been produced of any large, solitary-living cat and its principal prey.

Leopards are difficult to identify as individuals and, living as they do mostly in closed habitat and moving over many square kilometers of landscape in the course of the day and their lives, they are not easily amenable to detailed behavioral and ecological study. Few good studies were available prior to Bailey's work. Aided by radiotelemetry, Bailey was able to follow closely the leopards' movements and activities and to quantify the proximate conditions that influenced their behavior. The leopard has great ability to adapt to diverse environments—it has the largest geographical range of any mammal in the Old World—and a strength of this book is Bailey's identification of the leopard's behavioral and phenotypic adaptations to local conditions. I found his findings in regard to the dispersal system particularly insightful. Leopards, particularly subadults, regularly went on exploratory forays of up to 24 kilometers outside their normal home areas, presumably looking for better home areas. Knowledge of the dispersal system is important to understanding how a species will persist and spread or decline in the fragmented landscapes that are the leopard's range today. Bailey also addressed community- and ecosystem-level questions. For example, he concludes that leopard populations are "self"-regulating and that leopards do not "control" the numbers of impala (*Aepyceros melampus*), their most abundant ungulate prey, but may influence the numbers of less numerous species, such as steenbuck (*Raphicerus campestris*). History has taught, however, that a three-year study of a natural system of this size gives only a partial picture of how the system works over time.

Bailey concludes with a "synthesis." "The adaptable leopard" is a summary of behavioral and ecological adaptations to the diverse habitats the leopard inhabits through its vast range and a comparison of leopards with other large, solitary-living cats. In "The conservation of leopards," Bailey addresses factors affecting leopard numbers today, leopard status surveys, including those that have been highly controversial, and conservation strategies. He comes down on the side of tightly controlled trophy hunting as one means of making leopard conservation palatable to those who would otherwise simply poison or shoot leopards to be rid of them. His own data are from an unhunted population, and the numerical and behavioral response of a leopard population to the harvest regime he proposes remains untested. By the Adolf Murie standard and tradition of detailed descriptive studies of the large mammals living in national parks, Bailey has added a substantial and high-quality work.

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Saccharomyces and Company

The Early Days of Yeast Genetics. MICHAEL N. HALL and PATRICK LINDER, Eds. Cold Spring Harbor Laboratory Press, Cold Spring Harbor, NY, 1993. x, 477 pp., illus. $75.

The Early Days of Yeast Genetics is a work modeled on the very successful *Phage and the Origins of Molecular Biology* of 1966, also published by Cold Spring Harbor. *Phage and the Origins* was both a celebration of Max Delbrück's impact on the students of the phage school and a history of the science. Given a single person as a focus, the earlier work had a theme tying together the reminiscences of the essayists in a way that made the whole much more than the sum of the parts.

The Early Days of Yeast Genetics is by necessity a very different book because the field of yeast genetics has quite a different historical base from phage genetics. Like the phage volume, however, *Early Days* is a collection of essays by and about key figures in the development of the field. Some essays have the personal-reminiscence style of the earlier model, whereas others are much more focused on the science of their authors. The editing was light, allowing the styles of the contributors to show through, but at some cost in clarity.

The essays are grouped into eight sections: Beginnings; Recombination, Gene Conversion, Mutation, and Repair; Mitochondria and Cytoplasmic Inheritance; Mating; Cell Cycle; Gene Structure and Expression; Molecular Biology; and Institutions. All but two focus on *Saccharomyces cerevisiae*. Readers of the two essays on *Schizzosaccharomyces pombe*, by Urs Leupold and Murdoch Mitchison, will be amused that the remarkable development of *S. pombe* genetics came about as a result of a casual comment made by Øjvind Winge to Leupold.

From the standpoint of 1994, the reader will be struck both by the differences and by the similarities between the yeast genetics of yesteryear and today. Among the similarities are the accidental routes that have led so many of the major yeast geneticists to the subject. In several cases, an inspired teacher made a critical difference to a student, as reflected in the essays of Leupold, Donald Hawthorne, Elizabeth Jones, and Rochelle Esposito. Some came to yeast genetics as a result of being unable to grow corn in Seattle (Herschel Roman) or because their cornfield was converted to a parking lot (Seymour Fogel). Others made more deliberate decisions, choosing the expanded opportunities with a familiar technological base that yeast cells offered to former phage and bacterial workers (David Botstein, John Carbon, Benjamin Hall). A few conspired to convince anyone not working on yeast genetics to start (Fred Sherman, Gerald Fink). Among the differences, two stand out. First, the size of research groups was small by today's standards, with the norm being a few graduate students. Postdocs didn't figure prominently until the '70s. Second, people working on one aspect of yeast biology worked in isolation from those studying a different aspect. There was no common thread such as is provided today by DNA sequence matches. With the promise of complete genome sequence in the offing, the era when different biologists can study the same gene without realizing it will be gone. We will soon be able to learn immediately whether anyone else has stumbled into any particular corner of the genome by using the genome sequence as a bulletin board to report our own activities. This luxurious situation will give yeast geneticists the first chance to define how complete genome sequences can be used to improve existing experiments and open new possibilities. This stunning progress in the short time between the work of these essayists and today is ample evidence of the unpredictability of the course and extent of progress in science.

The two most revealing essays in the book, in my judgment, are the biographical sketches of Øjvind Winge and Carl Lindgren, both by Robert Mortimer. In his capacity as director of the physiology department at the Carlsberg Laboratory, Winge made many contributions to early yeast genetics, from working out the life cycle of yeasts and discovering the HO gene to characterizing the genes controlling the ability of yeasts to grow on different sugars. Winge's views on his work were remarkably modern and, although they preceded the molecular era, are completely congruent with everything that has come since. In contrast, Lindgren, a contemporary of Winge's, was clearly brilliant, yet for rea-

Boris Ephrussi and Herschel Roman, 13 rue Pierre Curie, 1953. [From *The Early Days of Yeast Genetics*]

sons hinted at in Mortimer's essay became entangled by hopelessly complicated explanations for his discoveries and never received the credit for his work, which inspired Beadle and Tatum and forced a number of labs to consider gene conversion as a real process. Lindgren is the tragic figure in these pages.

It is difficult today to appreciate the skepticism that non-Mendelian patterns of inheritance faced in the not-so-distant past. Essays by Ephrussi (an early reprint) and by Gottfried Schatz, Alexander Tzagoloff, Brian Cox, and David Wilkie do justice to the climate of intellectual resistance to both mitochondrial genetics and the underlying unity in the biochemistry of mitochondria from yeast to beef heart. Under the influence of Austrian charm and wine (Schatz), yeast mitochondrial DNA was discovered, making its genetics legitimate if initially confusing.

The pair of essays on yeast mating present an interesting contrast in approaches to genetic research. One style is represented in the essay by Vivian Mackay, in which she describes her pioneering work on the isolation of mutants affecting mating that has led to so many subsequent discoveries. Because mating is essential to genetic analysis with yeast, the idea of purposely isolating mutants that block the process as a first step in a genetic study must have seemed foolish to some, but the logic and luck that led to the success of this venture are described, if somewhat too briefly. The other style is revealed in the essay by Yasuji Oshima, whose lab's entire research program on mating-type switching was built on the study of natural variation between different strains. The fusion of these two approaches was most effective, and with molecular advances opening entire genomes to study we can hope that more labs will be inspired to explore the genetic basis of natural variation and benefit from, rather than be confused by, genetic differences between strains.

The introduction of molecular cloning was a watershed in yeast genetics. The essays by Benjamin Hall and John Carbon are unusually clear accounts of the early research achievements made through work with yeast, such as the proof that introns were in functional rather than in nonfunctional genes and the first description of eukaryotic centomeres and origins of replication. The essay by Botstein puts a more human face on the decision-making that led people from bacterial genetics to yeast, the trials and tribulations of the early molecular methods, and, remarkably, how the early aspirations were quickly outpaced by the reality.

Although yeast genetics has no Delbrück at its origin, the impact of Herschel Roman stands out in many of the essays. Roman had a strong influence on the origin of the yeast cell cycle studies as described by Leland Hartwell, on Rochelle Esposito's launching of her career on recombination and sporulation, and on Elizabeth Jones's studies on gene fine structure and the protease work to follow.

So what's missing from the book? It is a pity that the editors did not assemble a pedigree of who taught whom and when, such as is found in Sturtevant's *History of Genetics*. Some obvious potential contributors, among them Piotr Slonimski, Ronald Davis, A. W. Linnane, Julius Marmur, and Ira Herskowitz, are conspicuously absent, but not everyone can be convinced to contribute to such a volume in any field. The dearth of information on *S. pombe* is noticeable, but most of the *pombe* advances have been more recent than the time period covered in the book. The book contains information and perspectives that are hard to find elsewhere and therefore should be available to all labs whose research involves yeast. Although few graduate students will need a copy, most will enjoy access to one.

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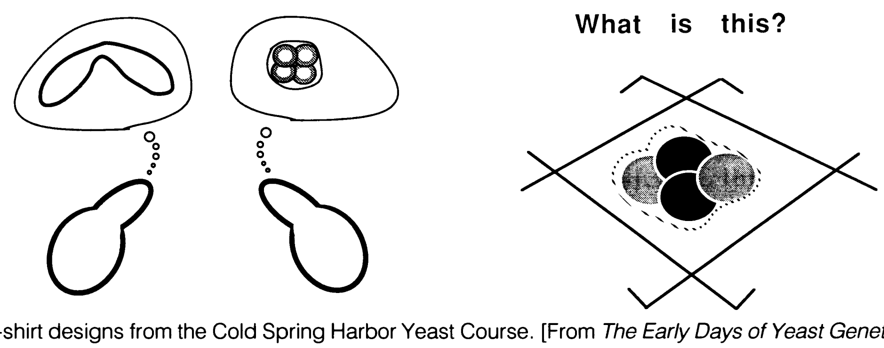

T-shirt designs from the Cold Spring Harbor Yeast Course. [From *The Early Days of Yeast Genetics*]

Evolution in the Fast Lane

The Evolutionary Biology of Viruses. STEPHEN S. MORSE, Ed. Raven, New York, 1993. xiv, 353 pp., illus. $69.

Nowhere in life are evolutionary processes as rapid, as easily observable, or as important for understanding short-term population dynamics as they are among the viruses. For instance, the current AIDS pandemic and the problems of combating its causes—the human immunodeficiency viruses (HIVs)—can only be understood in an evolutionary context. Not taking the evolution of HIVs into account would guarantee the failure of efforts to combat AIDS and in fact would likely lead to seriously counterproductive control measures. Understanding the biology of many other viruses critical to human welfare and basic research also requires a thorough knowledge of evolutionary processes, evolutionary theory, and evolutionary methods. At the same time, many viruses offer largely untapped experimental systems that are ideal for tests of evolutionary theory and methodology. It is both odd and unfortunate, then, that evolutionary biologists and virologists historically have had relatively little research overlap or scientific communication. Evolutionary biologists have been overwhelmingly eukaryocentric in their research programs, and only a few brave virologists have sought to apply or extend evolutionary principles to viral systems. The trend toward specialization and division of the biological sciences in academic environments has exacerbated the situation: evolutionary biologists and molecular