

X. *On the Height of the luminous Arch which was seen on Feb. 23, 1784.* By Henry Cavendish, Esq. F. R. S. and A. S.

Read February 25, 1790.

**T**HIS arch was observed, at the same time, at Cambridge by Mr. WOLLASTON; at Kimbolton in Huntingdonshire, by the Rev. Mr. HUTCHINSON; and at Blockley near Campden in Gloucestershire, by Mr. FRANKLIN; and is described in letters from those gentlemen read to the Royal Society in December 1786\*.

It has been remarked, that as the arches of the kind described in these Papers have usually but a very slow motion, their height above the surface of the earth may readily be determined, provided they are observed about the same time, at places sufficiently distant; and they seem to be the only meteors of the aurora kind whose height we have any means of ascertaining.

The three places at which this phænomenon was seen are not so well suited for this purpose as might at first be expected from their distance, because they lie too much in the direction of the arch; they however seem sufficient to determine its height within certain limits, and perhaps are as well adapted for it as any observations we are likely to have of such phænomena.

The latitude of Cambridge is  $52^{\circ} 12' 36''$ : that of Kimbolton is said by Mr. HUTCHINSON to be  $52^{\circ} 20'$ , and,

\* See p. 43—46. of this Volume.

according to the survey of Huntingdonshire, published by JEFFERIES, is  $52^{\circ} 19' 50''$ ; so that we may suppose it to be seven geographical miles north of Cambridge, and by the maps it seems to be about 18 such miles west of it: and Blockley is by the map 12 geographical miles south and 72 west of Cambridge.

At Cambridge the observations of its track seem to have been made at about 9 h. 15 P.M. or 8 h. sidereal time. At Kimbolton, allowing for the difference of meridians, they could hardly have been made more than 5' sooner; and at Blockley they were most likely made nearly at the same time as at Cambridge.

At Blockley the arch passed about  $7^{\circ}$  south of the zenith; but it is unnecessary to determine this point with precision. At Kimbolton it was found by a quadrant to pass  $11^{\circ}$  to the south of it; and at Cambridge it was observed to pass through  $\delta$  and  $\epsilon$  Tauri,  $\beta$  Aurigæ,  $\theta$  Ursæ majoris, Cor Caroli, and Arcturus. Now, if an arch was drawn through these stars, it must, I think, have appeared sensibly wavy to the eye; whereas Mr. WOLLASTON did not take notice of any crookedness in this part of its course. It is most likely, therefore, that the middle of the arch must have passed to the south of  $\beta$  Aurigæ, and to the north of  $\theta$  Ursæ; and if a circle is drawn through  $\delta$  Tauri, Arcturus, and a point one degree north of the zenith, it will differ but little from a great circle, will agree as well with the positions of these stars as any regular line which can be drawn, and will pass  $2\frac{1}{2}$  degrees below  $\beta$  Aurigæ, and as much above  $\theta$  Ursæ; which is not a greater difference from observation than may well have taken place, considering how much care and acquaintance with the fixed stars are required to determine a path by them so nearly.

The direction of the arch here described in that part near the zenith is W.  $18^{\circ}$  S. ; and if a line is drawn through Cambridge in this direction, Kimbolton is 12,8 geographical miles north of it ; and therefore, as the arch appeared  $12^{\circ}$  more south at Kimbolton than at Cambridge, the height of the arch above the surface of the earth must be  $61\frac{1}{2}$  geographical or 71 statute miles. If we suppose that the middle of the arch really passed through  $\beta$  Aurigæ, the height comes out 52 statute miles. On the whole, I should think, the height could hardly be less than 52 miles, and is not likely to have much exceeded 71.

The common aurora borealis has been supposed, with great reason, to consist of parallel streams of light shooting upwards, which, by the laws of perspective, appear to converge towards a point ; and when any of these streams are over our heads, they appear actually to come to a point, and form a corona. Hence, from analogy, it seems not unlikely, that these luminous arches may consist of parallel streams of light, disposed so as to form a long thin band, pretty broad in its upright direction, and stretched out horizontally to a great length one way, but thin in the opposite direction. If this is the case, they will appear narrow and well defined to an observer placed in the plane of the band ; but to one placed at a little distance from it, they will appear broader, fainter, and less well defined ; and when the observer is removed to a great distance from the plane, they will vanish, or appear only as an obscure ill-defined light in the sky.

There are two circumstances which rather confirm this conjecture : first, that though we have an account of another arch besides this \* having been seen at great distances in the

\* That of Feb. 15, 1750. Phil. Trans. XLVI. p. 472. and 647.

direction

direction of the arch, we have none of any having been seen in places much distant from each other in the contrary direction; and, secondly, that most of them have passed near the zenith, whereas otherwise they ought frequently to appear in other situations; for if they appeared near the zenith to an observer in one latitude, they should appear in a very different situation in a latitude much different from that.

I wish it to be understood, however, that I do not offer this as a theory of which I am convinced; but only as an hypothesis which has some probability in it, in hopes that by encouraging people to attend to these arches, it may in time appear whether it is true or not. If it should hereafter be found, that these arches are never seen at places much distant from each other in a direction perpendicular to the arch, it would amount almost to a proof of the truth of the hypothesis; but if they ever are seen at the same time at such places, it would shew that the hypothesis is not true.

Supposing the hypothesis to be well founded, the height above determined will answer to the middle part of the band, provided the breadth of it was small in respect of its distance from the earth, but otherwise will be considerably below the middle. If the breadth of the band was equal to the distance of its lower edge from the earth, the height of the lower edge would be three-fourths of that above found; and if the breadth was many times greater, would be half of it.

In the common aurora borealis, an arch is frequently seen low down in the northern part of the sky, forming part of a small circle. What this is owing to, I cannot pretend to say; but it is likely that it proceeds from streams of light which appear more condensed when seen in that direction than in any other, and consequently that the streams which form the arch to an observer

in one place are different from those which form it to one at a distant place, and consequently that no conclusion as to its height can be drawn from observations of it in different places. Attempts, however, have been made to determine the height of the aurora from such observations, and even from those of the Corona\*; though the latter method must surely be perfectly fallacious, and most likely the former is so too.

\* BERGMAN. Opusc. Vol. V.



February 17, 1859.

Sir BENJAMIN C. BRODIE, Bart., President, in the Chair.

The Lord Bishop of London and the Lord Bishop of Ripon were admitted into the Society.

The following communications were read:—

- I. "Statement of Facts relating to the Discovery of the Composition of Water by the Hon. H. CAVENDISH." In a Letter from J. J. BENNETT, Esq., F.R.S., to Sir B. C. BRODIE, Bart., P.R.S., dated February 12, 1859. Received February 14, 1859.

Since the death of our late excellent and lamented friend Mr. Robert Brown, several appeals have been made to his executors to publish certain evidence presumed to have been in his possession relating to the much-agitated question of the priority of Cavendish or Watt in the discovery of the composition of water. As the executor to whom Mr. Brown entrusted his papers, and having been for many years honoured with his entire confidence, I feel called upon to respond to these appeals, and I therefore request that you will kindly lay before a Meeting of the Royal Society the following brief statement on the subject.

The date and nature of Cavendish's communication to Priestley have always been considered as essential elements in the determination of the question; and it was the evidence which Mr. Brown possessed in regard to these particulars, which, in his estimation, "placed Cavendish's claims as the discoverer of the composition of water beyond dispute." That evidence, however, was not derived from any unpublished document, but formed part of a section of Deluc's "*Idées sur la Météorologie*," which although especially entitled,—"*Anecdotes relatives à la découverte de l'Eau sous la forme d'Air*,"—appears entirely to have escaped the notice of those

who have advocated Cavendish's claims. It is the more conclusive as coming from Deluc, the "*ami zélé*," as he justly terms himself, of Watt, and who, in relation to this question, believed himself "à portée d'en connoître toutes les circonstances."

The testimony of Deluc is as follows:—

Vers la fin de l'année 1782 j'allai à *Birmingham*, où le Dr. Priestley s'étoit établi depuis quelques années. Il me communiqua alors, que M. Cavendish, d'après une remarque de M. Warltire; qui avoit toujours trouvé de l'eau dans les vases où il avoit brûlé un mélange d'air inflammable et d'air atmosphérique; s'étoit appliqué à découvrir la source de cette eau, et qu'il avoit trouvé, "qu'un mélange d'air inflammable et d'air déphlogistiqué en proportion convenable, étant allumé par l'étincelle électrique, se convertissoit tout entier en eau." Je fus frappé au plus haut degré de cette découverte\*.

The italics and inverted commas are Deluc's own.

In this communication made by Cavendish to Priestley the theory of the composition of water is clearly indicated. The two gases (known to have been hydrogen and oxygen) were mixed together *in due proportion*, and by means of the electric spark were *entirely converted* into water. Referring to one of Cavendish's experiments, as recorded in his journal, Lord Jeffrey, the most candid and judicious of Watt's advocates, has said: "if he [Cavendish] had even stated in the detail of it, that the airs were *converted, or changed, or turned* into water, it would probably have been enough to have secured to him the credit of this discovery, as well as to have given the scientific world the benefit of it, in the event of his death, before he could prevail on his modesty to claim it in public.†" The evidence which this distinguished critic and judge regarded as sufficient to establish Cavendish's claim is now afforded, not by a note in his private journal, but by the testimony of the zealous friend of Watt, who states that it was communicated to Priestley towards the end of the year 1782, that is to say, several months before Watt drew his own conclusions from Priestley's bungling repetition of Cavendish's experiments. It was, moreover, published to the world, and suffered to remain uncontradicted, while

\* Idées sur la Météorologie, tome ii. 1787, pp. 206-7.

† Edinburgh Review, vol. lxxxvii. p. 125.

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