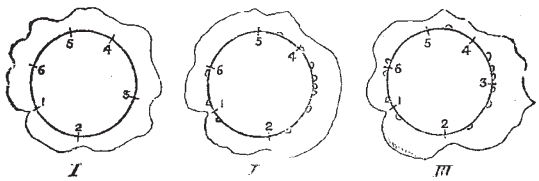
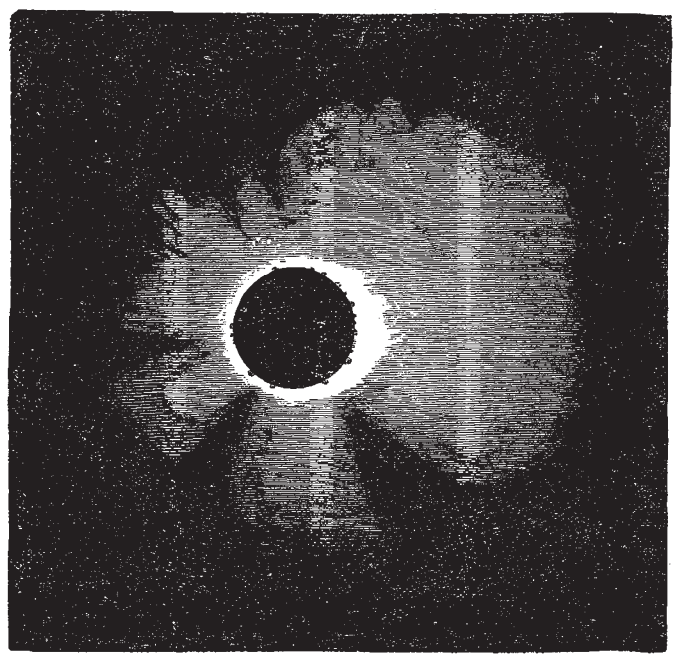
on the Eclipse Expedition. In Mr. Lockyer's article it is stated :—"Now at Syracuse Mr. Brothers also photographed rifts, three rifts, but the sketches did not record a single one;" forgetting, evidently, that at Syracuse no



From Prof. Watson's Drawing. From American Photo. taken at Cadiz. From Photograph taken at Syracuse.

attempt was made to sketch the Corona either by our own party or the Americans. At Agosta Mr. Brett was stationed, but as the Eclipse was only visible there for about *five seconds*, of course in that time no artist could pretend to make a drawing. It happens, however, that Prof. Watson was at Carlentini, and being favoured with a clear sky he succeeded in making a very careful drawing, which I had the good fortune to see and compare with my photograph No. 5 a few days after the Eclipse. An outline of this drawing I now give, so that it may be compared with the photographs made in Spain and at Syracuse.

There are two or three points which must be considered in comparing drawings and photographs. The photographs will differ according as they are made with a camera or telescope, and the drawings will differ according as they are made with the aid of a telescope or without. With the telescope the field of view is limited, and the eye is naturally attracted chiefly by the intense light of the red prominences and the corona near the moon's limit. Naked-eye drawings ought to be as valuable as photographs, but I doubt if any two artists will ever be found to make sketches agreeing in every particular. On photography must we depend for settling doubtful points of this nature, and it seems to me in this case to be absolutely settled that three rifts are identical. The outline sketches speak



THE LATE ECLIPSE, AS PHOTOGRAPHED AT SYRACUSE

for themselves. A pair of compasses applied to the points for ned by lines drawn from the moon's centre to the centres of the depressions (or rifts) in the corona, will show whether or not the places of the three gaps are the same.

It may be said that Lord Lindsay's photographs taken five miles from the station occupied by the American observers in Spain, do not show the rifts. This, I think, must be accounted for by the presence of cloud. The cloud may have been so thin as to be quite invisible in the feeble light of the Eclipse, but yet sufficient to prevent the photographic delineation of the rifts. Three of my photographs were taken through cloud, and they show us traces of rifts. The fifth plate shows three distinctly, and less plainly five or six others.

Professor Watson's drawing shows two gaps corresponding with 1 and 6 in both photographs, and depression in the corona agreeing very closely indeed with my picture.

This evidence seems to me to be absolutely irresistible as to the identity of the great rifts in the corona.

In explanation of the way the outline drawings have been made, I may say that the points marked from 1 to 6 have been pricked through the photographs, Professor Watson's drawing having been reduced to the same scale as the photographs, and pricked off in the same manner.

A. BROTHERS

EXPEDITION OF THE "DUQUESNE"

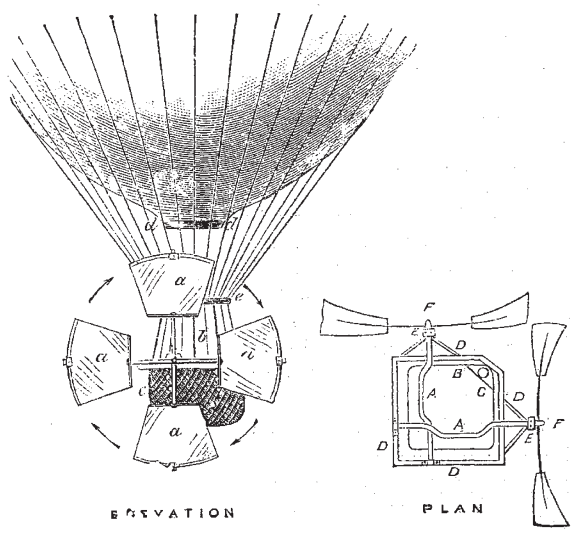
M. RICHARD, master in the Royal Navy, directed the Expedition, and is now attached to the Lille aëronautic station for the Department of the North. I have interrogated him and elicited from him the following details, which can without inconvenience be placed before the eyes of the general public. The French Republican Government having in view the promotion of general knowledge, as well as the defence of the national integrity, did not object to any communication which is not directly connected with warfare.

The aërostat, "Le Duquesne," was despatched from Paris on January 9, at three o'clock in the morning, before a large attendance, among them some members of the French Institute. The

reigning current was a strong S.W. wind, which was unfavourable for escaping the Prussian lines, as the intended directing power was only a motion of three feet per second. The experiment should have been postponed for a fair trial. Another drawback to the Expedition was the despatch of the balloon in the night-time, although the moon, being almost full at the time, afforded some light to the aërial travellers, being very low on the horizon when "Le Duquesne" left the Orleans railway station. There were in the car M. Richard and three sailors of the national navy, so that two could be kept pulling without interruption; three sacks of despatches, four pigeons, eight sacks of ballast, thirty kilogrammes each, were also in the car. The provisions were bread, wine, and chocolate. The weight of the machinery was 300 pounds. As will be seen by the accompanying diagram, the two screws were worked by a very simple contrivance, rotating only at a rate of twenty-five rotations per minute, but the diameter four yards, so that the motion in feet per second of the screws was about sixteen, five or six times more than the rapidity intended to be given to the balloon, which was three miles an hour.

Before leaving ground the car had been placed with its diagonal due north. But that precaution proved useless, as the balloon rotated when ascending before the screws could be put into operation.

We will give the explanation under M. Richard's own authority, with some remarks. We are certain of his perfect truthfulness, but it is very hard to say if he saw everything correctly for the whole length of the eventful journey.



THE DUQUESNE DIRECTING BALLOON, DESPATCHED FROM PARIS JANUARY 9, 1871

a a a a segments of the screw revolving in the direction of the arrow or the opposite direction, with variable rapidity; *b* valve line; *c* car; *d* appendage *e* hoop.

A A handle for moving screw, two men pulling at once; C place of the captain; B place of one man; D D board in iron tubes fixed to the car; E E extremities of the axes; F F fixing screw, which can be removed so that wheels may be thrown overboard when landing.

The observers on the ground had placed themselves in a right position to ascertain the effect of the screws, and the *compte-rendu* published in Paris gave the summary of their impressions. They suppose M. Richard succeeded in giving to his balloon a deviation of 15° from the due course of the wind during the earlier part of his journey. If experiments had been made during daylight, matters should have been more easy to ascertain. If I can procure authorisation, I will get an experiment tried anew at the Crystal Palace before an English audience. The fact is that two of the three sailors pulled with all their strength during a few minutes, after having exhibited some hesitation in the first instance. The scenery was so magnificent that it was necessary to call them twice before they began to pull.

M. Richard soon perceived that he was unable to ascertain the effect of the propeller. He resolved upon letting the balloon follow its way undisturbed, and he noted carefully the barometric altitude and the direction. When passing over Prussian lines at an altitude of $0^{mm}\cdot69$, some shots were directed at the balloon without any result.

At $3^{h}\ 10^{m}$ altitude 0·68° was reached; temperature 20° F. The Valley of the Marne was below.

M. Richard turned the screws and tried to pull towards the north, in order to go N.N.E; but the rotation was difficult to stop. It was necessary to work only one screw during a long time in order to rotate the aërostat in the right direction. It was only when some real torsion was established between aërostat and car that the required revolution took place. But when movement was given to the balloon it was difficult to prevent it, and the aërostat executed one entire revolution against the will of its captain. Then the two screws were worked together; the same effects were produced, but it was only with great difficulty that the car was placed in the right direction. When the two screws were worked together, the balloon was rotating sometimes in one way and sometimes in another. These observations, moreover, says M. Richard, were made at night by a man who had never before ascended in a balloon. As already said they cannot be considered as wholly reliable, as rotation depends on the changing of the fuel as well as on the friction in the forward half or on inequality in the pulling of the screws. But it is very easy to understand that the fact of the screws being able to rotate the balloon in a given direction is unquestionable, although men and captain were equally unable to move it in the right direction for escaping Prussian lines.

The difficulties experienced were so great that M. Richard stopped the experiment, and tried it again only after day-break, but with no other success than previously.

At 7 o'clock in the morning, altitude 65 centimetres, temperature 11° Fahr., cirrus was visible at a great height, cumulus down below, and stratus covering the earth.

At 11 o'clock only one sack of ballast was left. The balloon stopped its descent; voices of peasantry were heard from above the stratus. They cry "Versy; commune of Chigny." The balloon emerging in stratus, fell rapidly by condensation. All the ballast was thrown away, and the balloon ran horizontally for two miles. The guide rope only had been thrown; then the grapnel was thrown out. The wind was so strong that the grapnel rope was broken. The screws and wheels had been thrown overboard, but the car incliners and the screw axes project outside. One of these comes into contact with the ground, the car is upset, and the crew are dragged under it for a length of time, 600 yards. The balloon is stopped by the loss of gas and the peasantry. The three sailors are slightly injured, the captain is left for dead. The sailors, helped by the peasantry, make their escape, and conceal balloon, car, and despatches in the woods. The captain is brought like a corpse into a neighbouring town, but is found to be alive. He is cured, and sent secretly into Lille, where the balloon arrived two days later.

W. DE FONVIELLE

NOTES

AT a recent meeting of the American Philosophical Society it was resolved "That a committee, consisting of the President of the Society and five members, be appointed, whose duties it shall be to consider and report whether it is desirable, and if desirable, whether it be practicable, to establish in the City of Philadelphia, under the auspices of the Society, an Observatory, astronomical and physical, either or both; and if so at what cost, on what site, and what instruments are requisite for such purposes, and at what cost such instruments can be procured." We suppose such a proposal for England would be looked upon as a joke, and yet already we cannot compare with America in our observing power.

WE are informed that the Royal Commission on Scientific Instruction and the Advancement of Science have their First Report nearly ready.

DR. J. CLERK MAXWELL, F.R.S., was elected yesterday to the Professorship of Experimental Physics in the University of Cambridge.