justified large conservation areas over small ones.

[14] In this context, surrogates are empirically tractable measures for which distributional data are available or attainable that represent biodiversity in place prioritizations (see Sarkar, 2005, Ch. 6 for a philosophical treatment). Specific species with easily assayable distributions, or environmental factors for which georeferenced data can be acquired via remote sensing (Sarkar et al., 2005), are common biodiversity surrogates. The more general and empirically driven concept of surrogates has replaced ineffective and problematic appeals to flagship, keystone, and umbrella species in biodiversity planning (see Andelman & Fagan, 2000; and Caro, 2010 for a thorough review).

[15] Pressey and Nicholls (1989) and Margules (1989b) share the distinction of being the first to do this.

[16] "Wasted" with respect to the goal of maximizing biodiversity representation while minimizing the total area protected. Of course, there are other values protected lands may realize, and the cost to protect places may vary significantly for different regions. These qualifiers are implicit in our invocations of "waste" and "cost" relative to conservation.

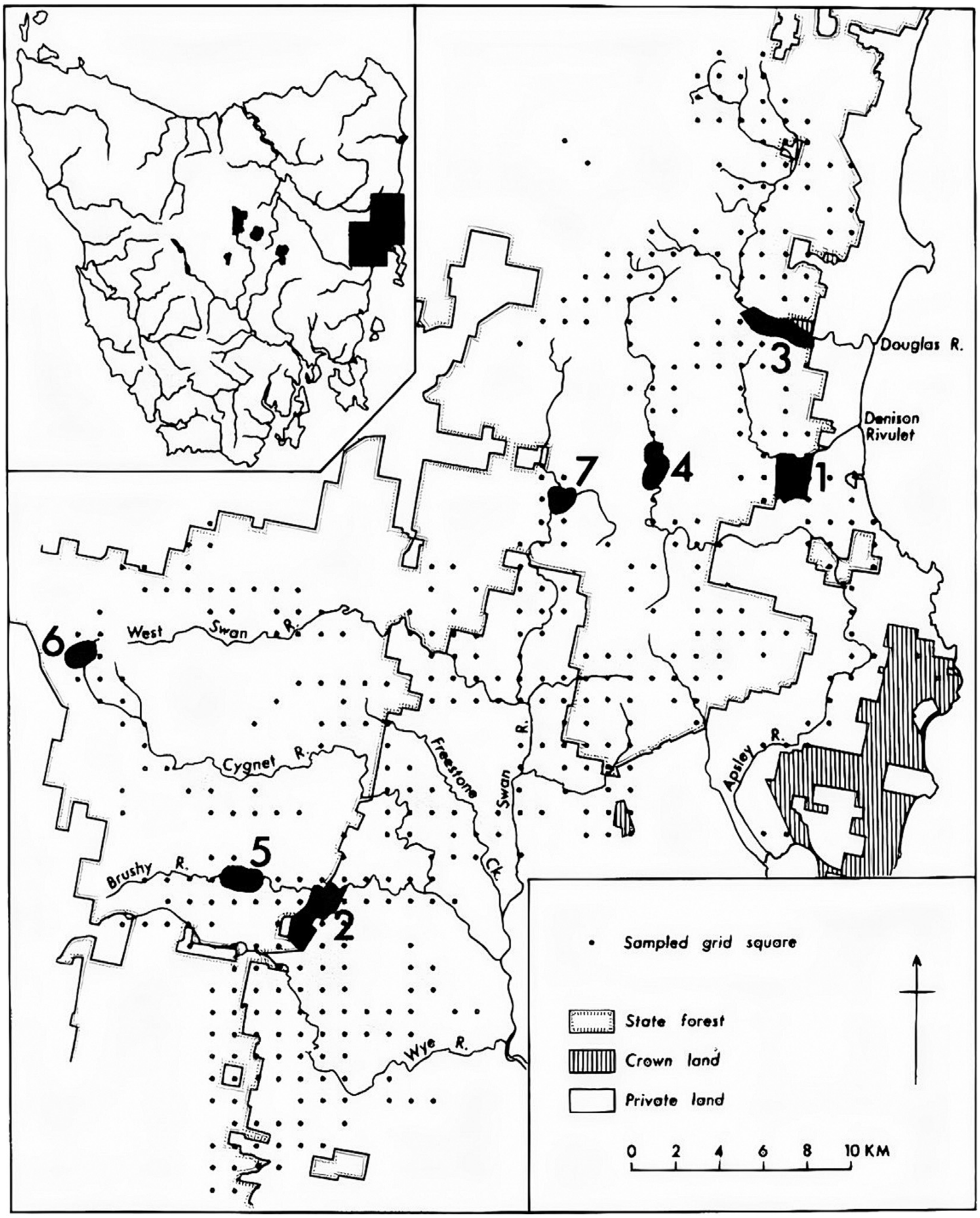

Fig. 2. Priority areas for conservation in the Crown owned lands of Tasmania. The number labels indicate the ordinal priority of each shaded area (From Kirkipatrick [1983], p. 133.).

Pressey and Nicholls (1989) also noticed a second negative aspect of the 13 already protected areas. Besides poorly representing land systems, they also hindered efficient construction of a fully representative reserve system. Even if areas complementary to the 13 conservation areas were protected, the resulting reserve system would still achieve representation targets much less efficiently than one constructed from scratch. Pressey and Nicholls did not recommend doing so but delisting some or all of these 13 areas might therefore improve efficiency of future reserve design in New South Wales.

Margules (1989b) reached similar conclusions about conservation areas in the mallee lands of South Australia. Using a slightly modified version of an algorithm from Margules et al. (1988), Margules (1989b) found that only 6 of the 18 areas required to achieve one representation of 45 vegetation "alliances" were included in existing reserves. Hence, 14 of the 21 conservation areas in the region were superfluous for this representation target. Furthermore, if these 21 areas were assumed protected, 81.50% of the region was required to represent each alliance, compared to 69.11% if the reserve system was constructed from scratch.

In their prioritization of the Cape Floristic Region of South Africa to protect 326 taxa (species and subspecies) of Fynbos vascular plants, Rebelo and Siegfried (1990) showed less than 27% of existing conservation areas and 50% of areas proposed for protection included areas of high Fynbos endemism. Furthermore, they suggested De Hoop Nature Reserve, which composed 55% of the Cape's total protected area, was in its least diverse part and should be deproclaimed in exchange for new areas containing more Fynbos plants. Although conservation biologists had considered deproclaiming reserves before, this was one of the first quantitative justifications for deproclaiment, based on a PPA. The "worthless lands" status of reserves could no longer be simply ignored by the powers that be.

Throughout the 1990s an Australian conservation biologist, Robert Pressey, publicized the capability of PPAs to quantify deficiencies of existing reserves objectively (Pressey 1990a, 1990b, 1992, 1993, 1994; Pressey, Bedward, & Nicholls, 1990, Pressey & Tulley, 1994). He also recognized that the speed and flexibility of PPAs, especially heuristic PPAs at the time, would make them powerful tools in policy making contexts. First, heuristic PPAs could rapidly and quantitatively demonstrate the requirements of various representation targets. This helped expose politicians' tepid commitment to conservation, and thwart the pattern of, "politically driven reservations [that] are often no more than environmental gestures of real concern" (Pressey, 1992, p. 20). Place-prioritizations quantified the often severe conservation costs of land-use policies, which could then be conveyed to the public.

Second, PPAs were flexible enough to implement several different kinds of constraints on place-prioritization. For example, PPAs could explicitly exclude inappropriate areas or mandate inclusion of others, as well as incorporate other criteria, such as adjacency or compactness, into prioritization (see Margules and Sarkar 2007). The adjacency criterion, first integrated into PPAs by Nicholls and Margules (1993), prioritizes adjacent areas over non-adjacent ones and tends to generate reserve systems with areas clustered around a few locations. The criticism, then, that "complementarity-based algorithms are biased in favor of selecting small, potentially isolated reserves containing relatively low species abundances" (Linquist, 2008, p. 541) is misinformed. PPAs are neutral on reserve size in two ways. First, the degree of "clumping" of selected cells can be intentionally varied with adjacency, compactness, and other selection criteria. It's entirely up to the algorithm user to specify the appropriate degree, which certainly can be quite high. Second, PPAs are neutral on cell size. It's entirely up to the algorithm user to determine their size when gridding the region of interest into cells. The size can be very large, and often is to accommodate the habitat requirements of some species. And if abundance data is available, prioritization can utilize that information in a variety of ways as well.

The criticism that, "complementarity-based algorithms overlook the habitat requirements of particular species," (Linquist, 2008, p. 542) similarly shortchanges PPAs. The nature of the claimed problem is clarified:

> "A complementarity approach might prioritize areas containing high levels of frog diversity, but inadvertently exclude suitable breeding habitats where these animals are rarely found. Although island biogeography theory does not take particular habitat requirements into account, its preference for large continuous land reserves errs on the side of caution. (p. 542)."

With respect to the first sentence, it "might" but it certainly need not. If knowledge of that habitat exists, the breeding sites can easily be included in any prioritization. In fact, their selection can be *mandated*. PPAs are very flexible tools that can integrate many different kinds of information. They are regularly integrated with species distribution models that would typically capture these habitat relations. If there is any potential problem here, it's a dearth of information. With respect to the second sentence, besides IBT omitting consideration of habitat requirements, it's deeply unclear what caution counsels given finite budgets for land acquisition. Large area *here* means smaller and/or fewer areas *elsewhere*. Absent distributional data, which scenario is superior is utterly unclear. In general, PPAs are tools that complement and can integrate, rather than compete with, sound input from ecological science.

A final supposed problem is worth briefly addressing. Linquist (2008, p. 542) says, "A final shortcoming of the complementarity approach is that it is sensitive to 'apparent novelties' or species that appear rare in a region but occur in large numbers outside the area being investigated." But as common sense dictates, such species should, and almost surely would, be excluded from the place-prioritization analysis, absent some overriding reason, such as a desire to protect bald eagles in the continental United States as a national symbol even given abundant populations in Canada and Alaska. Nothing about PPAs forces the hand of their users. It's up to them to make the obvious exclusion.

Given their speed and flexibility, conservation biologists could use PPAs to revise conservation planning strategies rapidly when new information about the analyzed region became available or politicians wanted to consider different representation targets (Pressey & Nicholls, 1989). Although heuristic PPAs did not guarantee optimally efficient reserves as exact PPAs did, a weakness first pointed out by Cocks and Baird (1989), in the 1990s the computation time the latter required slowed, or worse prevented effective conservation planning altogether, especially for analyses of large regions (Pressey & Tulley, 1994). This explains the early predominance of heuristic PPAs over exact PPAs in real-world conservation planning.

In policymaking contexts, these features of PPAs made negotiations between conservation and competing interests more explicit and quantitatively rigorous, especially when combined with the analytic and computational capabilities of geographical information systems.[17] This combination, in turn, resulted in unprecedented conservation successes (see Finkel, 1998). For conservation biology, it's difficult to imagine a clearer case of scientific progress. But the seeming indisputability of that judgment conflicts with the reining philosophical accounts of scientific progress.

5. "Progress" by any other name

In one of the new discipline's first manifestos, Soulé (1985) deemed conservation biology a "crisis" discipline with an explicit normative agenda: preserving biodiversity. Providing a scientific basis for efforts to do so was (and is) the overarching goal, and this substantially shaped its early aspirations, priorities, and real-world conservation strategies. But as the preceding discussion makes clear, without algorithmic innovations—place-prioritization algorithms in particular—supplying that basis, and its emergence as a *rigorous science* in general, would have been impossible.

Despite the impressive credentials of the advances recounted above, that they constitute real "progress" has been called into question. Linquist (2008, p. 531) is skeptical: "[T]he adoption of complementarity-based algorithms in place of theoretically motivated conservation guidelines has arguably not advanced the field of conservation biology." Some misconceptions about PPAs motivate this judgment, as well as an apparent underappreciation of how badly Diamond's design principles fare empirically (see §4). But the core of the criticism seems to be something else, a view of what scientific progress *should* look like. After noting how conservation biologists have largely abandoned Diamond's principles, rightly given their failings described above, Linquist (2008, p. 530) admonishes, "But instead of rigorous autoecological studies, *something much less scientifically grounded* emerged in their place. Conservation biology has become dominated by various 'fast and frugal' place prioritization algorithms for designing conservation reserves" (emphasis added). As the juxtaposition insinuates, PPAs comparatively lack a scientific grounding that IBT at least could have, even if numerous empirical studies suggest it does not.

There are two interrelated aspects of this contention. The first concerns the assumptions underlying the "instead of rigorous" and "much less scientifically grounded" insinuations. Linquist then says, "the core principle on which most place prioritization algorithms are based—the principle of complementarity—is not ecologically sound" (Linquist, 2008, p. 531). It's hard to cognize the motivation behind this claim. 'Complement' in 'complementarity' is set-theoretic. In the course of a

[17] Geographical information systems supplied a similarly powerful tool for reserve design, one which complemented and enhanced PPAs. For a detailed account of its emergence and impact on conservation biology see Justus (2021).

prioritization, the biodiversity surrogates in already selected cells are assumed to be protected. Selecting by complementarity requires selecting the cell that maximizes surrogate representation in the complement of biodiversity surrogates not yet assumed protected. Since the goal is protecting as much as possible with the usually severely limited resources available to do so, complementarity simply expresses how set-theoretic facts dictate what is instrumentally rational relative to this overarching objective. Given distributional data on biodiversity surrogates, PPAs take that information as input and then uncover what areas should be protected to achieve various specific conservation goals. Effectively, PPAs confirm some conservation-salient if-then statements while disconfirming others. The principle of complementarity is not an ecological principle whose soundness depends on findings in ecological science any more than the formula for a mean is biological and subject to evolutionary analysis when employed in studies of population genetics.

The second aspect concerns the much broader issue of how scientific progress should be conceptualized. Linquist's unfavorable depiction of PPAs as "not ecologically sound" because they lack "theoretical motivation" seems to evince a tacit theory-centric view of progress. This partiality is widespread. Niiniluoto (2017, p. 3300), for instance, insists that, "According to the realist views of scientific progress … science makes progress on the level of theories." Knowledge-first views also predominate. Kitcher (1995, p. 93), for instance, contends that, "The most obvious pure epistemic goal is truth. Talk of truth—or approximation to truth—has dominated philosophical discussions of scientific progress." In an influential analysis, two of the three approaches to scientific progress Bird (2007) recognizes—epistemic approaches that gauge progress in terms of acquiring knowledge, semantic approaches that gauge progress in terms of scientific theories approaching the truth, and "functional-internalist" approaches that gauge progress in terms of "problem-solving" á la Kuhn—embody the pervasive knowledge and theory focus. Bird decidedly favors epistemic approaches, but they (and semantic approaches) seem utterly ill-equipped to account for progress in applied, ethically-driven sciences. These sciences don't deliver anything resembling justified true beliefs about a mind-independent cosmos, at least as that idea is usually philosophically expressed about, say, particle physics. Instead, they supply data-driven, evidence-based, and in the present instance algorithmically-rigorous means for achieving ethical goals. It therefore seems only a functional account, appropriately stripped of unnecessary Kuhnian trappings, can correctly judge that these achievements constitute genuine progress.[18]

Unintuitiveness is Bird's main complaint against functionalist accounts—"puzzle solving" by false theories is intuitively not scientific progress (Bird, 2007, p. 83)—and that Kuhn's view of the nature of science is flawed. Although the latter might be correct, the former judgment (and standards underlying it) should be rejected. Intuition trading is, of course, a game for those who believe intuitions are philosophically important. But wading into that meta-philosophical debate isn't necessary to appreciate that Bird's intuition is indefensible, especially for ethically-driven sciences. First, what generates the supposed unintuitiveness is a theory's falsity. Anti-realists unencumbered by such semantic preoccupations are accordingly free to evaluate whether the putative advances theories afford constitute progress on other, less obscure grounds.

Second, the intuition arguably conflicts with the undeniable utility of false models in science. Highly-idealized models with unrealistic assumptions—ignoring system components and interactions, disregarding their spatially-explicit nature, treating interactions as instantaneous, representing discrete components with continuous variables, etc.—feature crucially in scientific inference (Cartwright, 1983; Godfrey-Smith, 2006). These models are common currency in ecology, and many other sciences, where "developing maximally simplified and predictively powerful principles and models—without concern for whether those, often patently false models, accurately represent things as they are in nature," (Donhauser, 2016, p. 74) is the norm. Conservation biology, which originally emerged from ecology, is even less tethered to veridicality as an applied science with an explicit ethical agenda (see Odenbaugh, 2003).

Third, given that agenda, truth-values and theory status are obviously ancillary for properly gauging progress in these sciences.[19] Uncovering fundamental truths about the world is not the point; delivering scientific insights and tools that promote achieving ethical goals is. Conceivably, a grand, unified, true theory of biodiversity might eventually emerge that would synthesize and consummate all of ecological science. That theory would be an unparalleled achievement in biology, perhaps rivaled only by Darwin. But its progressive import *for conservation biology* would be measured by how it advanced the agenda of conserving biodiversity.[20]

Although arguably the philosophical underdog, the original functionalist approach (Kuhn 1962, 1977; Laudan 1977, 1981; Lakatos, 1978) has several recent defenders (Longino, 1990, 1996; Douglas, 2014; Shan, 2019). Their details differ, but each defense tries to articulate an account of scientific progress that encompasses practical, non-theoretical advances like those discussed above. Some are more successful than others. For example, Shan (2019, p. 744), one of the most recent and developed, proposes that, "Science progresses if more useful research problems and their corresponding solutions are proposed." Besides problem-*solving*, the emphasis on problem-*defining* is supposed to circumvent deficiencies of the original view. Putting "unintuitiveness" aside, they include problems of "sufficiency," "quantitative-weighting," and "internalism."

Consider sufficiency first. For Kuhn, problem-solving power solely determines scientific progress: efficiency increases in this power constitute progress, decreases constitute regress. The supposed sufficiency problem is two-fold: (i) problem solutions derived from false theories mark progress, and (ii) Kuhnian shifts to a new true (or truer) theory that solves fewer problems mark regress. Shan goes to great lengths arguing the emphasis on problem-defining discharges the sufficiency problem. In our eyes, that effort is for naught because there's nothing to discharge. Ostensibly, (i) and (ii) are only problematic for those with realist inclinations who find the uncoupling of truth from problem-solving unintuitive and therefore philosophically unpalatable. The sufficiency

[18] Of course, if the conceptions of truth, belief, and (therefore) knowledge are relaxed away from mind-independent connotations, *some kind* of epistemic or semantic approach may not be impossible. If the human valued, mind-dependent if-then statements the PPAs evaluate are within an epistemic-semantic purview, perhaps some notion of progress could be developed. But as it stands, this would-be (and appropriately) non-standard account is conspicuously and unsurprisingly absent from the literature, unsurprising given that standard accounts overwhelmingly emphasize mind-independent targets of the "true belief" in "justified true belief." It's that overly narrow focus that favors the much more inclusive functionalist approach, which not only accommodates the mind-independent truths and knowledge claims at the heart of traditional epistemic-semantic views, but also the mind-dependent if-then truths that (non-standard) epistemic-semantic views *might* capture, *and* the practical, societally-valued achievements that we've argued propel progress in conservation biology and other ethically driven sciences.

[19] The ancillary import of ascertaining true beliefs and fundamental theoretical insights reinforces Elliott and Rosenberg's (2019, 1) defense of 'citizen science' against the criticism it is "not appropriately hypothesis-driven." A related objection complains the ubiquitousness of socio-political values undermines citizen scientists' ability to conduct honest scientific inquiry. Citizen science "is tainted by advocacy and is therefore not sufficiently disinterested" (ibid., 2). Besides the undeniable fact that values pervade even the classically descriptive sciences (ibid., 5), our analysis of algorithmically-based advances in conservation biology shows how a bias for Knowledge and Truth can, somewhat unintuitively, lead scientists and philosophers astray in judgments of scientific progress.

[20] Of course, achieving the goals of ethically-driven sciences—improving human health, increasing economic well-being, fostering cooperation and sociality, conserving biodiversity—are hardly akin to "puzzle-solving."

problem therefore seems derivative on unintuitiveness worries, not a new concern, and only worrisome for those favoring intuition-focused philosophical methodology.

Beyond that methodological point, Shan's proposed solution raises independent issues. He argues *research problem defining* is also an essential feature of scientific progress: "[w]ell-defined research problems should be at least as important as the good solutions in scientific practice … how to define a good research problem is somehow crucial to guiding the future explanatory and investigative study" (Shan, 2019, p. 744). Since problem-defining is "at least as important as" problem-solving, improvements in the former can outweigh losses in the latter. This avoids the supposedly unacceptable implications of Kuhn's (and Laudan's) exclusive focus on problem-solving, which Shan also criticizes for not appreciating the complexities of problem-defining: "Kuhn and Laudan implicitly assume that these problems are either simply pre-defined or defined in a straightforward way … the significance of problem-defining seems not to be fully recognized … Problem defining is much more than proposing a problem" (2019, p. 744).

Defining research problems is obviously crucial to scientific practice, but it will never be "at least as good as" problem-solving for ethically-driven sciences. The relevant progress metric for sciences with explicitly normative agendas is the degree they scientifically advance those agendas, principally by helping solve problems thought culturally, socially, or politically important enough to merit scientists' efforts to do so. Establishing land-sharing corridors to counteract habitat loss and fragmentation for Golden Tamarins grew the endangered population from fewer than 200 in the 1970s to over 2500 by 2003 (Buckley and Fernanda 2015); creating aquaculture and hydroponic methods for non-arable soils or small urban spaces achieves sustainable agricultural development goals (Goddek, Joyce, Kotzen, & Dos-Santos, 2019); discovering that an ineffective cancer drug (azidothymidine) was actually a successful HIV treatment catalyzed research on antiretroviral therapies that led to new drug classes and over 30 antiretrovirals now standardly used in combined HIV therapies (Broder, 2010). Developing new *research* problems and topics in Shan's (2019) sense—initially uncovering them, precisely defining and refining how they are characterized, determining how they are best investigated, etc.—is, at best, a positive prelude (or after-effect) to the main objective, actually helping solve ethical problems.[21] If the scientific insights acquired and tools employed fail in this regard, but somehow end up catalyzing and defining stimulating new research problems, that research success would certainly constitute some kind of epistemic-semantic advance (or facilitate one in the long run), but it would pale in significance against failing the main objective. Shan's priority on defining new *research* problems in assessing progress, "introducing new concepts … proposing new hypotheses … designing and undertaking new experiments" (2019, p. 755), still overemphasizes cognitive outcomes and undervalues enhancing the practical "capacity to predict, control, manipulate, and intervene" (Douglas, 2014, p. 62) that more accurately characterizes progress in ethically-driven sciences. In discussing Mendel's scientific achievement, for instance, Shan claims Mendel's problem-defining and problem-solving were "*more fundamental* to accounting for the nature of scientific progress" than those practical capacities because the former "underlie[s]" the latter (*ibid.*; emphasis added). Such a fundamentality judgment coheres well with an epistemic-semantic view that prizes crucial cognitive insights and deep understanding of natural phenomena.[22] But it makes little sense for a functionalist account intended to capture the advances in ethically-driven sciences that actually help solve important problems.

The quantitative-weighting problem confronting the original functionalist view concerns how identifying and solving problems of different significance is quantifiable into a single progress metric. For Laudan (1981, p. 149), for example, problem-solving power varies directly with the number of important empirical problems solved and inversely with the significant empirical anomalies and conceptual problems generated. Shan (2019, p. 742) criticizes this approach as, "too oversimplified and vague to be helpful," because empirical problems vary in importance, it's unclear their importance is quantifiably evaluable, and the significance of anomalies and conceptual problems are arguably incommensurable.

Shan's (2019, p. 746) solution is to abandon the ambition, and instead construe "useful" in "useful research problems and solutions" of his proposal as a strictly *qualitative* notion concerned with repeatability, reliability, and testability of research problem defining and solving. That move obviously complements Kuhn's (1977) powerful case against an algorithm for theory choice, with which we sympathize. But, our case study demonstrates that complete qualitative defeatism is premature. There are clear quantitative ways of measuring the significance of problems like biodiversity loss and underprotection, and their proposed conservation solutions. To reemphasize one example among many, place-prioritization algorithms (PPAs) and geographical information systems (GIS) furnished conservation biologists with rigorous methods for revealing, quantitatively and compellingly, the representational inadequacy of existing reserves (see §3 and §4). Given the overarching goal to conserve biodiversity, the significance of that inadequacy is transparent, measurable, and momentous.[23]

Shan's last issue with previous functionalist accounts is that they maintain scientific communities best judge genuine scientific progress, due to their (supposedly, Shan avers) unique expertise in recognizing and evaluating a solution's problem-solving power. But Shan claims the history of science undercuts this positive appraisal. Shan cites biometricians rejecting the progressiveness of Mendelian genetics as displaying the "problem of internalism" afflicting Kuhn and Laudan's view. For Shan, biometricians undervalued the *usefulness* of Mendel's understanding of heredity, and thus "the usefulness of a certain set of problem-defining and problem-solving is not obvious to the scientific community. The progress thus achieved is not known or judged by the community" (2019, p. 747).

Shan doesn't offer a solution to this problem. Instead, he simply stipulates that "usefulness is a relative concept." (ibid.), and that his account asserts nothing about who can or should judge progress. But this agnosticism seems premature. If *scientific* communities can't judge when *scientific* progress occurs, who or what possibly could? Relativizing

[21] Moreover, problem-defining in ethically-driven sciences is usually not the complicated undertaking it can be in other sciences. Given their disciplinary organization around achieving explicitly-stated objectives society deems ethically valuable, the metrics for success are much clearer and thus the potential problems thwarting such success more obvious. That habitat destruction and fragmentation adversely affect Golden Tamarin populations is obvious. Demonstrating that corridors would (and did) effectively mitigate those effects—i.e. solve the problem—required sophisticated scientific analysis. Problem-solving typically outshines problem-defining when gauging progress in ethically-driven sciences.

[22] Shan (2019), following Kuhn and Laudan, does distinguish his account from traditional epistemic-semantic views that favor theoretical knowledge and propositional truth. But he also clarifies that his view accommodates an epistemic emphasis on non-theoretical knowledge and semantic perspectivalism about truth (2019, 751–754), and stresses that it substantially differs from Douglas' practical capacities view, which he faults for overlooking the significance of theoretical understanding and conceptual innovations (2019, 755).

[23] A deeper pessimism might motivate this judgment. Shan (2019, 753–754) expresses additional pessimism that, "I do not see that there is any true or correct solution to a research problem, given its practical nature … it is implausible to claim that there is a true or correct way of experimentation or problem-refining." But that flies in the face of the undeniable success of PPAs and GIS, *contra* island biogeography theory for reserve design. To suggest there was no correct solution, in this case, would contradict the perspective of most conservation biologists. Of course, how 'true' and 'correct' should be construed is the critical question. Construed along epistemic-semantic lines, Shan's claim is understandable. But as pragmatists tirelessly counsel, these concepts are best characterized by precisely the kinds of successes we've described (James, 1907).

usefulness so completely ignores the fact that scientists are *usually* reliable arbiters of what constitutes progress in their domains of expertise. Conservation biologists recognized, rapidly and of course correctly, the vast superiority of PPAs and GIS over earlier approaches. Advances in other sciences are usually assessed and assimilated similarly, although the assessments sometimes require more time to stabilize. *Sometimes* scientific communities make mistakes, but the biometrician example appears to be an outlier exception that doesn't make the rule.

In an analysis primarily devoted to revealing the philosophical *in*significance of a pure vs. applied science distinction, Douglas proposes an increased capacities account of scientific progress that best reflects what PPAs and GIS achieved in conservation biology. The focus on the ability "to predict, control, manipulate, and intervene in various contexts," (2014, p. 62) captures the priority of the practical over the theoretical in ethically-driven sciences:

> This is the kind of [practical] success that translates well across paradigms, that is rarely lost with theoretical change, and which matters greatly to both scientists and the public. While paradigm change can create losses in understanding or losses in explanatory unification as clear conceptual structures are swept away, what is not lost is the ability to predict phenomena and/or the ability to control aspects of the world. (2014, p. 62).

The emphasis on practical success and what matters to the broader public embraces, Douglas (2014, p. 63) correctly stresses, a "more socially, ethically mediated conception of progress." Science is a human activity conducted in complex social-political contexts; judging its successes is fraught with contextual values (Longino 1990; Elliott 2007). Those values permeate ethically-driven sciences, and further magnify the focus on achieving societal goals: "[s]cientific inquiry directed at reducing hunger, promoting health, assisting the infirm, protecting or reversing the destruction of the environment, is valued over knowledge pursued … for knowledge's sake," (Longino 1996, p. 48).

Of course, not every advancement towards these goals is actually *scientific* progress. The adjective is essential and whether the proposed advance emerges from recognizably scientific activity is the key question. A sharp demarcation criterion is not needed (or indeed possible); the threshold is obviously vague. But there is a threshold, and scientific progress occurs, or doesn't, on either side of it. If belief in quinine's remedial power for malaria originated from a febrile individual ingesting water (unknowingly) suffused with *Cinchona* plant matter (Achan et al., 2011), that fortuitous happenstance obviously does not amount to a breakthrough of medical science. Accident rather than deliberate scientific investigation was responsible for the fortunate outcome. And if deliberate mystical or religious doctrine, rather than careful experimentation, observation, and inductive inference, yields a similarly felicitous outcome, that's also not scientific progress towards some ethical goal. The provenance of a method, outcome, practice, or belief is crucial to judging whether it counts as scientific. On those dimensions the judgment for PPAs and GIS could hardly be clearer: they were the deliberate product of scientists and supply data-driven, algorithmically-rigorous, evidence-based means for achieving the ethical goal of conserving biodiversity. Only a question-begging commitment that acquiring (theoretical) knowledge or accumulating true beliefs is necessary for scientific progress could deem them merely "technological progress" (Niiniluoto, 2017).

Interestingly, despite championing the epistemic notion, Bird endorses a broader understanding of progress towards the end of his analysis that surprisingly accords with Douglas' view. He says (2007, p. 83):

> Our conception of scientific progress is linked to what we take the aim of science to be. In general, something like the following principle holds:
> (A) if the aim of X is Y, then X makes progress when X achieves Y or promotes the achievement of Y.

Precisely. With respect to whether (A) holds when Y = biodiversity conservation and X = place-prioritization algorithms, let's review. Within roughly two decades, PPAs transformed reserve design from a problematically intuition and theory-driven affair into a data-driven, quantitative science. Specifically, this methodology demonstrated why conservation biologists should abandon principles "inspired" by island biogeography theory, and they provided a defensible alternative, an explicit methodology moreover, that maximizes representational efficiency given limited budgets for designating reserves. This methodology also produced quantitative (usually critical) assessments of existing "reserves" that helped expose Machiavellian subterfuge about being conservation-friendly by politicians and other power brokers. Finally, PPAs provided area-specific reserve design recommendations and rapid assessments of proposals in policy-making contexts that led to unmitigated, and unprecedented conservation successes. If this doesn't constitute progress in an applied science, nothing does.

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