work to yield a saturation based on flowing volume only. Figure 1 of the present report shows the relative permeability curves obtained for increasing nonwetting phase saturation. Curves A and C are the wetting and nonwetting phase relative permeability, respectively, plotted on the basis of total volume; curves B and D are the wetting and nonwetting phase relative permeabilities plotted on the basis of flowing volume. In this graph, as in all relative permeability graphs obtained from the network model, the nonwetting phase curve based on total volume seems too far to the left when compared with curves for sandstone. This difference is believed to be a result of the small number of tubes in the model (about 400) compared to the number of pores in a sandstone sample used for relative permeability measurements. Curve E in Fig. 1 is typical of sandstones and is shown here for comparison.

It is apparent from Fig. 1 that the wetting phase relative permeability curve is about the same when based on total volume of wetting phase present or on flowing wetting phase. Dead-end volume is not an important influence on this curve. The factors governing the shape of the wetting phase relative permeability curve can be summarized in descending order of importance as follows. Wetting phase permeability decreases with increasing nonwetting phase saturation because: (i) large pores are removed from the network that carries wetting phase; (ii) the path of the wetting phase becomes more tortuous; and (iii) some wetting phase is trapped in dead-end pores which are not available as flow paths.

Figure 1 shows that nonwetting phase permeability is greatly influenced by dead-end volume. When plotted on the basis of saturation of flowing volume, the nonwetting phase relative permeability curve is almost a 45° line, which indicates that nonwetting permeability is almost proportional to flowing phase saturation. The data of Fig. 1 are replotted in Fig. 2 to show the fraction of a given phase that is in dead-end tubes at any total saturation of that phase.

The factors that govern the nonwetting phase relative permeability can be summarized in descending order of importance as follows. Relative nonwetting phase permeability increases as the saturation of that phase increases because: (i) more pores become available to carry nonwetting phase; the additional pores are progressively smaller as the nonwetting phase saturation increases; (ii) less nonwetting phase is in dead-end pores as its saturation increases; and (iii) the flow paths become less tortuous.

This study of the influence of dead-end pore volume suggests several experiments on real porous media to check the predictions of the network model. Handy's (*5*) two-tracer miscible displacement study seems to offer the best experimental procedure. In Handy's experiment, both a fast diffusing and slow diffusing tracer are in the displacing fluid. The dispersion of the slow diffusing tracer is taken to be a measure of dispersion due to mixing only; dispersion of the fast diffusing tracers is a measure of both mixing and diffusion. Dead-end pore volume will increase loss only of the fast diffusing tracer from the displacement front. The two-tracer displacement tests should be carried out with two immiscible phases present and two tracers in each phase. Dead-end volume will cause a separation of the tracer concentrations. By repeating the experiment at different saturations, it should be possible to obtain a measure of dead-end volume as a function of saturation (*6*).

IRVING FATT

Miller Institute for Basic Research in Science and Department of Mineral Technology, University of California, Berkeley

References and Notes

1. I. Fatt, "The network model of porous media," *Trans. Am. Inst. Mining Met. Petrol. Engrs.* **207**, 144 (1956).
2. W. Rose, *Illinois State Geol. Survey Circ. No. 237 (1957).*
3. I. Fatt, *J. Phys. Chem.* **63**, 751 (1959).
4. ———, *Science* **131**, 158 (1960).
5. L. L. Handy, *Trans. Am. Inst. Mining Met. Petrol. Engrs.* **216**, 382 (1959).
6. Acknowledgement is made to the donors of the Petroleum Research Fund, administered by the American Chemical Society, for support of this research.

14 August 1961

Ancient Agriculture in the Negev

I wish to address myself in the main to the problem of the *teleilât el 'anab*, the gravel-stone heaps and mounds, which were referred to in an article in *Science* by Evenari *et al.* (*1*).

In two articles (*2, 3*) I have detailed my position on the possible function of the *teleilât*. Briefly, I maintain that the gravel-stone mounds and heaps (called "conical heaps and ridge mounds" in my articles) were the result of excavations of pits and ditches in which mainly vines were planted, and that the "flower-pot" heaps (not illustrated or described in the article by Evenari *et al.*; see *2*, pp. 24–26) were built to put a sterile area to the same use. Over the years, the action of the elements has filled up the excavations, leaving only the stone heaps in evidence.

My main point in connection with the pits and ditches is that they formed collection basins *primarily* for rain water. The undisturbed surface surrounding the pit or ditch served as a small runoff area which supplied the necessary supplementary supply of water for the vine planted in the pit or ditch. If hand watering were necessary, as might be the case in the event of a severe drought, any water applied would be concentrated near the roots of the plant. This would be the case regardless of where the vines were planted.

In reference 28 of their article (*1*), Evenari *et al.* make rather short shrift of my evidence (*2*) and make it appear that I considered that the only water the vines received came from hand irrigation from water stored in cisterns. I have written (*2*, p. 27): "We must *not*, above all, think that vines were planted *within* the mounds, but rather in pits or trenches. In other words, the conical heaps and ridge mounds came about as a result of digging holes into the ground in which vines, and in some cases, trees were planted. . . . Aside from enabling the farmer to cultivate his vine properly, each basin or trench, which could easily have been partitioned into a series of basins, would hold winter rains and irrigation water near the roots. . . . In addition to hand irrigating the vines from water stored in cisterns, it was also possible to run small channels on an oblique line from pit to pit or trench to trench, which would catch a portion of the slope runoff during the rainy season and direct it into the basins. It is for this reason, I believe, that one never finds ridge mounds following the contour of a slope but running down it; if they had been raised along the contour, runoff would have been denied to the areas below. Furthermore, because of the heavy downpours which often occur in this region, excess runoff could have been trapped at the base of a slope and channeled into a cistern where it

Table 1. Yields of barley and wheat in Bedouin agriculture in the Negev.

Description of yield	Yield (kg/dunam)	
	Barley (sown: 6 to 7 kg/dunam)	Wheat (sown: 5 to 7 kg/dunam)
Excellent	200	100
Good	70–80	50
Satisfactory	40–60	30–50
Poor	0–20	0–20

was stored for later use or directed into the terraced fields."

As for the number of heaps per hectare, Evenari *et al.* once more misread my material. I have stated (*2*, p. 22) that the number of "flower-pot" heaps, the only kind which would require hand irrigation exclusively, only number about 50 (*sic*) to the hectare, whereas the conical heaps, which represented the residue of excavated pits (planting holes), number about 600 to the hectare.

In order to support the above theory, I cited possible analogies, ancient and modern, from the Mediterranean and European regions; of particular significance were the citations from Apulia, southern Italy, where aerial photographs revealed ancient patterns similar to those found in the Negev. I also pointed out that when settlement workers in the Negev settlement of Ashalim planted eucalyptus trees on a hill top of their community, they dug pits into the *hammada,* placing the stone to one side of the excavation. As a result, a pattern of *teleilât* evolved not unlike the ancient.

With regard to the stone heaps in the Wadi Isderiyeh which do not run down from the slopes but are quite close to the wadi bed and run parallel to it, it is possible that they are leftovers of a relatively recent excavation by the Mandatory Government of Palestine for a telephone cable. But, I am puzzled over the fact that there are *three* rows of stone heaps, each row 2 meters or more apart (*2*, Fig. 5). Was more than one cable laid? Be that as it may, one thing is certain: an excavation of a ditch, whether for vines or telephone cables, results in a pattern of stone heaps very much like the kind that are seen on the slopes. (For further details regarding soil structure of the *hammadas,* adaptability of vines to stony soils, leaching out of salts, water storage, and arguments against the efficient-runoff theory of the Hebrew University and Nelson Glueck, see *2, 3*.)

Generally speaking, it is often difficult for scientists to think that the ancient farmer in a submarginal region would cultivate the worst soil when they never used all the "good" soils available. Aside from hydrographical reasons which at times makes it impractical to plant vines or trees in wadis in which heavy floods could uproot them and carry them off, we simply do not know enough about the terms under which the ancient farmer held tenure of his property. In other words, could the ancient farmer in the Negev have planted wherever he wished, or was he restricted to land which he owned or was assigned? The Byzantine papyri discovered at Nessana make it abundantly clear that every piece of property of the Nessanites was recorded at the district office at Elusa (modern Khalasa) and that taxes had to be paid—virtually guaranteed, regardless of sale or transfer—on that property. It is very likely that the ancient farmer, like most farmers over the world, attempted to put every bit of his land to productive use, even the "worst soils" by modern standards. Take an example from southern Italy: Why should the ancient farmer in Apulia have gone so far as to cut 1 cubic meter out of the living rock in order to create a container for his vines or tree? Yet he did it, and it is still being done, in spite of the fact that the land is, as stated by one investigator, impenetrable to the roots of trees and useless for orchards and vineyards.

Finally, the article of Evenari *et al.* and its subtitle give the reader the impression that until the recent experiments by the Hebrew University, agriculture had not been practiced in the Negev since the Byzantine period, some 1300 years ago. Again, I feel that this is quite misleading. Bedouins have been farming the Negev, even the very area of the experimental farms, for many years, and though lacking the energy and security of some of their predecessors, Bedouin farmers use virtually every method known to the ancients (particularly runoff farming) for supplementing a limited supply of water. Bedouin experience, in my opinion, is a far better index to ancient practices than is that of the experimental farmer.

Evenari *et al.*, without stating the rate of seeding, mention the "astonishing" yield of 125 kilograms of barley per dunam achieved on "selected parts" of the experimental area (*1*, p. 995). My own data, gathered from Bedouins who have long cultivated the wadi beds of the Negev, are, I believe, the proper index for the calculation of ancient yields—not modern capabilities—in the region. I have recorded the rates of seeding and yields shown in Table 1, making due allowance for exaggerations (*4*).

PHILIP MAYERSON
Department of Classics,
New York University, New York

References and Notes

1. M. Evenari, L. Shanan, N. Tadmor, Y. Aharoni, *Science* **133**, 979 (1961).
2. P. Mayerson, *Bull. Am. Schools Oriental Research* No. 153 (Feb. 1959).
3. ——, *ibid.* No. 160 (Dec. 1960).
4. A full report of my study of the ancient agricultural practices of the region will be found in a recently published monograph, entitled *The Ancient Agricultural Regime of Nessana and the Central Negeb* (British School of Archaeology in Jerusalem, London, 1961).

11 April 1961

Mayerson's discussion deals with two main points: (i) his explanation of *teleilât el 'anab* and (ii) the place of the Bedouin in the agricultural history of the Negev. We will reply briefly to the two points.

In presenting his variation of the century-old "grapevine" theory explaining the purpose of the gravel mounds and strips, Mayerson rejects our proposal that these mounds and strips were the result of clearing the surface soil of stone, in order to increase the rates of runoff from the hillsides with their poor soils and so produce sufficient runoff water for the farm units in the good bottom lands.

Mayerson infers that the gravel mounds and strips resulted from excavating pits and trenches on the hillsides for growing vines or trees or both. (For brevity we refer to Mayerson's system as "planting-pits" and "planting-trenches.") However, this speculation is not substantiated by any evidence to be found in the field. Why are there no remnants or even a semblance of remains to be found on the hillsides of one of these hundreds of thousands of planting-pits and planting-trenches that Mayerson presumes to have existed, while, on the other hand, the gravel mounds and strips have been preserved in excellent condition? If these features had ever existed cheek by jowl with the gravel mounds and strips, some outline or indication of their former existence must be found in the field today, since our own experience, as well as that of all recognized investigators in the Negev Desert, shows that man-made features leave their imprint on the desert surface for millennia. During our extensive field surveys we have not met with any evidence justifying the speculation that planting-pits and planting-trenches existed in conjunction with the gravel mounds and strips.

Mayerson suggests that these planting-pits and planting-trenches were used as "collection basins *primarily* for rain water" for growing vines or trees or both on the hillsides. However, Mayerson has failed to analyze the amount of water that would have been available to these vines if the planting pits worked as he suggests. The analysis is as follows. The total annual rainfall is 100 mm. The estimated effective annual rainfall (allowing for evaporation from soil surface, light showers of less than 3 mm, and runoff of 20 percent into planting-pits) is 50 mm, and this is probably a high estimate. This works out to an effective rainfall of 500 m^3 per hectare. The number of vines (that is, mounds) is 600 per hectare (Mayerson's figure). This means that about 0.8 m^3 of water would be available for the growth of each vine.

This 0.8 m^3 is the maximum quantity of water each vine could have received in the very best of years and this quantity is absolutely insufficient to ensure the survival of mature vines, let alone produce grapes. In the Gaza area, where the annual rainfall is 400 mm, the unirrigated vines are grown on sandy soils with 400 to 500 vines to the hectare, and each vine requires 5 to 6 m^3 of water (after allowing for evaporation from the soil surface) to produce 4000 kg of grapes per hectare. In the Lydda area irrigated vines are grown (1200 vines to the hectare) and each vine also requires 5 to 6 m^3 of water (after allowing for evaporation and irrigation efficiencies) to produce 12,000 kg of grapes per hectare.

Mayerson could now suggest that the ancient farmers had special secret varieites of vines or trees (unknown to modern science) that could grow in soils of questionable value (Mayerson's admission) and with only these subminimum quantities of water. But this he cannot do since he "feels that a distortion arises from viewing the Nabataeans and Byzantines of the Negev as 'scientific' and efficient agronomists" (*1*).

We hope that these figures will once and for all close the discussion as to the possibility of collecting sufficient water on the hillsides for growing vines on the hillsides.

Now we come to the most serious contradiction resulting from Mayerson's theory. If the planting-pits and planting-trenches were used *primarily* for the collection of runoff water for the hillsides with their poor soils, then they would have used all the available runoff on the slope and prevented any runoff from reaching the cultivated bottom-land fields with their good soils and also would have deprived the cisterns of their runoff water! This is untenable. The bottom-land farm units and the cisterns existed and were undoubtedly dependent on runoff from the hillsides, a fact contradicting any suggestion that the farmers renounced the opportunity of cultivating the good bottom lands in favor of utilizing the poor, saline, stony hillsides of questionable agricultural value.

Finally, in objecting to our theory, Mayerson refers to unpublished information he received, of a series of experiments carried out by the Hebrew University (by us) and the Soil Conservation Service. These experiments, Mayerson maintains, showed that "undisturbed hammada gave much more runoff than the adjoining piece of ground that was bared of its upper cover. The exact percentages have not been published, but they range from 100%–200%" (*2*).

This statement is false. Mayerson must have been misled by an unreliable "grapevine" informant. We cooperated on the planning of these experiments, but unfortunately they were not executed under sufficiently controlled conditions, and hence we did not feel justified in publishing the results as being accurate enough and final. Mayerson's somewhat unconventional method of fact finding and publication now forces us to submit some of the preliminary results for whatever they are worth. These simulated rainfall experiments showed that the impermeable crust was formed sooner on the bare plots than on the stone-covered plots and this will result in increased rates of runoff from the bare plots. We are continuing with these experiments and will publish all the results as soon as they are completed.

Turning now to the place of the Bedouin in the agricultural history of the Negev. Mayerson states that our article "and its subtitle give the reader the impression that until recent experiments by the Hebrew University, agriculture had not been practiced in the Negev since the Byzantine period some 1300 years ago," and in his opinion "Bedouin experience . . . is a far better index to ancient practices than is that of the experimental farmer." We stated in our article: "During the Nabataean-Roman-Byzantine period, desert agriculture reached its peak of development. After the Arab conquest, the ancient desert agriculture slowly disintegrated, and the Bedouins of the area at best merely utilize dilapidating old systems for patch cultivation." We stand by this statement and do not credit the Bedouins with being more than nomadic patch cultivators, whose primitive, backward agriculture cannot be used as an index for understanding the ancient settlers who established a permanent, intensive agriculture in the desert.

Palmer, who, unlike Mayerson, lived with the Bedouins and spoke a number of their dialects, can be regarded as an expert in this field. He has aptly summarized their role in the area and states: "The Arabs do occasionally practice agriculture, if sowing a little corn in a roughly ploughed field and leaving the irrigation to chance, can be so called, but it never occurs to them to take advantage of the works left them by their former owners of the soil. . . . By Arab I mean the Bedawi, the typical son of Ishmael. . . . The sympathy wasted already on the Red Man of North America warns me that I am treading on delicate ground, but I must nevertheless state my belief that the 'noble savage' is a simple and unmitigated nuisance. To the Bedawi this applies even more forcibly still, for wherever he goes he brings with him ruin, violence and neglect. To call him the 'son of the desert' is a misnomer; half of the desert owes its existence to him and many a fertile plain from which he has driven its useful and industrious inhabitants, becomes in his hands, like the 'South Country'—a parched and barren wilderness" (*3*).

Although this was written about a century ago, we agree fully with Palmer's observations and maintain that no investigator on the Negev has yet regarded the Bedouin as a civilization that contributed to the agricultural development of the Negev.

Mayerson submits "guestimates" of the barley crop of the Bedouin, and is not astonished that we reaped 125 kg of barley per dunam in a drought year of 40 mm rainfall. However, the Bedouins of the area *were* astonished, since they had a complete drought year and did not reap 1 kg per dunam. The purpose of our experimental farm is to replace these popular "guestimates" with scientific data.

M. EVENARI, Y. AHARONI
Hebrew University, Jerusalem, Israel
L. SHANAN
Tel Aviv, Israel
N. TADMOR
Ministry of Agriculture of Israel

References

1. P. Mayerson, *Bull. Am. Schools Oriental Research* No. 153 (Feb. 1959).
2. ———, *ibid.* No. 160 (Dec. 1960).
3. E. H. Palmer, *The Desert of the Exodus* (Cambridge, England, 1871).

31 May 1961

Formation of the Periostracum in Mercenaria mercenaria

Abstract. A series of histochemical tests performed on the mantle of the northern quahog *Mercenaria mercenaria* L. suggest that the epithelium of the inner surface of the first fold and the underlying secretory cells function together in the formation of the periostracum in this mollusk. The secretory cells supply a phenolic substrate which, when oxidized, provides quinones capable of tanning the periostracum.

A row of columnar epithelial cells along the inner surface of the outer—that is, first—fold of the mantle of bivalve mollusks is regarded as being functional in the secretion of the periostracum (*1–4*). Our studies indicate that a similar group of epithelial cells in the bivalve, *Mercenaria mercenaria* L., also functions in the formation of the periostracum (*5*), although a conflicting report states otherwise (*6*). The periostracum of bivalve mollusks consists largely of a quinone-tanned protein, but the nature of the tanning process is obscure. Our histochemical tests on the mantle of *Mercenaria mercenaria* suggest that material formed in secretory cells underlying the inner epithelium of the first fold is a precursor of the tanning agent in this organism. A summary of the tests and procedures follows.

Quahogs were dug from an intertidal flat in Delaware Bay, off Cape Henlopen, Delaware, fixed in Bouin's solution within an hour after collection, and tested with Millon's reaction (*7*) and the argentaffin reaction (*8, 9*). Millon's reagent reacts with proteins containing large amounts of phenolic amino acids, principally tyrosine, while the argentaffin test has been used extensively to detect polyphenols involved in the tanning of proteins (*4*).

Paraffin sections cut transversely from the mantle edge were deparaffinized and treated with Millon's reagent at room temperature until maximal color developed (approximately 6 hours). Similar sections were treated with ammoniacal silver nitrate (Fontana's solution) (*8*) for 18 hours at 37°C and counterstained in 1-percent aqueous neutral red for 30 seconds.

A test for a polyphenol oxidase (*10*) was carried out on whole mantles from quahogs taken off Slaughter Beach, Delaware, a few miles north of Cape Henlopen. The clams were refrigerated (5°C) overnight and then removed from the shells while they were still alive. Whole mantles were treated with 10-percent neutral formalin for 1 hour at 22°C to harden the tissues. They were then incubated for 13 hours in $0.0056M$ 3,4-dihydroxyphenylalanine (DOPA) in $0.1M$ phosphate buffer at pH 7.4. The DOPA solution was changed after the first hour of incubation.

After the mantles were incubated in the DOPA solution, pieces were cut from the mantle edge (see Fig. 1), fixed for 24 hours in Bouin's solution, and imbedded in paraffin. Sections were counterstained in 1-percent aqueous neutral red solution. Control mantles were treated in the same manner except that they were incubated only in phosphate buffer.

The inner epithelium of the first fold, as seen in Table 1, although not reacting with either Millon's reagent or ammoniacal silver nitrate of the argentaffin reaction, did give a positive DOPA oxidase reaction, particularly near the opening of the periostracal groove. The underlying secretory cells did not react with DOPA, but their behavior in Millon's reagent and Fontana's solution showed that they contain a protein substance that has large amounts of phenolic groups.

The secretory cells have been demonstrated by several histochemical and histological techniques (*5*) and do not appear to be artifacts of fixation or staining. Ducts or extensions of these cells feed through the inner epithelium of the first fold to the periostracal groove.

Table 1. Summary of reactions observed in histochemical tests on *Mercenaria* mantle.

Tissue	Millon's	Argentaffin	DOPA oxidase
Inner epithelium of first fold	−	−	+
Underlying gland cells	+	+	−
Periostracum	+	+	+?
Outer epithelium of second fold	−	−	−

The periostracum gives intense Millon and argentaffin reactions, but there were no discernible differences between the DOPA oxidase-treated periostracum and the untreated control portions. Since the periostracum is a tanned protein, it is difficult to tell whether a positive DOPA oxidase reaction is due to the enzyme or the already-tanned periostracum.

The epithelial cells themselves do not seem to contain appreciable amounts of phenolic amino acids. Apparently the only available material capable of sup-

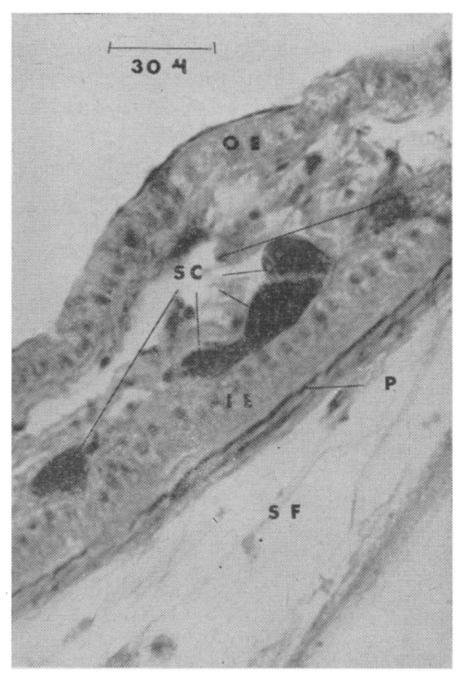

Fig. 1. Transverse section through the mantle edge of *M. mercenaria* showing the dark-stained response of five secretory cells to Millon's reagent. In this photograph the periostracum is adpressed to the epithelium of the inner surface of the first fold by a flap of tissue which arises from the outer surface of the second fold. *OE*, Outer epithelium of first fold; *IE*, inner epithelium; *SC*, secretory cells; *P*, periostracum; *SF*, flap from second fold.