Foreword

Ecology is not rocket science – it is far more difficult (Hilborn and Ludwig 1993). The most intellectually exciting ecological questions, and the ones most important to sustaining humans on the planet, address the dynamics of large, spatially heterogeneous systems over long periods of time. Moreover, the relevant systems are self-organizing, so simple notions of cause and effect do not apply (Levin 1998). Learning about such systems is among the hardest problems in science, and perhaps the most important problem for sustaining civilization. Ecologists have addressed this challenge by synthesis of information flowing from multiple sources or approaches (Pickett et al. 2007). Some major approaches in ecology are theoretical concepts expressed in models, long-term observations, comparisons across contrasting systems, and experiments (Carpenter 1998). These approaches have complementary strengths and limitations, so findings that are consistent among all of these approaches are likely to be most robust.

Ecosystem data are noisy. There are multiple sources of variability, such as external forcing, endogenous dynamics, and our imperfect observations. Thus it is not surprising that statistics have played a central role in ecological inference. However, with few exceptions the statistical approaches available to ecologists have been imported from other disciplines and were designed for problems that are simpler than the ones that ecologists face routinely. If you need to cut a board and all you have is a hammer, you might try pounding on the board until it breaks. Such a misapplication of force resembles some uses of statistics in ecology. But the metaphor is not quite right. It would be more accurate to say that ecosystem and landscape ecologists need to create and compare multifaceted models for large-scale processes, whereas the readily available tools were designed for testing null models that are usually trivial or irrelevant for this family of ecological questions.

The mismatch between the needs of scientists and the availability of statistical tools is acute in the analysis of ecosystem experiments. Ecosystem experiments have been an important contributor to ecological science for more than 50 years (Likens 1985, Carpenter et al. 1995). While humans have manipulated ecosystems since at least the beginnings of agriculture, if not longer, deliberate experiments for learning about ecosystems are traced to limnology in the 1940s (Likens 1985). The earliest whole-lake manipulations lacked reference systems, and so sometimes it was difficult to determine whether changes in the ecosystems were caused by the manipulations or by other environmental factors. In 1951, Arthur Hasler and his students divided an hour-glass shaped lake with an earthen dam, thereby creating two basins, Paul and Peter lakes. Peter Lake was manipulated, while Paul Lake served as an unmanipulated reference ecosystem (Johnson and Hasler 1954). The use of a reference or "control" ecosystem was a pathbreaking innovation (Likens 1985). It allowed Hasler and his students to separate the effects of the manipulations of Peter Lake from those of the environmental variability that affected both lakes (Stross and Hasler 1960, Stross et al. 1961). As a result of their experiences as students of Hasler, Gene Likens and Waldo Johnson were inspired to create two of the most influential centers of ecosystem experimentation in the world, the Hubbard Brook Ecosystem Study (Likens 2004) and the Experimental Lakes Area (Johnson and Vallentyne 1971).

Most ecosystem experiments involve spatially extensive systems (often observed at several spatial extents) over long time spans. Such experiments pose statistical challenges that cannot be handled by the methods of laboratory science or small agricultural plots (Carpenter 1998). It is not possible to substitute small-scale experiments run for short periods of time, because results of such experiments do not predict dynamics at spatial and temporal scales relevant to ecosystem science or to management (Carpenter 1996, Schindler 1998, Pace 2001). Instead, we must perform our studies at the appropriate scales – possibly at multiple scales. Then, we must learn how to learn from noisy observations of transient, heterogeneous, and non-replicable systems. This is a daunting challenge.

Thus many ecologists have broken free of the constraints of older statistical methods in order to explore new alternatives that seem better-adapted to the world of large-scale ecological change. The method of multiple working hypotheses (Chamberlain 1890) is now explicit in many ecological papers. Multiple hypotheses are expressed as quantitative models and confronted with data (Burnham and Anderson 1998, Hilborn and Mangel 1997). New approaches are explored for long-term monitoring data (Stow et al. 1998). Experiments are designed for critical tests of multiple alternative models to address fundamental questions about ecological dynamics (Dennis et al. 2001, Wootton 2004). Comparisons of multiple models are providing new insights about long-term field observations of big systems (Ives et al. 2008). These are but a few selections from a diverse and rapidly growing literature. This new phase of ecological research is turbulent and subject to rapid intellectual progress. Some of the emerging practices are nonstandard and are themselves objects of inquiry. Some approaches are tried, found wanting, and abandoned. New approaches are introduced frequently. It is an era of creativity, innovation, discarding of mistakes, and selection among alternatives – in a nutshell, a time of rapid evolution by the discipline.

The volume before you presents a sampling of case studies and syntheses from this fertile field of research. The authors and editors aim to improve our tools for ecological inference at scales that are relevant for fundamental understanding, as well as for management of ecosystems and landscapes. The book conveys the excitement and novelty of emerging approaches for learning about large-scale ecological changes.

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