be in liquid form and could be tapped simply by breaking the overlying permafrost without the decomposition of the clathrate (*3*).

Although a clathrate, rather than an ice, permafrost eliminates the need for an external heat source, it still requires a local heat source and thus the heat problem remains. In fact, since 150 cal is required to release 1 g of water from the clathrate compared to 80 cal required to release 1 g of water from ice, the heat problem is aggravated by the introduction of clathrate. What dispels the problem is not that the water is in the clathrate but that the water is already *liquid* at a temperature above its freezing point and no latent heat need be supplied at all!

Even if the only available water were bound up in the clathrate, in order to maximize the amount of water released the ratio of the clathrate to the rock matrix would have to be carefully adjusted at all points beneath the surface to just cool the surrounding rock to 0°C as the clathrate is exhausted. If a higher fraction of clathrate were present, once 0°C is reached, the necessary latent heat of decomposition would be obtained by the freezing of some of the liquid water just released, thus removing its contribution to the expected flow. In fact, even when excess water is already available in liquid form, clathrate decomposition would reduce the amount released because part of it would freeze.

Thus, regardless of the H_2O/CO_2 ratio, the presence of water in a CO_2 hydrate is detrimental to its release from an underground reservoir. The water must already be in liquid form if the release is to be rapid. As we have discussed (*3*), water trapped under a permafrost layer could be released by any meteorite impact or tectonic activity which ruptures the permafrost, without the necessity of heat transfer. Flow of water on the surface would not necessarily require that the martian atmospheric pressure be greater than the triple point of water, since at lower pressures a relatively thin skin of ice would form and prevent the water from boiling away (*3*).

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Polynesian Voyaging

At the end of the report (*1*) on the "Probable Fijian origin of quartzose temper sands in prehistoric pottery from Tonga and the Marquesas," Dickinson and Shutler conclude with the following point: "The implication for Polynesian origins is a continuing infusion of Melanesians into Polynesia from Fiji (and perhaps elsewhere). . . ." While I would not disagree with the generality of their statement, in the particular case in question I offer an alternative viewpoint based on work in Western Samoa.

In Western Samoa we have shown that if at an early stage in the sequence the inhabitants possessed pottery and then abandoned it, a few sherds from the early levels tend through subsequent disturbances to be incorporated in later deposits (*2*). The same phenomenon, for example, led some archeologists to initially misinterpret the duration of ceramics in the Tongan sequence by as much as 1000 to 1700 years (*3*). It has also led to a similar misinterpretation of the Marquesan evidence, where pottery has been dated to much later periods than is warranted (*2*). In my view, the handful of Marquesan sherds, most of them of local origin, plus a few imports, derive largely from secondary excavation contexts or, as at location M, from the surface. They are an indication that excavations in Marquesan sites have not yet sampled or, in one possible case at location M, have inadequately sampled those basal deposits with abundant pottery which lie at the early end of the Marquesan sequence and date before A.D. 300.

The alternative viewpoint I offer then is this: The few imported sherds from Fiji found in the Marquesas represent pots brought by the founding population, in contrast to those in Tonga, which are correctly interpreted as evidence of two-way contact at the earlier end of its prehistoric sequence, just as the pottery observed by the 19th-century explorers in Tonga attests to such contact at the late end of that sequence (*3*). This is understandable, for Tonga, Samoa, Futuna, Ellice, and other islands of western Polynesia are, together with Fiji, part of an historically well attested area of two-way voyaging in Polynesia (*4*). If the founding population of the Marquesas came from western Polynesia, as is generally agreed, the geological evidence that some of the pottery they brought with them was imported from Fiji is a further indication that this pattern of voyaging goes back to some point before A.D. 300. That should occasion no surprise, as it was probably from Fiji around 1100 to 1200 B.C. that founding populations moved into and rapidly colonized the adjacent western Polynesian island groups of Tonga, Samoa, and Futuna. Thereafter, there is evidence for imported adzes in contexts predating 500 B.C. in Tonga, indicating contact with Samoa or some other island of western Polynesia across the andesite line (*5*). In addition, Davidson and Shutler review other evidence from Samoa that supports a continuation of contact with Tonga and Fiji during the last 1000 years (*6*). On this basis I conclude that we are now fairly close to demonstrating archeologically that two-way voyaging in one of the two areas of Polynesia where it was known historically has been maintained since the time of settlement nearly 3000 years ago. In one case it led to the carrying of items exchanged in that contact to a new homeland in the Marquesas before A.D. 300.

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