Surprise!

J.S. Bell

Surprises in Theoretical Physics. By R. Peierls. Pp.166. (Princeton University Press: Princeton, 1979.) Hardback $19, £8.40; paperback $5, £2.10.

THIS is a fascinating book. About thirty situations are presented, scattered widely over theoretical physics. The surprise is sometimes that the obvious answer is wrong, sometimes that the devastating criticism of the obvious answer is wrong, sometimes that a very great man has gone wrong, and so on. Some of the surprises are pleasant, as with the validity of the nuclear shell model, or with the simplicity of Landau's diamagnetism. Some of the surprises are unpleasant, as with the non-existence of the Bogolyubov expansion for the diffusion coefficient, or even with the difficulty of estimating the adequacy of the JWKB method in barrier penetration. Some of the distinguished men who slipped up were very distinguished indeed. Debye was quite wrong about thermal conductivity in non-metals, and the "reasonable agreement with experiment fortuitous". Heisenberg was wrong about the resolving power of his γ-ray microscope, and was corrected by Bohr. Einstein forgot his own gravitational redshift in an attack on the time–energy uncertainty relation, and he also was corrected by Bohr. But Peierls himself remains surprised that Bohm and Aharonov have successfully attacked a version of this "fourth uncertainty relation". He conjectures that the interaction they invoke "for some reason cannot be permitted in quantum mechanics".

Sometimes the important reference is to early work of Peierls himself. It is the 'Umklapp' process of his 1929 thesis which resolves the difficulty with Debye's thermal conductivity. Sometimes the surprise has been generated by colleagues in his own department. But the choice is catholic, and indeed the two longest sections in the book are concerned with two perennial surprises — irreversibility in statistical mechanics, and the problem of interpreting quantum mechanics.

It seems to me that the non-technical account of macroscopic irreversibility, contrasted with microscopic reversibility, brings out admirably the essential point often concealed in lengthier and more mathematical accounts. That is to say that the situation is intelligible when we suppose boundary conditions to be imposed in the past, rather than the future, and with no great care — or at least without the fantastic and conspiratorial care that could have ensured the exceptional decrease of entropy rather than the normal increase.

As regards the problems of quantum mechanics, Peierls begins with an account of what went wrong with the celebrated von Neumann theorem on the impossibility of deterministic hidden variables. He continues with a nicely ironical account of subsequent work centred on the notion of 'locality'. Then he takes up the infamous 'reduction of the wave packet'. To my dismay he regards it as "clear that the significance of the state function is to represent our state of knowledge of the system". But he goes on to ask "whose knowledge . . . ", and is carried down into the depths. He finally conjectures that the Schrödinger equation does not apply to conscious organisms and (if I interpret correctly) that it is in the presence of such organisms that linearity fails and wave function collapse occurs. It seems to me that the reduction is then dynamical rather than actuarial. It is not at all to be equated with the mere adjustment of odds appropriate when a candidate for life insurance is seen to be over one hundred.

The bulk of the book is occupied with quantum and statistical mechanics. But finally there is one relativity problem. It is the old question of radiation — or non-radiation — from a uniformly accelerated charge. Here (surprise) there is something quite new, from unpublished work of Boulware. He has worked out carefully (for the first time, it seems) how it looks in the accelerated system in which the charge is at rest. I would have liked to have seen my favourite relativity surprise included in this book. I will yield here to the temptation to describe it (A. Evett, *Am. J. Phys.* **40**, 1170; 1972). Two identical spaceships, identically programmed, are initially at rest in some inertial system S, one of them 100 metres behind the other. At a given moment (in S) both motors start and off they go. With identical acceleration programmes they remain, of course, always 100 metres displaced from one another (in S). The ships are initially connected by a fragile (but incombustible!) thread. This thread would like to Fitzgerald — contract as the ships speed up, but as they do not come closer (in S) the thread cannot contract — so it breaks. Or does it? My experiences with this puzzle have convinced me that most relativity courses seriously damage the minds of most students.

There are no surprises here from elementary particle theory. Is that, then, only a dull plodding sort of subject? I think not. For example, there was the big surprise of a renormalizable theory of weak interactions. And I think also of the very beautiful surprise of the 't Hooft-Poylakoff magnetic monopole. And surely there are many others.

But let us be grateful for what we do find in this excellent book. One of the nicest surprises in it is the elegant simplicity with which nearly all of these topics are presented. The essays are mostly accessible to undergraduates with a first course in quantum mechanics, and to graduates who have not forgotten. □

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Memorial to the Ghetto

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Hunger Disease. Edited by Myron Winick. Pp.261. (Wiley: New York and Chichester, UK, 1979.) $19.80, £10.60.

THIS is an important book. It was conceived of early in 1942 by Dr Israel Milejkowski who, as chief of public health in the Warsaw Ghetto, was sure that death from starvation and disease awaited its inhabitants. Certain of this fate, and powerless to prevent it, he recruited 27 colleagues and persuaded them to undertake a scientific study of the effects of hunger that they were observing in the Ghetto. Such a study, he hoped, would be, at least, a memorial to the dead.

They had just five months to build that memorial. On 22 July 1942 the German authorities began to liquidate the Ghetto and its inhabitants. Milejkowski records that their already meagre hospital and laboratory facilities were destroyed, and, with mass deportations occurring around them, the scientists wrote up their work and prepared the manuscript that was smuggled out of the Ghetto and hidden.

It was a remarkable feat, but ironically it almost seemed as if their efforts were in vain. Though the manuscript survived the war, only limited editions were published in Polish and French, and though the title *Maladie de Famine* was often referred to in the nutrition literature, the document itself was largely unread by nutritionists.

In rescuing it from obscurity, and producing not only an English translation, but a detailed appreciation of the modern significance of the work, Professor Winick has ensured that the memorial Milejkowski intended has at last been built.

But the book must not be viewed as a purely historical document, and Professor Winick has wisely treated it as a work of current scientific merit. Of course it is a strange scientific document, exploring theories and using techniques of 40 years ago. But outmoded or not it is still of current interest, because, with the exception of Keys' *Biology of Human Starvation* (University of Minnesota Press, 1950), it is the only study in the field. Though by its brevity it is less authoritative than Keys' work, it ranks alongside it as a scientific document because it deals with some different aspects of starvation, and


Severe marasmus and death in the Warsaw Ghetto.

because, unlike Keys' experimental study, it deals with people starving to death.

I am sure that many of the results the authors obtained will still be of scientific interest.

For example, the chapter by Fliederbaum *et al.* on metabolic changes records that in severely malnourished patients BMR was 30–40% below normal and was not stimulated by protein feeding, but increased by 20–50% when sugar was fed. Or again, Fajgenblat's brief report on ocular changes in starvation, or Apfelbaum-Kiwalski's report on the pathophysiology of the circulatory system in starvation, will be read with interest by all workers in the field. All will, like me, be saddened by the brevity of the reports: in reading the book I often found myself wishing that all the authors had reported their raw data, or referenced the exact methods by which they worked out complex indices (like the degree of normality of BMR) but, given the conditions in which they worked, one can only be grateful that anything at all has survived.

That which has survived will be accorded a place of honour amongst many analogous nutritional studies on the pathophysiology of protein-energy malnutrition in children that have been produced since the Second World War.

However there will remain one crucial difference between this book and many other postwar studies on the biochemistry of malnutrition. Most scientists studying malnutrition since the war have done so because they believed that the causes and cures of malnutrition should be sought at the physiological level. The authors of this book had no such illusion. The cause of the malnutrition they describe is to them clear: it is the result of a systematic policy which, in isolating the Jews from the economic life of the nation, sentenced them to death. It was a policy which allowed a Jew only 800 kcal per day, under half of that allowed by the Germans even for people who did no work worth mentioning. It was in response to this policy that the authors undertook their study, not because they believed they would find a scientific cure for the Hunger Disease that this policy induced but because all they could do as scientists was to create a memorial to the dead, by their contribution to scientific knowledge.

Is there in this a lesson for our times? In the 40 years since the Ghetto was destroyed, immense scientific effort has been put into studies of the nutritional and metabolic aspects of protein-energy malnutrition in children without doing anything to reduce the prevalence of the disease. Perhaps the heroic efforts of Milejkowski and his colleagues should cause us now to focus more clearly on the cause of the disease and step outside a narrow scientific paradigm to seek a cure.

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Planets X and Pluto. By William G. Hoyt. Pp.302. (University of Arizona Press: Tucson, Arizona, 1980.) Hardback $17.50; paperback $9.50.

A YEAR from now we will be celebrating the two-hundredth anniversary of the discovery of the planet Uranus by William Herschel. Compared with, say, the Einstein centenary which occurred recently this might seem to be an event of limited interest. In terms of its contemporary impact, however, Herschel's discovery attracted as much public attention as the General Theory of Relativity did in this century. For the first time since prehistory, the scope of the Solar System had been enlarged. The natural reaction of astronomers was to look for other, as yet undiscovered, planets — a hunt which lasted for a century and a half.

Mr Hoyt's book is primarily concerned with the investigations that led to the last of these planetary discoveries — the detection of Pluto. But it is logical that he should introduce this search with a detailed discussion of the events surrounding the discovery of earlier planets before turning to Plutò itself. His account is readable, but highly detailed. Like his earlier book, *Lowell and Mars,* the narrative is partly based on material in the Lowell archives. Like the earlier book, too, it is intended as a partial history of the Lowell Observatory, and so covers matters other than planets. But it is the search for Planet X — as the supposed planet beyond Neptune was labelled — that dominates the story.

The main outlines of the hunt for new planets is fairly familiar, at least up to the discovery of Neptune. But this earlier history — and especially the problems facing the theoretical prediction of Neptune's position in the sky — provides a fascinating parallel with the subsequent search for Planet X. Neptune was sought because the newly discovered Uranus stubbornly refused to follow its predicted path. The postulate of an outer planet, gravitationally perturbing Uranus, became an increasingly attractive possibility. Ultimately, two theoretical astronomers, Leverrier and Adams, independently predicted a position for this supposed planet. Their results were in good agreement with each other, and Neptune was, indeed, picked up close to the predicted point. So far, this was a major success story. But it rapidly became evident that some of the assumptions made by both Leverrier and Adams in determining the new planet's position were far removed from reality. How, then, had they managed to pinpoint its place so accurately? It was very quickly suggested that their result was purely a 'happy accident', a conclusion which was equally quickly denied. The controversy continues today (it is, perhaps, a slight defect in this book that the subsequent analyses of the problem are not fully covered); but a modern consensus would give Leverrier and Adams the benefit of the doubt.

The search for Planet X possessed one immediate difference from the hunt for Neptune. Like the latter it was based on an examination of residuals, otherwise unaccounted for, in the orbits of the inner planets (in this case, Uranus and Neptune). The residuals now, however, were much smaller than those that had originally attracted attention to the existence of Neptune. It was less that these residuals forced a search for a new planet than that the desire to find a new planet motivated the investigation of the residuals.

The most detailed, though not the only, attack on the problem of Planet X was by Percival Lowell, and much of the book revolves round his activities. Lowell's final predictions appeared in 1915 — only a year before his death — but the astronomers at Lowell Observatory had already started on a photographic search for the supposed planet in 1905. After Lowell's death the search lapsed, only to be resumed at the end of the 1920s. It was then undertaken by Tombaugh, a new recruit to the observatory staff, and, early in 1930, he discovered Pluto close to the point indicated by Lowell's calculations.

This sounds like a repeat of the