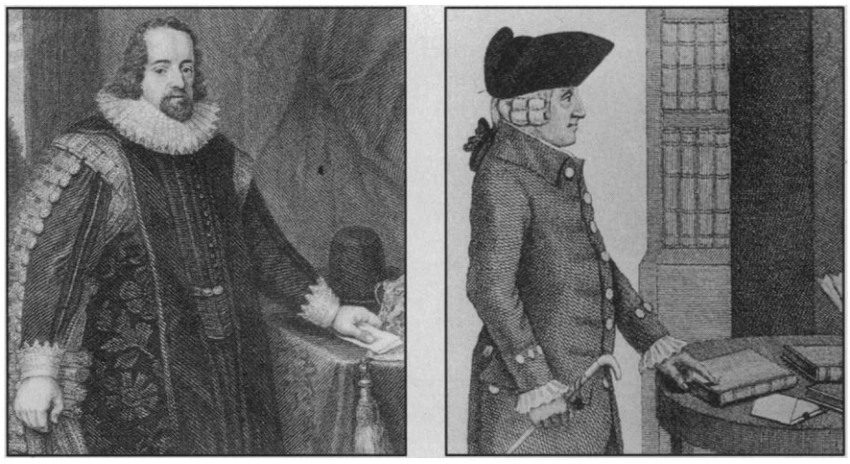
BOOK REVIEWS

Free-Marketing Science

The Economic Laws of Scientific Research. TERENCE KEALEY. St. Martin's, New York, 1997. xii, 382 pp. $75, ISBN 0-312-12847-9; paper, $19.95, ISBN 0-312-17306-7. Published in the UK by Macmillan. £47.50, ISBN 0-333-56045-0; paper, £16.99, ISBN 0-333-65755-1.

Oh my, what a book! In a *tour de force* whose interest and vitality are belied by a pedantic title, biochemist Terence Kealey has written a lively history and analysis of science funding that is an impassioned apology for capitalism and private as opposed to public patronage. This is a deeply conservative work whose principal *bête noire* is what the author sees as a bloated, state-subsidized science that threatens to undermine the habits that helped "the West grow rich" (to echo the title of a recent, and similar, book by Rosenberg and Birdzell). Kealey is opinionated, often historically glib, and at times seems mean-spirited (he would say "peevish"); but he is also smart, widely read, and profoundly pertinent. Many will be provoked by his book, especially those scientists and their advocates who have been made defensive by the down-sizing of federal science budgets, shifting national priorities, and cultural criticism of increasing variety. Nevertheless, the study is important and deserves to be read.



Francis Bacon (left) and Adam Smith (right). *The Economic Laws of Scientific Research* is, according to its author, "constructed as a debate" between these thinkers. "Who won? The world's answer, of course, is Francis Bacon: The governments of all industrialized countries now support their universities and their science much as Bacon prescribed. . . . But the world can be wrong. Indeed, it often is, particularly when it flouts Adam Smith." [Corbis-Bettman]

Kealey has many targets: the European Union, subsidized universities as nurseries of student revolution, hypocritical science bureaucrats, and Harold Wilson (whose science policies were, Kealey believes, economically disastrous). But chiefly Kealey assails two "myths" that he believes have become entrenched in contemporary policy. The first posits wealth as the consequence of technology, itself the product of academic science. On this view, by increasing the sponsorship of science, which only the state has the disinterestedness and comprehensive horizons to do responsibly, one guarantees the production of wealth and economic advance. The second myth assumes that public and private funding are additive and together increase the total pool of money for research. All wrong, Kealey argues, and he goes to great historical and statistical lengths to make the case, not without effect. Science *is* more often the consequence of technology than vice versa and the relationships between science and technology *are* more complex than the "linear model" suggests, as much work by historians of technology has demonstrated. Moreover, Kealey continues, public funding disproportionately displaces private funding, shrinks the R&D pool, and breeds ineffective science unfit for vigorous competition. The persistence of these myths, as he sees it, owes much to political bureaucracy, the exigencies of modern war, and a university/science lobby that has grown fat and happy supping at the public trough.

Kealey's claim that private patronage of science is more effective than public patronage hinges both on historical argument and, especially in key chapters, on a close, comparative analysis of quantitative indices of gross domestic product, funding for R&D, and scientific publication. The data he presents are too complex to be dealt with in a brief review, but those readers who, like the reviewer, trust statistics even less than the beneficence of the rich ("One of the great joys of capitalism," Kealey asserts, "is that it redistributes wealth in an egalitarian fashion") should pay close attention. This is, after all, history construed to prove a point. It is, indeed, Manichaean history, in which Adam Smith ("a good, as well as a great, man"), *laissez-faire*, capitalism, "hobby" science, and Margaret Thatcher are ranged on the side of light, and Francis Bacon (who "was not a nice man" and originated the "linear model"), *dirigisme*, Marx, and, of course, Harold Wilson on the side of darkness. Thus, the historical arguments often seem a bit Procrustean: Invention in the ancient world could only be the fruit of private enterprise and free markets (but what, one wonders, of the great "hydraulic civilizations" like China and Egypt?); thus the Roman Empire collapsed because it abandoned capitalism. Again, in Kealey's argument, the steam engine owed nothing to science (though highly philosophical discussions of the vacuum and the weight of the air in the previous century paved the way) and James Watt depended on none (even though he practiced, as a mathematical instrument-maker, the most scientific of all trades). And Darwin can be regarded as nothing but an independent-minded hobbyist only if one forgets his formative experiences as the official natural historian on one of the great state-sponsored scientific enterprises of the 19th century, the voyage of the *Beagle*.

Much of this analysis works only if one defines science, as Kealey does, in a narrow enough fashion: it would have come as a great surprise, for instance, to the industrialists who flocked to (and paid good money to attend!) public lectures on Newton and the mechanical philosophy that science had nothing to do with their projects. Likewise, Kealey treats the free market as an independent variable in his

economic "equations": governed only by Adam Smith's "invisible hand," *laissez-faire* will dependably produce wealth, philanthropic benefits, and the basic science that society needs as well as governments prosperous and powerful. But it's possible to tell the story differently. Another version would explain the West's dynamic capitalism as, at least in part, the result of the European discovery that guns and trade, business and politics, are a potent mix. In a world in which economies have expanded in the wake of Portuguese gunboats in the Age of Discovery and that has seen the establishment of global military hegemony after the Second World War, it might be questioned whether there is any such thing as the classically free market, or whether instead there are only degrees of managed economies and various arrangements between government and business. If this is the case, then Kealey's demonized account will do little to help us understand the ways in which the modern, ferociously war-minded world has blurred the boundaries between public and private and thus compromised the dynamics of scientific change.

Economic Laws is a great read. It's sure to provoke discomfort, if not anger, especially among those who find themselves skewered by Kealey's wit and arguments. But so what? One must be clear about the author's targets. Few, least of all the author, would deny that science is a powerful cultural good. But science policies that presuppose the dominance of public versus private initiative on the grounds of historical inevitability and the alleged disinterestedness of the public scientist are another matter. In fact, such policies constitute for the author yet another self-serving myth that masks the vested interests of a powerful elite grown strong in the wake of two world wars. In a world in which science has replaced religion as the most powerful of orthodoxies, a "Protestant Reformation" might be a healthy thing. And Kealey would surely enjoy being its Martin Luther.

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Speedbumps for Adaptationism

Adaptation. MICHAEL R. ROSE and GEORGE V. LAUDER, Eds. Academic Press, San Diego, 1996. xiv, 511 pp., illus. $69.95 or £52, ISBN 0-12-596420-x; paper, $34.95 or £24.95, ISBN 0-12-596421-8.

While the elucidation of adaptation in evolution has long been a main avenue of biological research, several ruts and potholes have become evident in the past 30 years. Chief among these is "The spandrels of San Marco and the Panglossian paradigm: a critique of the adaptationist programme" (*Proc. Roy. Soc. Lond. B* **205**, 581–98 [1979]), in which Stephen Gould and Richard Lewontin roundly castigated uncritical practices in adaptational studies, particularly the telling of "just so stories." "The spandrels" initiated an era of self-doubt and recrimination that had a chilling effect on adaptational biology. At the same time, however, it greatly enhanced receptivity to several positive developments that have smoothed the adaptationist road: new comparative methods rooted in phylogenetic systematics; an analytical framework that merges quantitative genetics, function, and natural history; and increasingly sophisticated study of ongoing adaptation in the laboratory, in nature, and in manipulated organisms and populations. Indeed, many of the criticisms made in "The spandrels" (directed, for example, at ignorance of history and disregard of constraint) have been coopted to generalize and improve the adaptationist program. The continuing fusion of molecular and evolutionary biology, moreover, should accelerate these developments.

With these welcome repairs, one looks for a corresponding road guide. The view from *Adaptation*, edited by Michael Rose and George Lauder, is bleak, however. The book is the first comprehensive examination of how adaptational biology is confronting the issues that "The spandrels" raised. It includes chapters on the major components of the field: argument from design, optimality models, quantitative and molecular genetic approaches, phenotypic manipulation, description of selection in the laboratory and field, phylogenetic systematics, and paleontology. Perhaps in continuing reaction to "The spandrels," most of the authors painstakingly emphasize the pitfalls of their approaches, the limitations of their assumptions, and the lacunae in our knowledge, and scrupulously avoid any sustained optimism. Michael Novacek concludes, for example, that paleontology provides no unique insights to the study of adaptation, and Rose's summary of the seminal advances yielded by studies of laboratory evolution (adaptation is a usual outcome of laboratory evolution, trade-offs sometimes occur, and different selection regimes yield different evolutionary outcomes) is damning with faint praise. Although Rose, Novacek, and the other contributors have provided affirmative and optimistic accounts of their approaches elsewhere, their effect here is to conclude that adaptation is just as problematic as it ever was, if not more so. They do this well!

Rose and Lauder encouraged their authors to present conflicting viewpoints. Not surprisingly for a field as heavily laden with jargon as evolutionary biology, the conflicts often concern terms and definitions. The authors seem to devote so much energy to defining what is or is not an adaptation that, to paraphrase contributor Geerat Vermeij, they overlook interesting biological phenomena for purely semantic reasons.

Not all in the book is pessimism, semantics, and faint praise. David Reznick and Joseph Travis, for example, provide an exciting account of studies in natural populations. The work they summarize furnishes abundant evidence that adaptation is ongoing in nature and amenable to analysis.

The book concludes with chapters on clade-level adaptation, subdivided populations, genomic parasites, and adaptive systems. Though these chapters are positive and interesting in their own right, collectively they make little contact with the treatments of organismal adaptation in the first part of the book. For that matter, *Adaptation* seldom advocates a multidisciplinary or pluralistic approach. Clearly, each approach represented in the book can contribute valuable insights, but none in itself suffices for understanding adaptation. The situation calls for a cogent articulation of how diverse approaches can be deployed in complementary fashion, rather than editorial isolationism.

Adaptation is a valuable and well-written cautionary work for those who would execute the adaptationist program. It culminates the current stage of post-"spandrels" adaptationism and, in assessing the state of the art, is likely to have a significant impact on the next generation of adaptational studies. This impact, however, may be achieved primarily by discouraging adaptational biologists rather than by presenting a vision of the next stage. Whether *Adaptation* is a milestone or a millstone, it shows clearly how "The spandrels," by raising the standard of proof, ultimately enhanced and energized the study of biological adaptation.

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