nately the readings of the barometer are not corrected for diurnal variation, although the given values of $\frac{dp}{dt}$ are so corrected; and I can only apply approximate corrections, and so obtain approximate values of Δp .

Mauritius, April 29, 1892.

	p	Δp	$\frac{dp}{dt}$	Time of arrival.
6 a.m.	29.668	0.282	0.018	2 p.m.
8 "	.597	.353	.029	2 "
9 "	.536	.414	.063	1 "
10 "	.440	.510	.094	1 "
11 "	29.304	0.646	0.131	1.30 "

The computed time of arrival is therefore 1.30 p.m., and the agreement in the last column shows that the centre was directly approaching the place of observation, and it really arrived there at 2, or 2.30 p.m.

Now at 6 a.m. the wind was 22.4 miles an hour: (4) gives $\frac{dp}{dt} = 0.0016$; (2) gives $r = 104$; and (5) gives $\Delta p_e = 1.5$, which is a little too small, the observed fall at the centre being about 2.0. If, however, we compute Δp_e for 9 a.m., we get 2.4, which is a little too large; and as in the case of time of arrival, we should be guided by a series when possible.

Jamaica, July 29.

MAXWELL HALL.

A Sparrow's Antipathy to Purple.

I HAVE but just seen your number for March 10. About five years ago I knew a tame sparrow with a great antipathy for purple. It was brought up in a room, but not, or seldom, caged. It lived four or five months. A piece of blue paper placed over its food would cause it to hesitate, though if hungry it would eventually draw the paper aside; a person coming into the room wearing a blue dress would make it quite wild, and a habit of mischievously pecking at a certain part of the wall of the room was successfully stopped by hanging a piece of blue paper there. This sparrow was taught to be cleanly in its habits. I had put off writing this to you in hopes that others who saw more of the sparrow would have written a more detailed account, but trust this letter may not be too late for any one interested to get a young sparrow from the nest this year and rear it. Sparrows have not yet reached Borneo.

G. D. HAVILAND.

Sarawak, June 17.

Bumping in the Lane Fox Mercurial Pump.

CAN any reader of NATURE favour me with a method by which the bumping in the Lane Fox pump may be obviated? I find that when exhaustion is pressed to a certain point, the bumping becomes so violent, in spite of the utmost care in lowering the reservoir, that the bulb of the pump is constantly cracked.

D. G.

Lahore, July 25.

CARL SCHORLEMMER, LL.D., F.R.S.

CARL SCHORLEMMER having been my friend and colleague in Owens College for more than thirty years, it is with a sad pleasure that I take up my pen to record in the columns of NATURE some few details of his character and work. He had not, like his predecessor Dittmar, been a fellow student with me in Heidelberg, but had worked at chemistry in Darmstadt, where he was born, and at Giessen. In 1858 Dittmar, who up to that year had been my private assistant, obtained the College appointment of Demonstrator, and he strongly urged me to offer his vacant post to his friend Schorlemmer, a young man of great promise. From the time of his arrival in Manchester until the day of his death I do not recollect that in all the intercourse of those years Schorlemmer and I ever had a single serious difference.

Whilst my private assistant he and I examined the relation which the aqueous acids exhibit as regards boiling point and composition, and I remember well the difficulties we had to contend with in distilling fuming nitric and hydrofluoric acids under pressure, and I also remember how successfully he met them. Once, I know, he got some fuming hydrofluoric acid on his hand, and he bore the scar of the serious burn to the end. This work with me was his apprenticeship. In a short time Dittmar left us, and Schorlemmer took his place as the official Laboratory Assistant, and as we had not many students at that time, he had leisure to begin the hydrocarbon work which has placed his name high in the list of organic chemists of the century. In 1861 the late Mr. John Barrow, of the Dalton Chemical Works, Gorton, brought me a sample of the light oils which he had obtained in the distillation of cannel coal. At that time our knowledge of the chemical composition of the low-boiling coal-oils was very incomplete, and I urged Schorlemmer to undertake the investigation. This was the beginning of the work which led to a result which altogether modified the existing ideas concerning the constitution of the paraffin hydrocarbons, and paved the way for the sound foundation upon which the organic portion of our science has since been successfully laid. In order to appreciate Schorlemmer's results let us for a few moments glance at the position of the question when he commenced work. Before 1848 the only known member of the paraffin series of hydrocarbons, was methane CH_4 . In the above year the researches of Kolbe on the electrolysis of the fatty acids, and of Frankland on the isolation of the alcohol-radicals, opened out new fields yielding a rich harvest. Each molecule of these latter hydrocarbons was supposed to contain two molecules of the radical methyl being represented as $\begin{Bmatrix} \text{CH}_3 \\ \text{CH}_3 \end{Bmatrix}$, whilst together with these a second

series of hydrides was believed to exist, $\begin{Bmatrix} \text{C}_2\text{H}_5 \\ \text{H} \end{Bmatrix}$ ethyl hydride standing in the same relation to the radical as an alcohol does to an ether. The truth of this view seemed confirmed by Wurtz's discovery of the existence of the so-called mixed radicals in which two molecules of different hydrocarbons, such as ethyl and amyl $\begin{Bmatrix} \text{C}_2\text{H}_5 \\ \text{C}_5\text{H}_{11} \end{Bmatrix}$ occurred. How was this question to be settled? Schorlemmer at once seized upon the correct method of solution and carried it out successfully. If, said he, the radical methyl $\begin{Bmatrix} \text{CH}_3 \\ \text{CH}_3 \end{Bmatrix}$ is identical with hydride of ethyl $\begin{Bmatrix} \text{C}_2\text{H}_5 \\ \text{H} \end{Bmatrix}$ not only must these two bodies possess the same properties, but both bodies must yield the same product, viz., ethyl chloride, on treatment with chlorine. This identity he proved, not only in the above—the most simple case—but in the more complicated cases of ethyl-amyl $\begin{Bmatrix} \text{C}_2\text{H}_5 \\ \text{C}_5\text{H}_{11} \end{Bmatrix}$ and of di-amyl $\begin{Bmatrix} \text{C}_5\text{H}_{11} \\ \text{C}_5\text{H}_{11} \end{Bmatrix}$ as these hydrocarbons yielded respectively chloride of heptyl and chloride of decetyl, $\text{C}_7\text{H}_{12}\text{Cl}$ and $\text{C}_{10}\text{H}_{21}\text{Cl}$. It is difficult to overrate the importance of this apparently simple discovery. It laid for ever the ghost of the existence of two sets of isomeric hydrocarbons of the paraffin series, and paved the way for Kekulé's theory of carbon combination, upon which the whole modern theory of organic chemistry is based. So to Schorlemmer belongs the credit of placing in position the foundation-stone of our science. And at once his name became known as a master wherever chemistry is studied; so that in 1871 the Council of the Royal Society admitted him to the Fellowship at once, an honour conferred nowadays on few.

But it was not only as an expert experimentalist that Schorlemmer excelled, and his thirty-two papers catalogued in the Royal Society list prove that he was a successful one. He possessed an exhaustive knowledge, un-