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Beneath the Straw: In Defense of Participatory Adaptive Management

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Abstract Our recent paper advocating adaptive management of invasive nonnative species (INS) in Kings Bay, Florida received detailed responses from both Daniel Simberloff, a prominent invasion biologist, and Mark Sagoff, a prominent critic of invasion biology. Simberloff offers several significant lines of criticism that compel detailed rebuttals, and, as such, most of this reply is dedicated to this purpose. Ultimately, we find it quite significant that Simberloff, despite his other stated objections to our paper, apparently agrees with our argument that proposals for alternative management of established INS (i.e., alternatives to minimization/eradication) should not be rejected on an a priori basis. We argue that more specific development and application of adaptive approaches toward INS management, whether in Kings Bay or other appropriate case studies, would be facilitated if ecosystem managers and invasion biologists follow Simberloff's lead on this key point. While Sagoff largely shares (and, indeed, served as a primary source for developing) our general arguments that challenge common moral and scientific assumptions associated with invasion biology, he does question our suggestion that participatory adaptive management provides an appropriate framework for approaching environmental problems in which science and politics are inherently entangled. We attempt to answer this criticism through a brief sketch of what participatory adaptive management might look like for Kings Bay and how such an approach would differ from past management approaches.

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Introduction

We are very grateful that Daniel Simberloff and Mark Sagoff both took time to respond to our recent paper (Evans et al. 2008) about adaptive management of invasive nonnative species (INS) within the context of Kings Bay/Crystal River, Florida. Indeed, we find it encouraging that an ecologist as respected as Simberloff is willing to further entertain the "notion of some adaptive management approach for Kings Bay/Crystal River along the lines" that we outline (Simberloff 2009), while an environmental ethicist of Sagoff's caliber apparently agrees with our argument that attempts to define harm from invasive non-native species (INS) "need not be arbitrary" (Sagoff 2009).

Although we believe that areas of common ground are perhaps the most interesting in terms of moving forward, we are also compelled to answer several criticisms developed in the two responses. Because Simberloff's critiques are considerably more extensive in scope and provocative in tone, we have organized the sections of this response around four charges that Simberloff makes: (1) that we construct a straw man out of invasion biology and, moreover, Simberloff himself; (2) that our discussion of controversies over the severity of extinction threats from INS is "incorrect" and, moreover, irrelevant in the context of Kings Bay; (3) that our support for adaptive management of INS such as water hyacinth and hydrilla in Kings Bay is fundamentally flawed because we do not explicitly consider the threat posed by spread of these INS to nearby ecosystems; and (4) that adaptive management has not only proved difficult to achieve in practice, but that our proposal for Kings Bay cannot be thoroughly evaluated because it is not specific enough in terms of scientific and administrative detail.

Sagoff generally offers a much more sympathetic overall reading of our paper and uses much of his reply to expand upon his ongoing critique of economic and scientific studies commonly used to justify control of nonnative species. However, Sagoff does take issue with our suggestion that participatory adaptive management offers a coherent method for dealing with environmental problems (such as aquatic plant management in Kings Bay) in which science and politics are inherently entangled. While the specifics of Sagoff's skepticism toward adaptive management are somewhat different than Simberloff's, we attempt to answer both authors' objections through an expanded outline of what participatory adaptive management might look like in the specific context of Kings Bay and other ecosystems in which aquatic plant control is a source of political conflict.

Grasping at Straw

Simberloff (2009) quite provocatively opens his response by charging that the conceptual framework of our paper is hinged upon a straw man that grossly mischaracterizes the general views of invasion biology. The charge is strikingly personal, as Simberloff (2009) claims that he is altogether "surprised" to see himself discussed in our paper, ostensibly because he has "never been a party to arguments about management of Kings Bay." Admittedly, we find this to be a somewhat puzzling protestation, as we never imply that Simberloff personally was involved in the issues at Kings Bay. Instead, our paper quite clearly couches the discussion of

Simberloff's thought within an overall review of arguments between prominent invasion biologists and recent critics of invasion biology.

Simberloff (2009) further extends the straw man charge by noting that he has never called for "a *carte blanche* against all alien species in natural environments." The apparent point of this statement is to dismiss the claim that invasion biologists (including Simberloff) too often fall into the trap of regarding "invasive non-native species (INS) a priori harmful" (Evans et al. 2008). This gets into quibbling over semantics, but Simberloff appears to be implying that we have accused him of advancing the position that *all nonnative* (i.e., "alien") species are a priori harmful and, as such, should be targeted for control. However, we must point out that the initial paragraphs of our conceptual framework make a clear distinction between INS (i.e., nonnative species that are invasive in the sense of spreading rapidly and, presumably, reducing the populations of certain native species in areas where they spread) and the vast majority of nonnative (i.e., "alien") species that do not exhibit invasive behavior in areas in which they have been introduced. Given the context of this explicit distinction, we plainly do not make the specific accusation that Simberloff is defending against. Instead, our much more nuanced claim, and one that we stand by, is that Simberloff and other invasion biologists tend to take the default position that the effects of that subset of nonnative species that *do become INS* are harmful.

Simberloff also seems to take issue with this latter claim by proclaiming that "few if any invasion biologists have argued," as we suggest they do, that "INS represent an ecological bane that should be aggressively countered to preserve local and global biodiversity." This again gets into semantics, but we simply do not see any meaningful difference between the description of INS as an "ecological bane" and Simberloff's own description of INS as, for example, a "global scourge" (2003, p. 83). Similarly, we fail to see any difference between describing INS as something that should be "aggressively countered" versus the idea that they are "one target of resource management at which it is often better to shoot first and ask questions later" (Simberloff 2003, p. 88) or that effective control of INS requires "(a)ggressive state action (Schmitz and Simberloff 1997, p. 38). Assuming that the preservation of local and global biodiversity is one of the major justifications advanced for the control of INS, it is very difficult to understand why Simberloff is so vehement in his charge that we have used unfair and/or inaccurate vocabulary to summarize invasion biology's (and Simberloff's own) normative position toward INS.

Let us also be clear that we do not consider the normative view of invasion biology altogether unreasonable. Indeed, many of the economic, social, and ecological changes attributed to certain INS are generally regarded as harmful, although, as Sagoff (2009) points out in his response to our paper, some of the specific claims about the extent of these harms should be more carefully scrutinized. But regardless of the specific quantification of global harms, there is very good reason to believe that aggressive control to prevent and/or mitigate existing and/or potential harms from INS are well-justified in many circumstances. The subtle crux of our criticism, as also advanced by Sagoff (2005), actually lies in the tendency to define any effects attributable to an INS as "harmful" through circular reasoning hinged upon stipulative definitions. The form of this circular argument can be summarized in the following way: (1) INS are harmful; (2) this particular INS has been demonstrated to cause such and such effects; (3) because such and such effects are caused by an INS, these effects are harmful.

One of the major points here, again echoing Sagoff (2005), is that invasion biologists (and ecosystem managers) should utilize metrics that define "harm" through criteria that are not dependent upon stipulative moral assumptions about INS themselves. Such metrics need not be based upon ecology or conservation (even though they can be); the most

important point is to dispense with the tautological argumentative form. For example, harm from a particular INS could be coherently defined using the following argument that frames ecological findings within a framework of aesthetics and/or historical values: (1) such and such INS displaces a large percentage of such and such meadow plant species; (2) our community values such and such meadow plant species; (3) anything that displaces what we value is harmful; (4) such and such INS is harmful. Aside from avoiding the problem of stipulative circularity in ascribing harmfulness to INS, this latter formulation has the advantage of placing judgments about harm within the changeable context of ongoing deliberations in a moral community with pluralistic value sets. To paraphrase Norton (2005), whose conception of adaptive management we largely adopt in our paper, this allows for the possibility that experience (e.g., observations of effects associated with INS control) may well change how environmental values are prioritized in a specific place and, by extension, what sorts of things are characterized as harmful in that place over time. Our paper argues that such value shifts are precisely what has taken place among a significant subset of Crystal River residents with regard to water hyacinth and hydrilla: INS once regarded as harmful (because they spread rapidly, reduced native plant populations, affected navigation, etc.) are now regarded by many local citizens as less harmful than both the control activities that target them and the currently dominant ecological community in Kings Bay (i.e., one characterized by algae and potentially toxic cyanobacteria).

Based upon quotes from several aquatic plant managers and excerpts from an internal agency document, we argue in our paper that institutional resistance to alternative INS management as an experimental strategy for improving ecosystem conditions in Kings Bay is, at least in significant part, based upon a stipulation that any increase in population of an INS ipso facto constitutes harm. Although some of this resistance is justified by an assertion of statutory authority to maintain INS at low levels, the Florida Statute authorizing control of invasive aquatic plants clearly stipulates that control activities must be balanced with a corresponding duty to "protect human health, safety, and recreation and, to the greatest degree practicable, prevent injury to plant, fish, and animal life and to property" (Florida Statutes 2008). A plausible interpretation of this clause is that control policies based upon the goal or INS minimization can (should?) be realxed in those cases where higher levels of INS would benefit recreation, wildlife, and property.

The case of water hyacinth in the spring-fed St. Marks River (located in the Florida panhandle) provides an interesting precedent along these lines, in that efforts by local citizens led to a suspension of chemical control activities targeting water hyacinth in the St. Marks system in the late 1980s. Detailed ecological studies subsequent to the cessation of chemical control suggest that water hyacinth is not only maintained at moderate levels of coverage through natural processes in the St. Marks River, but that the nonnative plant also provided excellent habitat for desirable native wildlife such as the spring run crayfish, the Florida apple snail, and various small fishes (Bartodziej and Leslie 1998). It is an open question as to whether water hyacinth might play a similar habitat role in the Kings Bay system, but one that we believe is worth investigating given the clear consensus about the undesirability of current ecosystem conditions.

In any case, we argued that the resistance of the Bureau of Invasive Plant Management to citizen suggestions regarding alternative aquatic plant management strategies provides an example of the agency falling into a *caricatured* view of invasion biology that calls for the aggressive control of "all INS regardless of the wider socio-ecological context" (Evans et al. 2008). Simberloff takes particular offense to our suggestion that such a caricatured view of invasion biology also can be seen as representing his "default position" (Evans et al. 2008). On this point, Simberloff offers a cogent rebuttal in which he notes his support

for various management programs that are explicitly utilizing INS as "useful allies in conservation and restoration." We must admit that Simberloff's expression of support for such programs does indicate a willingness to subvert INS control (at least in some circumstances) to wider socio-ecological concerns. As such, our characterization of Simberloff's position on this particular point could be construed as somewhat unfair. In our defense, we must point out that Simberloff has, in other contexts, advanced the argument that unpopular INS control campaigns may often need to be supported by "expensive public relation campaigns" and government agencies that can "compel cooperation" (Simberloff 2002, p. 6)—a position that seems to imply that, at least in some cases, he does believe that the imperative to control INS generally outweighs even quite widespread social dissent. But to be fair to Simberloff, he seems to justify this somewhat authoritarian position through a genuinely-held conviction that conservation and socio-economic threats posed by unchecked spread of certain (particularly newly introduced) INS often demand quick, decisive action that would be much more difficult to achieve under a participatory approach.

Despite our own personal preferences for more participatory modes of environmental decision-making, the "ends-based" argument advanced by Simberloff is one that cannot be cursorily dismissed—particularly if one presumes that biological conservation is an end that may often trump insular human interests and that efficient INS control is critical for achieving such conservation. But even if one accepts such presumptions, we believe that the authoritarian position becomes less and less justifiable over time in the context of management interventions aimed at certain INS in particular places. Put another way, we believe that the general movement should be away from authoritarian precaution and toward participatory adaptive management as a function of the time elapsed since the initial introduction. In practical terms, this switch toward adaptive management essentially means that, beyond any initial "crisis" stage, the actions of ecosystem managers should be put before the scrutiny of the wider community for the purpose of ensuring that their interventions are, indeed, providing greater benefits than costs. If the general consensus is that they are, then the interventions should continue. But if not, then adjustments in both goals and tactics should be made.

What we found at Kings Bay, by contrast, was a reflexive mobilization in defense of longstanding INS control policies and against any suggestion that conflicts with these policies. We and, more importantly, many local citizens believe that such a stance is hindering the development of novel approaches toward conservation and restoration given the specific conditions found in Kings Bay. A number of other recent studies advance the notion that the aggressive control of established INS can have socio-ecological effects that are generally regarded as negative (e.g., Shapiro 2002; Foster and Sandberg 2004; Krajick 2005; Hershner and Havens 2008; Kanowski et al. 2008; Lockwood and Latchininsky 2008), and we suggest that the history of aquatic plant control Kings Bay provides an example in which this may also be the case. While we do apologize to Simberloff for oversimplifying his position on the particular point of flexibility with regard to INS control in certain circumstances, we—for all the reasons discussed above—must otherwise reject his charge that our conceptual framing of invasion biology as an applied management discourse is a strawman built upon "brazen" exaggeration.

INS and Extinction

Simberloff's (2009) second criticism is to suggest that our review of Sagoff's (2005) critique of presumed relationships between invasive nonnative plants and extinctions, and

Simberloff's published response to Sagoff, is both generally "misleading" and specifically "irrelevant" to the situation at Kings Bay. Taking the irrelevance argument first, we agree with Simberloff in this sense: there is no reason to believe that the introduction of water hyacinth, hydrilla, or Eurasian milfoil into Kings Bay poses an extinction risk to any species found in this system. In fact, Kings Bay—as Simberloff readily acknowledges—represents a situation in which INS are, in fact, used by a charismatic endangered species, the West Indian manatee. Given the lack of outward extinction issues in Kings Bay, Simberloff then rhetorically asks why would we "raise the matter of extinctions at all?" The straightforward answer is that our conceptual framework attempted to provide a general review of ongoing controversies regarding the moral and scientific foundations of invasion biology. Arguments about how to frame the role of INS in the contemporary extinction crisis—as highlighted in the exchange between Sagoff and Simberloff—represent a primary example of such controversies, and thus were appropriately included in our review.

But was our discussion of the extinction issue misleading, as Simberloff charges? Our primary agreement with Sagoff (2005) is in the specific claim that nonnative plants (especially in continental ecosystems) pose far less extinction concern than introduced animals (particularly in island ecosystems)—a claim that is largely founded through citation of Gurevitch and Padilla (2004). On the one hand, Simberloff does correctly note that some of Gurevitch and Padilla's (2004) findings were challenged in a brief, but widely cited, response letter (Clavero and García-Berthou 2005). On the other hand, we must also point out that it is difficult to evaluate the relevance of the Clavero and García-Berthou (2005) response to the specific issue of extinction from nonnative plants, simply because these authors make no distinction between the type of introduced species (i.e., animal or plant) or the type of ecosystems (i.e., island, continental, marine) associated with the species extinctions they evaluate. Much more obviously relevant to the extinction argument is a paper by Sax and Gaines (2008) recently published in the *Proceedings of the National Academy of Sciences*. This paper found that "most extinctions have been on islands as opposed to mainlands," approximately "98% of all extinctions" associated with exotic species are a function of "predation acting alone and predation acting in concert with other factors," and that direct competition from invasive plants has led to "few extinctions" (Sax and Gaines 2008, p. 11491). All of this is generally consistent with the specific points about extinction risk in which we expressed agreement with Sagoff (2005).

To be fair, Sax and Gaines (2008, p. 11495) also note that better understanding of "how the transformation of large areas into exotic-dominated ecosystems influences extinction of native species through reduction in total available habitat" is presently a major research gap. While we agree that more research to better understand the interplay between plant INS, extinctions, and biological change (including invasion-induced speciation) on a global basis is needed, there is little reason to reject Sagoff's (2005) suggestion that attempts to use extinction risk from INS as *a general class* to justify control of invasive nonnative plants on continental ecosystems are—absent specific case study information that does suggest the existence of such a risk (including, for example, any continental bird species that may be primarily threatened by invasive plants)—ultimately hinged upon an inductive fallacy. Of course, there may well be any number of very good reasons—whether these are founded in religion, spirituality, aesthetics, economic utilitarianism (all of which are touched upon by Sagoff 2009), or precautionary aversion to unknown consequences (as suggested by Simberloff 2009)—for preferring native plants over nonnative plants in continental ecosystems. The bulk of scientific evidence simply suggests that major extinction risk cannot be confidently counted among these.

Risk of Dispersal

The third criticism of our proposal that Simberloff (2009) develops is that, in his view, we "propose to facilitate the local growth of water hyacinth, hydrilla, and Eurasian water milfoil without consideration of the fact that they all cause enormous ecological and economic problems in many places." On the one hand, we plead guilty to giving more attention to the relative benefits that local stakeholders identified from these species as compared to their perception of negative effects of aquatic plant control, while also falling short of an explicit and exhaustive list of the problematic socio-ecological effects associated with each of these species in all the contexts in which they are found. On the other hand, our point was not to give a detailed account of problems from these INS (as Simberloff notes, there are a number of sources that do list such problems), but rather to explore a particular case in which there is good reason to suspect that attempts to control INS may have been a major source of harm in their own right. Similarly, a focus on potential benefits from water hyacinth, hydrilla, and Eurasian milfoil—as "widely detested" as they may be in most other contexts—is not inappropriate in an ecosystem where the plants may reasonably be expected to assist with two of the primary conservation goals: (1) protection of the manatee population; and (2) reduce blooms of undesirable algae and cyanobacteria.

Simberloff (2009) is right to raise the general worry that boats and trailers from Kings Bay/Crystal River might serve as a vector for spreading invasive nonnative plants into other nearby aquatic ecosystems. However, there is little reason to believe that these risks are as extreme as Simberloff's response implies, mainly because almost all of the major public freshwater bodies in Citrus County and surrounding areas have ongoing control programs for (and, thus, already contain) water hyacinth and hydrilla (see Bureau of Invasive Plant Management 2007). In other words, the risk of dispersal into new systems for these two species is relatively low because such dispersal has, for the most part, already taken place. Given that both of these species have been established in the local area for many decades (and, as we noted in our paper, that Kings Bay apparently served as a primary initial vector for hydrilla's spread throughout the southeast), their ubiquity in regional water bodies with a history of significant boater recreation is not particularly surprising. Eurasian milfoil provides a somewhat different case in that it has been present in Kings Bay/Crystal River since the 1960s (SWFWMD 2004) and in the Homossassa River—a coastal springs ecosystem just south of Kings Bay/Crystal River—since at least the 1970s. Despite having ample opportunity for dispersal over this time, Eurasian milfoil—in clear contrast to hydrilla—has not become widespread in nearby freshwater lakes. Admittedly, there are no definitive answers as to why Eurasian milfoil has had little invasive success in Florida lakes up to this point in time, and precaution is certainly called for in the sense that actions should be taken to minimize the risk of spread into new ecosystems. Aside from our apparent disagreement in terms of how the dispersal risk should be framed, we do ultimately agree with Simberloff's suggestion that "the scope of any management scheme involving maintaining any or all of these species at Kings Bay should take into account the risk of spread beyond that site." There is no reason to believe that such a program would not do so as a matter of course.

Participatory Adaptive Management

Simberloff's (2009) most compelling line of criticism relates to our discussion of participatory adaptive management, which was largely influenced by lines of argument

developed by Norton (2005), and our general sketch for applying such a program through a participatory committee that would delineate and monitor different aquatic plant management zones in Kings Bay. Our discussion of adaptive management did, as Simberloff noted, conspicuously avoid any formal definition. Instead, we described the concept as being founded upon a recognition of six interrelated principles: (1) the inherent *variability* of ecosystems; (2) the *unpredictability* of this variability; (3) the fact that all management actions take place within some context of *uncertainty*; (4) all management actions should be treated as provisional *experimentation* from which new things can be learned; (5) *flexibility* is required to incorporate what is learned from past management actions into future actions; and (6) management goals, methods, and evaluation should all take place within a *participatory* framework that includes managers, scientists, and the local community. Taken together, the overarching suggestion is for resource managers to not become "stuck" in one mode of management on behalf of a fixed goal, but to instead iteratively adapt goals and future interventions based upon the learning that takes place from careful monitoring and participatory evaluation of past interventions.

A critical difference between this set of principles and the scientific adaptive management approach that Simberloff (2009) summarizes in his response is the explicit focus on stakeholder participation as a core component of the process. Simberloff is right to suggest that the early visions of scientific adaptive management rarely were achieved due to "the lack of resources or simply the impossibility of finding an adequate array of sites" for sufficient replication and/or experimental controls, and he probably is right to argue that the myriad socio-ecological complexities of Kings Bay may pose greater challenges for scientific adaptive management than the other, simpler systems in which it has been attempted. We also agree with Simberloff's assertion that definitive explanations about the root causes of *Lyngbya* outbreaks in Kings Bay likely would entail time scales of treatment and control that are beyond those typically associated with past adaptive management experiments. Similarly, his suggestion that formal ecological comparisons between suggested aquatic plant management zones would be extremely difficult due to the mobility of both nonnative plants (which could therefore contaminate native restoration zones) and herbicides (which could therefore contaminate no treatment zones) also seems quite valid.

With all that said, we find little reason to believe that the messiness Simberloff aptly points out with regard to formal scientific adaptive management provides a convincing argument against the participatory adaptive management approach that we advocate. Rather, our view is that the messiness should be acknowledged as an inherent part of the process and explicitly taken into account as interventions are monitored and qualitative comparisons are made. For the sake of argument, if significant *Lyngbya* reduction was documented in conjunction with certain kinds of experimental treatment zones (aside from direct algaecides in which the suppressive mechanism would be obvious), there may well be opportunity to develop novel insights into the ecological factors (some possible factors might include water quality, aquatic plant competitors, invertebrate types and density, boat traffic density, and manatee grazing pressure) that may promote *Lyngbya*. But even in the absence of such insights and definitive ecological explanations, consensual acknowledgment of success in achieving a desirable outcome would serve as sufficient basis for continuing and/or expanding the treatment method under the terms of participatory adaptive management. As Simberloff explicitly concedes, it is possible that "an informal management procedure in the spirit of adaptive management can achieve some resource goal without much scientific understanding."

Although specifically noting that he does not "object to substantial input from stakeholders" in the management of introduced species, Simberloff does hint at serious doubts about the practicality of a participatory approach in Kings Bay. For example, he wryly suggests that it would be "an interesting process to try to get all stakeholders to agree on how many replicates should be maintained for each treatment, where these should be located, and under what criteria a treatment should be changed or a replicate abandoned." While the suggestion that "all stakeholders" would have to agree upon the specifics of each experimental design is something of a red herring (i.e., there is an important distinction between reaching consensus and unanimity in collective decision-making), we do not think it is all unreasonable to include local stakeholders who utilize the ecosystem within the process of developing (and refining) relevant criteria for what constitutes management success and failure. As such, our answer to Simberloff's (2009) complaint that our paper does not make "completely clear" the goals and end-points for management experiments in Kings Bay is that these such goals and end-points are precisely the types of things that should be decided through a participatory adaptive management process. Ultimately, our disagreement, if any, with Simberloff on the point of participation may simply be the degree to which we believe that it would provide a feasible basis for effective management.

Sagoff's (2009) major critique of our paper is similarly related to the vagaries of participatory adaptive management. Most conceptually, Sagoff claims that adaptive management "punts" on the question as to whether ecosystem management is primarily a purview of science or of politics. The best short answer we can give here is to reiterate that the science behind ecosystem management is often so uncertain that the din of competing interpretations offered by experts is not altogether different in form from (and, indeed, is often influenced by) overtly political arguments, making it unfruitful to give a formal attempt at disentangling where science ends and politics begins. The participatory adaptive management we have in mind would instead take this entanglement as a given and, as such, attempt to find ways of making the socio-political processes of management more transparent and responsive to the concerns of the wider stakeholder community.

Citing both the long history of management interventions and the various forums in which citizens have had the opportunity to give input about management efforts, Sagoff wryly argues that managers unwittingly may "have been applying the method all along". The most obvious problem with this suggestion, however, is that the formal participatory discussion groups we describe (i.e., the Kings Bay Water Quality Subcommittee and the Kings Bay Working Group) were both formed within the last decade, while the history of aquatic plant management goes back for decades. But more fundamentally, participatory adaptive management would entail a much deeper utilization of stakeholder groups than has been attempted in Kings Bay. Following a general framework suggested by Berardi (2002), we would argue that the typology of stakeholder participation has been one of "consultation" in which agency officials give presentations and answer questions, but assume no obligation to accept and/or act upon any of the questions asked. The goal of participatory adaptive management, however, is what Berardi (2002) calls an "interactive participation" that involves local citizens directly in research, development, analysis, and implementation of ecosystem management plans. There is certainly a risk that such a process could, as Sagoff worries, collapse into a "group grope" of different scientists aligning with different stakeholders, causing a morass of deepened confusion, indecision, and ineffectiveness—i.e., the much-feared "paralysis by analysis." Call us liberal optimists, but we think that the upside of an effective program of participatory adaptive management is much greater than such a downside risk.

While we obviously do not have space here to offer a detailed plan for how participatory adaptive management might be implemented over time in Kings Bay, an expanded sketch of such an approach probably will do more to answer Simberloff's and Sagoff's questions than the conceptual issues discussed above. The first step we suggest is holding a collaborative workshop mediated by a neutral facilitator for the purpose of listing the multiple values that stakeholders have for Kings Bay, attempting to order these values through detailed discussion and consensual votes, and developing hypotheses about what kinds of management interventions might support the preservation/recovery of these values. If we can then assume (as experience suggests it is probably safe to do) that there would be multiple hypotheses about how to best achieve widely held values such as restoration of native plants, reduction of *Lyngbya*, and protection of manatees, the next step would be to identify alternative experimental approaches and suitable monitoring regimes for evaluating these approaches. Of course, the issues of scarce resources and, as mentioned above, an inherently variable ecosystem would both constrain the extent to which such an approach would fit within a formalized framework of controls and replication called for by scientific adaptive management. To reiterate, the failure to achieve such a formal experimental method is something that should be acknowledged, but does not by itself invalidate the general participatory approach.

The idea of establishing aquatic plant management zones in which different values are emphasized (and treatment approaches applied) is one that we suggested as a potentially promising framework for participatory adaptive management in Kings Bay. Other approaches may well prove to be more suitable, but we must note that collaborative establishment of aquatic management zones has been used as an apparently quite successful way of resolving quite bitter conflicts about hydrilla and Eurasian milfoil management in Lake Guntersville, an Alabama reservoir (Yokum 2004) that is many times the size of Kings Bay. The overall strategy in Lake Guntersville is for aquatic plant managers to maintain navigation channels around boat docks and certain areas of the lake used for recreational boating (as advocated by many lakeside homeowners), while allowing for large beds of the exotic plants to persist in certain areas of the lake for the purpose of fishery enhancement (as advocated by local fishermen). At the very least, we stand by our suggestion that this is a promising participatory (and, indeed, conflict-resolution) model that deserves some consideration for Kings Bay and other ecosystems in which aquatic plant management has become a major controversy.

Conclusion: Common Ground

As mentioned at the beginning of this reply, we are quite grateful that Simberloff and Sagoff took the time to respond to our paper. Albeit in quite different ways, both responses provided an invaluable opportunity to reflect upon, clarify, and, when appropriate, defend our work.

Although it is clear that we share many points of agreement with Sagoff (2005, 2009) in terms of questioning invasion biology, it may seem odd that we also find that our points of agreement with Simberloff (2009) are of more practical significance than our disagreements. Despite our philosophical disagreements, we do not doubt Simberloff's overall commitment to biological conservation, and truly believe that his response serves as a helpful addition in the conversation about conservation and management at Kings Bay. Clearly, there are legitimate concerns that should be discussed and debated with regard to any proposal for alternative management of invasive aquatic plants in Kings Bay, and

Simberloff performs a valuable service by raising at least some of these in his response. However, Simberloff's express willingness to at least further entertain the idea that Kings Bay may well be a suitable location for alternative management of invasive nonnative plants provides an important piece of common ground for moving toward a framework of participatory adaptive management of INS in this (and other) system(s). While we agree that there are non-trivial challenges associated with "testing" the general claim (or set of embedded claims) that alternative management of nonnative plant species would provide socio-ecological benefits for Kings Bay, or even testing much more specific hypotheses about the effects of nonnative plants on manatee production and/or filamentous algae/cyanobacteria such as *Lyngbya wollei*, the fundamental point that Simberloff concedes is that there is no justification for rejecting any such scientific hypotheses or inferred benefits due to a priori claims about the harmfulness of nonnative plants. We cannot help but find it significant that here we have found clear agreement between Simberloff (2009) and Sagoff (2005, 2009), two thinkers who otherwise agree about very little regarding the science and ethics of INS management. Moving forward, we believe that the difficult path towards participatory adaptive management in Kings Bay would take a large step forward if aquatic plant managers were to follow Simberloff's lead on this key point.

References

Bartodziej, W., & Leslie, A. J. (1998). *The aquatic ecology and water quality of the St. Marks River, Wakulla County, Florida, with emphasis on the role of water hyacinth: 1989–1995 studies. Bureau of Invasive Plant Management, TSS* (pp. 98–100). Tallahassee: Department of Environmental Protection.

Berardi, G. (2002). Commentary on the challenge to change: Participatory research and professional realities. *Society and Natural Resources, 20*, 847–852.

Bureau of Invasive Plant Management (2007). Status of the aquatic plant maintenance program in Florida waters: Annual report fiscal year 2005–2006. Tallahassee: Florida Department of Environmental Protection. http://www.dep.state.fl.us/lands/invaspec/2ndlevpgs/pdfs/aquatics05-06.pdf. Accessed Sep 2008.

Clavero, M., & García-Berthou, E. (2005). Invasive species are a leading cause of animal extinctions. *Trends in Ecology and Evolution, 20*, 110.

Evans, J. M., Wilkie, A. C., & Burkhardt, J. (2008). Adaptive management of nonnative species: moving beyond the "either-or" through experimental pluralism. *Journal of Agricultural and Environmental Ethics*, *21*(6), 512–539.

Florida Statutes. (2008). Non-indigenous plant control, Chapter 369.22. http://www.leg.state.fl.us/statutes/index.cfm?App_mode=Display_Statute&URL=Ch0369/ch0369.htmAccessed Sep 2008.

Foster, J., & Sandberg, L. A. (2004). Friend or foe? Invasive species and public green space in Toronto. *The Geographical Review, 94*(2), 178–198.

Gurevitch, J., & Padilla, D. K. (2004). Are invasive species a major cause of extinctions? *Trends in Ecology and Evolution, 19*, 470–474.

Hershner, C., & Havens, K. J. (2008). Managing invasive aquatic plants in a changing system: Strategic consideration of ecosystem services. *Conservation Biology, 22*, 544–550.

Kanowski, J., Catterall, C. P., & Neilan, W. (2008). Potential value of weedy regrowth for rainforest restoration. *Ecological Management and Restoration, 9*, 88–99.

Krajick, K. (2005). Winnnig the war against island invaders. *Science, 310*(5753), 1410–1413.

Lockwood, J. A., & Latchininsky, A. V. (2008). Confessions of an entomological hit man. *Conservation Magazine, 9*, 15–19.

Norton, B. G. (2005). *Sustainability: A philosophy of adaptive ecosystem management*. Chicago: The University of Chicago Press.

Sagoff, M. (2005). Do non-native species threaten the natural environment? *Journal of Agricultural and Environmental Ethics, 18*, 215–236.

Sagoff, M. (2009). Environmental Harm: Political not biological. *Journal of Agricultural and Environmental Ethics, 22*(1). doi:10.1007/s10806-9127-4.

Sax, D. F., & Gaines, S. D. (2008). Species invasions and extinctions: The future of native biodiversity on islands. *Proceedings of the National Academy of Sciences, 105*(1), 11490–11497.
Schmitz, D. C., & Simberloff, D. (1997). Biological invasions: A growing threat. *Issues in Science and Technology, 13*(4), 33–40.
Shapiro, A. M. (2002). The California urban butterfly fauna is dependent on alien plants. *Diversity and Distributions, 8*, 31–40.
Simberloff, D. (2002). Today Tiritiri Matangi, tomorrow the world! Are we aiming too low in invasives control? In C. R. Veitch & M. N. Clout (Eds.), *Turning the tide: The Eradication of Invasive Species* (pp. 4–12). Gland, Switzerland and Cambridge, UK.: IUCN.
Simberloff, D. (2003). How much information on population biology is needed to manage introduced species? *Conservation Biology, 17*(1), 83–92.
Simberloff, D. (2009). Moving beyond strawmen and artificial dichotomies: Adaptive management when an endangered species uses an invasive one. *Journal of Agricultural and Environmental Ethics*, *22*(1). doi:10.1007/s10806-008-9126-5.
SWFWMD (2004). Crystal River/Kings Bay technical summary. Brooksville: Southwest Florida Water Management District. http://www.bocc.citrus.fl.us/commdev/scc/cr_kingsbay_summary.pdf Accessed 1 Dec 2006.
Yokum, K. (2004). A victory for bass fishermen. BASS Times. http://sports.espn.go.com/outdoors/bassmaster/news/story?page=b_fea_bt_0411_fish_mng_victory Accessed 16 Sep 2008.