Preface

This book has had three successively more proximate causes. In general I had become increasingly dissatisfied through the years with the logical empiricist analysis of science that had been so popular for over a generation. I did not find the "received view" so much mistaken as too far removed from the ongoing process of science. Too much of science was being left out. But I was no happier with the critics of the logical empiricist analysis of science. Some of these critics, operating within the logical empiricist tradition, presented convincing objections to the received view but offered nothing to put in its place. A second group of critics was bent on debunking science at all costs. Science was the enemy and had to be smashed for the "good of the people." Because science did not possess the ideal characteristics that the "positivists" insisted that it should, knowledge-claims made by scientists had no more warrant than those of magicians, faith healers, and politicians. All is one, and anything goes. Something had to exist, other than the rarefied abstractions of logical empiricist philosophers of science and the wild-eyed proclamations of the more radical critics, but what? That was the problem.

Then, in April 1973, Bert Rowell, the editor of *Systematic Zoology,* sent me a manuscript by Gareth Nelson to referee. In this manuscript, Nelson (1973c) complained of the way that the views of Leon Croizat had been treated through the years by such authorities as G. G. Simpson and Ernst Mayr. I decided that the sort of thing Nelson was investigating with respect to Croizat was the sort of thing I would like to do in philosophy of science. What is the relative importance in science of reason, argument, and evidence on the one hand, and power, prestige, and influence on the other? I thought that answers couched totally in terms of one sort of influence or the other were sure to be wrong and that the interplay between the two was likely to be fascinating. Because I had been involved in the systematics community, I decided to study the fate of a particular group of systematists—the pheneticists, or numerical taxonomists, at the University of Kansas in Lawrence, Kansas. To study these systematists, I had to interact with them—attend meetings, become involved in professional societies, referee papers, etc. While I was studying the pheneticists, I realized that Nelson, without intending to, had given rise to a new "school"—the cladists, or phylogeneticists. I decided to study them as well.

The scientists I have studied investigate such things as fruit flies, fossil fish, and slime molds. As diverse as these organisms are, they have one thing in common: they cannot

read the conclusions published about them. My subjects can. One of my goals in this book has been to present a fair and balanced estimate of the influence of various factors, including professional allegiances and alliances, on the course of systematics and evolutionary biology. I obviously have my biases and personal preferences. I also have my own professional goals. Perhaps there is no such thing as objectivity in the abstract, but at least there can be third-party objectivity. For most disputes, I have no strong preference for one position or the other; for some I do. When I do, I say so explicitly and try to present all sides as fairly as possible. One sign that I may have attained this elusive goal of fairness has been that members of all camps find that I have treated their enemies too gently.

I have not even tried to neutralize the effects of my professional goals. I could not, on pain of contradiction. I very much want others to accept my view of the factors that make science function as it does. I did not begin my research with the general view presented in this book fully worked out. As I studied science and scientists, I was forced to change my mind on a variety of topics, but I also did not proceed inductively. There was a complex interplay between my convictions and my experience, the sort of interplay that characterizes all scientific investigations. One conclusion I found inescapable is that the professional relations among scientists influence, at least locally, the content of science. As a result, I spend a good deal of time discussing these relations. I have tried not to indulge in gossip for the sake of gossip without omitting details that are relevant to understanding how science works. I suspect that I have erred on the side of conservatism rather than excess, but others will have to decide this. In consequence of my sustained professional and personal relations with many of the scientists whom I discuss in this book, I have acquired a responsibility to them. I hope I have not compromised it. If I am mistaken on this score, I have total confidence that my lapses will not go unmentioned.

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on, changes have occurred. The most grievous is the untimely death of Donn Rosen. We all have our own memories of our relationship with this complex and engaging man. I appreciate the time and effort that all of these readers generously contributed to making this book more accurate and well balanced.