

associated with primary tissues of different sources and ages may limit the large scale use of our culture technique. The human coronaviruses, however, were initially only capable of growth in organ culture, but after a period of adaptation by this means they could be grown on standard continuous cell cultures. It may be that a period of adaptation on foetal liver organ cultures could produce the same effect for the virus of serum hepatitis.

This work is supported by grants from the World Health Organization (Virus Diseases), Pfizer Ltd, the Medical Research Council and the Department of Health and Social Security. We thank Dr H. E. M. Kay and his staff at the Royal Marsden Hospital for help with material.

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Received December 23, 1971; revised January 4, 1972.

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Fig. 1 The naked hamster.

## New Semi-dominant Mutation in the Syrian Hamster

SEVERAL different mutations causing hypotrichosis are well known in both rats and mice<sup>1,2</sup> but none have so far been described in the hamster.

In November 1968 a spontaneous semi-dominant mutation causing hypotrichosis occurred in a litter of hamsters (*Mesocricetus auratus*) maintained by one of us (M. K. W.). A single male animal with a "sparse coat" was noted among normal littermates. The mating was repeated twice, but no further abnormal young were observed.

The mutant male was mated to normal females of the same strain, and approximately 50% of his offspring had a sparse coat. The original male was then mated to his sparse-coated daughters, and in May 1969 the first hairless young were observed. Breeding data given in Table 1 indicate that the condition is caused by an autosomal semi-dominant mutation resembling "naked" in the mouse. Because of this resemblance,

the name "naked" with the gene symbol *N* is proposed for this mutant.

Homozygous naked hamsters are completely devoid of hair apart from a very short and soft down (Fig. 1). Vibrissae are present but are soft and tend to be bent. Naked hamsters which reach weaning age are viable, though they have a reduced body weight, and are probably more sensitive to environmental factors than normal hamsters. Both male and female homozygotes are fertile though the few females that have produced young have so far been unable to rear them. The breeding data shown in Table 1 indicate that there is about a 30% deficiency of homozygotes at weaning, thus the effect on viability is much less marked than the naked mutant in the mouse<sup>3</sup>.

The onset of the condition has not been studied in detail, but naked individuals are identifiable at about 14 days of age, and there is no evidence of hair growth from then onwards. Heterozygotes are identifiable from about 14 days of age onwards by their sparse coat, which is particularly deficient on the ventral surface, though the nature of the deficiency has not yet been determined. Microscopic examination of hair clippings from heterozygotes reveals no obvious deficiency in either type or diameter of the hairs. Viability and breeding performance of the heterozygotes are normal, though there may be a slight reduction in body weight.

According to Kirkman and Algard<sup>4</sup> the epidermis of the hamster is responsive to a variety of chemical carcinogens, producing the same types of tumours as other laboratory animals. The naked hamster may prove useful in studies of such neoplasia as it eliminates the need to shave the hamsters before applying the carcinogen.

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- <sup>1</sup> Robinson, R., *Genetics of the Norway Rat* (Pergamon Press, London, 1965).
- <sup>2</sup> Green, E. L., *Biology of the Laboratory Mouse* (McGraw-Hill, New York, 1966).

Table 1 Breeding Results

Mating		No. litters born	Young weaned		
♀	♂		+/+	N/+	N/N
+/+	× +/+	5	38	—	—
+/+	× +/N	22	95	81	—
+/N	× +/N	26	60	98	39
+/N	× N/N	15	—	61	42
+/+	× N/N	8	—	65	—
N/N	× N/N	2	None	None	None



<sup>3</sup> Gruneberg, H., *The Genetics of the Mouse* (Martinus Nijhoff, The Hague, 1952).

<sup>4</sup> Kirkman, H., and Algard, F. T., *The Golden Hamster* (edit. by Hoffman, R. A., Robinson, P. F., and Magathas, H.) (Iowa State University Press, Ames, 1968).

## Phloem Physiology and Protective Sealing Mechanisms

RECENTLY Dr Spanner, in a review "Transport in the Phloem"<sup>1</sup>, described certain aspects of my work<sup>2</sup> but reinterpreted my reported observations in accordance with his scheme to explain the mechanism of phloem transport. His description that the massage pretreatment applied to *Ricinus* stems in my experiments was "gentle" is not correct. The theory behind the method is to compress phloem sieve tubes containing sap under pressure of about 10 bar, so as to make it surge rapidly along the tubes; manual pressure was applied firmly though not so as to produce detectable damage during the application. Similarly the brief application of quite strong pressure is needed to stop exudation completely.

He describes as "contrived" my proposals for the sieve plates as control systems, governed by sealing and anti-sealing mechanisms, in view of the "obvious" alternative that the pretreatment merely disrupts the natural system. Yet callose deposition, one accepted sieveplate sealing mechanism, is known to be reversible<sup>3</sup>. Further evidence supporting my theory will be published shortly<sup>4</sup> in which illustrations of the complexity of the sealing system are presented. For example, a cut may exude, then stop completely for a time, begin again without interference and finally cease (the "re-bleed phenomenon"), which defies a simple explanation.

A further important finding is that *Ricinus* plants may exude considerable amounts of sap without pretreatment if they are grown sufficiently vigorously<sup>5</sup>. Such exudation can also be stopped by the temporary application of pressure. More than 3 ml. of phloem exudate has been collected from a single 5 mm cut in an unmassaged plant about 50 cm tall (Baker and Hall, unpublished result), whereas the corresponding maximum on record from a single cut in a vigorous massaged plant is now more than 10 ml. This rapid collection of phloem sap from plants in amounts too large to be accounted for by turgor release from single cells is a most serious obstacle to acceptance of an "occluded pore" mechanism. Not only is massage pretreatment inessential, but the amounts collected above and below cuts are in roughly equal amounts as one might expect of a simple pressure release. We should expect the output from a pumping system between leaves and roots to be polarized and produce dissimilar amounts.

It is surprising that the palm sugar industry, which produces thousands of tons of sugar annually from phloem exudate<sup>6</sup>, has been so ignored by plant physiologists. It was from Bose's account<sup>7</sup> of the collection system that my successful massage pretreatment was devised. There are local variations in technique but my enquiries into these have been curtailed by the Pakistan troubles. But an eyewitness, Mr Syed Hadiuzzaman of Dacca University, has described a method for the collection of *Phoenix* phloem sap without massage pretreatment, by "V" shaped incisions alone. Experts incise the upper trunks after removal of a series of leaf bases, which otherwise protect the tissues from injury. Cuts are renewed every few days and provide several litres of sap (about 14% sucrose) per day. Thus it seems that the mechanisms involved are similar in *Phoenix* and *Ricinus* and pretreatment by "disruptive" massage is not essential for exudation from either.

In terms of evolutionary development it can be argued<sup>3,4</sup> that such a rich potential food supply as phloem has become adapted to seal rapidly to withstand the onslaught of predators. Probably this has been largely achieved. Exceptions are small insects, for example aphids, which probably tap the sieve tubes at rates too slow to trigger the sealing response<sup>3</sup>, and

larger animals, for example the sapsucker, a bird which makes a succession of wounds so bypassing blocked tissues<sup>8</sup>. If man can succeed where most animals would fail, large resources of sugars, amino-acids and proteins should become available "on tap" from otherwise useless vegetation.

A serious problem in studying phloem physiology has been the difficulty of establishing if tissue is still capable of conduction after being treated experimentally. The sensitivity of the tissue to manipulation is now beyond question. If sieve plates have become adapted to seal rapidly, this probably explains why considerable efforts, using largely empirical techniques, to demonstrate "open" sieve plate pores have produced inconclusive or controversial results.

If the explanation proposed is correct the most efficient way to prove the mechanism of phloem transport is to elucidate further the physiology of the sealing mechanisms with a view to controlling them. We now have plants in which the system can be studied systematically (*Yucca*<sup>9</sup>, *Phoenix*, and *Ricinus*) and only when the sealing mechanisms are fully understood will it be possible to demonstrate convincingly the ultra-structure of sieve plate pores in the natural state.

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Received August 18, 1971.

DR SPANNER writes: I am grateful to Dr Milburn for correcting the wrong impression given by my use of "gently". Further thought on his interesting results inclines me to greater sympathy with his view. My reaction had been to conclude that his metabolic sealing-antisealing mechanism had involved P-protein, a conclusion against which strong arguments have been raised<sup>1</sup>. If he is thinking rather of callose formation I find his suggestions plausible. But in that case they seem to me to lose their relevance to the controversy about whether the sieve plate pores are normally occupied by P-protein, as my Fig. 1 indicated<sup>10</sup>. One only has to assume that the usual brevity of exudation hinges on the formation of callose rather than on the filling of the pores with P-protein to see this. For I believe that the electro-osmotically significant P-protein in the pores does not require very much force to displace it. (I am not thinking, of course, of massive protein plugs in the lumen.) Consequently the situation at the cut surface is dominated by the huge turgor gradient, and pays little attention to whether the pores are normally open or occluded. That exudation from the two surfaces of the cut should be more or less equal (not always found incidentally<sup>11</sup>) is understandable. Similarly the re-bleed phenomenon does not seem so hard to explain; pressure build-up might dislodge a proteinaceous accumulation at a callose-constricted sieve plate.

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Received September 5, 1971.

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