works such as ecosocial theory, feminist perspective, political economy, and the human rights point of view. The article shows that all frameworks converge to the disadvantage of women: economic disadvantage, racial discrimination, and gender inequality compound each other and result in a greater risk of HIV infection among women of poor economic background, and black and hispanic women. The authors suggest pathways through which these frameworks interact. The message is that AIDS prevention strategies should not concentrate on individual interventions; rather, they should move towards approaches to build a more equitable society.

The United States has gone through successive stages of the epidemic of coronary heart disease. Public health measures such as building awareness about the importance of diet, the need for keeping in check serum cholesterol levels, daily exercise and abstinence from smoking have paid rich dividends in reducing the incidence of coronary events. 'Aspirin in the prevention and treatment of cardiovascular disease' by C. H. Hennekens discusses the role of aspirin in prevention of cardiovascular events such as acute myocardial infarction, reinfarction, and angina, the mechanism of action, its role in women, and the optimum dosage. Though there is scientific evidence for the role of aspirin in prevention of coronary events, its actual use seems to be restricted. Strong evidence of benefit in primary prevention has been demonstrated in trials as far back as ten years ago; however, there is also a concurrent, small increase in risk for haemorrhagic stroke. This has perhaps restricted its universal use. Primary prevention of coronary events through drug interventions is a new strategy which seems to be catching up. Unlike the statins which have been shown to be beneficial in lowering serum cholesterol levels and thus decreasing the risk for coronary disease, aspirin, though scientifically proven to be of use, is not aggressively promoted. Perhaps the reason might be that promoting aspirin use does not lead to the possibility of realizing enormous profits through its sale, since it is no longer a patented medicine. This could be reflective of the extra-scientific considerations in clinical decision making.

The *Annual Review of Public Health 1997* provides an insight into the topics of current importance in international, especially US, public health. All practising public health professionals and doctors, besides health policy makers, would benefit from the volume.

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Dynamics of Cell and Tissue Motion. W. Alt, A. Deutsch and G. Dunn (eds). Birkhaeuser Verlag, Basel. 1997. x + 336 pp, price not stated.

Not all that long ago, if you spoke to experimentalists about theoretical cell and developmental biology, you were left with the impression that in their opinion there was something dubious about the whole enterprise. When asked to explain what they meant they would say that theorists appeared to operate solely on the basis of unfounded assumptions regarding how living systems worked; indeed, to revel in their ignorance concerning basic facts, let alone details (not that the experimentalists knew all that many details themselves: one reason why it is only recently that phenomenology has started coming into theoretical biology). The grander and more ambitious a theory, the more out of touch it was with reality. There is a telling anecdote that symbolizes this attitude: apparently the distinguished molecular biologist S. Spiegelman once told a prospective post-doctoral fellow that the only good that might come out of modelling a cell as an oil drop was lots of oil. Needless to say, theorists, who had their own notions of what constituted worthwhile science, did not take kindly to such views. But they tended to combine disagreement with the unpleasant habit of talking down to experimenters. As a result, communication between the two groups remained poor. This worked mainly to the detriment of the theorists; after all, biology was and is primarily an experimenrtal science.

For anyone not convinced that a wide gap separated these two sub-cultures, a study of the output of the Rashevsky school would be instructive. Those who are unsure of what this means are directed to *Mathematical Biophysics* by N. Rashevsky – cited in the book under review, to my pleasant surprise. That work epitomizes a great deal that was good as well as many things that were bad about the school. With hindsight, it is clear that one cannot blame Rashevsky and his co-workers all that much; by the standards of today, ignorance concerning the biology of intercellular transport, or signalling, or cell movement, was enormous. Because of this, model-building at the level of tissues or embryos or whole organisms was in certain respects simple-minded; also, it was carried out largely on the basis of a mix of hope and faith. If anything, Rashevsky must be looked upon as a pioneer and ahead of his time.

Both in terms of its achievements and in terms of the way people look at it, what is the present-day status of the theoretical biology of shape and form? Has the field advanced significantly since the days of D'Arcy Thompson? With regard to one important criterion, that of bringing theory closer to factual knowledge, progress has been spotty. The bulk of the new information that we have concerning cells, what they are made of and how they behave comes from genetics and molecular biology; and only a tiny fraction of that information has been assimilated into theories or models. The mathematical analysis of biological oscillations is a prominent exception to this generalization in that it demonstrates an impressive interplay of contemporary ideas from biochemistry, genetics and cell and organismal biology. Apart from the field of oscillations, the modelling of cell movement and tissue patterning has attracted a fair amount of effort in recent years. *Dynamics of Cell and Tissue Motion* offers testimony to the vigour of the effort.

This book is the result of a Workshop organized in Bonn in March 1995 under the aegis of the German Research Council (DFG). During a final plenary discussion the participants identified, as suitable for bringing out in book form, presentations pertaining to a set of 'hot topics'. What has come out is the result of a series of prolonged exchanges between the editors, reviewers and authors. Each chapter is embellished with quotations, many of them in German. The latter are accompanied by an offer to supply English translations on request, not that that is guaranteed to make them any the less baffling (here is one, attributed to Leibniz, that happens to be provided in English: 'A movement can only develop, naturally, from another movement').

The title sums up the theme well. Practically every contribution consists of a mathematical analysis of changes in the position and shape of an individual cell or a tissue, caused by an external stimulus or by factors internal to the system. For most authors partial differential equations constitute the technique of choice; I noticed just two papers using cellular automaton models. The phenomena that are covered include the movement of embryonic mesoderm and human keratinocytes, plasmodial streaming, chemotaxis in leucocytes and amoebae, cell division, morphogenesis of the lung and the growth and bending of plant roots. Overall, the treatment is impressive if not forbidding. The diversity of systems, not to mention the detail in which each is explored, makes it difficult to say more within the space of a brief review. However, one thing that stands out is the level to which models with a comparable scope (e.g., ignoring genetics) have approached reality since Rashevsky made his brave attempt to mathematize biology. The most striking demonstration of this is the manner in which mechanical forces can now be taken into account while writing down equations to describe morphogenesis.

A fair amount of space is devoted to models for chemotaxis and oscillatory aggregation in the social amoeba *Dictyostelium discoideum*. Fuelled by the conviction on the part of physicists and mathematicians that this is as simple an example of biological self-organization as one can hope for, the field has spawned a mini-industry. On the experimental front, announcements of new genes implicated in pattern formation keep coming at the rate of almost one a month. All this might have been expected to lighten the burden of modellers. On the contrary, the profusion of data has led to a curious and, from the viewpoint of 20–30 years ago, unexpected, impasse. The difficulty has been not so much one of finding models that work as of finding models that do not work; in other words, of being able to discriminate between models. Continuum approximations seem to do just as well as discrete treatments; linear stability analysis leads to results that are as satisfying (under the appropriate set of assumptions) as those requiring finite-amplitude perturbations; and using a solely cyclic AMP-based oscillator may be no worse than using an oscillator based on an essential interplay between cyclic AMP and calcium.

This curious situation – and it is one that we are increasingly having to come to terms with in biology – cannot be ascribed to a paucity of data. Rather, living systems seem to possess redundancy of a high order. An analogy might help in making the point: Kimura once illustrated his neutral theory of molecular evolution with a cartoon showing two men drinking. One of them, a serious darwinist, sat nursing the single drink that he was left with after having rejected many bottles; his motto was 'Only the best'. The other was a more jaunty-looking gent, evidently a neutralist; he had assembled a largish collection of liquors and carried the slogan 'So long as it isn't bad'. Redundancy implies that cells and organisms display a sort of neutrality with respect to the genetic pathways that they can make use of for getting along in the world. As long as it is not too fussy, a plant, animal or microbe can make do without many genes and gene products (albeit not all at the same time). This forces theoreticians to confront underconstrained systems. The situation is comparable to that of having more unknowns than equations.

One might say that evolution has so moulded the Dictyostelid amoebae – and a great many other systems besides – that they come equipped with a multitude of solutions to the same problem. (The most striking example to date is illustrated by the claim, made last year, that *Dictyostelium discoideum* could go through apparently normal aggregation and multicellular differentiation in the absence of cyclic AMP.) Obviously, this renders moot any discussion of what *the* correct solution might be among a set of alternatives; it all depends on the precise experimental situation. By and large, theoreticians are still to come to terms with the implications of this fact.

The contributors assembled by Alt *et al.* provide excellent examples of model-building in cell biology. Anyone with an adequate grasp of mathematics and an interest in problems with a biological flavour will find it useful, as will practising theoretical biologists. Having said that, I wish that the derivations had been accompanied by clearer verbal descriptions of the assumptions and conclusions. In the absence of such assistance, few experimenters are likely to get a feeling for the models, and even fewer to make use of them. In short, much as theorists will benefit from the book, I doubt whether it will contribute to a narrowing of the gap across the theory–experiment divide. That may not be entirely a bad thing if the end result is mutual coexistence (and not mutual deterrence).

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Our Evolving Planet: Earth History in New Perspective. Karsten M. Storetvedt. Alma Mater Forlag AS Publishers, P.O. Box 4213, Nygårdstangen, N-5028 Bergen, Norway. December 1997. 456 pp. Hardbound. Price: US $ 80 plus postage ($ 15 surface, $ 30 air).

This is a very unusual and interesting book, strongly nonconformist to plate tectonics. Storetvedt, Professor of Geophysics at the University of Bergen since 1973, is arguably one of the most distinguished European palaeomagnetists. After and during many plate tectonics-based contributions in reputed scientific journals in the earlier years, today he is one of the few diehards who completely dismiss plate tectonics. Storetvedt is the man who, in the mid-seventies, brought about a consciousness of remagnetization problems in palaeomagnetism – remagnetization is a major issue in analytical and experimental palaeomagnetism today. Another startling and revolutionary demonstration of Storetvedt a few years ago was that the well-known diverging APW paths for the various continents, usually cited as the 'ultimate' evidence for lateral continental drift, *do not* require or prove Wegenerian-type motions, but are fully explainable by continental *rotations in situ*, such that a continent rotates around an Euler pole located *within* it, not outside it. In brief, no palaeomagnetic data ever proved lateral continental drift.