There is a third scenario. For example, at the end of the Cretaceous the Dinosaurs became extinct, whereas the mammals survived and radiated. I do not know why this was so, but let us suppose that the mammals survived because they are homoiotherms, and that their ability to chew had nothing to do with it. It would still be true that the Cretaceous extinctions led to an increase in the proportion of species with secondary palates, but not because of any selection for the palate itself. This is what Elizabeth Vrba has called the 'effect hypothesis'. It is an analogue, at the level of organs, of the process of 'hitch-hiking' at the level of genes.

The hierarchical view of evolution, then, is that processes of selection and stochastic drift go on at the level of genes and of species, as well as of individuals. As an old-fashioned proponent of the modern synthesis, I have no difficulty in accepting this, so long as no one expects me to believe that adaptations of individuals can be explained by molecular drive, and so long as the concept of species selection is confined to qualities, such as speciation rate and evolutionary rate, that are properties of species and not of individuals. However, there remains plenty of room for disagreement about the relative importance of these various levels, or — what amounts to the same thing — about the relative effectiveness of selection at different levels.

In from the cold

This brings me to what I see as the greatest impact that palaeontology is having on the way we see the mechanisms of evolution. We have been familiar for a long time with the dramatic disappearance of the Dinosaurs at the end of the Cretaceous. It is now apparent that massive extinctions, involving many different taxa, have been a repeated feature of evolution. Adolf Seilacher (Tubingen University) and Anthony Hallam (Birmingham University), the two palaeontologists on the panel, agreed with Gould on this, although there was disagreement about whether these events have been periodic or irregular, and whether they are caused by extraterrestrial events (meteorites, asteroids) or by terrestrial ones (Hallam, for example, emphasizes the role of changes in the area of the continental shelf caused by continental drift — see *Nature* **308**, 686; 1984). The impact of these extinctions is not random; in any given event, some taxa are more affected than others: Seilacher stressed the need for a quantitative study of this, to replace the somewhat anecdotal picture we now have.

In addition to the problem of their causation (which at present seems to be a problem for geologists and astronomers, although Seilacher did not rule out the possibility that some extinctions had biotic causes), these extinctions raise questions for evolutionary biologists. Is it possible that evolutionary change would slow down and stop in the absence of changes in the physical environment? As Manfred Eigen has pointed out, the simplest evolving systems (populations of RNA molecules in test tubes) reach a global optimum and then stop. Are extinctions, then, a necessary motive force of evolution? A second question concerns the relation between extinction and radiation. Ecologists tend to see nature as dominated by competition. They would therefore expect the extinction of one species, or group of species, to be caused by competition from another taxon. Most palaeontologists read the fossil record differently. The Dinosaurs, they believe, became extinct for reasons that had little to do with competition from the mammals. Only subsequently did the mammals, which had been around for as long as the Dinosaurs, radiate to fill the empty space. The same general pattern, they think, has held for other major taxonomic replacements. Not all palaeontologists would agree, but I think this is the majority view. I find it surprising: I would have expected a major cause of extinction to be competition from other taxa.

The Tanner lectures were an entertaining and stimulating occasion. The palaeontologists have too long been missing from the high table. Welcome back. □

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Malarial immunity

Indonesian and Sudanese style

from F.E.G. Cox

RECOVERY from malaria is accompanied by the production of antibodies that block the entry of merozoites — the infective stage of the *Plasmodium* parasite — into red blood cells[1]. Alternatively, it can be accompanied by a mechanism that kills the developing parasites within the red blood cell. This phenomenon has been known since 1944, when the Taliaferros noted the appearance of disintegrating forms, 'crisis forms', of a simian malaria parasite, *Plasmodium brasilianum*, in the red cells of recovering monkeys[2], but received little attention until it became clear that crisis forms are associated with recovery from a number of different *Plasmodium* infections in laboratory animals[3]. Although crisis forms are not apparent in the peripheral blood of patients with malignant tertian malaria caused by *Plasmodium falciparum* (because the developing stages of the parasite disappear from the circulating blood), they can regularly be detected *in vitro* both visually and by a test in which the incorporation of radiolabelled hypoxanthine into growing parasites within the red cell is measured[4]. That test has recently enabled James Jensen and his colleagues to show that in the Sudan, and probably elsewhere, intraerythrocyte killing predominates over merozoite blocking as a means of defense against the parasite[5-7]. These observations should completely alter our concepts of the development of vaccines against the blood-borne forms of *Plasmodium falciparum*.

Jensen and his colleagues collected sera from some 300 people living in three regions of the Sudan and tested their ability both to block the invasion of red cells by merozoites and to induce crisis forms in erythrocytes containing 'ring stage' parasites. The population could be divided into those who had experience of serious attacks of malaria, of mild malaria or no experience of malaria[6]. The clinical status could not be correlated with the presence of merozoite-blocking antibodies but was correlated with the ability of the serum to induce crisis forms. The greatest number of crisis forms was induced by sera from the group with no experience of malaria and hence with the greatest immunity to the disease. The serum factor that causes parasites to die in the red blood cells is not antibody because antibodies on the merozoites are sloughed off as the parasites enter the red cells; moreover, activity is not lost with the removal of immunoglobulin[5]. Jensen and his colleagues call the factor 'crisis-form factor' (CFF)[6].

In the Sudan, then, clinical immunity to malaria can be correlated with the presence of CFF in the serum. But what of other parts of the world? Jensen *et al.* have now shown that Indonesian sera do not induce crisis forms *in vitro*[7]. In order to make a direct comparison, the earlier Sudanese studies were repeated with sera from an area where the prevalence of malaria was similar to that in Flores, where the Indonesian study was carried out. Overall, Sudanese sera have abundant CFF and little merozoite-blocking antibody whereas Indonesian sera lack CFF, and their anti-parasitic activity, where present, is associated with merozoite-blocking antibody. This indicates that genetic differences may be important in determining whether antibody or non-antibody factors predominate in immunity to human malaria.

These experiments are of great importance and should significantly alter our concepts of an antimalarial vaccine. It is conventional to identify and isolate blood-stage antigens of the parasite by means of monoclonal antibodies and immune sera[8]. Such techniques may well lead to a vaccine that elicits a high and specific antibody response. But will this be protective when sera with antibody titres greater than 1:10,000 have little merozoite-blocking effect[7]? And if merozoite-blocking is of relatively little importance in natural immunity in Sudan, could it be the basis of an effective vaccine there? If genetic differences between the populations at risk have to be taken into account in malaria vaccine development, it will greatly complicate the task.

What is CFF? The short answer is that we do not know. As long ago as 1953, Trager and McGhee identified a factor in the sera of birds immune to malaria that inhibited infection by Rous sarcoma virus[9]. From this has arisen the possibility that CFF is related to the ill-defined tumour necrosis factor[10,11]. Alternatively, it may be related to certain reactive oxygen intermediates that bring about the occurrence of crisis forms both *in vivo* and *in vitro*[12,13]. However reactive oxygen intermediates are probably too short-lived to produce directly the effects recorded by Jensen *et al.*, although they could be involved in reactions with lipids to produce toxic substances such as aldehydes[14].

What must happen now is a switch of attention away from *Plasmodium* antigens that elicit the production of antibodies to those that elicit CFF. Since CFF is probably a product of macrophages, these cells and the T cells that activate them require much more attention. At a very basic level, however, we now know that CFF kills malaria parasites and thus, if characterized, could be used to treat the rapidly increasing number of drug-resistant cases for which there is no obvious alternative treatment. Further studies of CFF might also provide clues to the development of new antimalarial drugs. □

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100 years ago

THE *Bolletino* of the Italian Geographical Society for May contains a brief account of Signor Maurizio Buonfanti's late expedition across North Africa. After some trips to Yakoba and other little-known parts of Sokoto, he made his way through Gando to the Niger at Say, about midway between Timbuktu and the Binue confluence. Here he turned north, and for the first time *ascended* the Niger as far as Timbuktu. This feat, hitherto supposed to be impossible, was performed *in the dry season*, and the problem thus successfully solved possesses considerable geographical and commercial importance in connection with the attempts now being made to establish regular lines of water communication between Western and Central Sudan and the Gulf of Guinea. From *Nature* **30**, 131, 5 June 1884.

Astronomy

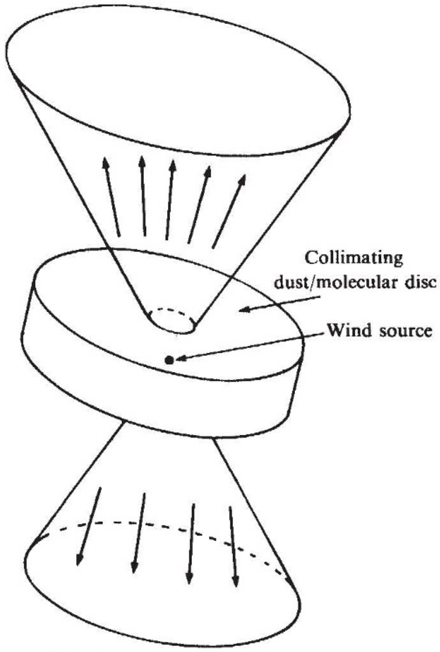
High-velocity winds in Orion

from Ben Zuckerman

THE biggest surprise to emerge from a decade of observational study of the formation of stars has been the realization that this process is accompanied by massive outflowing winds. The winds, which can be sporadic, are millions of times stronger than the solar wind and are generated by most, perhaps all, young stars either shortly before or, more probably, shortly after they initiate the burning of hydrogen into helium in their interior. Since it is natural to think of star formation as a process of collapse, or infall, the ubiquitous occurrence of an accompanying outflow comes as a bit of a shock. Now D.J. Axon and K. Taylor have described new optical spectra indicating very high-velocity winds in the most famous region of star formation in the heavens, the Orion Nebula (*Mon. Not. R. astr. Soc.* **207**, 241; 1984). Their spectra point towards the existence of a biconical pattern of outflow and suggest the presence of a substantial disc of molecular gas and dust grains close to a putative stellar source of excitation, located in the giant molecular cloud behind the nebula.

As a result of isolating the 6,300 Å wavelength forbidden transition of neutral oxygen, which is prominent in shock-excited regions but relatively weak in photoionized gaseous nebula such as the Orion Nebula, Axon and Taylor discovered a 'family' of six shock-excited objects which, in the plane of the sky, are located near a group of strong infrared-continuum sources discovered in the 1960s. The latter are believed to constitute a very young, but highly obscured, star cluster behind the Orion Nebula. The oxygen atoms display a very wide range of line-of-sight velocities extending from near 0 km s^{-1} (the velocity of the molecular cloud) to an approach velocity of nearly 400 km s^{-1}. When projection and other effects are taken into account, the implied wind velocities are of the order of 1,000 km s^{-1} — similar to that of winds from the most massive main-sequence stars in our Galaxy.

Taking into account previous observations of Orion, primarily at infrared and microwave frequencies, Axon and Taylor conclude that the most plausible model of the region is one in which a biconical protostellar wind originates from one (or more) of the massive young stars that are embedded in the molecular cloud, as in the figure. Although the star itself must be obscured from our view by an optically thick disc that lies, presumably, in its equatorial plane, the wind can escape from the polar regions. In their model, the shock-excited objects, detected by neutral oxygen emissions, are the result of the impact between the winds and gas blobs ('knots') located at the front face of the molecular cloud, just behind the Orion Nebula itself.



Model of biconical protostellar wind in the Orion Nebula. (From Mon. Not. R. astr. Soc. **207**, 246; 1984.)

These results bear on more general questions related to star formation and massive winds. Observations of many other regions of star formation indicate that bipolar or biconical outflow is the rule rather than the exception. Orion has now been reasonably securely added to this class. If the collimation is due to a disc of gas and dust located near the stars, as Axon and Taylor, like many others before them, suggest, then it is possible that at least the raw material necessary to make planets can be found in the vicinity of most young stars. Since not a single planet has been identified with certainty outside our Solar System, the implied ubiquitous existence of this raw material is of considerable interest.

Material which may be distributed in the form of a small disc with dimensions only about three times that our Solar System has very recently been seen directly (via scattered light at 2 μm wavelength) near the very young star, HL Tauri, by two different teams (S. Beckwith, B. Zuckerman, M. Skrutskie and H.M. Dyck, and S. Strom and G. Grasdalen, *Astrophys. J.*, in the press). However, it still remains to be shown how frequently a gaseous disc, as opposed to some other physical mechanism such as a magnetic field, is actually responsible for collimation of the winds that accompany star formation. □

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