in that book were first put forward in his Mathematical Analysis of Logic, (Cambridge 1847), reprinted Oxford 1948), a work published before he was appointed to the Chair of Mathematics (not Probability Theory) in Queen's College, Cork. An account of Boole's life can be found in W. Kneale, "Boole and the revival of Logic", *Mind*, lvii (1948), pp.149-75. Whilst setting the chronology to rights, I might also point out that Leibniz was not born until 1646, and so, in 1600, was dreaming neither of his *ars combinatoria*, nor of his *calculus de continentibus et contentis*.

More serious is Darwood's misreading of Venn and Boole. Despite the comments of Lewis Carroll (C.L. Dodgson), Venn does not insist on circles (or eclipses) for his diagrams, nor does he ignore situations involving more than six classes.

> *"With employment of more intricate figures we might go on for ever. All that is requisite is to draw some continuous figure which shall intersect once, and once only, every subdivision. The new outline thus drawn is to cut every one of the previous compartments in two, and so just double their number. There is clearly no reason against continuing this process indefinitely"* (Symbolic Logic, *London 1881, p.106*)

He goes further in a footnote on pp108-9,

> *"It will be found that when we adhere to continuous figures, instead of the discontinuous five-term figure . . . there is a tendency for the resultant outlines thus successively drawn to assume a comb-like shape after the first four or five. . . . Thus the fifth term of the figure will have two teeth, . . . and so on, till the (4+×) th has 2^x. There is no trouble in drawing such diagrams for any number of terms which our paper will find room for."*

It is not the geometry of his diagrams that cannot cope with large numbers of classes, rather it is the perception of the human eye and the human brain.

> *"the visual aid for which mainly such diagrams exist is soon lost on such a path."*

What is more, Venn's diagrams, unlike those of Carroll, Marquand, Veitch or Karnaugh, would maintain the contiguity of all areas belonging to any one class.

Regarding Boole, there are several mistakes. In The Laws of Thought, the variables are introduced as classes, just as Venn and Euler had interpreted their areas, and as most European logicians from Leibniz back to Aristotle had interpreted their symbols. This is the logic of the syllogism, the classical predicate calculus. The objects which Darwood calls "Boolean statements" are propositions, the domain of the functions of the classical propositional calculus. Boole called these "abstract" or "secondary propositions", regarding them as statements about the truth values of propositions, or rather "primary propositions", which were about things (i.e. classes). He introduces secondary propositions as a model of his algebra, although he interprets them in terms of classes, regarding his symbol "×" as denoting the class of times at which some proposition, X, is true. Later in the book he offers, as another model, an interpretation of the variables as measure of the probability of events.

As to the "mystery" of why Boole uses "+" for disjunction, Boole himself writes (regarding classes),

> ". . . we have expressed the operation aggregation by the sign +, . . ." (p.33).

What would be more natural for a mathematician than the use of the sign of addition for aggregation? Earlier, Leibniz, in his Non Inelegans Specimen Demonstrandi in Abstractis uses the sign "O" for something like the union of sets.

Lastly, in his exposition of Boole's algebra, Darwood seems to confuse the modern mathematical conception of a Boolean Algebra with the algebra of Boole. The former uses "+" in a way which can be interpreted as inclusive alternation, i.e. "A+B" means "A or B or both A and B".

On this basis, he is correct when, having derived

$$A+B\bar{A}=A+B$$

from

$$A+BC=(A+B)(A+C)$$

he refuses to subtract A from both sides to obtain the incorrect

$$B\bar{A}=B$$

Boole, however, takes disjunction in an exclusive sense.

> *"The expression, "Either y's or z's," would generally be understood to include things that are y's and z's at the same time, together with things that come under the one but not the other. Remembering, however, that the symbol + does not possess the separating power . . . we must resolve any disjunctive expression which may come before us into elements really separated in thought, and then connect their respective expressions by the symbol +."(p.56)*

In other words, "A+B" is only a well-formed expression in Boole's system if we have already assumed the truth of $B=B\bar{A}$. Then, of course, it is not surprising that we can deduce the true statement $B\bar{A}=B$. On Boole's interpretation, subtraction will work in his system as it does in ordinary algebra.

As a final point, it is possible to fill the gap between Lull's use of linked circles, for in De Censura Veri (1555), Ludovicus Vives uses a diagram to indicate that if all B is A, and all C is B, then all C is A. If one compares this with an Eulerian diagram of the same proposition, then the link is clear.

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MICHELSON – MORLEY

The saga of the M.M. experiment must surely be one of the strangest tales in the history of science. It is a story of such monstrous oversights and omissions that when those defects are repaired the experiment is found to prove exactly the opposite of that which is taught.

In the 1887 paper[1] M.M. admit to an earlier experimental omission, the effect of the aborration of light in the transverse axis, which was pointed out by M.A. Potier. They also admit that it was an analysis by H.A. Lorentz which led to the idea that the transverse axis would reduce the originally anticipated result by half.

At the present time we are not taught that it was Lorentz who did half of the calculations for M.M. and we must remember that at the time Lorentz wanted a particular amount of length contraction, the reason being that he would repair the equations of J.C. Maxwell.

Did Lorentz secretly predict a null result to himself: If he did, and on the evidence he surely must have, then he certainly did not divulge his ideas to M.M. otherwise *they* would have claimed a comfortable experimental confirmation instead of the nebulous uncertainty that science has tried to sweep under the carpet ever since.

Let us pretend that there was in fact a null result, let us further pretend that Lorentz did not fully appreciate the implication of Fig. 1 in the supplement of the paper which describes graphically just how aberration of light occurs.

The mathematic of the experiment was designed to reveal the difference in time taken by both rays of light in their respective paths.

The error made by M.M. was that they did not measure, directly, the difference in arrival time of the light wavefronts. They chose instead to interpret a phase difference in light waves as being the same thing as a measure of a difference in time.

A phase difference is a proportion of a wavelength expressed either as a spatial displacement or alternatively as an angular displacement which in itself is a form of spatial displacement. The introduction of time into the notion of phase difference is clearly ridiculous for it would allow phase difference the dimensions of velocity.

So, we now have a situation where we have slid, with magnificent ease, from the mathematic comparison of time into the experimental comparison of distance and there is no bridge joining the two things.

Now we must consider the experiment in the terms in which it was conducted, those of wave theory and practice.

First let us deal with the transverse axis. There are two points of view to be considered.

To an observer moving with the experiment the light is seen to travel straight out and back to its origin but to an observer at rest in space the light covers a triangular path as a result of the aberration which occurs when light is reflected into a sidways path by a moving mirror.

Now, the important thing to remember is that both observers are looking at the *same ray of light* and that they both see the same number of waves. The phenomenon of aberration extends the wavelength on the triangular path by an amount which conforms to the Lorentz transform. Regardless of the velocity of the experiment it is quite impossible for the number of waves in this axis to vary.

In the longitudinal axis we have again two observers looking at the same thing, one sees two equal paths and the other two unequal length paths but they both see the same number of waves. There is no mystery here because it is well known that with the Doppler effect there is, whether light be blue or red shifted, an additional element of red shift which accords with the Lorentz transform[2]. Because the wavelengths are extended and because that fact has been overlooked it became popularly accepted that the length of the experiment itself varies with velocity.

So, we see that by using interferometry and *invariant* length the experiment must always yield a null result.

Had length in fact varied as supposed by Lorentz then the result would have been both obvious and spectacular.

What will the scientific establishment do to rectify their error? Or will they just sit tight and hope that reason will continue to be driven away from the explanation of Nature?

A. Jones,
Swanage,
Dorset.

1. Philosophical Magazine December 1887.
2. Einstein's Universe, N. Calder.