that the law of large numbers, sometimes augmented by topological considerations, may allow one to measure quantities such as the flux quantum *h*/*e* more accurately on many-body systems than we could on microscopic ones. All of the above are valid and important points, but all of them were being made in the literature long before emergence gained its current buzzword status—and by others besides Philip Anderson and Ilya Prigogine, whom Laughlin explicitly acknowledges as his intellectual precursors.

So where's the extra beef? What *is* emergence? For the first three-quarters of *A Different Universe* the reader has to live with the definition given in chapter 1: "a physical principle of organization." What exactly are these allegedly ubiquitous principles of organization? Let's consider one of Laughlin's showcase examples: superconductivity. In chapter 8, the organizational principle is explicitly identified as "superfluid symmetry breaking," and he puts great weight on the existence of multiple "equivalent solutions," by which he presumably means solutions corresponding to different overall condensate phases.

My problem with this point of view is that, for all its historical importance, "spontaneous breaking of gauge symmetry" and the consequent multiple solutions are quite simply myths. The Meissner and Josephson effects, which Laughlin identifies as the essence of superconductivity, can be derived just as simply, and with much less danger of generating pseudo-problems, by using a much more transparent postulate originally formulated explicitly by C. N. Yang—namely, that there occurs a sort of Bose–Einstein condensation (BEC) of Cooper pairs (or, more technically, that the two-particle reduced density matrix has a single macroscopic eigenvalue). The point becomes even more evident in the case of recently stabilized weakly repulsive Bose alkali gases, in which the analogs of the Meissner and Josephson effects are manifestly just single-particle effects amplified by BEC.

So what are we to conclude, that superconductivity/superfluidity is an emergent phenomenon in the case of superconductors but not in the case of the alkali gas? Or that BEC is itself an example of emergence? If the latter, then presumably, a fortiori, just about any application of the central limit theorem in physics will qualify. Where has the "organizing principle" gone?

The example of superconductivity seems to me to epitomize the central weakness, at least in a condensed matter context, of Laughlin's thesis. The concept of emergence, touted as a revolutionary new paradigm, turns out at the end of the day to be little more than a catchall label for a miscellaneous collection of things we all understood perfectly well already. As an explanatory or even heuristic principle in its own right, "emergence" is completely vacuous.

Still, we all had a lot of fun along the way, so maybe we shouldn't complain.

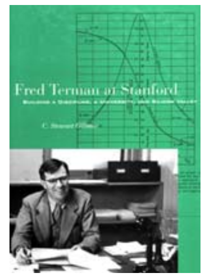
Fred Terman at Stanford: Building a Discipline, a University, and Silicon Valley

C. Stewart Gillmor
Stanford U. Press, Stanford, CA, 2004. $70.00 (642 pp.). ISBN 0-8047-4914-0

The closer you get to Stanford University, the more you hear that Frederick Terman was the "father of Silicon Valley." Stanford can be truly proud of the many achievements of this electrical engineer who became one of its most accomplished administrators after World War II, building what was already a strong university—albeit regarded by many as somewhat of a country club—into an intellectual powerhouse now recognized as one of the leading academic institutions in the world. Along the way, he encouraged close interaction between industry and academe, an intercourse that almost all universities studiously avoided during the 1950s and 1960s. The resulting exchanges of people and ideas did much to foster the growth of high-tech industry in the Santa Clara Valley, which was until the late 1960s much better known for its apricot orchards than for semiconductor manufacturers.

In *Fred Terman at Stanford: Building a Discipline, a University, and Silicon Valley*, C. Stewart Gillmor, a historian of science at Wesleyan University in Connecticut, has written what will be regarded as the definitive biography of Terman. It is so thoroughly researched and so richly detailed that I cannot imagine anyone attempting another. After having glanced through the Terman archives at Stanford while conducting research for articles and a book on semiconductor history, I know what a vein of gold Gillmor has mined. And I can attest to the care and thoughtfulness with which he has used the material.

Terman was born at the dawn of the 20th century and grew up the son of Lewis Terman, a now-famous psychologist who pioneered the measurement of intelligence and the development of the Stanford–Binét IQ tests. Early in young Fred's life, while he studied at Palo Alto schools and played on campus streets, he became absorbed with the notion that quality could be measured and quantified. As Gillmor recounts, the idea was to remain with him the rest of his life, shaping his approach to building Stanford and Silicon Valley.



But instead of following in his father's intellectual footsteps, Terman turned to engineering when he matriculated at Stanford during World War I. As an early ham-radio enthusiast, he drifted naturally into the emerging discipline of electrical engineering and did well at it. So well, in fact, that in 1922 he headed east to MIT for graduate work in the field, becoming Vannevar Bush's very first doctoral student.

That apprenticeship, and a well-deserved reputation as one of the nation's best radio engineers, helped plug Terman into the leadership of the US scientists and engineers who, under Bush, rose to meet the technological challenges of World War II. From 1942 to 1945, he directed Harvard's Radio Research Laboratory (just upriver from the famed MIT Radiation Laboratory) and oversaw the development of such radar countermeasures as jamming and chaff. It was a career-molding experience for him.

Terman found himself among a select, privileged group of what historians have come to call "master builders"—scientists and engineers who had served their country during the war, grasped its organizational lessons, and formed extensive networks of trusting government and military contacts to turn to during the postwar years. He also was among the first to understand that universities would have an important research role to play in the military–industrial complex that was emerging during the ensuing cold war. As Stanford's dean of engineering from 1946 to 1959 and provost from 1955 to 1965, Terman drew upon his extensive contacts and funding sources to build what he called "steeples of excellence" in fields such as electrical engineering, physics, chemistry, biology, and medicine. The Microwave Laboratory, a joint effort of the physics and electrical engineering departments, was established shortly after the war during Terman's tenure as dean. As provost, he oversaw the construction of the Stanford Linear Accelerator Center, which was the focus of much conflict among Stanford physicists in the early 1960s but the locus of three Nobel Prize–winning experiments during the next decade.

Gillmor is less thorough in discussing Terman's central role in the formation of Silicon Valley, which the author treats in several sections sprinkled throughout the latter chapters of the book. It was a huge role that deserved its own chapter. In the 1930s, Terman convinced his Stanford graduate students William Hewlett and David Packard to remain in the Bay Area and start their own electronics firm, in which he invested $500, rather than take jobs in big, established companies on the East Coast. Terman also played a large part in the formation of the Stanford Industrial Park, where Hewlett–Packard, Varian Associates—another entrepreneurial Stanford spinoff—and other technology-intensive companies set up operations during the 1950s.

Terman deserves much of the credit for the high-technology character of the park and the rest of Silicon Valley, which he saw as a natural, synergistic complement to Stanford's research activities in science and engineering. Except for MIT and Stanford, most universities at the time kept industry at arm's length. Yet Terman recognized that nearby entrepreneurial, high-tech companies could take the products of Stanford research to the commercial marketplace—and that the principals of these successful companies would, later in life, reward the university richly with generous contributions.

My greatest criticism of *Fred Terman at Stanford* is that Gillmor does not step back enough from the many interesting details of Terman's highly productive life to offer readers a broader analysis of his impacts beyond Stanford. We also don't get much feel for the historical context of the cold war amidst which all the steeple building was taking place. We see too many trees and not enough forest.

But those are small flaws in an otherwise fine book, and other historians such as Robert Kargon, Stuart Leslie, and Rebecca Lowen have already addressed these issues in depth. Gillmor has gone beyond their work to give us an engaging portrait of the farseeing, hard-working engineer who "proved himself a master builder of a career, a profession, a university, and a regional economy."

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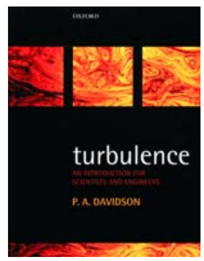
Turbulence: An Introduction for Scientists and Engineers

P. A. Davidson
Oxford U. Press, New York, 2004. $174.50, $74.50 paper (657 pp.). ISBN 0-19-852948-1, ISBN 0-19-852949-X paper

In *Turbulence: An Introduction for Scientists and Engineers*, Peter Davidson begins the first chapter by writing "The study of turbulence is not easy, requiring a firm grasp of applied mathematics, and considerable physical insight into the dynamics of fluids. Worse still, even after the various theoretical hypotheses have been absorbed, there are relatively few situations in which we can make definite predictions!"



I agree with Davidson's assessment. Historically, there have been two main schools of thought on the theory of turbulence. One emphasizes the study of coherent vortical structures in a turbulent flow; the other emphasizes a statistical approach. Until recently, researchers who followed one of the two methods were reluctant to acknowledge the importance of the other. My own approach is definitely statistical and looks for analogies of turbulence with other problems in statistical physics. Davidson does a nice job in presenting both points of view and in explaining how those views can come together. He keeps the discussion lively with an elegant writing style, frequently quoting a range of experts from Winnie the Pooh to Richard Feynman to Keith Moffatt.

The first three chapters of *Turbulence* give a clear introduction to the classical picture of turbulence. Chapter 4 concerns turbulent shear flows and simple closure models. In chapters 5 through 8, the author develops the physics of homogeneous isotropic turbulence in further detail. For example, chapter 6 covers isotropic turbulence in real, physical space and gives a good description of the conflict between statistical phenomenology and deterministic cartoons. Davidson gives considerable thought to the conflict and explains the problem clearly. In chapter 7, which covers the role of numerical simulation, Davidson summarizes research that was current a few years ago; however, the field is moving very fast, so it is hard for a textbook on the topic to remain current. In chapter 8, on isotropic turbulence in spectral space, Davidson conveniently collects many of the important results.

Chapters 9 and 10 are unusual and welcome additions to a textbook on turbulence. In chapter 9, Davidson focuses on the influence of rotation, stratification, and magnetic fields on turbulence. In chapter 10, he covers two-dimensional turbulence.

I do have a fundamental disagreement with Davidson: He largely ignores the Lagrangian description of turbulence, in which one follows a fluid particle, in favor of the Eulerian description at a fixed spatial point. Considerable evidence exists that a valid statistical theory of the turbulent cascade requires a Lagrangian description. The idea that the smaller eddies are swept by the larger ones without dynamical distortion is fundamental to turbulence dynamics and can be expressed only in Lagrangian terms. A more radical position to which I subscribe is that a turbulent cascade does not exist dynamically but is instead a descriptive property of a statistical steady state.

Davidson's book can be compared with another text on turbulence, *Turbulent Flows* (Cambridge U. Press, 2000) by Stephen B. Pope, a book intended primarily for graduate engineering students. The first half of Pope's book largely overlaps with the first eight chapters of Davidson's. The second half of Pope's book is devoted to the engineering modeling of turbulence. As one might expect, Pope's presentation of the fundamentals is straightforward, and he spends less time on the subtleties and basic difficulties of the subject. Another difference between the two books is in the layout: The layout of Pope's book is easy to read, and the type size and fonts are clear. By contrast, Davidson's book is visually unsatisfactory, and just two-thirds of the width of each page is used for text. The remaining third contains captions and occasional figures. In addition, the book's type is unpleasantly small and the font too light for easy reading. The color plates in the middle of the book, however, are nice.

In brief, Davidson presents a thoughtful and detailed discussion of