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### Title

Nucleosynthesis in barium stars

### Permalink

<https://escholarship.org/uc/item/6bj5k87v>

### Journal

Nature, 310(5979)

### ISSN

0028-0836

### Author

Trimble, Virginia

### Publication Date

1984-08-01

### DOI

10.1038/310633b0

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Peer reviewed

## Photosynthesis

# Novel carbon supply on land

from Peter D. Moore

AN adequate supply of carbon is a fundamental need of all autotrophic organisms, and not surprisingly, an intriguing range of techniques have evolved to satisfy it. A number of aquatic plants have been reported to use a carbon assimilation pathway of the type normally associated with desert succulents, named crassulacean acid metabolism (CAM; see Moore, P. D. *Nature* **304**, 310; 1983), which permits accumulation of dissolved CO<sub>2</sub> and bicarbonate ions during the night, when there is a better supply than in daylight. The CO<sub>2</sub> supply in the dark ocean waters is tapped by chemolithotrophic microbes which obtain energy from the organic rain of dead material sinking from the euphotic zone (Karl, D. M., Knauer, G. A., Martin, J. H. & Ward, B. B. *Nature* **308**, 54; 1984). Terrestrial plants obtain their carbon from the atmosphere as gaseous CO<sub>2</sub>, but often it is in such short supply that it limits the rate of photosynthesis. When the light supply is adequate, the rate of photosynthesis increases linearly with increasing CO<sub>2</sub> from 0.03 to about 0.1 parts per 10<sup>6</sup> (Hall, D. O. & Rao, K. K. *Photosynthesis*, Edward Arnold, London). Obviously, therefore, it would be a selective advantage if a plant could locate alternative supplies of CO<sub>2</sub>. What about the abundance of CO<sub>2</sub> released in the soil by decomposer and root respiration; would it not be advantageous for plants to supplement their atmospheric supply with uptake of this resource through their roots?

A plant which has adopted this strategy is described on page 694 of this issue of *Nature*. It is *Stylites andicola*, a member of the same pteridophyte family as the quillworts, *Isoetes*. *Stylites* grows on decomposing peat in the Andes, a carbon reserve which is definitely worth exploiting. In the opinion of Keeley, Osmond and Raven, who have examined the species both anatomically and physiologically, the thick cuticle which covers the aerial parts of the plant is essentially impermeable to CO<sub>2</sub> and, since there are no stomatal pores, CO<sub>2</sub> must enter the plant entirely through the roots. This fact was confirmed by exposing the root system to labelled <sup>14</sup>CO<sub>2</sub> and showing that the rate of CO<sub>2</sub> incorporation was up to 50 times as fast as during the exposure of green tissues.

Ecologically, it is understandable that this type of adaptation should be found in a habitat where there is an accumulation of organic material in the soil and also where a seasonally fluctuating water table stimulates the respiratory activity of microbes and generates a rich supply of CO<sub>2</sub>. Other peatland plants need to be

examined for this root uptake of CO<sub>2</sub>, though none is likely to be as dependent as *Stylites* with its lack of stomata.

Keeley *et al.* also observe that, as in other members of the Isoetaceae, there is a diurnal acid metabolism of the CAM type. In the genus *Isoetes*, this characteristic is associated with an aquatic or seasonally aquatic habitat and becomes less marked when the plant is taken out of water (Keeley, J. E. *Am. J. Bot.* **69**, 254; 1982). *Stylites*, it seems, gains advantage from CAM in a rather different way. The authors imply that diurnal variation in acidity was observed in this species only in the aerial chlorophyllous tissues and it does not appear to be associated with dark short-term carbon assimilation in the

roots. A lot more needs to be known about carbon transport in the plant before this can be adequately understood.

The evolutionary significance of the discovery is considerable, particularly since it may offer some clues to the initial colonization of land by plants. Were the early colonists, like some modern aquatic plants, able to take up CO<sub>2</sub> through their roots? And was this capacity lost with the development of stomata? How widespread was CAM among primitive land plants, and what was its adaptive significance? Perhaps it was a hangover from aquatic environments and was later deactivated in what Van Valen (*Evol. Theory* **4**, 143; 1979) has called the 'switchback evolution' to which CAM/C4 strategies have been subjected in the course of geological time. A more detailed study of *Stylites* should throw light on at least some of these questions. □

Peter D. Moore is a Senior Lecturer in the Department of Plant Sciences, King's College, 68 Half Moon Lane, London SE24 9JF.

## Astronomy

# Nucleosynthesis in barium stars

from Virginia Trimble

A CLASS of stars characterized by moderate excesses of barium, yttrium, carbon and the rare-earth elements might not seem to present any dramatic feature likely to excite astronomers, but the fact is that the barium stars provide a rare opportunity to see that nucleosynthesis really is taking place in stars in our Galaxy. The anomalous elements are just those that should be produced by the s-process (slow capture of neutrons by iron nuclei) in a star whose main energy source is the burning of helium to carbon. The only trouble is, the nucleosynthesis seems to be happening in the wrong sorts of stars. The barium stars are too small and faint and too low in mass to have reached the evolutionary phase (burning of hydrogen and helium in thin shells around a carbon-oxygen core) when, according to current models, the anomalous elements ought to be produced and dredged up to the surface for us to see them. We still do not really understand how the stars do it.

Restrained rejoicing nonetheless occurred when Robert McClure and his colleagues at Dominion Astrophysical Observatory showed that essentially all the traditional barium stars are members of binary systems<sup>1,2</sup>. They have separations of ~ 2 AU and inconspicuous companions of near 0.5 solar masses that could be, and in a few cases definitely are<sup>3</sup>, white dwarfs. The obvious solution, then, was that the nucleosynthesis had occurred in an initially rather massive star, the large separation permitting this star to expand sufficiently to reach the evolutionary stage needed to make carbon and the s-process elements

and to bring them to the surface. Then, when the massive star ejected its outer layers (in a wind and/or planetary nebula, leaving a white dwarf core behind), it polluted the surface of its previously normal neighbour. A variant scheme locates the synthesis on the surface of the white dwarf, where matter accreting from the barium star's wind can trigger nova-like flashes. These blow the material, now modified in composition, back where it came from. This would presumably happen at intervals throughout the system's life. Although the basic pollution scheme has some enthusiastic defenders<sup>4</sup>, recent observations<sup>5-9</sup> present several difficulties for it and, instead, appear to favour an arrangement whereby the companion has somehow merely triggered mixing in the star we see.

First, the variant scheme can probably be ruled out, because the ratio of radioactive <sup>93</sup>Zr to stable <sup>93</sup>Nb, as well as the absence of unstable Tc, argues for the completion of the processing at least 3 × 10<sup>6</sup> yr ago<sup>9</sup>. Second, a small group of stars (including such bright ones as ε Peg) shows a pattern of enhancement of only a few s-process nuclides, strongly suggesting that a very large fraction of the envelope has been exposed to only a few neutrons per seed nucleus, thereby juggling abundances within the heavy elements rather than making fresh ones out of iron<sup>7</sup>. It is probably not practical to transfer most of the envelope from one star to another. This is, then, an objection to the main binary model, as are the following points.

Lithium abundances present a third

difficulty. The element is fragile and easily destroyed in all but the coolest outer layers of an evolving star. Yet most barium stars have the same lithium abundance as normal K giants of the same mass (1–2  $M_{\odot}$ ), temperature (4,000–5,000 K), luminosity ( $M_v = +2$  to  $-2$ ), iron abundance (solar to 1/3 solar) and evolutionary phase (core helium burning)<sup>8</sup>. Thus, the supposed material to have been transferred must either have been pure carbon and s-process material (in which case so little is required that no other abundances are disturbed) or have carried with it a Li/H ratio appropriate to the mass and condition of the recipient, not the donor (supposed to be more massive and more evolved in the binary transfer model). The lithium data really only make sense if most of the present giant envelope has always been on the surface of the star, with modest amounts of pollutants convected into it. The idea that the star has been completely mixed, as already suggested<sup>10</sup> for the still fainter CH subgiants (also carbon- and s-process-enriched), is even more at odds with the data.

A fourth difficulty arises in a detailed analysis of the ratios of abundances of 15 s-produced elements in seven assorted barium stars<sup>6</sup>. Different phases of stellar evolution provide different neutron sources:  $^{13}\text{C}(\alpha, n)^{16}\text{O}$  during core helium burning and  $^{22}\text{Ne}(\alpha, n)^{25}\text{Mg}$  during the phase with two thin shells. Both make s-process products, but with different abundance patterns, and only during the latter phase do models predict that the products should get mixed to the stellar surface. Thus, we would expect the  $^{22}\text{Ne}$  pattern for barium stars formed by binary pollution. Instead, we seem to see the  $^{13}\text{C}$  pattern<sup>6,11</sup>.

The fifth problem is the lack of evidence for a white dwarf companion in many of the systems and for stars that have been polluted in the advertized way at any evolutionary stage except ~ KOIII core helium burning<sup>5,6,10</sup>. For the few barium stars where there is evidence of a white dwarf companion, it has come from excess UV flux detected by the International Ultraviolet Explorer satellite (IUE)<sup>3-5</sup>. Perhaps, then, other systems have white dwarfs that are too cool to be detected by IUE. However, white dwarfs take time to cool, and giants evolve rather rapidly. Dominy and Lambert therefore conclude that a white dwarf whose immediate predecessor has polluted the remaining star will not yet have had time to cool below IUE detection limits<sup>5</sup>. Böhm-Vitense *et al.* do not agree<sup>4</sup> and propose instead that increasing the mass of a star by transferring material to it both speeds up and slows down its evolution. The Space Telescope should be able to ferret out any sort of white dwarf ever thought of in this context and so resolve the issue.

What about polluted stars in other evolutionary phases? A massive star will inevitably complete double shell burning

and shedding when it is good and ready, regardless of the state of its companion. Objective prism searches<sup>10</sup> have uncovered a few old metal-poor, high-velocity stars near GO,  $M_v = +4$  that are rich in carbon and s-process products, (but have failed to turn up anything at all at adjacent colours or luminosities). These are the CH subgiants mentioned above. Their discoverers interpret them as products of complete mixing at helium core flash in stars of solar mass 1. The mixing shakes them up and puts them back on the main sequence near the turn-off point for old stars like those in M67. Thus, the CH subgiants seem to be largely irrelevant to our problem, except that they ought, in due course, to evolve into something like barium giant stars. Moreover, they are not known to be binaries — an IUE survey<sup>12</sup> of 17 CH subgiants rules out white dwarf companions hotter than 14,000 K — and they are too rare and restricted in occurrence to give rise to most of the barium star population.

The supergiant carbon stars of types R, N and S are also not relevant. They are sufficiently bright and massive for the conventional wisdom to apply, and their s-process excesses are presumably a result of reactions in their own interiors, followed by dredge-up to the surface. Similar remarks apply to analogous older stars, the CH giants, although many of them seem to be members of binary systems<sup>13</sup>.

As for main-sequence stars that might have been polluted by binary companions, none is known. Yet, if 1 giant in 100 is affected (the ratio of barium stars to normal K giants), so should be 1 main-sequence star in 100 (it will capture less of the ejecta, but it also has a much smaller convective envelope to dilute what is captured). Perhaps we just have not looked carefully enough for them<sup>4,6</sup>, although an awful lot of people have looked at an awful lot of main-sequence stars over the years!

Given all the difficulties with the notion of transfer, the possibility that the binary companion merely triggers unexpected mixing in the star is worth looking at. But, immediately, we hit the difficulty that the companions of the barium stars are all of low mass and inconspicuous. Yet a large fraction of binaries contain two stars of comparable mass. Thus, either a little bit of triggering works better than a lot — which seems silly — or it only works after the stars have had a chance to interact tidally through several evolutionary phases, and no effect is seen until the secondary is ascending the giant branch — which is almost as hard to believe, though perhaps not impossible<sup>5</sup>. There is also a difficulty of time scale 4. The s-process waits for  $\beta$  decays and so goes slowly. Thus, some  $10^4$  yr of processing are needed before the mixing event (which is assumed to be the violent phase of helium core flash). Yet an adequate neutron source will not be available until helium burning begins; and existing models indicate that violence sets

in only a few years after helium ignition<sup>14</sup>.

Further confusion is introduced by the (apparently single) K5 V star  $\epsilon$  Indi. It is the closest star of its kind to us and one of the 15 closest stars altogether, with a parallax of  $0.29''$ . It ought, therefore, to be of the common or garden variety. Yet a detailed spectral analysis<sup>15</sup> shows it to have excess carbon and s-process elements in a pattern very like that of the barium stars and CH subgiants (in combination with high space velocity, absolute magnitude and low iron abundance, all suggestive of an old subdwarf). Kollatzschny advised caution in accepting these results at face value. Ionized and neutral species of the same element typically gave discordant results, and the apparent anomalies in  $\epsilon$  Indi may thus reflect only our incomplete understanding of the structure of very cool atmospheres. Where only one ionization state is seen, as in the group of stars studied by Holweger and Kovacs<sup>7</sup>, improper interpretation of atmospheric structure could be responsible for all the anomalies reported<sup>15</sup>. And even for the frequently-studied bright K giant Arcturus, there are still large error bars on values for its temperature, surface gravity and abundances<sup>16</sup>.

Misinterpretation of observational data would be the easiest way to resolve the problem of apparent nucleosynthesis in barium stars, though the soul quakes at the number of wasted astronomer-hours. I do not think that this is the whole answer, but neither mass transfer from a companion nor anomalous mixing, triggered by the companion, is entirely satisfactory either. Thus, although the nuclear physics of the s-process is in good shape, we still do not seem to know its astrophysical context very well. □

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Virginia Trimble is Professor of Physics at the University of California, Irvine, California 92717, and Visiting Professor of Astronomy at the University of Maryland, MD 20742.

## Erratum

In the article 'Antibodies to the rescue' by David Hanke (*Nature* **310**, 272; 1984), in the third paragraph, auxins are described as 'plant hormones'. This description was introduced during the editing of the article; the author would prefer auxins to be referred to as 'plant growth substances'.