Preface

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The centenary of the Ecological Society of America inspired us to ask ecologists their thoughts about the next century, specifically on the broad question of "What are the Unsolved Problems in Ecology?" We imagined that they might identify two classes of problems: (1) Those people have wrestled with, but where solutions have remained elusive and (2) problems that someone may have just recognized as being potentially huge yet unexamined. The motivation for the book stems from a deep conviction that ecology will be a central defining science of the twenty-first century, just as physics defined the twentieth, and chemistry the nineteeth. Consequently, we put our authors in the position of defining what they think the key agenda for ecology will be within their area of research for the next decades to a full century. Sutherland et al. (2013) honored the centenary of the British Ecological Society by compiling a list of key unanswered questions—in effect, a series of bullet points aiming at future progress in the discipline. We, instead, asked authors to provide a more discursive reflection on open, important questions in the form of essays, providing a more expansive vista across possible future intellectual landscapes.

A strong motivation for the book was a previous volume of essays published in the 1970s that simply asked "What are the unsolved problems for the 20th Century" (Duncan and Weston-Smith 1977); there were only two biological chapters, including one by John Maynard Smith, who astutely pointed out that we did not know why sex had evolved. Curious as it seems, no one had explicitly realized that this was a problem prior to Maynard Smith's explication of the inherent "cost of sex" (1971, 1977; see Bell 1982); although Darwin as early as 1862 presciently remarked "we do not even in the least know why new beings should be produced by the union of two sexual elements, instead of by . . . parthenogenesis" (cited in Kirk 2006), and Bonner (1958) and others do adumbrate some aspects of the issue. This book chapter helped spark the genesis of a whole subdiscipline of studies within evolution, behavioral ecology, and epidemiology. We are ambitious enough to hope that at least one of our chapters in this volume can likewise unearth an intellectual goldmine that transforms

thinking within ecology and the broader disciplines of evolution and environmental science. The other biological essay in the 1977 compilation was by Peter Grubb, who pointed out that our knowledge of leaf structure and function at the time was woefully inadequate. This chapter also led to multiple developments in plant physiology and ecology. We were delighted when Peter accepted our invitation to write a chapter for the current book, and doubly so, when he decided to write a chapter that describes how much we still need to know about leaf structure and function, some four decades after his initial distillation of this question.

Some unsolved questions that the authors in this volume bring up are radically new, but others are longstanding. Robert MacArthur towards the end of his life sketched an array of outstanding problems in ecology (MacArthur 1972), focused around the theme of species coexistence, many of which are still with us and touched on in the current volume, including for instance the need for network perspectives, and the importance of understanding "why are some species able to adjust niche widths rapidly when put in a new situation while others are rigid?"; the latter question foreshadowed current concerns with themes such as niche conservatism and evolutionary rescue. MacArthur argued for intellectual pluralism and suggested that ecologists needed to get beyond the biological sciences (including in particular, he notes, the earth sciences) to really come to grips with the issue of species coexistence. These insights resonate today.

We initially planned to obtain three temporal perspectives on the unsolved problems identified by the authors, corresponding roughly to different stages in the trajectories of careers. To this end, we split the set of authors we invited into three broad and overlapping categories: (1) We asked younger researchers whose careers are expanding rapidly as to what they see as the major conceptual challenges facing their research, (2) we asked midcareer scientists to describe what they plan to focus on as the major targets of opportunity in their own careers, and (3) we asked individuals who have helped to define the study of ecology over the last 30 to 50 years to describe the problems they have found intractable or continually challenging, given available techniques and methodology. The skeleton of this structure is faintly discernible within the chapters we received for the final volume, although we perceive two distortions, one of which can fairly readily be dealt with, the other of which presents a significant "unsolved problem in ecology." The first distortion is that we tended to ask people whom we knew personally to write chapters. Although we have all been active in the Ecological Society of America, the British Ecological Society, the American Society of Naturalists, and the Society for Conservation Biology (among others) for more than 30 years, we surely (if unconsciously) are biased in asking friends and colleagues, rather than a broader array of people we may have admired from a distance in these

and other ecological societies. This is partly because it is so much easier to pressure and cajole friends and colleagues to deliver manuscripts and forebear with us when the editorial process slows down.

We could have assembled and organized the book in different ways, for instance by soliciting chapters that focused on specific areas of current or historical controversy, or by dividing the contributions papers into those that focus on specific issues, versus more general scientific and societal problems. We again resisted this, partly because such approaches would reflect our own personal knowledge and biases. One notable feature of our collection of essays is that they are all are each by one to two authors. Yet many of the problems identified by these authors will require collaborative efforts among many scientists, and indeed the ecological literature is becoming dominated by multiauthor publications.

In past years, the National Center for Ecological Analysis and Synthesis (NCEAS) in Santa Barbara) was an excellent forum for pushing synthesis in ecological theory and practice. NCEAS supported working groups, postdoctoral fellows, and visiting scholars working on themes such as coexistence theory, ecological networks, and phylogenetic perspectives on community structure. After the ending of core National Science Foundation (NSF) funding, NCEAS metamorphosed into an entity more tightly focused on critical applied issues, such as global food systems sustainability, ocean health, conservation practice, and also providing a venue synthesis across the Long Term Ecological Research Network (LTER) network of sites. These are of course all very important issues to address, but this change in focus means that ecology does not have a think tank where groups of ecologists—often with dissenting opinions—can convene to identify common ground and hatch new perspectives on key conceptual problems in the ecological sciences. The traditional forums of symposia and talks at annual conferences do not at all fill this niche. It is too easy for opposing parties to posture and defend their position rather than work with each other cooperatively and constructively. The long time delays inherent in production allow differences of opinion to fester, slowing the development of new and vital knowledge. Current debates in the ecological literature, ranging from subtleties in coexistence theory, to articulating the biodiversity consequences of habitat fragmentation, to the dilution versus amplification effects in host–parasite ecology, to priority effects and alternative stable states, could all benefit from the working group environment provided by the original avatar of NCEAS.

As pointed out by one of the referees of this volume, “Few if any significant debates in ecology have ever been resolved. People either die or get tired of arguing them. This is not a good thing!” This was much less of a problem when we had NCEAS as a facility to host discussions of areas of controversy which often led to an emerging consensus. The absence of

such a concrete center—a think tank for the basic ecological sciences—is in our view a major unsolved problem in ecology.

Just as each essay reflects the personal stance of the author, the selection we have ended up with reflects our own collective vision as to important directions for future research in ecology. A different set of editors might well have ended up with a different suite of unanswered questions in our discipline.

The second distortion is harder to deal with and reflects a broader problem in science. Our initial list of potential authors was well-balanced by subdiscipline and as well-balanced as we could between male and female authors. Most of the more senior women we approached felt themselves too overcommitted with other work to be able to contribute a chapter. Most expressed frustration at the limited amount of time they had available to write primary research papers or even grant proposals, given the heavy loads they experience in terms of being asked to participate in a broad range of administrative—but not directly scientific—tasks. This is a parlous state of affairs that excludes important and insightful voices from not just our compilation of thought pieces, but broader discussions of ecology and other academic disciplines. We hope this situation can be resolved over the next decade, as the different ecological societies, academic institutions, and funding agencies nurture and mentor the next generation of younger scholars. Nonetheless, it is a major unsolved problem that we still need to address with increasing vigor in both the scientific and policy arenas of ecology and the environmental sciences.

We hope the book will appeal to at least three different groups of ecologists: (1) Graduate students at early stages of their careers, who are looking for new and exciting areas in which to develop their research careers. (2) Established ecologists, who are thinking about different directions to take their research, or simply inquisitive about new ideas to include in their courses and symposia. (3) Historians of science who are interested in the forces that shape the development of new ideas within different scientific disciplines.

We thank the authors of each chapter for their contributions, particularly those who also acted as referees for chapters other than their own and provided insightful comments that further enhanced the quality of these chapters. We humbly also thank the two anonymous referees who read the complete volume for Princeton University Press. Their vital insights are reflected in the expanded title, in some of the threads of this introduction and in a modified organization of the order of the chapters. As is inevitable, the task took longer than we first assumed; grappling with the task of compiling and editing this lively set of essays greatly increased our respect for the editors, reviewers, and authors of the ecological journals that keep our discipline vibrant and rigorous. We finally thank every-

one involved for their patience, and hope that the final product matches our and your expectations. We have learned a lot and thoroughly enjoyed reading these contributions, and hope that you, and the broader readership of our community, may likewise profit from careful perusal of these essays.

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