Einstein's Miraculous Year – Five Papers That Changed the Face of Physics, edited and introduced by John Stachel, Princeton University Press, 41 William Street, Princeton, NJ 08540, USA. 1998.

A long time ago the theoretical physicist George Gamow wrote a book with the title *Thirty Years that Shook Physics*. Inspired by his phrase, one may well say that 1905 – *annus mirabilis* – was the one year when Albert Einstein shook the foundations of physics with his five papers, four of them in the then prestigious *Annalen der Physik*, heralding the dawn of a creative career scarcely matched in the annals of science.

A chronological listing of the papers, their respective dates of receipt by and publication details in the *Annalen*, all in 1905, is (in English translation): (i) 'On a heuristic point of view concerning the production and transformation of light' (18 March; vol. 17, p. 132–148); (ii) 'A new determination of molecular dimensions' (University of Zurich dissertation; 30 April); (iii) 'On the motion of small particles suspended in liquids at rest required by the molecular kinetic theory of heat' (11 May; vol. 17, p. 549–560); (iv) 'On the electrodynamics of moving bodies' (30 June; vol. 17, p. 891–921); and (v) 'Does the inertia of a body depend on its energy content?' (27 September, vol. 18, p. 639–641). All these have appeared in 'The Collected Papers of Albert Einstein – Volume 2 – The Swiss Years: Writings, 1900–1909', English Translation, published by Princeton University Press in 1989. What John Stachel's *Einstein's Miraculous Year – Five Papers that Changed the Face of Physics* offers are modern English translations of these classics, an extended introductory essay along with individual ones for each paper, and historical and editorial notes for each. There is a brief preface by Roger Penrose where he points out that we in this century have been privileged to witness two major revolutions in physics, and Einstein had so much to do with both.

Stachel's general introduction compares Newton's and Einstein's personalities, states of preparation and mathematical abilities in their respective miracle years 1664–1666 and 1905. Comparisons are always difficult, especially when the individuals are separated by two and a half centuries, but Stachel's account is most illuminating. While Newton was twenty two and just beginning his creative efforts in 1664–1666, Einstein in 1905 was twenty six and already a mature physicist with five publications behind him. Newton's innate mathematical strengths were of a supremely high order, while in this respect Einstein often depended on others for help. Stachel groups the five papers of 1905 into three major areas in increasing order of 'distance' from classical Newtonian physics: papers (ii) and (iii) in the above chronological listing (Nos 1, 2 in Stachel's sequence) are in the tradition of classical mechanics and show Einstein's mastery in using statistical ideas and fluctuation methods; papers (iv) and (v) (Nos 3, 4 in Stachel's arrangement) are in the tradition of the classical field theory of electromagnetism, and show the way to the reformulation of mechanics to be in harmony with the former. These four papers bring to a culmination the classical legacy of Galileo–Newton–Faraday–Maxwell–Boltzmann, and display Einstein's extraordinary understanding of this legacy. Paper (i) (No. 5 in Stachel's list) stands by itself, constitutes area three, and in Einstein's own judgement is the only one of the five that is truly revolutionary!

Paper 1 was Einstein's dissertation submitted to the University of Zurich for his Ph D. Here he suggests using arguments based on phenomena in liquids rather than in gases to arrive at reliable estimates of molecular sizes. He uses detailed knowledge of classical hydrodynamics to calculate the effect of a solute on solvent viscosity, and on the diffusion rate of solute molecules. Comparing these two results with experimental data, he was able to estimate both Avogadro's number and the size of solute molecules. What is rather surprising about this paper, apart from his unerring intuition and choice of physical approximations, are the innumerable errors – in symbols, numerical factors and even in inferences from his own formulae – that are present! His own student Ludwig Hopf was later to recheck and correct many results, in the midst of extensive correspondence with the experimentalist Jean Baptiste Perrin.

Paper 2 on the Brownian motion is a jewel of the kinetic theory of heat. Its influences on statistical physics, theory of stochastic processes and probability theory have been simply enormous. It was written at a time when there was considerable scepticism concerning the reality of atoms – 'atomism may have a heuristic didactic utility' alone. (In passing one recalls Gell-Mann's initial attitude to quarks in the early 1960s.) Einstein invented and recognized fluctuation as the key concept; and that the most appropriate measurable quantity is the mean square displacement of a Brownian particle, not its speed. The attentive reader will see on page 96 of Stachel's book the seed of the idea of the Dirac delta function – this in 1905! For a long time the experimental studies on Brownian motion were quite poor in resolution, and it took a while before Perrin beginning in 1908 could improve their accuracy and verify Einstein's predictions.

Papers 3 and 4 are landmarks in the evolution of physics. They established the special theory of relativity and one of its most stunning and fateful consequences – the equivalence of mass and energy. Paper 3 is so carefully crafted that it already reads like a review rather than a research paper! Stachel's introduction is an excellent historical overview of the principle of relativity, its origins in mechanics, the conflict with electromagnetism, and the final resolution. It transpires that Einstein had been possessed of these questions for seven long years, until finally a discussion with his friend Michele Besso, during which he aired his difficulties, suddenly showed him the way. This is a splendid instance of the psychological fact that clear enunciation of a problem to a willing listener can itself lead to the way out of the darkness. In a bare six weeks was the paper then composed. Einstein's analysis of space and time measurements (philosophically influenced by his reading of Hume, Mach and Poincare'), his enunciation of the two postulates underlying special relativity, the physical derivation and interpretation of the already known Lorentz transformation equations, are all part of legend. His deep unravelling of the meaning of simultaneity shows a courage in thinking and conception that was to inspire many others. The paper itself – the longest of the five – is made up of Part A setting up the new kinematics of space time, and Part B applying it to electromagnetism. Einstein realized early

and stated clearly that, like thermodynamics, special relativity was a framework theory to which all specific physical theories had to conform – until of course his own general relativity came along and superseded it. The following paper 4 on mass energy equivalence is very brief. It is only necessary to point out that neither the phrase 'rest mass energy' nor the formula '$E = mc^2$' occur in it explicitly. The statement however is made that 'The mass of a body is a measure of its energy content'.

The 'revolutionary' paper 5 is historically the second most important paper in the development of quantum theory, after Planck in 1900 had announced his law of black body radiation. Here Einstein's mastery of statistical thermodynamics, the roles of probability and entropy and the Boltzmann principle, are just stunning. His examination of the high frequency Wien limit of Planck's radiation law, and extracting the volume dependence of the entropy of such radiation, are strokes of sheer genius: he knew that it was here that departures from classical ideas would show up. It was the similarity of this volume dependence to the case of a gas of free molecules that led him to the idea of quanta of light. This link to the Wien limit of Planck's law, an approximation, is mentioned by him repeatedly. And after having arrived at the light quantum hypothesis in this way – expressed in the historic statement 'monochromatic radiation of low density (within the range of validity of Wien's radiation formula) behaves thermodynamically as if it consisted of mutually independent energy quanta of magnitude $R\beta\nu/N$' – he looks at three different situations where his ideas can be checked: Stokes rule for photoluminescence, the photoelectric effect, and the ionization of gases by ultraviolet light. As everyone knows, it was not relativity but the explanation of the photoelectric effect that was cited in his Nobel award in 1921.

This is a precious volume, meant for both the mature and the gifted young. There is an indescribable thrill in reading these classics of science, and in reminding oneself that physics was different before them. We see the sources from which Einstein learnt his physics, the connections he made in his mind among its various areas, the new conceptions that sprang from his imagination, all in the historical context. Remembering what he said about Mahatma Gandhi, one can scarce believe that such a one as this walked this earth and achieved so much in the span of a single year, while earning his living as a patent office clerk. But then volume 17 of the *Annalen* exists to convince the incredulous!

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Therapeutic Strategies for Modulating Inflammatory Diseases. B. M. Weichman (ed.). Birkhauser Verlag, Postfach 133, CH 4010, Basel, Switzerland. 1998. Price: SFR 78. 107 pp.

One of the many problems with this book (and others of its genre) is of dates: The date of publication of the book is 1998 (although the catalog entry is from 1997), the editor's preface is dated August 1997 – a year ago –, and the book is based on a conference in October 1996 – almost two years ago! So the question is: What is new?

It is a generic problem, rooted in the cumulative nature of scientific endeavour, that books in science become outdated. This is especially true of books whose major, if not sole, *raison-d'etre* is their novelty or 'currency',—they suffer as terribly as bell-bottom trousers. Since the major reason (or at least, the major academic one) for conferences is topicality, a publication of the proceedings two years after the event is a sorry spectacle. If one is bent on publishing the proceedings of ephemeral conferences, should one not do it within months, at least, of the event itself?

The second difficulty with the book is in deciding what to expect, even after one has finished reading it. The blurb on the back cover says that it presents the latest results from molecular, preclinical and clinical investigations on the mechanisms of inflammatory diseases, by international experts in the field of inflammation research, and should be a valuable resource for pharmacologists, immunologists, molecular biologists, and medicinal chemists involved in inflammation research and drug discovery. That is indeed a tall order to live up to! Then one starts at the beginning and discovers that the book simply presents 'many highlights of the eighth international conference of the inflammation research association'. That is not a letdown; it is a crash.

Associated quibbles also arise: There is no uniformity in the pieces presented in the book. While some are reviews, others are actual data reports. Some of the data reports are those of clinical phase I trials, meaning preliminary studies of new drug toxicity and pharmacokinetics in volunteers, saying nothing about efficacy. Why on earth are these 'valuable resources'? A large part of the book is poster presentation summaries, which in the nature of things are even more ephemeral. Another quibble, admittedly a minor one, is about hype. 'International experts', the blurb proclaims, yet with one exception from the UK, all authors are from the USA. One hopes at least that the poster sessions had a more international flavour. All the five editors are from industry, as are a majority of the authors. This sets the tone for the writings which, despite claims to the contrary in the blurb, say nothing whatsoever about mechanisms of inflammatory diseases. What are 'inflammatory diseases', anyway? As far as the book is concerned, this is not a etiological or mechanistic definition but only a clinical therapeutic one, meaning all those diseases where anti-inflammatory therapy helps. Thus from so-called 'autoimmune' inflammation to degenerative diseases, like osteoarthritis, a wide range of pathophysiological states is grist to this mill. Every piece exhorts the troops about the 'immense therapeutic possibilities' in this that or the other,—the industry-based authors with pushy vim, the academics with chilly caution.

So why do academics write in such books at all, given its exceedingly limited readership? This is a 'non-peer-reviewed publication', and therefore of no great use to the bio-data of a working scientist. One reason could be to pad the bio-data of a junior colleague such as a doctoral student or a post-doctoral associate, but the junior collaborator authors in the book do not appear to need that. Another reason could be to write a review of currently controversial preoccupations in the field,