

valves, the whole area being greater than that of any other section of the column. Now, the question at issue is, whether by this arrangement the semilunar valves bear any less pressure because a portion of the base of the column rests upon the wall of the ventricle. That they do not may be sufficiently proved by the following considerations.

It is a generalisation from Pascal's law that "when a liquid enclosed in a vessel is submitted to an external pressure, every plane surface that we can imagine in the interior of the vessel experiences a pressure proportional to its area." As a consequence of this law, it follows, if the force impressed upon our imaginary surface represent the total force of reflux, that the pressure sustained by the whole area of the base will be considerably greater than the actual force of the column, and this increase of pressure will be proportional to the difference between the areas of the two surfaces. Also, the pressure upon the semilunar valves will be entirely independent of the pressure upon the rest of the base, and will be directly proportional to their own extent. It may be concluded, therefore, that whatever the condition of things at the base of the aorta may be, no mechanical advantage is gained thereby; indeed, if the area of the valves be equal to that of the surface we have taken, they will sustain a pressure equal to the total force of reflux of the column. Hence, by extending the area of the base over the wall of the ventricle, the only effect is to increase the total amount of pressure sustained, without at all lessening the pressure upon its original extent.

It is true that if the aortic orifice contract with the muscular substance of the ventricle, that in this way, *i.e.* by decreasing the area of the valves, a varying amount of advantage would be gained which would be greatest at the time of greatest contraction. This condition is, however, the only one that can at all favour the idea that "the reflux is most efficiently sustained by the muscular substance of the ventricle," and as this condition is doubtful, it must still seem that the main feature of Mr. Savory's theory cannot be supported.

W. PERCY ASHE

Phœnician Characters in Sumatra

IN a short communication to the Anthropological Institute in December last (*NATURE*, vol. xi. p. 199), Phœnician characters were stated by me to be still in use in South Sumatra. As many of your readers may be glad to have more information on the subject, I write to say that the district above alluded to includes Rejang, Lemba, and Passammah, between the second and fifth parallels of south latitude. Several manuscripts, on bamboo, from this region are preserved in the library of the India Office; and a Rejang alphabet is given by Marsden in his "History of Sumatra," third edition. Some of his characters, however, appear to have been incorrectly copied. About half the Rejang letters are admitted by all the Oriental scholars to whom I have shown them to be Phœnician of the common type; others being similar to forms found in Spain and other Phœnician colonies. Most of the letters are *reversed*, a peculiarity which is explained by the fact that the Rejang writing, according to Marsden, is read from left to right, contrary to the practice of the Malays generally. The matter is of great interest, and, it is to be hoped, will be investigated by Phœnician scholars.

J. PARK HARRISON

Ring Blackbird

IN my letter in *NATURE*, vol. xi. p. 187, I did not refer to the Ring Ousel, for it did not occur to me that anyone would suppose that, with the apparatus of so many standard works on birds, I could fail to identify my bird, if he were a Ring Ousel, male or female. I therefore add that my bird is in no respect (save the prevailing colour) like that species of *Turdus*. It is *exactly* like a female blackbird, save that it has a white ruff, in the position of the Barbary Dove's ring, and white spot under the chin. I have never seen a Ring Ousel, or the picture of one, with those characteristics. Besides, the Ring Ousel is migratory, and would hardly be seen till the spring.

Athenæum Club, Jan. 16

C. M. INGLEBY

[Considering the time of year at which this specimen was obtained, it is more probable that it is a pied variety of the blackbird (which is far from uncommon) than a Ring Ousel. If our correspondent will forward the specimen to us, for examination, we will settle the point for him, and return it.—ED.]

OUR ASTRONOMICAL COLUMN

THE TOTAL ECLIPSE OF THE SUN ON APRIL 6.—Dr. Janssen's station for the observation of this eclipse is mentioned as probably Hué, the position of which place, as laid down on the Admiralty Chart of Cochin China, is in longitude $107^{\circ} 38'$ east of Greenwich, and latitude $16^{\circ} 29'$ north. For this point the *Nautical Almanac* elements give the following figures:—

First contact at 1h. 38m. 6, local mean time, 130° from the sun's N. point towards the west, for direct image. Totality begins at 2h. 57m. 2s., and continues 3m. 12s., the sun at an altitude of 46° .

ENCKE'S COMET will no doubt be within reach as the moon withdraws from the early evening sky. The positions subjoined are reduced to 8h. Greenwich time from the ephemeris of Dr. von Asten, of Pulkova, published by the Academy of Sciences of St. Petersburg:—

		R.A.	N.P.D.	DISTANCE.
		h. m. s.	° ' "	
1875—Jan.	24	23 23 31	85 40'6"	1'989
	25	— 24 53	85 32'9"	
	26	— 26 16	85 25'0"	
	27	— 27 40	85 17'0"	
	28	— 29 6	85 8'8"	1'977
	29	— 30 33	85 0'4"	
	30	— 32 2	84 51'9"	
	31	— 33 31	84 43'3"	
Feb.	1	— 35 2	84 34'5"	1'961
	2	— 36 34	84 25'5"	
	3	— 38 8	84 16'4"	
	4	— 39 43	84 7'1"	
	5	23 41 20	83 57'6"	1'940

Mr. Otto Struve writes that Dr. von Asten's calculations show the last three revolutions of this comet can be perfectly represented by a uniform mean motion, without the hypothesis of a resisting medium, and even with greater precision than all the previous observed returns with that hypothesis. At the same time, during more than one revolution, something like acceleration has been indicated, and nearly to the same amount as Encke had supposed. This was the case between 1862 and 1865. Again, in other revolutions, as between 1845 and 1848, the acceleration has been subjected to very considerable changes. In the actual state of his researches Dr. von Asten is inclined to conclude that the existence of a resisting medium is not proved by the motion of Encke's comet, and that the observed acceleration in several returns ought to be attributed to the action of other forces; for instance, repulsive power produced by the approach of the comet to the sun, the effect of which might vary considerably, according to the conditions in which the return to perihelion takes place. A short paper by Dr. von Asten on this interesting subject is in the press.

WINNECKE'S COMET OF SHORT PERIOD, last visible in 1869, will also be observable in the morning sky from about the next new moon. The ephemeris calculated by Prof. Oppölzer of Vienna will be found in No. 2,016 of the *Astronomische Nachrichten*. This comet will probably be faint, while it remains visible at the present return. It arrives at perihelion on March 12, and at its least distance from the earth on February 15. It is Comet 1819 (3), and Oppölzer thinks he has identified it with one of the imperfectly observed comets in 1808. The elements which have been determined for 1875 show that the comet now makes a very close approach to the orbit of Jupiter; indeed, in heliocentric longitude $109^{\circ} 25'$, the distance between the two orbits is less than 0'06 of the earth's mean distance from the sun; this point is passed rather less than two years before perihelion passage. So far as can be judged at present, the comet will not be liable to great perturbation from the attraction of Jupiter till the year 1907, when it is possible a complete