

have always been the aggressors, where the dark populations have always represented the passive or recipient element. On the other hand, wherever it has escaped from Malayo-Polynesian influences, or wherever it has been able to preserve its original speech in spite of those influences, this dark element will certainly be found speaking languages organically distinct from the Malayo-Polynesian. Mr. Man's recently published account of the Andamanese dialects shows that they differ in their morphology, in their glottology—in fact, in every respect, from those of Malaysia.

Mr. Lawes makes the same remark respecting the Koiari people, who occupy the highlands back of Moresby Bay in South-East New Guinea. And the authors of the work under review now find that the Mafór of Geelvinck Bay betrays, with many striking resemblances to the Malayo-Polynesian, "an astounding peculiarity of structure."¹ The "resemblances" are of a verbal character, due to known contact with the Malays, who have long frequented the waters along the north-west coast of New Guinea. The "peculiarity of structure," involving root modifications and something even approaching to inflection ("Quasiflexion"), as understood in the Aryan family, belongs to the organic Papuan linguistic type. This type is thus demonstrated to be fundamentally distinct from the Malayo-Polynesian, which shows no trace of these peculiarities. And thus also disappears the fancied antagonism hitherto supposed to exist between the linguistic and anthropological elements in the Oceanic regions.

A. H. KEANE

THE SOLAR-COMMERCIAL CYCLE

IN an article printed in NATURE (vol. xix., pp. 588-90) I gave a table of the prices of wheat at Delhi, from 1763 to 1835, quoted, or rather calculated from data given in a brief paper of the Rev. Robert Everest, contained in the *Journal* of the (London) Statistical Society for 1843, vol. vi. pp. 246-8. Between the years 1763 and 1803 there was evidence of wonderful periodicity in the recurrent famine and abundance at that part of India. When recently engaged in examining more minutely the relation between these prices and the variations of solar activity, as indicated by Prof. Wolf's numbers, it has occurred to me that an inference may be drawn which I overlooked on the previous occasion.

In the accompanying diagram I have exhibited the prices in question together with Wolf's numbers as stated in the *Monthly Notices* of the Royal Ast. Soc. vol. xxi. pp. 77, 78. I have also indicated the dates of the Commercial Crises of the time according to the article on the subject in Mr. H. D. Macleod's "Dictionary of Political Economy," vol. i. pp. 627-8. It need hardly be said that the coincidence between the three classes of recurrent phenomena is of a very remarkable character, and goes far in supporting the relation of cause and effect which I had inferred to exist, both on empirical grounds and from the well-known fact that it is the cheapness of food in India, which to a great extent governs the export trade from England to India. But although the coincidence of commercial Crises in Western Europe with high corn prices at Delhi is almost perfect, it will be noticed that after 1790, the correspondence of the solar curve with that of prices is broken. Wolf does not recognise the existence of any sun-spot maximum between 1788 and 1804, and he believes that there was a minimum at 1798. According to Wolf's later researches (*Memoirs* Roy. Ast. Soc., vol. xliii. p. 302), these dates are respectively, maximum 1788·1, minimum 1798·3, and maximum, 1804·2.

But now arises the question to which I wish to draw attention. If the eleven-year solar periodicity was really interrupted in this long interval of 16·1 years, how comes

¹—"Cerade das Mafoor'sche aber wird in seinem Baue bei manchen auffälligen Aehnlichkeiten eine erstaunliche Eigenthümlichkeit im Bildungsprinzip aufweisen," p. 4.

it that the meteorological periodicity, as manifested in the corn prices at Delhi, was not interrupted. It is true that the price maximum of 1803 was a comparatively small one; but this was quite to be expected, considering that if there were an intervening solar maximum, it must have been a small one. May we not reverse the argument and infer that the evident relation between the previous sun-spot maxima and the succeeding scarcities at Delhi, would lead us to expect a minor solar maximum about the year 1797?

Standing alone, the presumption thus created would, doubtless, be of a somewhat slight character. But it is in the first place well known, that the data upon which Wolf based his numbers about this time, are less conclusive than in other parts of his series. His results, too, from 1801 to 1807 are expressly marked as doubtful, so that extrinsic information which might have little weight where there was abundance of reliable solar or magnetic observations may come in very usefully where doubts already exist. Now it happens that the late Mr. J. A. Broun inquired very carefully into the facts known about the solar variation at this time, his results being given in the *Transactions* of the Royal Society of Edinburgh, vol. xxvii. pp. 563-594, and in his article printed in NATURE (vol. xvi. pp. 62-64). Broun inferred from the observations of Gilpin, and from other data, that there was a small maximum about 1797, and that there were grounds for believing that the subsequent maximum "may really have occurred after 1806, when Gilpin's series terminated." Now, what Broun deduced from totally different data, is exactly what we should infer from the Delhi prices. If we are to believe that Indian meteorology depends upon solar variations, then it almost follows that there was a solar maximum about 1797. The consequence of this inference, however, is very important, because it goes to support the views of Lamont, Broun and others, that the solar period is about 10½ (10·45) years and not 11·1 as calculated by Wolf. It should also be pointed out that the temperature observations of Prof. Piazzi Smyth lead to a like result. The epochs of the heat waves are, according to him (NATURE, vol. xxi., p. 248), 1826·5, 1834·5, 1846·4, 1857·9, and 1868·8, giving an average interval of 10·57 years.

I may take this opportunity of asserting that the progress of events confirms belief in the eastern origin of the great commercial Crises.¹ In his important work, the "Précis du Cours d'Economie Politique" (vol. i. pp. 604-5), M. Cauwès while partially accepting the doctrine of periodicity criticises the particular views here advocated. He says:—

"Depuis longtemps les économistes ont signalé la périodicité de ces évolutions: MM. Juglar et Jevons prétendent même pouvoir la calculer d'une manière précise. Selon M. Jevons, l'ensemble des phénomènes serait renfermé dans un cycle de dix années et demie. De fait, les grandes crises économiques du siècle (1806, 1817, 1825-7, 1836-37, 1847, 1857), s'échelonnent à dix années d'intervalle ou à peu près, mais les dernières, 1866 et 1873, seraient venues un peu avant l'heure, et celle de 1873 s'est prolongée au delà de toute attente." M. Cauwès in short accepts the six earliest crises of this century as sufficiently agreeing with the theory. The crisis of 1866 no doubt came about a year before it would be expected, which is a divergence of reasonable amount. The year 1873, however, is one which it would be impossible to introduce into the series. Now there doubtless were both in America and England in that year, a state of commercial stringency, a relapse of prices and other disturbances which might be mistaken for the signs of a

¹ As it is impossible to reproduce the explanations and qualifications contained in the article quoted above, or that at pp. 33-37 of the same volume of NATURE (vol. xix.), it is assumed that this article is read subject to those qualifications and explanations. In p. 588 col. 6 of the same volume, a *seer* of wheat was by a typographical oversight stated to be equal to 21 lbs. instead of the true weight 2 lbs.

true crisis. But such as it was, this crisis turned out to be just one of those exceptions which prove the rule. The following statistics of bankruptcy in the United Kingdom, as collected by Messrs. Kemp, and published in the *Mercantile Gazette*, show conclusively that the real collapse came in exact accordance with the decennial theory in the autumn of 1878 or early in 1879:—

Year.	Number of bankruptcies.	Year.	Number of bankruptcies.
1870	8,151	1876	10,848
1871	8,164	1877	11,247
1872	8,112	1878	13,630
1873	9,064	1879	15,732
1874	9,250	1880	12,471
1875	9,194	1881	11,632

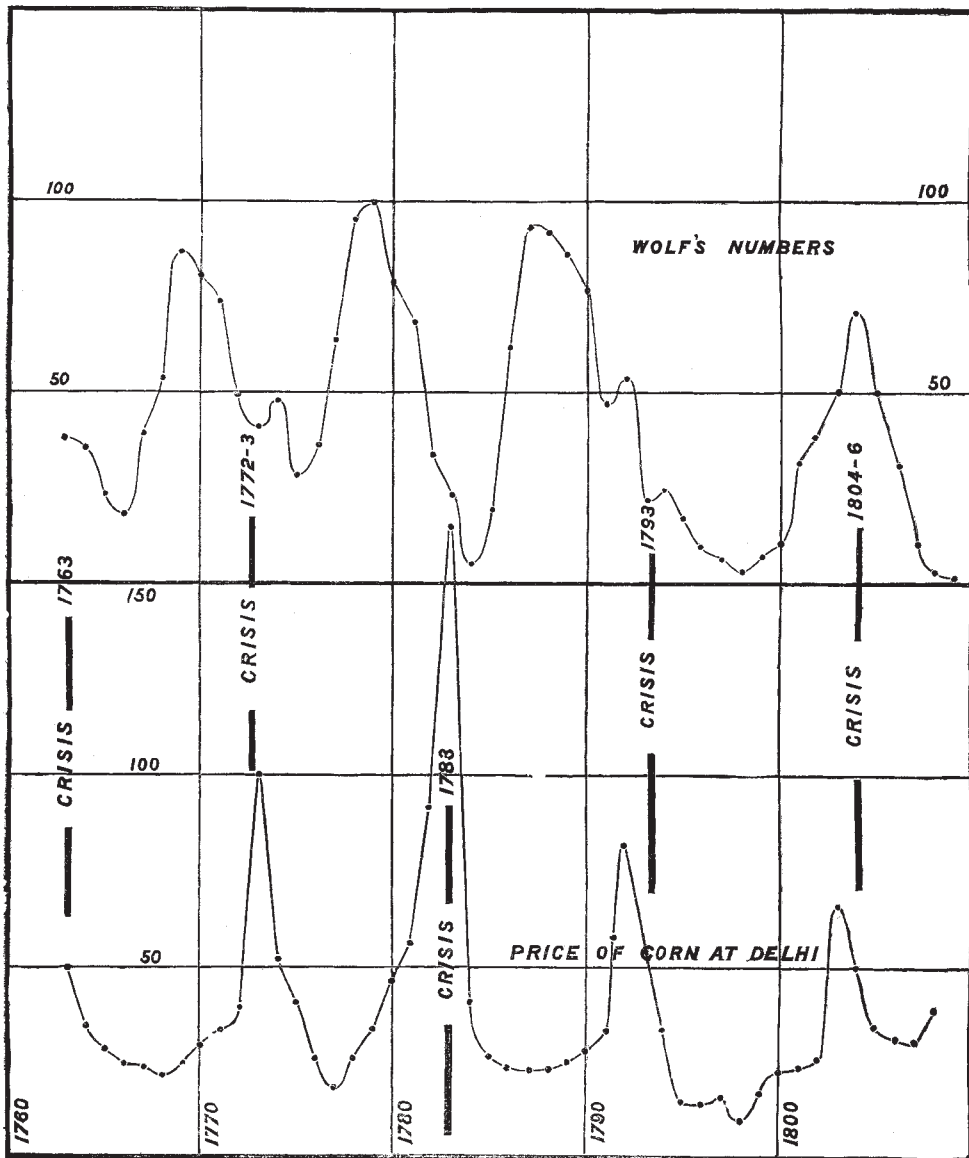
It will be remembered that the crisis of 1878 was precipitated by the failure of the City of Glasgow Bank owing to great losses of their customers in the Indian

trade, the depression of that trade being caused by the recent famine in India.

As a good deal of misapprehension has arisen concerning the American Crisis of 1873, it is well to quote the following valuable statistics from the Annual Circulars of Messrs. R. G. Dun's mercantile agency:—

Year.	Number of failures.	Amount of liabilities in dollars.
1873	5,163	228,589,000
1874	5,830	155,239,000
1875	7,740	201,060,353
1876	9,092	191,117,786
1877	8,872	190,669,936
1878	10,478	234,383,132
1879	6,658	98,149,053
1880	4,735	65,752,000
1881	5,582	81,155,932

Although the amount of liabilities involved in the failures



of 1873 was larger than in any subsequent year except 1878, the number of failures was less than in any year named except 1880. The average liability of each failure in 1873 was \$14,274 compared with 22,369 in 1878. It is

thus apparent that the crises differed entirely in character, and I believe that the collapse of 1873 was mainly due to the breakdown of values of properties necessarily following sooner or later upon the contraction of the paper

currency. In any case there was a very distinct maximum of failures in 1878, succeeded by a sudden reduction, and it occurred at a time differing by less than a year from the corresponding collapse in England. In the Dominion of Canada there was a very strongly marked maximum of failures at the same time as in England, namely, in 1879.

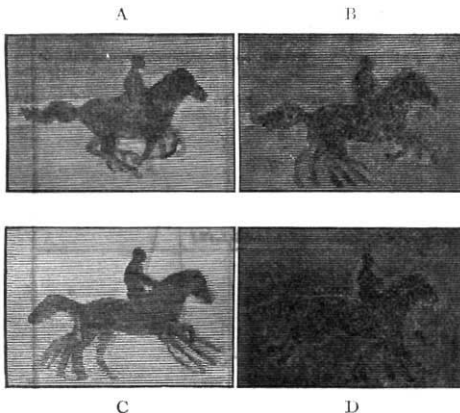
The theory of the solar-commercial cycle and of the partially oriental origin of decennial crises has received such confirmation as time yet admits of. I am, however, fully alive to the weight of some of the difficulties and objections which have been brought forward against the theory. These objections are far from being conclusive, and I may hope to give them in due time a satisfactory answer. But such answer must involve more detail than can be put into a brief article.

W. STANLEY JEVONS

CONVENTIONAL REPRESENTATION OF THE HORSE IN MOTION

IT is of interest to analyse the reason why artists represent a galloping horse in a way unlike any of its real attitudes, as they have been photographed by Mr. Muybridge, and why the critical public have so long acquiesced in these incorrect representations without remonstrance. Partly, no doubt, it is owing to prevalent errors of conception which govern the judgment in its interpretation of a movement that is hard to follow. An excellent instance of this is to be seen in the Academy, in the diploma picture of Mr. Riviere, R.A., entitled "The King drinks." It is a lion lapping water in the wrong way, by spooning his tongue outwards and upwards instead of curling it backwards, like the fingers of the half-closed hand when the knuckles are to the front, an action that may be conveniently studied in the kitten. The error of preconceived ideas partly explains the conventionally extended figure of the galloping horse; but I find the latter to be largely justified by the shape of the blur made on the eye by his rapid and various movements. I wish I could reproduce on a scale, however small, any one of the many plates published in "The Horse in Motion;" but it appears that the copyright of the photographs is disputed, and there are difficulties in the way of doing so, and I must make shift without them.

I find that taking the attitudes of the galloping horse, Phryne, as an example, published in Plate XVI. of the book just mentioned, that her stride has the duration of about six-tenths of a second, and that it has been photo-



graphically analysed into twenty momentary attitudes. Also, that these may be arranged in four groups, which I will call A, B, C, and D. I have made photographic composites of each of these groups, and copies of them by the wood engraver are annexed.

A contains six attitudes, in which the legs are crumpled below the body.

B contains four attitudes, in which one or both of the hind legs are on the ground, and the fore legs are pawing in the air.

C contains five attitudes, in which both the fore and hind legs are extended.

D also contains five attitudes; the hind legs are flung back and the fore legs are on the ground.



G

G is the general composite of all the attitudes. It will be observed that in the general composite the blur somewhat justifies the conventional representation, because though the lower parts of the limbs leave no

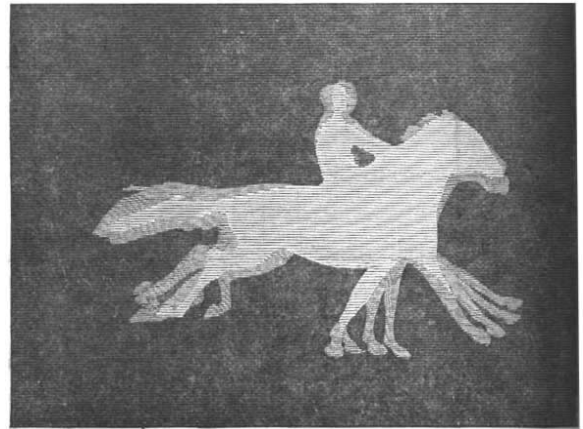


FIG. 1.

definite image at all (less so in the photograph than in the engraving), the upper portions have a distinctly outflung look, and as the artist lies under the same unhappy necessity that plagues the geographer, who,

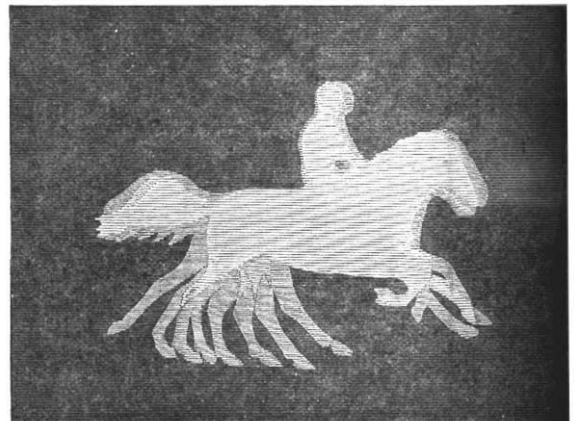


FIG. 2.

when he has to put down a lake or river on the map must put it *somewhere*, although its real position may be uncertain, so the artist thinks he must put the lower parts of the four legs of the horse *somewhere*, and he is guided