Panel Response to the Delta Independent Science Board Discussion Paper:

Toward a Preemptive Ecology for Rapid, Global, and Increasingly Irreversible Environmental Change

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Background

The Delta Independent Science Board (Delta ISB) is in the process of assessing the adequacy of research in the Delta, how well that research is addressing changing conditions and emerging challenges, and how it might be improved. To help stimulate discussion, the Delta ISB has drafted a discussion paper, titled "Toward a Preemptive Ecology for Rapid, Global, and Increasingly Irreversible Environmental Change."

On January 30, 2020, the Delta ISB will moderate a panel discussion with leading experts to scope out and address the challenges of conducting research under rapid environmental change. To help inform the panel discussion, individual experts were asked to draft a written response on the paper, which can be found in the appendices of this report. At the meeting, each panelist will have the opportunity to share highlights from their written response. After every panelist has shared their highlights, the Delta ISB will moderate a discussion with the panelists and other attendees.

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Appendices are ordered in which panelists will present their highlights. These written responses do not reflect the views of the panelist's affiliation. Note that Dr. Erika Zavaleta will only be providing comments and will not be at the panel discussion.

Appendix A: Dr. Jeff Mount Comments

Adapting to Rapid Ecological Change in the Sacramento-San Joaquin Delta

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Introduction

The Delta Independent Science Board discussion paper *Toward a Preemptive Ecology for Rapid, Global and Increasingly Irreversible Environmental Change* identifies a fundamental challenge for science in the Sacramento-San Joaquin Delta. Ecological change is accelerating and the paper rightfully acknowledges that science—and the management it informs—is poorly prepared for this.

The discussion paper gives a broad view of possible approaches to ecological research and related management that can adapt to these changes. Four general methods or fields of study are identified—resilience ecology, horizon scanning, expert judgement, and scenario assessment. The paper does not offer a comparison between approaches nor does it make specific recommendations for which would be best.

In this comment, I offer some observations that build upon those presented in the discussion paper. Although I am in broad agreement with the conclusions of the paper, the authors have not addressed a key concern. It is neither simple or easy to transform our current approaches to Delta science to meet the objectives the discussion paper lays out. Institutional inertia—driven by both policy and law—and deeply ingrained cultural traditions in science make it difficult to change. I suggest that recommendations for new approaches to science be bolstered by recommendations for reforms needed in order to carry them out. I also provide a brief comment on use of scenarios as a possible best approach.

Preemptive Ecology in Context

Delta science has improved significantly over the last 20 years. The stewards of this science—including state and federal agencies, the Interagency Ecological Program (IEP), the Delta Science Program (DSP), the Delta Stewardship Council (DSC), and the Delta Independent Science Board (DISB)—deserve credit for this progress. But most of that progress has been on understanding current conditions and historic change, or what former DSP Lead Scientist Johnnie Moore referred to as “crime scene investigations.”

[1] Contact information: mount@ppic.org. All opinions expressed in this comment paper are solely those of the author and do not represent the views of the Public Policy Institute of California.

Preemptive ecology will logically build upon current data, models, and understanding derived from crime scene science in the Delta. To be most useful, however, the DISB should offer a pathway to incorporating the approach into existing scientific efforts. In addition, the DISB should tackle the very difficult task of thinking through how preemptive ecology would shape decision making at the agency level, given the array of constraints faced by those agencies.

For example, most science and monitoring in the Delta is done by state and federal agencies to meet multiple regulatory and operational objectives. A suite of policies and laws govern these actions (Table A-1). Additionally, institutions tasked with carrying out this science have organized governance, administration and funding around meeting these objectives.

Several laws in particular—such as the state and federal Endangered Species Acts (ESAs), the federal Clean Water Act, and the California Porter-Cologne Act—are the primary driver of science and monitoring, using the vast majority of agency science resources. For better or worse, meeting the regulatory mandates of these laws encourages crime scene science—not preemptive ecology—because all focus is on historical changes in condition (crime scene), identification of a cause of those changes (criminal), and the need to craft a near-term fix or to set regulations (rehabilitation). There is very limited impetus or resources to scan the horizon.

Arguably, there are strong disincentives to practice preemptive ecology. State and federal ESAs do not require agencies to take into account future conditions (although agencies have leeway to consider it and the courts have supported this). Most agency effort in the Delta is focused on managing existing “take” and restoring or preserving critical habitat for a handful of listed species close to extinction. This makes it doubly difficult to practice preemptive ecology.[2]

Additionally, state and federal water quality and water rights programs focus on protecting current beneficial uses of water. There are no provisions within water quality laws to anticipate and act upon future ecological conditions (although to my knowledge nothing prevents that). And the state’s complex riparian and appropriative water rights system is built upon historic hydrology and water use without regard to future conditions. This too makes it hard to incorporate future conditions into management planning and regulation.[3]

Finally, the disincentives to practicing preemptive ecology are magnified by rule making under the current federal administration which seeks to reduce or eliminate planning for climate change in infrastructure development and species management.

[2] For a more extensive discussion of this issue see Mount et al., 2019. A Path Forward for California’s Freshwater Ecosystems*.* Public Policy Institute of California.

[3] The disincentives to practicing preemptive ecology are magnified by rule making under the current federal administration which seeks to reduce or eliminate planning for climate change in infrastructure development and species management.

Table A-1: Relevant Laws for Ecosystem Science in the Delta

The actions of agencies that manage species and ecosystems of the Delta are influenced by numerous laws and regulations. These will impact any "preemptive ecology" effort because all are focused principally on management of current problems, not future problems, with the exception of planned infrastructure. Here are just a few of the most important.

Law	Description
Federal Clean Water Act Section 303	Requires the state to adopt water quality standards that define designated uses of water and the water quality criteria to support those uses, including water supply, fish and wildlife, recreation, and others. It is principally focused on protecting existing uses.
California Porter-Cologne Act	Implements section 303 of the Federal Clean Water Act. The State Water Resources Control Board and regional boards define ecological and water quality objectives and set flow and water quality standards through Water Quality Control Plans. As with the federal act, the focus is on protecting existing uses.
California Water Rights, Public Trust Laws	The state board issues permits and licenses for surface water diversions that meet the standard of "beneficial use" as currently defined. In addition, the state constitution allows the board to determine what uses are reasonable and, using their water rights authority, protect public trust uses of water, including ecosystems.
Endangered Species Acts (ESAs)	State and federal ESAs are designed to protect and, if possible, recover species at risk of extinction. Although there are differences between the state and federal acts, their overall objectives are broadly the same: to conserve habitat and to reduce harm to listed species in order to avoid extinction. The ESAs are narrowly focused on species currently at risk and have limited provisions for preemptive ecology. Exceptions may be possible in Habitat Conservation Plans (federal) and Natural Community Conservation Plans (state), but are rarely addressed.
California Fish and Game Code Section 5937	Requires all dams to bypass or release sufficient water "to keep in good condition any fish that may be planted or exist below the dam." The State Water Board (with the California Department of Fish and Wildlife) determines how best to restore and protect various fish species. All of these actions are focused on current conditions of fish and do not anticipate future conditions.
Federal Power Act and Federal Energy Regulatory Commission (FERC) Licensing.	All non-federal hydroelectric power projects must be licensed by FERC, which must give "equal consideration" to power production, energy conservation, recreation, and the protection, mitigation, and enhancement of fish and wildlife. Licenses last as long as 50 years, requiring licensees to address changes in condition that may occur during that time. However, it is rare for licenses to address changing ecological conditions.
NEPA and CEQA	The National Environmental Policy Act (NEPA) requires the preparation of an environmental impact statement for major federal actions that may "significantly affect the quality of the human environment." The California Environmental Quality Act (CEQA), the state counterpart to NEPA, requires state and local agencies to prepare an environmental impact report to identify effects of a project, alternatives to it, and whether effects can be mitigated. Both acts allow for consideration of climate change and its impacts on a project.

To be fair, Delta science—under good leadership—has been increasingly emphasizing forward-looking science, including promoting investment in modeling that can test scenarios for future conditions. This has shown up in the science agenda for IEP, is regularly addressed by DSP grants and awards, is prominent in the Delta Science Plan and the Delta Plan, and appears in multi-agency efforts, including the CaSCADE program led by Jim Cloern of the USGS.[4] And there is strong leadership on the part of California for climate change mitigation and increasingly, adaptation.

But all of this progress does no good unless it grapples with how to translate this outstanding work into actual decisions within the framework of existing policies, laws and institutions.

Personal experience at PPIC says that it is worth expanding the circle of investigators working on this kind of problem in order to get at whether proposed solutions are feasible.[5]

An Additional Observation on Scenarios

The four approaches to preemptive ecology outlined in the discussion paper appear to be given equal weight, including a suggestion that a "hybrid" approach be tried that merges all four. I encourage the DISB to compare and contrast these approaches as they develop guidance for Delta research. Logically, the approach chosen will most likely be tailored to the problem being addressed. But the discussion paper does not cover this or present examples.

Based on years of working with scientists in the Delta, it is my personal observation that scenario testing is likely to make the best use of existing expertise, data and models, and is the most productive way to organize a problem. The skill is in setting up the scenario and formulating the questions.

First on the list should be severe drought scenarios. Space limitations here preclude a full discussion, but in a recent PPIC paper we conclude that severe drought—particularly warmer drought—is driving ecological change, whether through tipping points, thresholds or simply amplifying how current management actions work against the natural drought adaptations of many native species (for example, see Table A-2).[6] Indeed, we conclude that the 2012 to 2016 drought was a window into droughts of the future and their likely ecological consequences. What is most important here—and not emphasized enough in the discussion paper—is the combination of historic change, current management approaches, and increasing drought severity are together driving ecological changes. This is not just a global change problem.

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[4] Summary of the program on the CaSCADE website.
[5] For example, the diverse 30-author team in Mount et al., 2018. Managing Drought in a Changing Climate: Four Essential Reforms*.* Public Policy Institute of California.
[6] See discussions in Mount et al., 2017. Managing California's Freshwater Ecosystems: Lessons from the 2012-2016 Drought. Public Policy Institute of California.

Table A-2: California Freshwater Biological Adaptation to Drought, and Why They Are No Longer Working as Climate Warms

Drought Adaption	Common Species	Why Not Working
Anadromy. Anadromous fishes spend a portion of their life cycle in the ocean. This ensures that some of their population is in the ocean when inland conditions are poor, enabling them to return when spawning and rearing conditions improve. This is a hedge against poor ocean conditions, when good inland conditions can support populations.	Chinook and coho salmon, steelhead, cutthroat trout, green and white sturgeon, Pacific and river lamprey	Populations decline if conditions are consistently poor in one environment or the other. Ocean conditions—when good—cannot compensate for long term changes in inland conditions due to dams, river habitat loss, and watershed management. This leads to population declines.
Fecundity. Although populations decline during drought, fecund fishes take advantage of abundant habitat during wet conditions through exceptionally high rates of reproduction.	Longfin smelt, Sacramento splittail, salmon, green and white sturgeon, suckers	Abundant, high quality habitat is no longer available during wet periods due to land use changes, flow regulation, and diversions. Species cannot recover populations during periods of favorable conditions.
Longevity. Long-lived fishes wait out droughts and reproduce during periods when conditions improve for spawning and rearing.	Green and white sturgeon, Sacramento splittail, pikeminnow, suckers, tui chubs	Land management and water storage and diversion practices leave rivers and estuaries in drought-like conditions in most years. This lengthens the time between good years for reproduction.
Tracking. Some estuarine fishes are able to migrate with changing salinity gradients when freshwater runoff declines.	Delta smelt, longfin smelt, splittail, prickly sculpin	Reductions in inflows to estuaries, physical transformation of the Delta, and changes in food web productivity limit habitat availability during drought.
Long-distance movement. Some anadromous fishes are able to travel long distances to reach suitable habitat during drought, such as headwater areas with reliable cold water springs.	Spring- and winter-run Chinook salmon, steelhead trout	Dams have blocked access to most headwater areas that have reliable cold water sources and flows critical to drought survival.
Dispersal. During dry years, fish may be confined to reduced habitat areas; during wet years they disperse to improved habitats quickly, through movement and reproduction.	Most native fishes	Dispersal is blocked by dams, diversions, and perpetually dry streams.

SOURCE: Mount et al., 2017. Managing California's Freshwater Ecosystems: Lessons from the 2012 to 2016 Drought. Public Policy Institute of California

NOTES: Fish and wildlife have a variety of life-history strategies that allow them to adapt to and recover from droughts. Modern land- and water-management practices work in contravention of many of these adaptations, inhibiting their recovery from drought. As droughts become more severe, this limitations will play an important role in changing ecology.

To date, the agencies have not conducted a full retrospective on the ecological effects of that drought. Indeed, perhaps a “crime scene investigation” of that drought could become the basis of future scenario testing. The drought certainly tested water management institutions. We really should understand what it did to the ecology and build on that.

Conclusion

The DISB’s discussion paper on the need for preemptive ecology hits on something undeniable: the rate of ecological change in the Delta is faster than the scientific community—as currently configured—can adapt. Although the paper makes recommendations for new scientific approaches, it does not offer a prescription for how to implement this new science. Tackling that will require greater attention to constraints and factors that motivate current approaches to science. There are many reasons that agencies may resist the call to embed preemptive ecology into their research programs and decision making. At the top of list is the need to satisfy an array of laws and policies that may inadvertently discourage investing in forward-looking science. It is my view that the discussion paper would benefit from including the views of social scientists and experts in environmental law and policy and identifying what institutional reforms may be needed.

Appendix B: Dr. Sam Luoma Comments

Science for the future: A review of “Toward a Preemptive Ecology for Rapid, Global, and Increasingly Irreversible Environmental Change”

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“It is better to be roughly right than precisely wrong.” John Maynard Keynes

The Delta ISB white paper presents a stark view of the challenges that face human civilization, earth’s ecosystems and the future of the Delta. One can argue about details, but I “roughly” agree with the paper’s basic premises. The rate of change is accelerating. The changes will include frequent extremes, of all types, and change will be discordant in time, space and context. Intransigence is a characteristic of a changing earth (there is no guarantee that a changing system will, or can be, returned to its original state), therefore some changes will be irreversible. At least some aspects of change in the Delta will be globally driven (out of regional control). The least uncertain thing about the future is that unanticipated consequences are likely.

Several things go unsaid in the paper. First the emphasis is on climate change. But the challenges stem from an accelerating increase in the effects of climate change piled on top of environmental challenges from the past that we have only partially addressed (e.g. population growth, industrialization/urbanization, industrial agriculture, engineered water systems). Second, an accelerating pace of change has been a trait of human civilizations since the beginning and a characteristic of how humans affect the ecosystems (including the Delta). Human ingenuity has facilitated (partial) adaptations that have thwarted at least some potential disasters in the past. The question the paper raises is are we capable of such adaptation this time around? The first step is raising awareness and the paper does that. Third, history shows that serious disruptions of civilization can occur if the unanticipated consequences of disrupting “social-ecological linkages” are not addressed until too late (from the bubonic plague of the dark ages in Europe, to Boko Haram in Nigeria and war in Syria today). These are extremes but recent events like horrific wildfires, extended drought, and extreme weather events illustrate California’s vulnerabilities and the urgency of addressing the environmental challenges cited by the ISB.

Finally, ecology is behind in understanding ecosystems disturbed by human activities. This may be one result of the ISB’s observation that ecologists long assumed change in Nature is slow. The importance of understanding disturbed ecosystems and the deficiency of our understanding are increasingly evident. We already live with the immediate threat of extinction of a well-known species (Delta Smelt). Accelerating change will multiply risks to iconic species, like salmon. Predictions of future change will require greater sophistication in our understanding of how different disturbances interact with ecosystem structure and function in a changing world. Reversing or slowing the impacts of such interactions will require a new level of political cooperation as well as new knowledge.

Indeed, scientific research may be challenged by the future described. I agree that science must produce results (and predictions) faster as we enter a world where change is occurring faster. This is not a new demand from environmental science. But the most effective ways to accomplish such a goal remain a subject of discussion. Specific knowledge gaps are not a mystery (see documents accompanying the Delta Plan). The process steps in turning those gaps into scientific output are difficult to speed up (ISB 2019). But can't we increase the rate of scientific output by increasing input (funding) and building the body of work from which new ideas will spring? Trying to squeeze more efficiency out of modest investments in Delta science is not consistent with the need to meet the increasing challenges of the likely future. Wikipedia estimates the gross state product of California as $3 trillion; the fifth largest economy in the world. At present we invest 0.003 percent of the gross product on science studying and assessing the hub of issues (water, fire, floods, climate, oceans, extreme events) that underpin (or could threaten) large sectors of the economy. The science for the future will require a sizable increase in investment, but it seems to me that is consistent with the stakes.

It is also asked if investments in science are effective. Growing recognition of the threats to ecosystems, water supplies and stability of water governance led to a state and federal joint water accord in the mid-1990's. The Record of Decision that followed included creation of the Delta Science Program (now 20 years old) in recognition that controversies were accentuated by a lack of data to test assertions of different parties. A pulse of approximately $70 to 100 million of "new" money (not re-programmed agency money) was invested in agency, stakeholder and university science between 1997 and 2004. The step-increase in knowledge that followed underpins what we know today about hydrodynamics, pelagic ecology, wetlands ecology, contaminants, life cycles of iconic species, climate change and other subjects. Growth of science infrastructure and the talent pool applied to Delta problems was rapid. This core provides the base from which regulations and plans were improved. It still leverages today's modest science funding. The investment that was made 20 years ago was effective. If the goal is to increase the rate at which knowledge is generated then greater investments in Delta Science is the least uncertain way to do that. The ISB report does not pull any punches when it comes to identifying the technical challenges of the future. It should take a similar approach to science funding.

The ISB report only partly addresses the question of what kinds of investment in science could result in increasing outputs. Continuing comprehensive documentation of how the system is changing is critical. New approaches focused on better predictions like horizon scanning and scenario assessments, decision support tools like structured decision making, and "devoting energies… to the most pressing problems of the day" also make sense. But those approaches mostly look for solutions in the existing knowledge of how the system works. Definition of today's problems also changes constantly in a variable world. Constant re-direction of science slows progress. If we have learned anything from our 20 years of science in the Delta it is that the existing knowledge base is not sufficient to satisfactorily address the complex problems of today, much less those of the future the ISB predicts.

The ISB report asserts that systems become more difficult to understand when rapid change is continuous and thresholds or tipping points are approached. “Replication, or even complementary research that helps confirm earlier findings will be more difficult or impossible.” There is an element of truth to these assertions. But to me this view of science is too narrow. There is no mention of the value of mechanistic understanding of the causes of change. Similar basic mechanisms to today, in different configurations or states, are the most likely drivers of future change. Thresholds and tipping points, also are not new in the Bay-Delta ecosystem. The mechanisms underlying some regime changes are not yet fully known, like the pelagic organism decline. However, other regime changes have been explained by a combination of long-term assessment and basic system understanding (e.g. Cloern et al 2010). Deeper understanding of the mechanisms driving change will aid understanding regime change and provide a more solid basis for predictions and preparations whatever the rate of change.

We must be careful that calls for work on relevant problems do not exclude growing the underpinnings of such work. Integrated system studies aimed at building basic knowledge of how the disturbed Bay-Delta system “works” (mechanisms) must be a component of the science of the future. Decision makers and managers can be slow to value such science. It is argued that mechanistic studies do not target immediate solutions; the problems are too complicated; the delivery of useful knowledge is too slow; we already know all we need to know. The short-sightedness of that skepticism is shown in the long history of scientific discovery. For example, in California, skepticism about climate change in the late 1990’s was reversed by the rapid growth of knowledge since then. Today, knowledge about climate change underlies many state policy decisions, considerations that are demanded by the citizens of California.

A fourth component of a robust science of the future is adaptive management; a concept with a mixed history, at best, in the Bay-Delta. However recent successes with active adaptive management actions that included modeling, monitoring, assessment, reporting and feedback are encouraging. These included experiments with Fall outflow, injection of water through Yolo Bypass, extermination of aquatic vegetation, the Delta salinity barrier and management of the salinity gates into Suisun Bay (reports in progress). To some degree, this is a new approach to science in the Bay-Delta. Broad collaborations is an under-appreciated but critical element. The typical reductionist restriction to changing one variable at a time and exact replication from year-to-year is not possible in such experiments. Even so, every one of the experiments above changed the state of knowledge about the question at hand. Inclusion of feasible experiments in nature as part of science program for the future could help grow knowledge faster than at present.

Recent history has also shown that addressing complicated Delta problems requires collaboration. This means involvement in planning, permitting, implementing, interpreting and learning from the outcomes. All must be integrated across every relevant party. Agencies, university and stakeholder scientists have all made important contribution to the existing knowledge base. Incentives to encourage collaboration across institutions and disciplines will become even more important in the future the ISB predicts.

A final need identified in the ISB-projected future was improving the value of science to inform decision-making. “Translational research” and “actionable science” indeed have value in addressing this long-standing challenge. Broad acceptance of an outcome is critical to its influence on policy. Broad acceptance is more likely when non-specialists better understand complicated research outcomes (translation). Acceptance is more likely when managers, technical staff and scientists from multiple interests work together in every step of a study or an informative workshop (part of actionable science).

Trust is also critical to improving the bridge between science and policy. Decision makers are most influenced by technical managers they trust and those managers are most influenced by scientists they trust. Institutions, work groups, and forums that bring together management and scientists from multiple interest groups to discuss or evaluate science issues are effective in building trust; as long as the discussion is structured to minimize arguments about who is right. Collaborative efforts also can expand the array of interpretations that contribute to management decisions. This is especially important for issues where decisions are important but uncertainties are large. The Delta Science Program and the Collaborative Science and Adaptive Management Program (CSAMP) are working, with mixed success, on collaboration, trust and using science to inform policy. Much remains to be solved. But acceptance of new tools, dedication to the concept that collaboration better informs decisions, and “stubborn optimism” about the process (Christiana Figueres, interview from World Economic Forum, 2020) seem necessary in achieving the most informed policies.

The future envisioned by the ISB is daunting. The ISB paper does a major service by raising awareness of the characteristics and challenges of a future we should prepare for, not only as environmental scientists but also as responsible contributors to governance. The future will challenge the capabilities of science. But gathering the political will, and sufficient trust from the public and among interested parties, to invest in a science for the future is also important. Science for the future will require greater investment than today. It should include training for those that can teach the urgency of the problem and help students understand the challenges that lie ahead. A robust program must be balanced among monitoring, studies of immediate relevance, longer-horizon mechanistic study and feasible experiments in nature. Stubbornly working at building trust among interest groups, decision makers and scientists is essential. None of this may seem that new. That is because we have begun to adapt to some degree. But the ultimate unified science for the future will require vision, persistence, wisdom and cooperation beyond what we have mustered in the past.

References

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Independent Science Board. 2019. Toward a preemptive ecology for rapid, global and increasingly irreversible environmental change. Draft discussion paper.

Appendix C: Dr. Margaret Palmer Comments

The Bay Delta system is an extremely complicated system biophysically and socially. There are many species of interest and multiple invasive species that may be difficult to eradicate ever. The extent to which the hydrology has been manipulated and engineered exceeds that of most any ecosystem. These combined with the competing interests, the socio-political and economic forces at play, and the multi-layered natural resource governance network make the work of the Bay Delta science groups extremely challenging. The rapid change discussion paper (hereafter, “the paper”) asks panelists to place these challenges in the context of rapid global change some of which may result in irreversible ecological changes within the Delta system. Specifically, they asked us to respond generally to what is presented in the paper, the need for proactive environmental science that supports preemptive management. I preface my comments by saying that I present a variety of perspectives on the topic of futures, uncertainty, and management and try to provide literature references along the way so the Delta science group can choose to investigate all or none. I am *not* an expert in any of these and so cannot provide detailed information on them rather I can perhaps provide an entrée to the topics.

In general, I do agree with the paper’s assertion that predicting socio-environmental futures has become increasingly difficult. I also agree that we need wise and scientifically based plans to manage natural resources given uncertainty. However, the paper came across as a bit alarmist grounded more in general ideas and global trends than in what is known about the Delta region and what we do know scientifically about expected futures and the diverse ways to conceptualize them. I note that many of the citations in the paper are very general (e.g., IPCC reports, Rockstrom’s *Planetary Boundaries* paper, Groffman’s *Ecological Thresholds* paper, and Scheffer et al.’s regime shift work). I would say they do not draw on what is known from the specific region of interest (the Delta) but also do not reflect progress in scholarship to: conceptualize and model socio-environmental systems to inform planning, the variety of ways futures are studied or conceptualized, and a number of other related topics that I touch on below. Not having worked on the Bay-Delta system, I do not know what all has been done [by the science committees] nor the environmental management priorities which would have helped me immensely in thinking about needed scholarship.

On the use of models

If a model of the components including drivers and assumed causal pathways for the Bay Delta exists, I am not aware. Having worked on Chesapeake Bay watersheds for years, I do not think the Delta system is very comparable. The latter is much more complex! Further, the Chesapeake model does not include social drivers and feedbacks. I am not suggesting that developing a strong model of the Delta system/subsystems (biophysical, technological, & social components) that is linked to management actions will be easy but taking the first step to build a strong and well-informed *conceptual* model of the Delta socio-environmental system with proposed drivers and feedbacks will help in identifying science priorities. In the process of developing this, just identifying assumptions about drivers and pathways that result in different outcomes can be very useful. And eventually in guiding management. So this should be a science priority.

Starting with a conceptual model is essential however; some have gone well beyond this. There are many mathematical and modeling challenges that still make modeling SES systems difficult (e.g., see Elsawah et al. 2020). Despite this, there are some strong examples of making progress in doing so – for example the work by Maja Schluter and many others on the collapse of the Baltic Cod is a really nice example of socio-environmental science that contributed to moving from being able to predict dynamics based on a biological understanding of the system to identifying the socio-political drivers contributing to the collapse ("regime shift") – see Lade et al. (2015). This in turn revealed some leverage points that could be used to alter management. They combined qualitative and quantitative social and ecological data/information in the context of dynamical systems theory to understand the system. As they describe in the Lade paper, they show that:

> "*generalized modeling, which is well-suited to collaborative model development and does not require detailed specification of causal relationships between system variables, can help tackle the complexities involved in creating and analyzing social–ecological models.*"

A variety of other modeling approaches often applied to complex socio-environmental systems to help understand the Delta system include for example: agent based modeling (e.g., Gotts et al. 2019), fuzzy cognitive mapping (which could be combined with participatory modeling approaches (e.g., Giabbanelli et al. 2017), and system of systems modeling (e.g., Little et al. 2019).

Futures to guide planning

The Bay-Delta group may want to consider shifting from a mindset of restoration, reference sites, and quantitative or semi-quantitative models to development of an environmental futures program (e.g., Gibbs and Flotemersch 2019). Futures research is a social science field that has resulted in many different methods for creating foresight (sensu Bengston 2019) that can be used to guide management. This approach to planning is no longer <u>only</u> driven by quantitative predictions of the future (e.g., based on earth system models or downscaled versions). Use of models like GCMs or ESMs (downscaled or not) are one approach and are extremely common but there are other ways to plan for the future. Rather than a predictive approach, a more exploratory approach may be wise at this time for the Bay Delta. A variety of ways to describe futures approaches include for example, **envisioning** preferred futures, **horizon scanning** to identify emerging patterns or opportunities (e.g., Sutherland et al. 2016), etc. and **scenario development** in which modelers accept that it is impossible to calculate futures given high levels of uncertainty so the goal is so identify (imagine) a credible future.

It is often framed in terms of “what would happen if xyz did not (for e.g., stop the spread of a particular invasive species in the marshes of the Delta). The answer to that question may determine what or if at all, management actions are planned. There is now a large scholarly literature on scenario planning and the Delta scientists may want to consider this type of expertise.

But scenario planning is only one Futures approach and the field has grown significantly over time. As Bengston (2019) discusses:

> *“Methods relying heavily on expert knowledge dominated early futures research in the US, but participatory methods that engage stakeholders with diverse perspectives and expertise have rapidly gained ground in recent decades. The development and use of participatory futures research methods (e.g., scenario workshops and public Delphi) have been spurred by growing evidence that diverse groups are more effective at solving complex problems than leading individual experts.”*

Using such an approach requires the scholars working together with the decision makers in an iterative fashion rather than linear in which decision makers request pieces of science from the scholars and those are delivered with little active conversation or engagement. Future methods are evidence based and can be quantitative but mostly they are qualitative.

Uncertainty, attribution science and moving from bottom up approaches to estimate risks

Much of the focus on global change (e.g., warming, magnitude and frequency of extreme events) and natural resource management has been on quantifying the magnitude pace, and direction of change. The 2018 California Climate Change Assessment report states:

> *“climate change is making extreme conditions more frequent and severe. California’s temperatures are already warming, heat waves are more frequent, and precipitation continues to be highly variable.”*

A thorough analysis of the uncertainty around such changes in the Bay-Delta region’s climate is essential and it is also critical that if such changes are real (e.g., compared to historic patterns) they can be attributed to climate change (e.g., see Williams et al. 2015 paper on CA droughts and natural variability). Without these, management actions may proceed under the assumption that some changes are unavoidable and inevitable when they are not (or vice versa). Knowing how much of the change is attributable to human actions vs. interannual variability is important.

It is not easy to demonstrate anthropogenic effects on for example extreme events because events like drought are usually caused by multiple factors acting together (Trenberth et al. 2015). I am not suggesting that the Delta program invest is a lot of science to try to pinpoint probabilities of certain events or environmental outcomes (see next section) but rather decisions are not based on weak science that assumes attribution.

Brown and colleagues have pushed forward the idea of "decision-scaling" which asserts that rather than seeking probabilities, climate modeling be used to estimate the relative likelihood of different climate states and that this be considered in the context of different potential future states, the vulnerability each brings, and what is considered unacceptable … then management actions are compared. The idea is to identify a management strategy that is best across the broadest range of scenarios (likely futures). Most specifically, this approach does not rely on *having to estimate risk (e.g., estimate precise future probabilities). For more on this, I suggest looking into work by Casey Brown (University of Massachusetts) and colleagues* (Spence and Brown 2016; Brown et al. 2019)*. As he has written on his University of Massachusetts website:*

> *"Decision Scaling uses stakeholder engagement, a decision analytic framework, and systems analysis to tailor the scientific investigation to the variables that are most influential for decisions. The stress test is an analytical process by which we perturb the external factors or uncertainties affecting a system to identify the factors or combinations of factors that cause that system to fail. In this way, vulnerabilities are identified. The vulnerabilities can be defined in terms of those external factors and the thresholds at which they become problematic. This results in scenarios that are directly relevant to your decision and which serve as the basis for any necessary scientific investigation."*

Science on managing for resilience

While the last section on attribution science suggests the need to fully understand what is anthropogenically driven vs. natural variability another direction scientists have taken is to stress managing for resilience. Obtaining better and better estimates of likely changes in precipitation is extremely common and indeed the hallmark of the IPCC process and many state climate change programs. However, many scientists are arguing this is a waste of time specifically because uncertainty is so high…so why not manage for resilience to change. This is a very different approach than for example trying to predict magnitude and frequency of droughts/floods and build infrastructure or manage water allocations based on these predictions. Instead the assumption is that uncertainty is so high that management should focus on ensuring that management decisions do not lock one into a certain path but rather allows for "adaptive pathways" (e.g., see Haasnoot et al. 2011; Haasnoot et al. 2013). Grantham et al. (2019) say, this "incorporates flexibility, the precautionary principle and no-regrets strategies."

The idea is to consider the potential impact of certain management decisions on the resilience of ecosystems. Under this concept, the system becomes managed for maximizing the ability of it to recover most valued ecosystem services in the face of disturbances rather than specific endpoints (e.g., specific species). This recognizes that ecological systems will change hopefully adapting rather than collapsing. As Johnson and Geldner (2019) describe concerning natural resource management in the face of uncertainty:

> *"the predict, then act paradigm is gradually being displaced by monitor and adapt. Contemporary decision-analytic frameworks diverge from traditional methods in two distinct yet complementary ways. The first is to broadly sample plausible future states of the world (SOWs), exploring a wider range of potential system responses in order to seek solutions which are robust to a wide range of potential futures. The second is to use more sophisticated techniques to identify strategies that deliberately adapt to the inevitable occurrence of unexpected events."*

Restoration reference sites

Most restoration scientists no longer adopt the mindset that there is "a" reference site or that some historical state that provides models of how the "disrupted site might best be ecologically restored" (page 3 of the Delta ISB Discussion paper). This is increasingly recognized as a somewhat futile exercise because uncertainty in such 'quantifications' is so high. Further, the idea of reference conditions typically denotes a system in equilibrium i.e. removing the stressors or mitigating them through restoration actions will result in return to a former state or a state that is similar but perhaps less disturbed i.e., a reference site. As I wrote in 2009:

> *"Incorporating nonequilibrium dynamics into restoration planning suggests that many pathways are likely possible and may depend on restoration actions and thus, it is important for restoration "targets" to be based on an array of possible outcomes or states. As Hughes et al. (2005) have argued, using reference systems can give a false sense of predictability of ecological outcomes. Rather than selecting reference 'endpoints', a desired trajectory should be defined that takes into account a range of values for key system attributes that are inherently variable; e.g. ranges of flow and sediment inputs, variability in the location and number of habitat types, and changes in the species composition of assemblages through time and space (Hughes et al. 2005)."*

I would update my writing to now include the desired endpoint may not be based only on biophysical attributes and indeed may be very different in species composition, etc. than other similar less disturbed sites. The goal may be to prevent collapse of some component or process within the system or to recover some of the functional attributes without attention to the species. The use of trait-based approaches may be helpful in this regard. The concept of restoration for resilience shifts the focus from basing reference conditions on past or predisturbance states to using history to ask what conditions might allow the species or functional groups to persist. e.g., as Falk and Millar (2016) say:

> "*Rather than emphasizing only time-specific historical ranges or predisturbance species assemblages, compositions, structures, and landscape patterns, a resilience approach to restoration embraces landscape macrodynamics that have characterized populations and species over long timeframes. . . . The question for restoration ecology thus becomes not if these [climate] changes will occur, but whether the restoration response will be to resist, stand back and watch, or facilitate such change."*

An additional problem with using the un- or least-disturbed reference site framework is that it does not help in understanding the causal mechanisms behind recovery or failure. A more effective approach is to identify or hypothesize causal pathways associated with a suite of trajectories the system (or some component) could take and how those pathways related to the desired state (e.g., using Futures approaches). Note, my critiques of the use of a reference site as a measuring stick for progress does not mean that I believe one not need goals that are well articulated. The latter are critical. Rather it means working to hypothesize what the most important drivers (i.e., most limiting recovery) of degradation are, generating ideas on causal pathways and how they may be linked to the goal, and given this context the alternate trajectories the system/component might take when some management action is taken. Then measuring so-called "success" over time should be based on steps in the trajectories, no on the final goal. Example: in stream restoration, the most common goal for *ecological restoration* (not channel stability for say protection of infrastructure…which is very common) is recovery of native aquatic insect assemblages. The problem with comparing progress to how many native species are recovered or not is that this tells you little about why recovering is incomplete or absent.

A related idea is that of novel ecosystems as future endpoints rather than reference sites. This has been widely debated and I have been a critic of the concept (e.g. Palmer et al. 2014) while those in favor of it argue that it is often not possible to return to pre-disturbance conditions and novel ecosystem are a reality of today and the future (Hobbs et al. 2013). My concern has been this suggests "anything is acceptable" and while this is not what the proponents of the concepts intended, it has been adopted in that way already. The value of the novel ecosystem concept lies in its attention to perhaps what is possible when so called historical or contemporary reference conditions are not possible.

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Appendix D: Dr. Tessa Hill Comments

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January 23, 2020

Delta Independent Science Board
Elizabeth Canuel, Chair

Dear Delta Independent Science Board Members,

Thank you for this opportunity to review and discuss the Delta Independent Science Board discussion paper, *Toward A Preemptive Ecology for Rapid, Global, and Increasingly Irreversible Environmental Change*. I look forward to future discussion of this topic at our meeting on January 30, 2020.

The premise of this discussion paper is to provide information on how research and management actions in the Delta can be improved to address emerging challenges, specifically those related to rapid (anthropogenic) environmental change. I found the discussion paper interesting to read, thought provoking, and in general I am in agreement that we face management and scientific decisions that are unprecedented relative to the way we approached these problems over the past century. My suggestions for improvement of the discussion paper fall in five main categories and some additional suggested changes, described further below.

1. Consider the needs and goals of the discussion paper

The goals and target audience of the discussion paper should be clearly stated in the first few paragraphs. For example, this may (or may not) include the desired outcomes below, which could structure and frame the paper:

- Review the “changing reality” of management under rapid environmental change
- Review available science & history of management approaches,
- Identify data gaps and needs – what critical pieces of ecological information do we not yet have about the Delta?
- Prioritize action and identify management choices

An important section that currently addresses this is found on page 5, which includes: “…how they will interact with each other during rapid, global and irreversible environmental change are only beginning to be studied and are not yet to the stage where future scenarios can be developed. This means that it is difficult to assess which ecological factors can be managed or which stresses mitigated or ameliorated… It is also not clear what goals might be desired for management actions in a rapidly changing ecological system." (underlining is mine). This section clearly outlines data gaps, management needs, and a question about future management goals. In my opinion, these kinds of statements should be the start (and the framing) of the document – everything else should follow from there. Similarly, on page 6, the document states: "Lags and gaps between scientific understanding and meeting the needs of managers persist." What are those, and why do they exist – how can we tackle them for the Delta?

2. *Introduce the important changes that are of concern*

While I understand that the intent of this document is not to review the physical forcings that are changing ecosystems in the Delta, it is challenging to discuss "rapid environmental change" without specifically addressing what those changes are. Is this document intended to be limited to changes in the carbon/climate realm, or extend beyond that? On page 5 there are several potential impacts that are discussed, but no formal paragraph or section that specifically addresses what rapid environmental changes are of concern for the Delta, both physical and ecological. Again on page 8 in the "Rapidly Accelerating Environmental Change" section, there is no discussion of *what* changes are of concern.

The document would be strengthened by having an introductory section explaining what changes are of concern, and bolstered with significantly more citations/resources. Several that I would recommend that are directly relevant (and written with a policy/management audience in mind) are:

- California's Fourth Climate Assessment
- Indicators of Climate Change in California
- National Climate Assessment – Southwest Region
- UN IPBES Biodiversity Report

3. *Add specificity where possible*

There were several sections of the discussion paper that seemed to rely up on overgeneralizations. The strength of these assertions and ideas can be improved by adding specificity, making examples as pertinent as possible to the Delta, and adding citations.

- For example, statements like "what is different is that ecologists concerned with change in one place will no longer be able to look to other places with comparable but less disturbed habitats to provide models of how disrupted places might be ecologically restored" (page 3), is true, but this has been the case for quite a while now – this is not a new realization or phenomenon.

- Similarly, statements like "there will be rapid change everywhere" (page 3) are generally true, but in this case the specifics matter – the rates of change in different environments are different, and are driving different adaptation and management strategies.

- "…No one expects forests to return to their previous state" (p. 4) is another example where this section/paragraph could be made more specific, with citations. The literature shows that forests are unlikely to return to their previous species composition, which is a subtle yet important difference from what is stated in the paper.

- "Trends are steeper, extremes are more extreme and frequent, and systems are undergoing major shifts…" is included in a paragraph on p. 5 – this entire paragraph is quite vague. All of these statements will be stronger and more meaningful if grounded in specifics (trends in what? Extremes in what? What kinds of systems?).

- On page 9, the third and fourth points (about variability and extremes) need citations or support for these ideas – to my read, they are making the same point (increased extremes) and it would help if the document clarified how these are two separate points of change.

4. Spend more time looking forward

The historical justification for past management actions (page 5 to 7) could be significantly shortened. The main point here is that management action has to catch up to what we have known scientifically for several decades. This section, while it may seem important as a lead up to suggestions starting on page 8, seems to again rely on significant overgeneralizations. In a nutshell, there has been abundant evidence both in the geological and ecological realm for very fast environmental change, and ecosystem perturbations and recovery, for a long time. To that point, the historical narrative could be significantly shortened and then start with text on page 7 ("By the 1970's…") because this section describes that we have known about the potential for rapid environmental change (in both ecological and geological realms) for a while.

Further, this document is an opportunity for the Delta Independent Science Board to request or guide scientific investigations that will be of use to managers in filling data gaps and providing decision support (see my earlier comments about gaps/needs/goals guiding this document). Rather than stating "scientists…need new ways of thinking about the Delta to better anticipate and preemptively respond to forseeable accelerations in its dynamics…" (page 6), this is an opportunity for those involved in management of the Delta to call for science that fits those needs.

The section beginning on the bottom of page 9 is forward looking and should continue to be a central focus of the document. However, I would argue that rather than these changes be seen as a shift in how science is done, they are instead efforts to synthesize and understand the complexity of rapid environmental change and resilience. Organizations like NCEAS and SESYNC are experts at these synthesis efforts; I look forward to the comments of the other panel members on these topics.

5. Acknowledge the role of political and social systems

I was challenged by nearly all of the statements made on page 8 under Section 3 "A Closer Look…" because they suggest that the primary issue in management and response to rapid environmental change has been a flaw in the scientific method, rather than addressing how scientific information is taken up into policy and management action. It is, in fact, via the scientific method that we have discovered and documented all of the rapid environmental change that is the basis for this document. Importantly, and not mentioned here, what has slowed the response to massive anthropogenic impacts on the environment has been driven primarily by political and social forces, and by the complexities of human decision making.

Additional comments not addressed in sections above

I think the discussion of potential (and past) regime changes was interesting, and could be expanded (page 1 to 2). Regime changes have happened prior to our knowledge of the POD (perhaps not in the Delta, but there is environmental/ecological precedent for this). Sometimes these regime changes happen too rapidly for management response, and at other times they don't. It might be good to consider and review in this section other potential regime shifts that may impact the Delta, for example the outbreak of disease/pathogens, or shifts forced by an oil spill or similarly significant pollutant.

The idea that "drivers are beyond regional control" (page 4) is accurate, but the document misses an opportunity here to advocate that local and regional processes do contribute to these drivers. This discussion paper could explicitly support state efforts at greenhouse gas reductions, for example, as critical for the long term recovery of the Delta.

The section on forest ecosystems is fascinating and should be clearly described as a 'case study' of relevance for the Delta. I think it should be formatted in this document in a way to call it out as separate from the rest of the document – in a box or subsection – to make it clear that this is a valuable case study to learn from. The forest ecosystem example calls out three approaches*: monitoring*, *investigating* (understanding) and *adaptively managing* the system – that are directly relevant for the Delta. These three take home messages from the forest ecosystem example could be used to structure recommendations later in the document for the Delta system. As a side note, these approaches are not really discussed again later as recommendations for the Delta, and could/should be.

On page 6, the document states: "interpreting the implications of new scientific findings for management and communicating significantly new interpretations to policy makers and managers will become even more challenging." Based upon what? Over time, scientists, managers, and policy makers have only become more savvy in understanding rapid environmental change and communicating the consequences of this change – on what basis do we know that this is going to become more (not less) challenging? This is reiterated again on page 9, "scientific understanding has become more difficult…" – based upon what?

Hill Comments

On page 8, the paper states: “the rates of change are much higher than they were during the past century,” but in fact they are higher than essentially any interval in the geologic record as far as we understand (with regards to the carbon cycle).

Regarding the section on page 13 that discusses speeding up the translation of research to decision making: while I agree with what is written here it neglects to mention that all of the work done by Cooperative Extension at UC and other campuses does exactly this – this approach of translating science to decision making has existed for a long time and certainly could be supported further. Along these lines, the newest generation of ecologists are highly skilled at this translational work, are well trained, and are more interested in crossing boundaries of science, management, advocacy, communication, etc. What currently holds these efforts back is a university system that doesn’t necessarily reward these behaviors in scientists and has not fully incentivized this work of boundary crossing.

Finally, I would recommend that the discussion paper explicitly call out the need for knowledge and management action around rapid environmental change to incorporate the needs of vulnerable human communities, and to acknowledge the value of learning from indigenous communities that have lived and managed resources in this region for thousands of years.

Sincerely,

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Appendix E: Dr. Carrie Kappel Comments

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January 23, 2020

Introduction

The members of the Delta Independent Science Board rightly point out that systems all over the world are experiencing ecological and sociocultural changes that are increasingly rapid, global, beyond regional control, and irreversible. Our time, the Anthropocene, is characterized by directional and interacting trajectories of global change, affecting every corner of the planet. Global climate change is warming the Earth, acidifying the oceans, raising sea levels, and disrupting weather patterns worldwide. Human population and associated natural resource consumption continues to climb. Climate refugees contribute to mass human migration, which is increasingly disrupting societies and leading to unrest around the world. Species losses mount, as we find ourselves in the midst of a mass extinction event of our own making, driven by habitat loss and fragmentation, overexploitation, invasive species and climate change.

Ecology as currently practiced is being sorely tested as it attempts to keep up with the pace of change and provide relevant science to inform decisionmaking in the face of high uncertainty and large scale, rapid transformation of complex social-ecological systems. Natural resource managers, too, face unprecedented challenges that call into question their status quo operating mode. Whereas managers might once have focused on resisting or preventing ecological changes, they increasingly recognize that ecological transformation is widespread and in many cases inexorable. It will be impossible to resist all of the change underway. Faced with this reality, some managers and scientists are looking for new tools to help them determine which changes to try to resist, which to accept and adapt to, and which to attempt to direct to influence their outcome (Schuurman 2019).

As managers approach these difficult questions, the legal mandates under which their agencies operate may themselves be called into question. Do managers have the authority to choose acceptance and adaptation? How flexible are their goals and targets? Do they have the nimbleness to respond quickly and boldly to direct change? As species, habitats, ecological functions, and the people and economies that depend on them shift their distributions with climate change, how should agencies with jurisdictions tied to particular geographies adapt their goals and activities? Do they have the means to collaborate across boundaries? These are tough policy questions without easy answers.

Given the deep-rooted nature of the problems and the complexity of the interactions that impede addressing them, we need to be looking for opportunities for real systems change - in the practice of science, in the policy and practice of ecosystem based management, and in the solution space. In this context, science has a real role to play (necessary, but not sufficient for positive change). Understanding these complex systems will be critical to increasing our chances of avoiding unintended negative consequences and disruptions and adapting to change. In this context, recent work in *resilience science and tipping points, social-ecological systems thinking, ecosystem thresholds and forecasts*, and *ecosystem risk assessment* may provide useful insights and tools. At the same time, we need to accelerate the science-to-action pathway and decrease the time it takes for new knowledge to inform decisionmaking. Here progress may be made by adopting *open science practices, co-creating research with end users, broadening our definitions of what is research and who is a researcher, and investing in the people and structures to support collaboration and learning*. In all of these areas, there's an urgent need for bold experimentation and better communication to accelerate collective learning and action.

Resilience science and tipping points

Resilience thinking is not actually very new, having its roots in the 1970s writings of Buzz Hollings and contemporaries. However, while the ideas have been around for a while, putting the science into practice has been slow. More recent work by the Stockholm Resilience Centre, the Ocean Tipping Points project, and many others has centered on trying to operationalize key resilience concepts and develop tools and approaches to support their implementation in management. Here I share a few key insights from that recent work.

Most of what we know about tipping points, which occur when small shifts in human pressures or environmental conditions bring about large, sometimes abrupt changes in a system – whether in a human society, a physical system, an ecosystem, or our planet's climate, has come from hindsight after already having crossed them. The regime shifts that result from crossing tipping points have been observed across a wide variety of aquatic and terrestrial habitats around the globe. They tend to be persistent and difficult to reverse, particularly when characterized by hysteresis (See Figure E-1). Recovery times may be on the order of decades. And they are often associated with the cumulative effects of multiple stresses acting on a system either simultaneously or over time. In this context, methods for identifying tipping points prospectively will have most value. To date, scientists have approached this task either through analogy to similar systems, analysis of the past, modeling, or the development of early warning indicators.

The terms thresholds and tipping points are often used interchangeably, though tipping points are generally associated with 'ecosystem thresholds' that when crossed lead to fundamental shifts in ecosystem structure and function. Not all thresholds will lead to such dramatic change when crossed. Thresholds are not limited to the biophysical components of a system. Socioeconomic and cultural aspects of the system may also exhibit threshold behavior, and biophysical and social thresholds may interact. Finally, a small scale event may trigger a tipping point with cascading effects at larger scales or in other components of the ecosystem due to linkages and interactions.

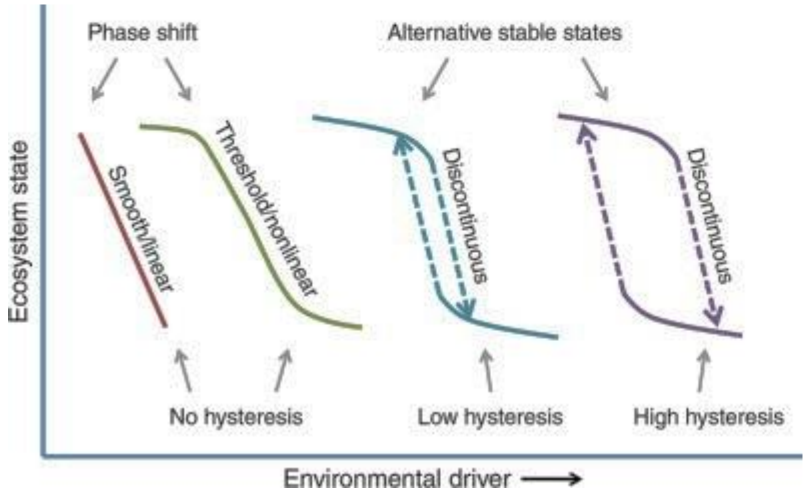

Figure E-1. The type of regime shift is mediated by whether or not there is hysteresis in the ecosystem, in which the pathway for recovery is different from the pathway of degradation and there is a different threshold that must be crossed to return to the previous ecosystem state (Suding and Hobbs 2009). Reprinted with permission from Selkoe et al. 2017.

While resilience science may be unfamiliar to some managers, all of the major US environmental laws allow for the use of thresholds in management, and many management systems already rely on quantitative thresholds to trigger monitoring or management action (though not all quantitative thresholds are linked to a known ecosystem threshold – some are set based on other criteria, like human health or technological capacity) (Kelly et al. 2014). In reviewing 51 case studies around the world from systems with known tipping points, we found that those who had developed a quantitative understanding of thresholds in their system and were using those thresholds in management tended to have better ecological outcomes (either success in avoiding crossing a tipping point or progress in reversing one) (Kelly et al. 2015).

In a paper led by Mary Hunsicker, Ocean Tipping Points scientists examined the nature of the relationships between stressors and key ecological components in pelagic systems and quantified the frequency of nonlinear responses, which could represent thresholds or tipping points (Hunsicker et al. 2017). We found that over half the responses were nonlinear, and many of them strongly so, suggesting the possibility of a detectable threshold that could be relevant for management. Further, we found that nonlinear responses were under-reported in the literature because statistical methods for quantifying nonlinear relationships developed more recently.

The frequency of threshold responses in these systems and the potential improvement in management outcomes that could result from integrating quantitative thresholds into management suggests that more effort should be invested into detecting thresholds and designing monitoring and decision support systems around them. This can be a data- and computation-intensive effort and the variety of methods available can be daunting. Samhouri and colleagues offer a valuable integrative framework and open source code to apply multi model inference to ecosystem threshold detection (2017). Successful threshold detection requires rich time series or spatial datasets (or combined spatiotemporal data), putting a premium on ecosystem monitoring efforts.

One must note the challengeof applying thresholds quantified from historical data to a system that is rapidly changing and may be moving into unprecedented conditions that have no analogue in past time series. This is definitely a concern, though some threshold relationships (e.g., physiological thresholds like responses to ocean chemistry) are fundamental enough that they will apply across a range of conditions.

Once quantified, thresholds can be used in combination with ongoing ecosystem monitoring and, where available, near real-time forecasts of physical conditions to provide benchmarks against which to monitor ecosystem change. This potential is beginning to emerge for some well studied systems (Tommasi et al. 2017). As a system approaches a known threshold increased investment in monitoring and preventive or adaptive action is warranted (Selkoe et al. 2017).

Given how common tipping points are and their disproportionate impact, in the absence of evidence to the contrary, one should assume nonlinearity is at play (Selkoe et al. 2017). Further principles for management of ecosystems prone to tipping points are given in Table E-1, reprinted with permission from Selkoe et al. 2017 and modified for accessibility.

Table E-1 from Selkoe et al. 2017

Social-ecological observation	Management principle
Tipping points are common.	In the absence of evidence to the contrary, assume nonlinearity.
Intense human use may cause a tipping point by radically altering ecological structure and function.	Address stressor intensity and interactive, cross-scale effects of human uses to avoid tipping points.
Early-warning indicators of tipping points enable proactive responses.	Work to make transparent the effects of tipping points on benefits, burdens, and preferences.
Tipping points change the balance between costs of action and inaction.	Tipping points warrant increased precaution.
Thresholds can guide target-setting for management.	Tie management targets to ecosystem thresholds.
Tiered management can reduce monitoring costs while managing risk.	Increase monitoring and intervention as risk of a tipping point increases.

The importance of cumulative effects in the context of tipping points and accelerating global change cannot be understated. Given the lack of time for recovery post-disturbance, many systems are transforming due to compounding and cascading changes. For example, as the frequency of massive wildfires increases and the fire return interval shortens we may see broad-scale habitat type conversion in California landscapes. Our current monitoring and management tools for assessing cumulative effects are inadequate to the task (but see Holsman et al. 2017 for a review of ecosystem risk assessment tools that apply). Understanding and quantifying cumulative effects and detecting related tipping points remains an important research frontier.

One final insight from recent work on tipping points is worth highlighting. While scientists or natural resource managers may view the ecosystem state on one side of a tipping point as more desirable, preferences for ecosystem state are complex and vary among stakeholders. Any major ecosystem shift will have winners and losers, and there may be stakeholders on each side, advocating for either the persistence or reversal of that state. The regime shift between kelp forests and urchin barrens mediated by California sea otters illustrates the point.

Following near extirpation of sea otters across their range during the fur trade, urchins were released from predation and overgrazed kelp forests, resulting in denuded rocky reefs called urchin barrens. As otter numbers in California recovered post-reintroduction, they crossed a threshold density where they could again control urchin populations, allowing kelp forests to thrive. Advocates for otter recovery place value in the biodiversity, finfish fisheries, recreation, and carbon sequestration the kelp forests support. But in Southern California, where valuable sea urchin fisheries developed in the absence of otters, the urchin barren state is preferred by urchin fishermen who have fought the reintroduction of otters vehemently. We need science and management tools that allow us to understand diverse viewpoints and values and weigh the tradeoffs of alternative management decisions in complex social-ecological systems (e.g., White et al. 2012, Shelton et al. 2014, Lester et al. 2018, Okamoto et al. 2020).

It’s worth noting, that while this was once a classic example of a relatively easily reversible marine regime shift, the kelp forest-urchin barren system is now experiencing a new set of alarming dynamics. Ocean warming and a warming-related disease that caused widespread seastar die-offs (another important urchin predator) have combined to cause booms in urchin populations and catastrophic declines in bull kelp across large swaths of the California Coast (Rogers-Bennet & Catton 2019). Once valuable urchins are no longer even saleable because they are so over-abundant they are starving. And the extent of kelp forest loss is stalling otter recovery, further reinforcing the urchin-dominated state of the system.

Ecosystem function as the target

As managers and scientists grapple with high levels of uncertainty and rapid directional change, we are seeing a shift in focus toward preserving and restoring ecosystem *functions* versus particular species or ecosystem configurations (Ingeman et al. 2019). This necessitates deeper mechanistic understanding of how ecosystems work, even as they are changing before our eyes. And it elevates the need for functional ecology to develop better methods and models to inform how ongoing or future shifts may affect ecosystem function and associated ecosystem services.

We will also need to draw more deeply on social science as we work to anticipate changes in ecosystem function and their impacts on people. Integrated social-ecological methods will be needed to weigh alternative interventions and adaptation strategies (e.g., Shelton et al. 2014). Sociological surveys, ethnographic interviews, and participatory processes can help to uncover how stakeholders perceive ecosystem functions, what represent substitutable ecosystem goods and services in their eyes, and how shifts in ecosystem function and services are likely to affect human wellbeing. Integrated approaches to ecosystem assessment like NOAA's IEA are building social and ecological indicator sets, data collection methods and risk assessment methods that can help to inform this kind of inquiry (Breslow et al. 2017, Holsman et al. 2017). The "biocultural approach" to monitoring and ecosystem assessment particularly emphasizes the need to *start with and build on local cultural perspectives*, including local people's values, various types and sources of knowledge, and needs (Stirling et al. 2017).

Futures thinking for ecologists and natural resource managers

The panel's discussion paper highlights two very useful futures thinking tools (horizon scanning and scenario analysis) that could prove useful in coping with the rapid, global, and increasingly irreversible changes that we face. To our somewhat limited predictive toolbox, I suggest we also consider adding extreme events as natural experiments, early warning indicators, and ecosystem risk assessment.

While there may be no perfect analogue for the ecosystem one is charged with managing, we can nonetheless learn from comparison to other places undergoing similar changes to those which are anticipated. This may mean looking to other parts of the world that are experiencing more rapid change or being opportunistic about drawing insights from chance events that mimic what could happen. Extreme events such as the prolonged near-surface ocean warming in the Northeast Pacific observed in 2013 to 2015, known as the "warm blob," offer a possible lens into what could become future average conditions. At its peak, SST anomalies reached as high as two to three degrees Celsius and penetrated 180 meters below the ocean surface, causing dramatic effects on weather, air quality and coastal ecosystems (Liang et al. 2017 and references therein). Marine impacts included delayed onset of upwelling, species range shifts, and changes in productivity (Liang et al. 2017 and references therein), patterns which may well presage future shifts in the California Current Large Marine Ecosystem under continued warming. While specific, place-based knowledge of the ecosystem under study and management is essential, ecologists need to simultaneously keep a broader view and attend to dynamics elsewhere that could help them understand future local conditions.

Universal early warning indicators (EWI), which could warn when an ecosystem is approaching a tipping point have been something of a Holy Grail for resilience scientists. The theory of complex adaptive systems predicts that certain statistical properties (such as variability, autocorrelation, and critical slowing down of key variables) change as a system approaches a critical transition (a specific type of tipping point) (Scheffer et al. 2009). By closely monitoring for these statistical signatures in temporal or spatial datasets, one might, in theory, gain advance warning of a major shift in the system. Whether such warning would come in time for managers to respond and whether they would have the means to do so are separate questions (Biggs et al. 2009). Mathematical models, microcosm experiments and a few well controlled large scale experiments suggest that EWI may hold promise. However, their application is limited to the specific case of regime shifts with hysteresis (aka critical transitions), requires very high resolution monitoring data, and depends on our ability to monitor appropriate variables at the right scales (Dakos et al. 2015, Litzow and Hunsicker 2016). Selection of the specific variables to act as EWIs is not a simple task. Variables that are less naturally noisy / stochastic make better indicators, but increasing overall variability in natural systems may make even these variables difficult to use as EWI. The lack of high resolution continuous monitoring data of the variables of interest may represent the biggest hurdle to taking advantage of EWI's potential.

Ecosystem risk assessment can complement these approaches and provide a formal framework for weighing the probability of undesirable events and assessing their consequences. Methods range from qualitative to quantitative and can be used to assess simple (e.g., single species) to complex scenarios (e.g., effects of multiple stressors on multiple species or ecosystem components) (Holsman et al. 2017). Qualitative and semi-quantitative risk assessments are useful where information is needed rapidly, or when capacity for quantitative risk assessment is restricted by lack of data or limited resources (time, staff, computing, etc.). Quantitative risk assessment depends on specific thresholds tied to acceptable probabilities of risk and is used to characterize risks and tradeoffs associated with alternative management strategies. Techniques include food-web models, multispecies size-spectrum models, multi-species assessment models, and fully coupled physical-biological-social-economic models (reviewed in Holsman et al. 2017).

Investing in understanding social-ecological systems

Social-ecological systems (SES) research is required to understand the large scale dramatic shifts underway in coupled human-natural systems. Understanding systems in this multi-dimensional way requires deep interdisciplinarity and integrated approaches and tools. Getting trained in this way is still the exception in most ecology programs. New and practicing ecologists could benefit from cross-training to build flexibility and fluency in methods across disciplines, whether that training is formal or informal. Perhaps even more important and more overlooked are the collaboration skills that are needed to bridge disciplinary divides of language, philosophy, and methodology. Active facilitation and process support can help teams work more effectively together. As noted by the panel, the trust, open communication, and shared understanding that must be in place to enable successful interdisciplinary or transdisciplinary collaboration requires time to develop. Synthetic thinkers who are adept at translating and connecting knowledge across the disciplines can help weave collaborations and accelerate successful integrative research projects. People who are adept at such weaving are also critical to enabling successful collaboration between scientists and managers. While it may be hard to imagine how this interaction-intensive approach will speed science-to-action, this may be a case of going slow to go fast. If, as ecologists, we ignore the human dimensions of these systems, we risk missing important sociocultural, economic, or governance factors which interact with the ecology. All too often a siloed approach leads to management recommendations that are misguided, irrelevant, or even counterproductive.

Tackling social-ecological problems doesn't just mean having social scientists onboard to answer pre-determined sociocultural and economic questions and natural scientists to answer known biophysical questions. An integrated SES approach leads to novel questions, both disciplinary and transdisciplinary in nature. For example, in recent work in Haida Gwaii, British Columbia on Pacific herring, dialogue with local decisionmakers, community engagement, and ethnographic interviews surfaced multiple novel testable hypotheses about the ecology of herring, people's relationship to herring, and the consequences of threshold changes in their abundance in space and time (Poe et al. in prep). Similarly, patterns emerging in ecological analyses of herring spatial dynamics provoked new inquiry into how those shifts might be affecting people (Stier et al. in prep, Okamoto et al. 2019, Okamoto et al. 2020). Addressing these hypotheses led to new lines of both social and ecological research and theoretical and practical insights. Results of the work are now being used to inform ecosystem management of the Gwaii Haanas National Park, National Marine Conservation Area Reserve and Haida Heritage Site.

As alluded to in the Haida Gwaii example, structures that allow for co-creation of research with end users and incorporate local and traditional ecological knowledge can surface new questions and help to focus science on the questions that really matter to managers and local communities. Participatory research methods can also help to build a platform for citizen science. Broadening the research enterprise in these ways may yield new perspectives and more sources of data, enriching our collective ability to sense and respond to environmental changes.

Accelerating the pathway from science-to-action through open science

So how do we shorten the time from data collection to application and increase decisionmakers' access to relevant science? Broader adoption of open science practices could help. Open science is an umbrella term encompassing a variety of practices and movements intended to democratize science and enhance research collaboration and replicability. This may include sharing questions, methods and preliminary results (Open Notebook), data (Open Data), software (including analytical code and workflows) (Open Source), or results (Open Access). An increasing number of biologists are experimenting with sharing pre-publication manuscripts ('pre-prints') through open access websites like bioRxiv. When the average time to publications is months to a year, preprints can allow end users (like natural resource managers) to access scientific information within more relevant timescales.

Beck and coauthors argue the specific value of an open science approach for the field of bioassessment, which aims to develop indices and other tools for monitoring the status of natural resources and ecosystems (unpublished ms). They argue that bioassessment researchers should be open and transparent about how they developed their methods, and should make their data, results, and tools open access and intuitive to maximize their public benefit and utility to managers. Others have made similar arguments for the field of ecology in general (Hampton et al. 2015, 2016, Lowndes et al. 2017). Finally, as networks of autonomous sensors for environmental monitoring are more widely deployed, continuous data streams representing environmental conditions are becoming more and more available. These platforms should be open by design to maximize their usefulness in a world of rapid change.

Translational ecology also seeks to increase decisionmakers' access to relevant science through collaboration and exchange. The idea here is to shorten the path between scientists and policymakers or managers, but like other forms of collaborative research, translational science takes time. As a community, we need to get better at sharing what works and what doesn't work to avoid wasting time on ineffective strategies. We could also invest more in training young scientists in translational practices, including design thinking and effective communication. Further, we could create and foster fora for policymakers, managers, stakeholders, and scientists to interact and exchange ideas more regularly.

Individuals (e.g., project coordinators) or institutions (e.g., boundary organizations or synthesis centers) often provide critical collaboration support, infrastructure, translation/communication, convening, and other functions that enable scientists and end users to work together effectively. The funding community has been slow to recognize and pay for this critical role. Shifting that is essential, but may require institutional change.

Funding institutions aren't the only ones that need to evolve to foster more cocreation of research between end users and scientists. Both management institutions and research institutions present numerous barriers to collaboration across sectors, including but not limited to: differences in timescales for decisionmaking and action, limitations on how funding or staff time can be spent, and differences in how individuals are incentivized or evaluated in their jobs. Systemic change is needed to address these barriers.

Be bold, experiment

The challenges raised by the panel and discussed above are deep-rooted and entangled. They have no silver-bullet solutions, but they beg urgent action of all of us. We cannot know what the future holds or how our future selves might direct us in this moment if they could. But my guess is that universally, those future selves would exhort us to be bold. To get moving. To experiment. With ways of working. With ways of organizing ourselves. With management strategies and interventions. With the ecological systems we study and manage and love.

Bold management actions could include experimenting with assisted migration (translocation of species), genetic manipulation, and artificial habitat creation (Kareiva and Fuller 2016). Centuries of landscape change have happened by default, but we are moving into an era of more deliberative manipulation, restoration and 'design' of ecosystems, including active intervention into disturbance regimes, connectivity, and other landscape-scale factors. Our landscape conservation and restoration efforts could be much more innovative, holistic, and forward-looking.

We cannot keep imagining that the world is going to return to the way it was when we were young. The baselines have more than shifted - they're careening wildly and out of control. We need both risk management or triage - to help us focus our limited resources and time effectively - and audacious, unconventional ideas to explore entirely new terrain. This represents an 'act, sense, and respond' approach to experimentation and learning, appropriate to the constant change that characterizes the world around us (Kareiva and Fuller 2016). To get started, we may borrow from Lean Engineering principles and ask, "What's the lightest weight, rapid prototype we could set in motion to learn quickly about the system and inform the next steps?"

As we move forward, I hope we can also acknowledge the emotional burden many of us carry. As ecologists and natural resource managers, most of us have been aware of the changes underway for much longer than the general public. That knowledge has weighed heavily on me and many of my colleagues, but it hasn't always spurred us to action. Now, we are joined by a global community, united in fear and sorrow for the global transformations underway. How we support each other in grappling with the inevitable grief, resistance, and fatigue will be critical to our success in confronting the crisis.

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Appendix F: Dr. Melanie Harsch Comments

The Delta Independent Science Board outlines in *Toward a Preemptive Ecology for Rapid, Global, and Increasingly Irreversible Environmental Change* the scientific basis of climate change and the need for preemptive planning. The challenge faced by the Delta Stewardship Council is how to plan for the impacts of climate change to preserve ecosystem function and the intrinsic value of the region. This challenge is not unique to the Sacramento-San Joaquin Delta; climate actions plans are being developed globally. The effectiveness of preemptive planning in climate action plans is limited by the availability of relevant and usable science. The science that builds the foundation of management plans cannot, as is, keep pace with understanding the implications of observed ecological change. In addition, the primary form of scientific communication, peer-reviewed journal articles, is not ready accessible or useable by most decision makers. Addressing the research framework for gaining knowledge on the impacts of climate change and how science is translated into usable formats for decisions makers are necessary steps for preemptive management in a changing climate.

Earth's climate is changing. Nonsynchronous responses by species to observed climatic changes reflect meaning and useful deviations from expectations not evidence that climatic change will not have limited effect on ecosystems and species. Species' responses to climate change vary by position in geographic range (Harsch & HilleRisLambers 2016), are impacted by multiple climatic factors that may, at times, be conflicting (Harsch & HilleRisLambers 2016), are limited by biotic interactions (Ettinger and HilleRisLambers 2017), past human actions (Mantyka-Pringle et al. 2012), or by life history and physiology (Gimona et al. 2015). Any of these factors may delay or inhibit a species from responding to climate change in the expected manner. In addition, current species' responses to observed climatic changes may not be indicative of future responses or species ability to persist if the velocity of climate change continues to increase (CITE). Such knowledge should not dismiss the reality of climate change but should be used to inform management actions.

The challenge with planning for and mitigating the impacts of climate change is understanding the complex biological end environmental interactions that govern whether a species will shift their distribution or phenology, acclimate or adapt to changes, or go extinct (Aitken et al. 2008; Cleland et al. 2012; Valladares et al. 2014). Planning for the impacts of climate change cannot wait as doing so may result in irreversible impacts that are environmental and fiscally costly, incur high damage costs, and result in damage to infrastructure and functioning (Ackerman & Stanton 2008). Management plans must provide means to increase natural resistance, build resistance, and proactive steps to facilitate responses that enable, at the least, ecosystem functions to persist. What remains is to identify management measures that provide scope for adaptation to future climate conditions and ensure that diverse perspectives are considered when assessing risks, impacts and tradeoffs. The latter relies on both understanding of biological trajectories of change as well as understanding and considering social, cultural and economic implications and scope for adaptation in the intricately coupled social-ecological Delta ecosystem

Planning for rapid environmental change requires, at a minimum, understanding how climatic patterns will change, how species will respond, and how interactions between species and the environment will modify expected responses. Understanding these complex interactions takes time, especially when research is based on observational studies. This means that decisions makers are frequently reactive rather than proactive and opportunities to mitigate costs are lost.

For science to effectively inform management plans, a research framework is needed that unites ecological theory with data-centered modeling and conservation decision support that can be developed at the rate required by decision makers. For a framework to be useful in developing preemptive management plans in light rapid ecological change it must utilize species and environmental data that is locally relevant (dispersal distance, drought tolerance, precipitation, water holding capacity, etc.) and can be collected quickly, such as over a field season. The framework be flexible. Flexibility is required because of the human and temporal cost in developing a framework. Ability to quickly adapt the framework to location, species, or new application minimizes costs required to develop and share. Finally, the framework must allow for identifying novel, often unexpected, outcomes that can be tested. Building a framework that allows for projections is not sufficient. Preemptive planning requires planning for the unknown and not just the observed. A framework that allows for the unknown and generates testable hypotheses can identify mechanisms leading to irreversible changes (Chen et al. 2015).

Applied mathematical models are, arguably, the quantitative method that best meets these requirements. Mathematical models are built upon decades of ecological theory and reflect observed biological dynamics such as dispersal, life history, and biotic interactions (Chen et al. 2015; Harsch et al. 2014; Servedio et al. 2014). Because they use equations parameterized by data they allow for exploration of novel dynamics and can generate testable hypotheses of mechanisms. Numerous models exist, each with their merits and limitations. The key attribute is that they can be parameterized with real data that can be collected over short time periods and that because they use real data and established equations describing ecological phenomena, they provide a useful tool to assess species response to climate change, especially to unforeseen changes, such as mechanisms leading to tipping points.

Harsch Comments

The Moving Habitat Model (Harsch et al. 2014; Harsch et al. 2017) is an example of extending a mathematical model developed for one purpose (species invasion) to evaluate the effect of species habitat movement due to climate change. The base of the model is an integrodifference equation. Data from real species is included in the dispersal parameter and life history parameters. Climate change is incorporated as rate that the habitable space along which the species occurs moves at each time step. There are a number of key elements to this model. First, most required data, such as dispersal, can be collected over short time periods (a season) or obtained in the literature. Second, the model is transferable. The model can be adjusted to other species and areas by adjusting the parameters. Third, the model is highly flexible. The Moving Habitat Model has been modified to include stochasticity in the rate that the habitat moves at each time step, biotic interactions, and inclusion of range effects. It is possible to extend the model to consider other elements, such as phenological changes. The model provides testable hypotheses, several of the testable hypotheses have been published in peer reviewed literature (Ford & HilleRisLamber 2020). The rate that the habitable space shifts, due to climate change, can be increased to assess the impact of as yet unseen rates of climate change.

Limitations exist in utilizing mathematical models as a framework. Developing mathematical models requires training in mathematics and time to develop novel methods. Overcoming the training and time hurdle requires interdisciplinary coordination and collaboration by a wide range of disciplines, such as ecologists, mathematicians, physical environmental scientists, and the decision makers. Bringing together experts ensures that the critical interacting pieces affecting species persistence and ecosystem functioning are included at the onset to build an informative and relevant model that can adapt with changing climatic conditions and knowledge.

Developing the methodology and tools to effectively inform adaptive management plans and address rapid ecology will require significant coordinated efforts. Historically, workshops sponsored by organizations such as NCEAS, NIMBIOS, and BANFF, to name a few, have provided opportunities to advance ecology as a field by focusing on timely topics such as historical data or developing models to address complex and timely ecological problems. Workshops should be organized that focus on developing the field of rapid ecology and preemptive management. Specifically, interdisciplinary workshops should focus on developing mathematical model frameworks and effective ways to communicate the model framework to decision makers.

The development of a framework to address ecological changes on a rapid time frame requires more than just a flexible, theory and a data informed quantitative method. Rather, it requires a change in the mental framework that science is conducted. Science, to date, has largely focused on basic, curiosity-driven research with the primary end product of the research being peer-reviewed publications. Unfortunately, many decision makers lack the time and access to incorporate peer-reviewed papers (Bainbridge 2014; Cook *et al*. 2013). In addition, results in journal articles are often presented at scales not relevant to decision makers (Roux et al. 2006; Cumming et al. 2006).

Bridging the disconnect between the goals and needs of institutional researchers and decision makers is a necessary step towards developing a framework to inform ecological research in a rapidly changing environment. Institutional demands currently limit the ease in which cross-communication can occur as sustaining translational interactions involves extensive social interactions which occur substantial costs in time and energy (Jacobs et al. 2010). The cost of social interactions on translating scientific results into useable formats is a realistic cost that must be addressed through changes in professional incentives and performance measures (Hallett et al. 2017; Ball 2016). Incentivizing translational ecology is worthwhile as when research is presented in more accessible forms, it is readily incorporated into management decisions (Walsh et al. 2015).

Translating research requires utilizing tools to effectively communicate results with decision makers. There are a number of ways to translate scientific research into useable formats, including making code for models easily available through website or web-based clearing houses, synthetic articles and fact sheets in formats easily found and downloaded from websites; policy briefs and short white papers written for the public and policy makers, web-based decision-support tools, web-based collections of case studies and analyses of effectiveness of management plans, and continual communication. The case for publishing computational code and developing web tools requires further details.

Computational advances have led to more ecologically relevant analyses and models but at the cost of simplicity and ease in uptake. These models often require complex statistical methods that are in a rapid state of development. Developing these potentially highly useful models from descriptions in methods sections of journal articles is beyond the purview and time of most decision makers. Publishing model code allows decision makers to adapt the code for their own area or species of concern. Publishing code helps avoid generating errors when trying to interpret vague or jargon-laden methods. Preparing code for publication requires documenting the purpose of the code, who wrote the code, and should include comments on the function code sections. This documentation requires minimal additional effort beyond what is required for archiving code for personal use.

Web-based interactive tools provide many beneficial outcomes. First, they a tool that allows decision makers to readily access results and models at a scale relevant to them. They can facilitate greater public engagement and provide greater transparency in decision making. Web-based applications allow other scientist, decision makers, and the public to explore models without having a deep understanding of the model or knowledge of the coding language. For example, the [Moving Habitat Model] outlined above was developed into a web application. Users define critical parameters, such as mean dispersal distance and the rate that the climate moves at each time step, that are relevant to the species and area they are interested in (Figure F-1). The mathematical model is run in the background and the output presented on the screen.

Figure F-1: Example of output from the web-based application of the Moving Habitat Model.

The power of web based applications is that they can take complex models and make them quickly and easily relevant to decision makers. In addition, it is possible to make web applications so users can upload their own data and analyses can be done on that data. In contrast to desktop application, web applications function across platforms, do not require installing additional software, do not require learning a coding language (at least for the end user), and can be free. The ease of developing web applications has changed with the development of the Shiny R package. This package wraps JavaScript, HTML, and CSS so developers only need to know R. Because R is a popular computer programing language that many scientists regularly use for data analysis, Shiny enables a relatively straightforward transition from standard scientific workflows involving data analysis, modeling, and visualization to the prototyping of decision-support tools. There is, of course, a learning curve but developing web applications is an extremely effective tool for making science relevant and useful for broad audiences.

The challenges facing decision makers in planning for the impacts of rapid ecological change are not minimal. Focused, collaborative workshops are needed to develop flexible models that incorporate ecological theory with data and allow for identifying potential unforeseen impacts and their triggers, and developing plans to mitigate impacts of climate change. Translating these models into easily accessible and understandable tools, such as web-based applications, is a necessary step to inform adaptive management plans and address rapid ecological change.

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Appendix G: Dr. Erika Zavaleta Comments

The world is in a period of rapid, accelerating environmental change. Environmental scientists should not be preparing for it, because it is already here. We should be responding to it now - with adaptive approaches to coupled action-research, and with systems tailored to early detection and rapid response. Scientists cannot respond to these challenges alone, because research is a means to an end. Science needs to directly serve the needs of decision-makers, communities and practitioners. To do so, it needs continual engagement (Reid et al. 2016) with those entities.

That continual engagement is hampered at present by institutions that separate science from practice, reward science for its own sake, and avoid confronting tough solutions by “studying it some more.” In particular, the emphasis on needing to know more before taking action is often overstated. We often know what needs to be done.[7] Fish need more water; forests need clear understories; sage grouse need more habitat. The trouble is that political, legal and social demands constrain our ability to provide it.

The unstated problem

Undoubtedly, environmental science could better support pre-emptive management in a number of ways, a few of which I discuss below. The need for data-intensive models to support pre-emptive management, though, is not a given. Rather, it stems from societal pressure to push biophysical systems to their limits to maximize short-term utility. When fisheries scientists run increasingly data-hungry models incorporating demography, climate, oceanography, illicit harvest, and multiple dimensions of stochasticity and uncertainty into identifying maximum sustainable yield limits (Bjorkstedt et al. 2016), they do so under pressure to identify with precision a level that maximizes harvest while keeping risk of harm (or of more severe future limits on harvest) acceptably low.

[7] From the original paper, I’m not sure I agree that “… it is difficult to assess which ecological factors can be managed or which stresses mitigated or ameliorated under rapid, global, and irreversible change.” We might not need to extensively study every species to understand, for instance, what stresses can be mitigated to provide capacity for adaptive persistence. I am also not sure that developing future scenarios requires knowing; scenarios are tools to select interventions that are robust to this uncertainty.

"Safe" harvest levels could be identified with far less data and precision by developing models with much larger uncertainty and keeping harvests lower.[8] However, society rejects a cautionary approach like this in favor of pushing for tighter bounds around a higher "safe" exploitation level.

The problem we face arises more from this societal mandate than from the sheer biophysical pressure of today's climate and atmospheric changes. To be sure, there are absolute limits to nature's capacity to adapt to rapid change. If sea level rise eliminates fresh water from an atoll harboring a freshwater endemic, we would have to move the species (elsewhere, or into tanks) for it to persist, regardless of efforts to mitigate additional stressors. I wonder, though, how often mitigation of additional stressors would suffice to safeguard a species threatened by directional climate change and its impacts. I suspect that more often than not, it would. But we do not live in a world where we are free to reduce other stressors. Farmers want water, and people and the pools and fluxes of their daily lives are now parts of the systems we seek to sustain.

On the other end of the spectrum, I wonder whether aiming scientific effort at narrowing the bounds of precision around maximized targets such as fish harvest or water withdrawals is a reasonable expectation. How narrowly are the bounds of uncertainty constrained around other factors that potentially affect, say, farm profits? Prices, tariffs, weather, and so on have impacts on them at least comparable to water deliveries. All are unpredictable to varying degrees. Environmental science might ask: how are we doing relative to the ability of economists, meteorologists and political scientists to predict important parameters in complex, uncertain systems? Are we being unrealistic in pushing for narrower uncertainty? Or is the needed environmental science today the one that says, "We cannot predict what's going to happen here, only define plausible scenarios. The course of action chosen must be robust to all those plausible scenarios."

We often know what needs to be done to remain robust to the plausibilities. The problem is that permission to do it is attached to a high standard of evidence and a precise characterization of risk or uncertainty. A different route could be to pursue alternative legal and governance approaches that shift focus from discrete resources

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[8] An analogy: If I had a car without a gas gauge, I would have a range of options to avoid running out of gas. At one extreme, I could develop a complex predictive model of gas consumption and keep track of speed, tire pressure, road surfaces, slopes, mileage, temperature, acceleration rates, etc. and take my tank down near zero before refilling. (As long as nothing unexpected happened like a traffic jam.) At the other extreme, I could skip all data collection and err on the safe side, heading to the pump very frequently. The best solution would be somewhere in between, with data on the size of my tank, miles traveled, and some conservative estimate of fuel efficiency. Fewer trips to the pump are better, but at what cost?

(water, timber, threatened populations) towards whole systems. For example, granting legal standing to places - whole ecosystems, as New Zealand, Ohio, and India have done lately for various bodies of water such as the Ganges River and Lake Erie - could shift focus from precise prediction of individual parts to overall ecological resilience and integrity.[9]

So what do we do?

Having said all of this, ecology and environmental science need to forge ahead in the context of existing legal and societal constraints. We can work with other disciplines and sectors to change the constraints, but ecological systems will not wait for that. Rapid, directional, global changes require adjustments to how we set goals, approach and integrate the systems we seek to change, and focus research from describing dynamics to understanding interventions.

Set goals

Clarity about what we are striving for in ecological systems should guide priorities for research. Much has been written about appropriate goals at local, regional and global scales in the context of directional environmental change (e.g. Cole & Yung 2012, Heller & Zavaleta 2009, Hobbs et al. 2017). Generally, recommendations include sustaining species with flexibility around location and emphasis on larger spatial scales; and targeting broad functional attributes at both local and larger scales. The latter attributes can include desired functions such as, in forests, the provision of wildfire containment, habitat diversity, and hydrological buffering. These in turn suggest desired structural features, such as a landscape mosaic of different-age stands. Targeted attributes can also include features associated with adaptive capacity (ecological and evolutionary) to cope with accelerating change. These attributes can include elements like genetic diversity and connectivity, looking to historical levels as baselines not of details like bill dimensions or distribution but of adaptive population, community and landscape processes (Zavaleta & Chapin 2010, Chapin et al. 2010).

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[9] Environmental personhood confers the rights of individuals (such as basic rights to life and health) to environmental entities. See for example: "What Is the Lake Erie Bill of Rights?" Retrieved 2020-1-22;Te Anga Pūtakerongo - Record of Understanding, 5.5.2 Retrieved 2020-1-22; Roy, Eleanor Ainge (2017-03-16)."New Zealand river granted same legal rights as human being". The Guardian. ISSN 0261-3077; Safi, Michael (2017-03-21)."Ganges and Yamuna rivers granted same legal rights as human beings". The Guardian. ISSN 0261-3077.

Several concepts such as resilience exist to capture the nature of management targets that are “flexible within bounds.” “Ecological integrity,” used by Parks Canada (Woodley & Kay 1993, Wurtzebach et al. 2016, Cole et al. 2008, Hobbs et al. 2010), provides an alternative or complement to the goal of resilience by defining more specifically, but from an ecological systems perspective, what processes and structural features characterize a system or landscape with this quality. All of these concepts share a shift away from trying to keep things the same (e.g. in terms of composition, structure and state) towards maintaining functions and processes within bounds. Accordingly, they share emphasis on monitoring and understanding functions rather than composition in order to manage systems.

A danger of this focus shift is that we lose attention to biodiversity and the conservation of unique taxa. Most change is reversible on time scales relevant to humanity - thousands of years or less. But extinction cannot be reversed (theoretical ideas about genetic resurrection of whole populations notwithstanding). We should be able to agree on species persistence as a goal alongside functions, structural diversity, and attributes like resilience or adaptive capacity. Species, however, might need to be conserved at very different spatial scales than we are accustomed to - with assisted movement and in new associations.

Though the concept of transformation is not new (e.g. Pelling 2010), the idea of transformation ecology has not been written about yet to my knowledge.[10] It is somewhat a matter of degree to distinguish between resilience and transformation: resilience is change within bounds, emphasis on the bounds; transformation is change within bounds, emphasis on the change. Transformation ecology would focus on understanding how to facilitate change through technical approaches (e.g. managed relocation, managed retreat) and systems approaches (changes in political and social systems that increase agility and responsiveness, e.g. by tightening information feedback loops). Given the new need to conserve biodiversity on the move and processes as communities continually change, there could be value in defining core questions and needs for this new domain.

Integrate systems

New conceptual, modeling and management approaches can help us tackle the scale challenges brought about by the need to manage moving species and whole regions in the context of global changes. Complex systems thinking has much to offer at the level

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[10] Though I discussed it in Zavaleta, E. (2009). Adapting conservation to climate change. Keynote presentation, The Nature Conservancy’s system-wide Climate Adaptation Clinic, Salt Lake City, UT, Sept. 1 to 3.

of characterizing the systems we intend to manage or change (Meadows 2008). Nested system models can not only integrate ecological, social and other subsystems but also bring broader, external factors into explicit ecological thinking (e.g. Chapin et al. 2006). Explicit discussion of desired system qualities could also guide formulation of models that emphasize the right parameters and metrics or that emphasize structural system improvements rather than ever-greater system description.

Describe and build systems with the qualities we want

Toward a Preemptive Ecology ends with: “The new challenge is to foresee change and be able to respond preemptively.” To the extent that competing societal goals make it practical, a complementary approach is to *assume* change and develop buffered, adaptive systems robust to a range of potential changes. A pre-emptively expanded characteristic of the managed system could be that it is more resilient to change or to surprises (Chapin et al. 2010). The specific nature of this resilience could be, for instance, that the system avoids extreme low outliers in water flows. This might come at some cost to mean annual flows, while reducing risk of dips below a critical threshold.

The world has examples of evolved systems that trade off, in similar fashion, higher yearly means for lowered risk of critical outlier years. In Ethiopia’s Gamo Gofa highlands, traditional agricultural systems are highly diversified, spatially dispersed, low-input, and structured to tightly cycle nutrients among sub-systems (Samberg et al. 2013). Average yields are low, but variability is also low in response to unpredictable external forces (such as weather, illness and trade variability). Average yield may be a fine measure of production if in bad years you have some backup system to buffer shortages. Agricultural extension efforts in poor countries sometimes miss that these backups, such as government food programs, do not exist. Extension efforts may replace diverse systems that have low productivity and variability with “modern” systems that have high productivity but much higher variability. This is out of a failure to realize that catastrophic hunger is a function not of low means, but of low outliers. Similarly, if we want to avoid catastrophic consequences in systems like the Delta that need only crash once to suffer irreversible losses, our spotlight should be on avoiding outlier lows - such as by reducing sensitivity to uncertain forcing factors. One direction is to look at very old management systems that emphasize resilience to variability over short-term utility maximization and to ask, what qualities do these systems share?

Integrate management across system components

Toward a Preemptive Ecology also characterized water management as simpler and better positioned to respond to rapid change than species or Delta management. This highlights the problem with balkanized ecosystem management. Water planning is about managing ecosystems. Species conservation is also about managing ecosystems. If they are not integrated processes, treated as parts of one system, they easily can end up working against each other. The situation is riskier for species and ecological communities because they are lower societal priorities, even if laws require their protection. We will be allowed to say: “Climate change was just too much. This species went extinct despite our best efforts.” But those “best efforts” likely were deeply constrained by other societal priorities. So integrated planning that requires multiple objectives to be met is critical.

Integrate science and practice

A critical element of translational ecology is the continual engagement (Reid et al. 2016) of scientists with managers, decision makers and community makers in reciprocal dialogue. Ecologists’ framing of the foundations of translational ecology emphasize this as a replacement for conveyor-belt and trickle-down models of communication (Enquist et al. 2017). Reid et al. (2016) provide a similar taxonomy of conceptual models for engagement among scientists, practitioners and communities. Their case study of continual engagement is valuable because it provides graphical models of these communication approaches and addresses how boundary-spanning looks at nested local, national and global scales. They also address important issues around power, trust and sustainability that need more attention for strong science-practice partnerships to persist. Participatory action-research (Kemmis & McTaggart 2005) also emphasizes the nature of these relationships and the roles they play in shaping research agendas as well as the legitimacy of research findings to practitioners and communities.

Representation is poorly addressed in translational ecology and in ecology in general, with implications for research agendas and the legitimacy of our field to much of society. It is a fraught, but real, issue to have society poorly represented among the scientists deciding what research to pursue and defining boundary-spanning approaches. This issue is better studied in biomedical science, which acknowledges how societal representation among researchers and medical practitioners helps link their work to societal need. For example, African-American biomedical scientists are much more likely than their counterparts to propose problem-solving research themes addressing societal issues like health care, socioeconomic barriers and risk (Hoppe et al. 2019). In ecology, low diversity likely also constrains problem definition and solutions. It likely constrains public trust and value of our field and what it has to say about the relationships of nature to people. An effective, preemptive ecology needs to better include and represent society.

Tighten feedback loops

Another area for development in our field is around building systems for early detection of new changes, and then for rapid communication of and response to these changes. We are very focused on technologies for collecting and processing information, but less on systems for disseminating and responding to it. Tight feedbacks from detected changes to needed rapid research could guide acquisition of specific knowledge to guide responses, in an iterative cycle. Preemptive management of emerging ecological challenges and changes could look to public health systems built to respond to new epidemics. For example, the World Health Organization (WHO 2016) maintains and deploys, as needed, a multi-part system to respond to and contain Ebola outbreaks. When the disease first emerged, this Ebola system was erected rapidly as part of the WHO's broader rapid outbreak response system (WHO 2020).

Build the science of what works

Documenting change - even understanding what ecological responses to it will be - is not the same thing as understanding which interventions work in response to change. Some researchers have shifted to treating ecological and social systems as integrated. Most however, focus on study of the integrated system in which (exogenous) management might intervene. We should expand the system of interest to include -- and focus strongly on -- potential, pilot, or past management interventions. The question to ask, relentlessly, in the study of this enlarged system is: *does the intervention work*?

The fields of agronomy, public health and development economics are built around this question to a greater degree than anything in the environmental or conservation space (Pullin & Knight 2001). Similar ideas have been developed for years, such as evidence-based conservation (Sutherland 2003) and decision support for environmental management (Dicks et al. 2014). However, they have not met the need for tailored research on key interventions and their effectiveness at achieving specific goals. We need a highly-valued community of practice that focuses on the science of interventions, like the Jameel Poverty Action Lab (JPAL)[11] -- which is highly-enough valued in economics that its founders won the Nobel Prize.

Focus on a science of interventions could yield a somewhat different line of questioning for research, such as:

1. What changes could reduce stresses on the focal system? Changes in agricultural water use? Changes in vegetable consumer behavior? Changes in industrial or

[11] Poverty Action Lab. Retrieved 1-22-20.

residential water use? Use or release of pollutants? Invasive species control measures?

2. What is known about how to achieve these changes? What interventions have successfully influenced these or similar changes, elsewhere or here?

3. How can we adapt those interventions to this context? How much change can we expect to achieve? How does that compare to the amount of change desired? (See for example JPAL's evaluations of interventions in Energy, Environment and Climate Change.[12])

4. When implementing the designed pilot intervention, how will we rigorously study it to quantify its impact on the central goal of the intervention? (Study of impact on anything else is fine in addition, but cannot substitute for measures of impact on the main goal.)

5. If no amount of alleviation of existing stresses will protect the targets in question, such as because even a pristine system with large, diverse, adaptable populations of key species is potentially going to change too much, too fast for them to persist there, what are other options?[13]

In summary, environmental science must rise to meet the challenge of accelerating, directional global change. The biggest challenges it faces have to do with the need to integrate efforts more strongly -- at larger spatial scales, with practitioners and more of society, and with legal and policy efforts that support rapid response and coordination of effort across previously separate domains. Fields outside of ecology and environmental science, ranging from systems thinking to public health and development economics, provide useful examples of what we could do. Stronger public and policy support for the value of sustaining healthy, functioning ecological systems would bolster our ability to build comparable systems in our field.

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[13] Such as managed relocation, engineering to resist change, or ex-situ conservation.

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