nals. For example, probes can contact civilizations that are not listening, that is, those that do not have radio technology. Probes can be used to explore and colonize uninhabited systems.

Carl Sagan has argued that "perhaps [extraterrestrial intelligences] just don't care to strip-mine every site in the Galaxy". In his books, Sagan himself shows that all communication has costs; as he repeatedly says, the bare fact of a received signal from an alien civilization would change ours drastically. But the purpose of any communication is to change the knowledge of the person to whom the message is directed: to colonize a mind with memes (complexes of ideas). There is no fundamental distinction between colonization with memes and colonization with genes. But in the case of genes, it is at least possible to limit colonization to uninhabited systems. Meme colonization necessarily occurs in inhabited systems, and necessarily extinguishes other memes. It is necessarily imperialistic.

"Those engaging in radio searches like to argue that absence of evidence is not evidence of absence. I totally agree, but we have evidence: extraterrestrial intelligences are not here."

Davoust discusses at length the anthropologist Ben Finney's comparison of interstellar colonization with historical human migrations. Finney claims: "No specific migration has ever gone unchecked. Ecological barriers, the slowing or cessation of innovation, flagging motivation, or the opposition of those in the way of expansion have . . . stopped every . . . colonization movement so far." Finney infers that interstellar colonization would stop short of the entire Galaxy.

But Finney's own data indicate the opposite. The analogue of the ecological-innovation barrier is the lack of a suitable robot probe, and our own civilization is near to overcoming this. With a probe, there is no natural barrier to stop a colonizing species short of the entire Galaxy. By definition, there is no opposition of those in the way for the first intelligent species to evolve. Finney's data indicate that motivation flagged once the other three barriers made further expansion difficult. Finney's picture of the evolution of Polynesian society is exactly what John Barrow and I predicted (*The Anthropic Cosmological Principle*, Oxford University Press, 1986) would be the behaviour of a colonizing extraterrestrial intelligence: an *r*-strategy characterized by rapid expansion in numbers would be typical of those at the frontier, whereas a *K*-strategy characterized by fluctuations in numbers around an equilibrium would be typical of those in the interior.

Those engaging in radio searches like to argue that absence of evidence is not evidence of absence. (Davoust repeats this slogan at least twice.) I totally agree, but we have evidence: extraterrestrial intelligences are not here. We just have to interpret this fact. Most astronomers cling to a belief in extraterrestrial intelligence against the evidence because of a philosophical principle: the copernican idea that our place in the cosmos must be completely typical. But we know this idea is false. The Universe is evolving: the cosmic radiation shows that there was once a time when no life existed because it was too hot. Thus, our place is atypical in time. In particular there must be a first civilization, and it happens to be ours.

Davoust does not mention Brandon Carter's argument, based on the weak anthropic principle, for the nonexistence of extraterrestrial intelligence. This is unfortunate, because some of the most interesting new developments in particle physics use Carter's argument, which is derived from the fact that the time it took to evolve intelligence on Earth is within a factor of two of the lifetime of the Sun. Carter explains this approximate equality by assuming that the average time needed to evolve intelligence on an Earth-like planet is actually much longer than the lifetime of Sunlike stars. Biological evolution will cease when the star of an Earth-like planet dies, because the dying star destroys its planet. But the longer evolution can proceed, the more likely it is that intelligence will evolve. Thus the most probable time for the appearance of intelligence would be near the end of the time that evolution has had to operate on an Earth-like planet; that is, we expect approximate equality between the Sun's lifetime and the time needed to evolve intelligence. By making Carter's argument quantitative, S. Weinberg obtained an upper bound to the cosmological constant, whereas M. Shaposhnikov (*Modern Physics Letters* **59**, 2607; 1987) made a prediction of the Higgs boson mass. This prediction failed, but it is a fascinating thought that there may be a connection between the Higgs mass and the rarity of intelligent life in the Universe. Pursuing this idea would be far more scientifically productive than doomed-to-fail radio searches. The original French title of Davoust's book was *Silence au point d'eau*. Silence there will be. □

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Mosquitoes and backbiting

Len Goodwin

The History of Yellow Fever: An Essay on the Birth of Tropical Medicine. By François Delaporte. *MIT Press: 1991. Pp. 181. $30.25, £20.25.*

RESEARCH scientists are human, and like to receive credit for their discoveries. But when recalling the development of their hypotheses, they can forget or omit to mention whose ideas really triggered them off. François Delaporte has made a detailed epistemological study of the classic discovery of the mode of transmission of yellow fever, accounts of which differ between South and North America. Cuban historians affirm that Carlos Finlay deserves most of the credit, whereas in the United States Walter Reed and his team are the true heroes. Both, of course, made vital contributions, but Delaporte, by noting what they read and wrote, reveals some facts that lead him to give credit for the seminal ideas to four Britons — Patrick Manson, Ronald Ross, Herbert E. Durham and Walter Myers.

Finlay, who had Cuban, French and Scottish blood, was an ingenious observer who had advanced several theories for the transmission of yellow fever, such as that involving increased ammonia in the atmosphere. In 1880 he had access to two important North American documents, the 'Plymouth' and the Chaillé reports, which showed that a ship needed contact with a port for an outbreak of yellow fever to occur and that some time elapsed between exposure at a port and an outbreak on board — the germ was "not like the poison of smallpox, but is produced and developed outside the body". He was also aware through the British medical press of Manson's work in China, showing that microscopic larvae of filarial worms in a patient's blood taken up by a mosquito developed in the insect to an infective stage. Finlay made the important observation that the 'Culex mosquito' (*Aedes aegypti*) could bite 12 times and lay three batches of eggs before it died; it could therefore convey infective material from one person to another, leading him to put forward his theory of mosquito transmission of yellow fever. But numerous attempts at transference from a patient to human volunteers failed because he kept the mosquitoes for only 3–5 days between bites. Finlay was regarded as a crank and it was 20 years before he was vindicated.

By 1900, yellow fever had visited 100 US cities and had killed thousands of

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soldiers in the Spanish–American War. Reed's team was sent to Cuba in June; their main concern was with the bacterium *Bacillus icteroides*, claimed by the Italian bacteriologist Giuseppi Sanarelli to be the cause of yellow fever, and they spent a month disproving this hypothesis. But in mid-July they were visited by Durham and Myers from the Liverpool School of Tropical Medicine. These workers were familiar with the recent discoveries of Charles Laveran and Ross that the malaria parasite used the mosquito as an intermediate host, and took time to complete its cycle in the insect. They also knew from W. S. Carter in New Orleans that there was an interval of 2–3 weeks between primary and secondary cases of yellow fever, and in a report published in the *British Medical Journal* in 1900 they wrote: "This curious and somewhat prolonged interval is suggestive of a development of the infective factor in or about some agent or matter in the domicile. . .. The suggestion propounded by Dr C. Finlay of Havana some twenty years ago, that the disease was spread by means of mosquitoes hardly appears fanciful in the light of recent discoveries in ague convection. . .".

The US team took action immediately. On 1 August, Reed, James Carroll and Jesse Lazear went to Finlay and obtained the eggs of his Culex mosquito. Reed was recalled to Washington, and the first, not very well controlled trials with mosquitoes were made by Lazear. Carroll was bitten on 27 August by an insect that had been fed on a yellow-fever patient on the second day of illness and that had been kept for 12 days; he had a sharp attack of fever. The rest of the story is well known — Lazear was bitten accidentally and died, and Reed took charge and showed conclusively that the infective agent passed through a bacterial filter and took 12 days to reach the infective stage in the mosquito.

But then politics took over and the controversy that developed, ostensibly a dispute over scientific credit, actually reflected a struggle for power. The defeat of Spain had not brought freedom to Cuba; the Platt amendment, which placed the country under the safeguard of the United States, humiliated the Cuban people and opened their eyes to the meaning of US intervention — the United States had supplanted Spain. The names of Reed and Finlay became convenient symbols for a clash of values, and mythical, political versions of the discovery were spread through the media. The two official pictures painted to celebrate the discovery, by Estevan Valderrama in Cuba and by Dean Cornwell in the United States, depict not real events but fictions.

Delaporte's study provokes thought as to the real origins of successful ideas. The translation from the French is good and retains a Gallic flavour with few hiccups. But you can't help feeling sorry for Finlay. Eccentric, devious and a bit of a showman, he had the brilliant, original conception of the mosquito as a vector but had no way of seeing it as a second host. With a little more luck, or a little less care, he might have kept one of his mosquitoes for a few more days: medical science would have been advanced 20 years; Finlay would have the credit; a Cuban volunteer, not Lazear, would have been martyred; and there might never have been a Walter Reed Army Institute of Research in Washington DC. □

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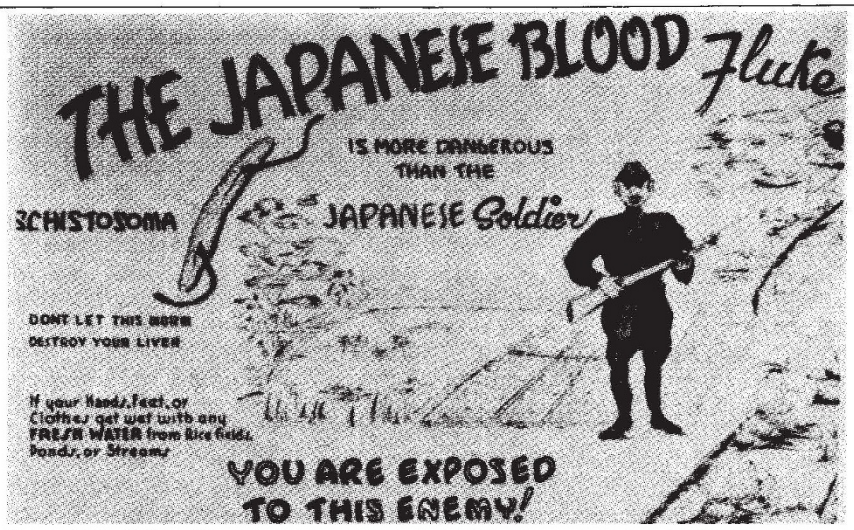

Second World War US Army poster warning of bilharzia — another debilitating tropical disease caused by parasitic worms. From *Bilharzia: A History of Imperial Tropical Medicine* by J. Farley. Published by Cambridge University Press, £40, $59.50.

Room at the top

John W. Galloway

The Outer Cycle: Women in the Scientific Community. Edited by Harriet Zuckerman, Jonathan R. Cole and John T. Bruer. *Norton: 1991. Pp. 350. $24.95.*

WOMEN feature much less prominently in science than men. This book, a collection of research papers and reviews, sets out to tell us why.

Of course, this lack of prominence is hardly peculiar to science — it is broadly true of most public and creative walks of life. But science at its best should surely subvert the established social order and lean towards enfranchisement. To find it apparently so reactionary in this respect is a bit disturbing. The phenomenon is a ready subject for sociological analysis, an opportunity that has here been enthusiastically embraced.

What exactly is the phenomenon that the authors explore and try to explain? One of their concerns is the consistent paucity of women in science's upper reaches. Women form only two to three per cent of the membership of any number of national academies of science (although in the United States the proportion is now rising and has reached a heady four-and-half per cent). And so it is perhaps not unexpected that the proportion of women Nobel science prize winners is about the same. The figure rises a little if you throw in the prizes for literature and peace. Further, the deficit cuts across social barriers. For example, in the United Kingdom there are few women Fellows of the Royal Society or women Judges. Yet Fellows represent a complete spectrum of social origins, whereas judges are drawn mainly from the better-off classes. So in this regard at least, women do seem to be sisters under their skins.

The other concern of the authors is that this low showing is not simply a result of not enough women entering science to begin with. The other day I visited a large Scottish cancer-research laboratory. Women clearly outnumbered men among the research staff. But the women tended to be young and the older staff tended to be men. In general, and this is largly the issue that the book addresses, women as a whole do progressively less well the higher up the scientific hierarchy you look — they publish less, achieve less and get paid less than their male counterparts. Similarly, in the United Kingdom, those educated at independent schools do better the further up they are in the legal hierarchy.

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