precipitation, evaporation and runoff that might affect the availability of water for irrigation. All of the authors see these changes as potentially serious, but only Bach agrees with a recent assessment by the US Environmental Protection Agency (*Can We Delay a Greenhouse Warming?*, which was also published last year) that the implications are unequivocally catastrophic.

● *What should we do?* If we side with all of the authors except Bach, giving weight to the huge uncertainties and the adaptive capabilities of life on Earth, then the wise course is to be alert for signs of nasty consequences (none is yet evident), to take sensible steps to reduce damage to the environment, and to develop a range of alternative options for energy supply and for living in a changing climate. If we share Bach's assessment then a much more active policy is called for. He proposes aggressive energy conservation measures, reforestation, soil conservation and the expeditious development of renewable energy resources. In the short term, the cautious and activist prescriptions may differ little in their practical effect, and by the end of the century, the NAS report proposes, we will know a lot more if we keep up the pace of research. In the meantime, the implementation of elements of Bach's prescription can only be to the good — fear of climate change, like fear of cancer, may lead us into healthier habits that could spare us from many other hazards.

Reading these books leads one to conclude that the CO_2 "cottage industry" is serving the public well by clearly illuminating the complexities of this web of scientific and social issues. Others in the business have different strengths. In a popular vein, John Gribbin's *Future Weather and the Greenhouse Effect* (Delacorte, 1982) has much to recommend it, if its more mystical elements are disregarded. Wilfrid Bach and colleagues have given us two earlier and excellent reviews — *Man's Impact on Climate* (Elsevier, 1979) and *Carbon Dioxide: Current Views and Developments in Energy/Climate Research* (Reidel, 1983). Societal aspects were explored by W.W. Kellogg and R. Schware in *Climate Change and Society* (Westview, 1981), while a detailed scientific analysis was orchestrated by Gordon J. MacDonald (*The Long-Term Impacts of Increasing Atmospheric Carbon Dioxide Levels*; Ballinger, 1982). A most comprehensive review, edited by William C. Clark, is *Carbon Dioxide Review: 1982* (Clarendon, 1982). And the NAS report, *Changing Climate*, a compendium of reviews, original contributions and synthesis, has already been mentioned.

The books of current vintage contribute much to this literature, and all can be recommended. Both Jäger and Bach are distinctive and important additions. Jäger's comprehensive view of climate through the prism of energy is unique and illuminating. Bach, though regrettably impeded by abominable computer typesetting, presents an impressively scholarly and exhaustive treatment. While his road leads to but one conclusion, it is broad and solidly built.

The next few years will bring further contributions. The US Department of Energy will soon release an assessment of the knowledge and conclusions emanating from its massive research programme, and an international effort is laying the groundwork for a 1985 intergovernmental conference. Thus, as was observed in the NAS report, while CO_2 may be a worrisome issue, it may also be a healthy stimulus which forces us to address seriously the long-term maintenance of our planetary home. □

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"Beluga or White Whale", from William Scoresby's *An Account of the Arctic Regions, with a History and Description of the Northern Whale-Fishery,* (1820). The illustration is reproduced from *Voyage into Substance: Art, Science, Nature, and the Illustrated Travel Account, 1760–1840,* by Barbara Maria Stafford, recently published by MIT Press. Price is $39.95, £41.75.

Cells: bags of water or blobs of jelly?

D.A.T. Dick

In Search of the Physical Basis of Life.
By Gilbert N. Ling.
Plenum: 1984. Pp. 791. $79.50, £67.57.

QUANTITATIVE membrane theories, such as those of Boyle and Conway and the Hodgkin–Katz–Goldman equation, achieved spectacular success in the 1940s. These, along with Hodgkin and Keynes's demonstration of rapid potassium diffusion in the squid axon, induced a widespread view among physiologists that the cell is simply a membrane-bounded bag. In it, so the story ran, is a fairly homogeneous, aqueous soup of proteins and inorganic ions, the composition and electrical potential of which is controlled by the membrane. By contrast, a heterodox group of physiologists — led in particular by Troschin in Russia and by Ling in the United States — has maintained that the cell is essentially a blob of jelly, held together by controlled intermolecular forces between protein molecules to which water and the ionic constituents are variably bound. This view, called by Ling the association–induction theory, has been pursued to the extent of denying any significant role to the cell membrane.

Neither of these extreme stances is of course correct. In addition to the ample evidence for the function of the cell membrane, it is now known that the cellular matrix possesses a structure of microtubules and microfilaments (see for example *J. Cell Biol.* **99**, No. 1, Pt 2; 1984), that in some cells the distribution of ions (particularly sodium and potassium) is not uniform within the cell, and that at least some cellular water is bound. Thus there is sore need of an impartial review of alternative theories of the cell.

Dr Ling's book is divided into five sections, the first of which surveys concepts in cell physiology in a historical framework (here the most important chapter is the fifth which deals with the evidence for and against the membrane theory of the cell). The second section gives a full account of Ling's association–induction theory, particularly in relation to the control of the sodium, potassium and water contents of the cell. The third is largely devoted to the experimental work of Ling and his colleagues in support of the association–induction theory, while the fourth gives an account of mitochondria, muscle and epithelial transport, usually rejecting conventional physiological views and reinterpreting the data in terms of the association–induction hypothesis. The final section describes recent work on protein synthesis, growth and differentiation, and cancer, and offers speculative interpretations of these phenomena again

based upon the association-induction hypothesis.

Dr Ling describes his book as "at once a monograph, a science history tract, and a textbook". A monograph, it certainly is, but it has little of the objective judgement and impartiality normally associated with a history or a textbook. For example there are 106 references to the work of Dr Ling while some quite distinguished cell physiologists are represented by a couple of citations only. A distinctive historical feature is a series of portraits, most of them of justly prominent figures but with some bizarre omissions (and inclusions); often one cannot help feeling that the space occupied by the photograph would have been better employed by a more adequate treatment of the work of the scientist portrayed. For example Ling's survey of the sodium-potassium ATPase of the cell membrane, embracing the work of J.C. Skou, E.T. Dunham, I.M. Glynn, R.L. Post and S.L. Bonting, occupies but a page and a half.

Dr Ling appears to be constitutionally an advocate, rather than a judge, and he has set out to win his case by presenting every conceivable argument in support of his own position and giving the other side inadequate and unbalanced treatment. This is a pity. Since the 1950s majority views of the cell have moved significantly nearer to those of Dr Ling, but in this book he harms his own case by overstatement. What we need now is a judge, not an advocate. □

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From the laboratory to the clinic

W. F. Bynum

Science and Medicine in France: The Emergence of Experimental Physiology, 1790–1855.
By John E. Lesch.
Harvard University Press: 1984. Pp.276. $25, £22.

A SCIENTIST's proclaimed self-image may conceal as much as it reveals. Isaac Newton likened himself to a boy playing on the sea-shore, "diverting myself in now and then finding a smoother pebble or a prettier shell than ordinary, whilst the great ocean of truth lay all undiscovered before me". This is a charming image, but one which hardly does justice to Newton's complex psychology, or to his belief that he had seen into the heart of things. François Magendie compared himself to "a rag-pricker: with my spiked stick in my hand and my basket on my back, I traverse the field of science and I gather what I find". Equally charming this, but equally inadequate as an account of Magendie's philosophy of discovery. Certainly he did not possess the philosophical sophistication of his pupil Claude Bernard (1813–1878), by whom this statement of Magendie was recorded and who rather liked to cast his teacher into the role of John the Baptist as foil to himself as Messiah. But Magendie (like Bernard) was concerned with the dialectic between basic science and clinical medicine, and it is this which is a central theme in John Lesch's book.

Magendie (1783–1855) was a child of the French Revolution, a product and a participant in the "hospital medicine" which, particularly after the reorganization of the Paris Medical School in 1794, came to dominate French medical education. This form of clinical medicine was firmly rooted in the hospital, where medical students were to be based from the first day of their education and where they were to "read little, see much, do much". The high priests of hospital medicine — Corvisart, Laennec, Louis — stressed the importance of the local lesion as opposed to the generalized symptom, the lesion to be discovered by careful physical diagnosis (made more systematic after Corvisart popularized percussion and Laennec invented the stethoscope) and confirmed by autopsy. Every hospital physician was to be his own pathologist, permitting diseases such as phthisis, pneumonia and typhoid to be conceptualized in terms of more constant lesion than more variable symptom. For many doctors of the French school, the sciences of physics, chemistry, physiology and even microscopy were "accessory" rather than "basic" to the medical enterprise: accessory and potentially dangerous, for they encouraged speculation and system-building in advance of the "facts" demonstrable by pathological anatomy.

This aspect of "hospital medicine" has been elucidated in books such as E.H. Ackerknecht's *Medicine at the Paris Hospital, 1794–1848* (The Johns Hopkins University Press, 1967) and Michel Foucault's *Birth of the Clinic* (published in English translation by Tavistock in 1973). But while Lesch recognizes the cogency of their work, the value of his own lies in its patient examination of an equally rich tradition in early-nineteenth-century French medicine: a tradition which recognized the value of experimental science. He scrutinizes three sciences, chemistry, pharmacology and physiology, in each of which Magendie was a key figure.

A critical chemical tradition in French science dates back to the formation (in 1776) of the Société Royale de Médecine and to the rigorous attempt by prominent members of that Society, especially Antoine Fourcroy (1755–1809), to apply the newer analytical methods of chemistry to isolate and identify the active ingredients of common medical preparations. From the turn of the century the discovery of plant alkaloids—including emetine, strychnine, codeine, quinine and morphine—had demonstrated the value of the "analytical method", and had provided Magendie with the justification for producing his own *Formulaire* (1821), a therapeutic manual based on the notion of pure substances rather than polypharmic preparations. In using the word "pharmacology" in 1805, J.B.G. Barbier (1776–1865) had insisted that the pharmacologist had to be both chemist (to identify the active substance) and physiologist (to elucidate its precise actions). It was particularly in the latter guise that Magendie pursued his experimental investigation of strychnine, prussic acid and ipecacuanha. His relations with the Paris hospitals were chequered, but various clinicians helped him in his researches; and, in 1830, he became director of a women's ward in the Hôtel Dieu, where he was able to conduct more systematic clinical trials.

Magendie's clinical work (some of it simply attempting to cope with the seriously ill, as in the 1832 cholera epidemic when this most gifted experimentalist of his generation treated 594 cholera victims between March and August) reminds us of the complexity of the relationship between science and medicine during the period. The interplay between the two can be seen in Magendie's concept of "pathological physiology", for much of his most famous research—on the cerebro-spinal fluid, on the functions of the posterior and anterior roots of the spinal nerves, on localization within the central nervous system—was laboratory based but clinically informed. Lesch establishes continuity between the methods and problems of the late Magendie and the early Bernard, just as he also sees important legacies for Magendie from the experimental work of Xavier Bichat (1771–1802).

Much of Lesch's monograph consists of rather detailed expositions of the experimental chemical, pharmacological and physiological results obtained by French medical scientists of Magendie's era. Some of this may be of specialist historical interest. But the general problem he addresses—what place has experimental science in medicine?—is one of great topicality. Our own answers may not be the same as those proposed in nineteenth-century France, but Lesch's book does illuminate an important historical moment in that continuing dialogue. □

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● Subscription rates were omitted from the review of the journal *Molecular Biology & Medicine* (*Nature* **311**, 318; 1984). Publisher is Academic Press, and rates for 1985 are: institutional £60 (UK), $100 (elsewhere); personal £30 (UK), $50 (elsewhere).