

**Chromosomes in *Camellia thea*  
(The Tea Plant).**

THIS short note deals with the chromosome numbers in four different *jats* of tea growing in the Experiment Station plots. Previous references on the subject of chromosome numbers are available for some Chinese *jats* (varieties)—known generally as *Thea sinensis* or *Camellia sinensis*. The diploid number in these as determined by (Cohen Stuart) Zaldastanishvili,<sup>1</sup> and Ono<sup>2</sup> is thirty ( $2X = 30$ ) and the haploid number according to Zaldastanishvili<sup>1</sup> and Karasawa<sup>3</sup> is fifteen ( $X = 15$ ).

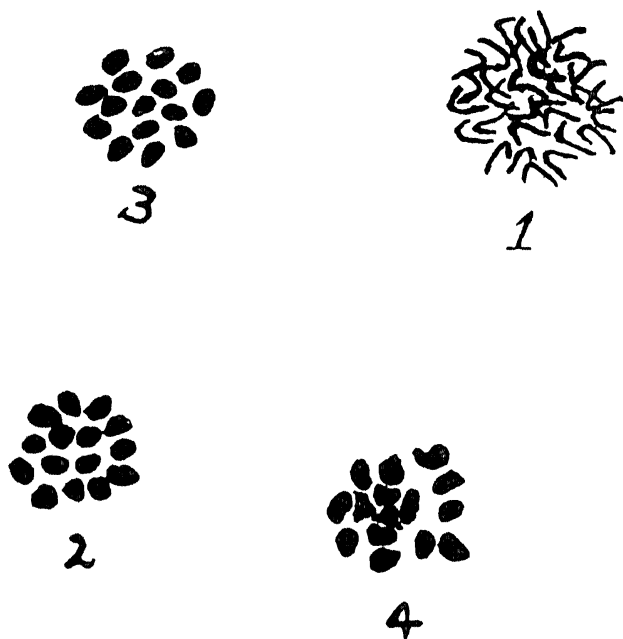


Fig. 1.

Chromosomes in *Camellia thea*.

1. Somatic Chromosomes in a South Indian Variety,  
2, 3 & 4. Reduced number in P.M.C.'s varieties  
"Bonaccard" "Chapleton" and "China" ?  
respectively.

Chromosome numbers counted in the four *jats* in South India have confirmed the numbers recorded by the earlier workers.

Variety — <i>Jat</i> .	2 X R.T.	X P.M.C.
1. South Indian Seed (A) ..	30	..
2. Bonaccord ..	..	15
3. Chapleton ..	..	15
4. China (?) ..	..	15

A paper in greater detail dealing with

mitosis and meiosis in one of the varieties (Chapleton) is under preparation.

M. K. SUBBA RAO.

U.P.A.S.I. Tea Experiment  
Station, Devershola,  
February 11, 1938.

<sup>1</sup> *Plant Breeding Abs.*, 1932-33, 3, 134.

<sup>2</sup> *Ibid.*, 1932-33, 3, 134.

<sup>3</sup> *Ibid.*, 1932-33, 3, 68.

**Sulaiman's Predictions.**

RECENTLY two adverse references and one criticism of my theory have been published in the columns of the *Current Science*.

I. In the September Number, Sir C. V. Raman referred to "numerous reports and publications emanating from Allahabad regarding the alleged demolition of Einstein's theory of relativity" and relied on Dyson and Woolley's "solar eclipses" for the view that "Einstein's prediction from his general theory of relativity is verified by observations". R. N. Rai<sup>1</sup> has already pointed out that the book had been published before Royds' results of the 1936 eclipse observations were announced.

I submit that the remark of Dyson and Woolley, which was confined only to the relative values of the deflection of light according to Newton's Law and Einstein's theory, has been slightly misunderstood. Even as regards the 1919 observations, which were responsible for the reputation of the General Theory of Relativity, what the authors have actually said is—"The value of 1.98" as given indicates that the deflection is 1.75" at least and possibly somewhat larger" (p. 47). Commenting on the observations of Campbell and Trumpler (1922), which are supposed to confirm relativity exactly, they remark: "There can be no doubt that the observations show a deflection as great as and possibly rather more than Einstein's predicted value" (p. 48). Later after, discussing Freundlich's observations, they express their final opinion in the following terms:—

"The conclusion is that the displacement is at least as great as 1.75", and possibly a little greater but not more than 2.0" (p. 50). Thus a marked excess over Einstein's value has been conceded, which cannot mean verification. I may also quote Evershed's opinion relied upon by the Editor of the *Current Science* in the December Number, p. 304, "These predictions (*i.e.*, Sulaiman's

might be thought to be confirmed by my measures of Iron lines in the red and by Freundlich's observed value of the deflection of stars near the edge of the Sun". According to my theory there should be an excess of at least 33%.

Prejudices in favour of an accepted theory died hard and it is no wonder that in spite of successive observations at the last few eclipses showing an undoubtedly excess over Einstein's value, the question should still remain controversial. Although the 1936 eclipse did not offer a favourable opportunity, nevertheless the results are still awaited. A really good opportunity will be furnished at the total solar eclipse of October 1, 1949 in South Africa, Brazil and Columbia, which will be the final test.

I maintain that observations so far made have been too low because owing to the non-corpuseular conception of light, no correction (which varies inversely as the shortest distance from the centre of the Sun) has been made for the pressure of solar light on the star light. As the pressure increases closer to the edge, in taking the mean the stars appearing nearest to the Sun should either be left out or corrected for. My theory predicts that without such a correction there would be a progressive increase in deflection over Einstein's value as one recedes from the edge. My prediction will stand or fall by this test.

II. In the December Number of the *Current Science* extracts from Evershed's paper in the *Observatory* have been quoted. I am obliged to Evershed for referring to the theory of light propounded by me and the two predictions which follow from it.

Evershed has suggested that "Were we situated on the planet Pluto . . . , we should certainly be satisfied about General Relativity", for the medley of shifts in the spectrum can be reconciled by taking the mean of all. I would go further and say: Were we at Sirius where the solar spectral shift from the edge would not be distinguishable from that from the centre, we would persuade ourselves to be convinced of the brilliant triumph of Relativity. Unfortunately for Relativity, we are in a nearer region where we can distinguish one from the other. I have repeatedly pointed out that owing to various causes there is a deficiency in the measurements; up to half the distance from the centre the values, though gradually increasing, are below Einstein's theoretical value; and for the other half, they are

above it, and still gradually increasing. The novel method of taking a mean of all values for the purpose of procuring confirmation amounts to shutting one's eyes to the patent fact that the spectral shift is not the same at all points, as Einstein's theory would show, but that there is undoubtedly a rapid and progressive increase as one proceeds from the centre towards the edge. As shown by me at the Science Congress, the effect becomes patent when a curve is drawn for the difference between the spectral shift at any point and the spectral shift at the centre. Royds' results leave no doubt for the value at the edge being double of Einstein's value and in exact accordance with my theory. Even Evershed concedes, "The limb effect remains, however, an unsolved problem."

No doubt Evershed has referred to his measures of the sodium D lines. One solitary swallow cannot make a summer, but I suppose that one set of sodium lines can confirm Relativity. As Professor Stratton of Cambridge declared in his public lecture at the Science Congress, the sodium lines are "minority lines" and do not justify the conclusion that Relativity is confirmed.

My objections to Evershed's observations of the sodium lines are as follows:—

(i) Evershed had not the advantage of a solar eclipse, and had to make observations in full light when the effect of scattering due to light from the centre is great.

(ii) Evershed made measurements in the vaporous atmosphere of England, when scattering due to terrestrial atmosphere is large.

(iii) The sodium D lines are rather broad, which make it more difficult to measure the shift exactly.

(iv) As lines of small wave-length are subject to great refrangibility, measurements of these lines are much more unreliable than those in the red region of the spectrum.

(v) Undisputedly there are convection currents at the surface of the Sun, and measurements of lines at one latitude only may be quite wrong if no correction is made for the possible convection current.

I have repeatedly emphasised that the true test will be furnished by the measurements of the spectral shift of lines in the red region (least subject to refrangibility), preferably at the time of a total eclipse. Royds' observations furnished such a test. Judged by it, my theory has had a cent.

per cent. confirmation, and the Relativity value is wrong by 100%.

III. I shall reply to the criticisms separately.

S. M. SULAIMAN.

New Delhi,  
January 28, 1938.

---

<sup>1</sup> *Science and Culture*, 1937, pp. 322-23.

I CRAVE the hospitality of your columns to say a few words regarding the communication of Sir S. M. Sulaiman appearing elsewhere in this issue. Since it was I that brought to your notice the article by Evershed in the *Observatory* (Oct. 1937, p. 266) extracts from which have been published in the December issue of your Journal, I feel it necessary to make some remarks.

It is with great reluctance that I have to call your attention to the title which Sir Sulaiman has chosen for his rejoinder. I may be pardoned for saying that such a procedure, which is unfortunately becoming rather too common, savours more of personal advertisement than of a genuine scientific controversy.

Sir Shah Sulaiman says: "According to my theory there should be an excess of at least 33 per cent." A theory which, on his own statement, is so indefinite can scarcely be expected to be taken seriously. To seek support for it in residual discrepancies between observation and the magnitude of the Einstein shift does not appear to be a useful procedure.

Sir Sulaiman attaches the greatest importance to Dr. Royds' recent eclipse observations and claims that, judged by them, his theory has had a cent. per cent. confirmation. But Dr. Royds' own conclusions<sup>1</sup> do not at all support this contention; for according to Dr. Royds,

"the relativity theory, or any other theory requiring a general displacement of solar wave-lengths is not adequate to furnish a complete explanation of solar displacements."

Sir Sulaiman has misunderstood Evershed's reference to a hypothetical observer on the planet Pluto. Evershed has used the illustration just to emphasise the point that relativity gives a satisfactory explanation of the solar spectral shift in a general sort of way. It appears therefore hardly fair to remark

that he advocates the ignoring of the observed discrepancies altogether, and to suggest that an observation of the Einstein shift be made from Sirius. One might as well suggest an observer on the Andromeda Nebula wherefrom the solar system itself would not be seen!

One thing that is clear from Sir Sulaiman's reply is his belief that every failure of the Relativity Theory automatically means a triumph for his own. If the solitary swallow of the "minority sodium lines" does not make a summer for relativity, a few stray swallows suffice to constitute a summer for his theory. Even the remark of Evershed that the limb effect remains an unsolved problem is seized upon as an evidence indirectly supporting this new theory. When competent and experienced astronomical observers like Dyson and Woolley make the authoritative and unequivocal statement, "But there can be no doubt that Einstein's prediction has been verified" (p. 50 of their book), an attempt has been made to show that their remarks have been misunderstood and that they really do not mean what they say!

It is true that prejudices in favour of an accepted theory die hard, and it is therefore all the more necessary that a new theory which seeks to supplant a reputable one should be entirely free from all the objections raised against the old theory. Sir Sulaiman's theory cannot certainly be said to have achieved this measure of success. Moreover his theory has not yet gone beyond the stage of initial criticism, and, except for a few papers by the author, nor has it been developed in a comprehensive manner worthy of a fundamental physical theory. Until it proceeds beyond the region of mere tinkering with residual discrepancies of observation, and until it produces some striking achievements compelling its acceptance, it is idle to expect that one would throw overboard such an eminently successful and philosophically satisfactory theory as the theory of Relativity.

B. S. MADHAVA RAO.

College of Engineering,  
Bangalore,  
February 25, 1938.

---

<sup>1</sup> *M.N.R.A.S.*, 1937, 97, 695.