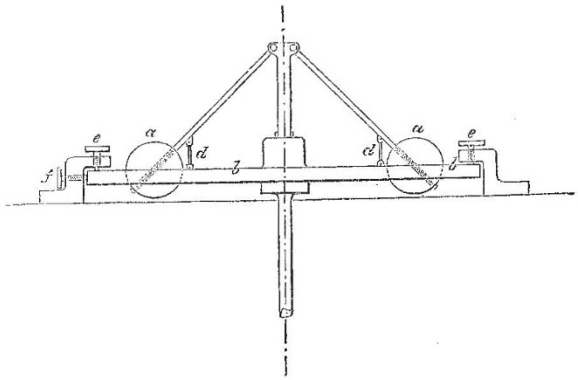
in use on the clock of the apparatus of Cape Bon, Tunis, an apparatus exactly similar to that now standing in the International Exhibition. It consists of a shaft making 170 revolutions per minute, to which the balls *a a* are hung, and on which the disc *b b* can slide, guided by a feather key. When the clock is below speed the disc rests upon a collar fixed on the shaft, the pull exerted by the balls through the links *d d* being insufficient to raise it; but as soon as the proper speed is attained, the disc rises and comes in contact with the screws *e e*, which are tipped with leather and fixed to the frame of the clock. Spaces are cut out of the disc to admit the balls, avoiding unnecessary height. The screw *f* serves as a brake to stop the clock at pleasure. I



calculate that work to the extent of five foot-pounds per minute must be done on the governor to accelerate the clock one second per hour. This form possesses two advantages over that in which the rubbers are carried by the balls—1. It checks any acceleration of the clock more powerfully; 2. It is easier to adjust. In the older form it is necessary to ascertain by careful experiment that *each* ball shall bring its rubber into contact exactly when the speed is correct, whereas in this it is immaterial that the arms of the balls should be exactly equal; it is only needful that they should *together* raise the disc to contact when the speed is right. J. HOPKINSON

Glass Works, near Birmingham, Sept. 1

Rainbows

As a pendant to my note inserted in NATURE, vol. x. p. 437, I may mention that an exceedingly fine lunar rainbow was observed here at 8.40 P.M. on September 29.

Though the moon was near the last quarter, the bow was bright enough to appear reddish on one side and greenish on the other. It is the only one, of some five or six lunar rainbows I have seen, which appeared to show any trace of differences of colour.

I may also mention that about the end of August I saw, two hours after sunrise, a dazzlingly bright and gorgeously coloured parhelion in a small ice-cloud to the right of the sun, the rest of the sky being almost perfectly clear. There had been a sudden and considerable fall of temperature during the previous night.

St. Andrew's, Oct. 2 P. G. TAIT

IN NATURE, vol. x. p. 438, Mr. Schuster complains that in text-books no mention is made of supernumerary rainbows, and that the theory of them is to be sought in original memoirs, not generally accessible. Allow me to mention that in Sir John Herschel's Meteorology (a little work published by Black, price three and sixpence, and originally an article in the Encycl. Britann.), a complete explanation of the rainbow, and of the supernumerary bows as well, on the principle of interference, is to be found. F.M.S.

U.S. Weather Maps

IN Prof. Loomis's "Results of an Examination of the U.S. Weather Maps for 1872 and 1873" (published in the *American Journal of Science and Arts*, and recently noticed in NATURE, I am struck not only by the general agreement but by the almost verbal coincidence of one or two of his "Results" with some of the rules laid down in my work on the "Laws of the Winds prevailing in Western Europe," which was published in the beginning of 1872.

In "Laws of the Winds," Part I. p. 56 and following, I have shown that "we are unable to account for the eastward progress of depressions by attributing it to prevailing westerly upper-currents," but that "each system of depression appears to travel eastward with a kind of self-developed motion," and that the precipitation on the east side of the centre "is the principal agent in producing the change of geographical position." Prof. Loomis writes: "The progress of a storm eastward is not wholly due to a drifting resulting from the influence of an upper-current from the west, but the storm works its way eastward in consequence of the greater precipitation on the eastern side of the storm."

Prof. Loomis also appears to attribute the formation of some depressions, primarily developed in the United States, to the collision of moist air from the Pacific with the mountains in the north and west, in the same way as I have attributed the primary formation of some of our depressions to the collision of the vapour-laden atmosphere from the Atlantic with the high-lands in the west and north of the British Isles.

I am glad to observe that Prof. Loomis is no advocate of the "circular theory" of storms as still held by some meteorologists. He intimates the mean inclination of the wind towards the lower isobars as "more than 45°" in the United States. In the Journal of the Scottish Meteorological Society, No. xxxix. I have shown that at stations in the British Isles the mean inclination is 21°, but that it appears to be considerably higher in continental Europe.

In the work previously alluded to I have shown that depressions appear to travel most to the south when the atmosphere is warmer in the west than in the east, and most to the north under contrary circumstances, but that this influence is interfered with by another, viz., the tendency of depressions to travel so as to have the highest general pressures on their right. A less limited acquaintance even than I can claim with the U.S. Weather Maps would go far to show which of these two influences is the predominant, the general atmospheric conditions of the United States presenting a better field for their investigation than is to be obtained in Europe. Prof. Loomis finds that in North America storms tend most to the south in July and to the north in October. It would be interesting to inquire whether this observation holds good of depressions on the Pacific coast, as well as near the Atlantic. But a two years' average is insufficient to settle such questions.

On the whole it is satisfactory to find that some important results obtained from a study of European weather-charts are found, on good authority, to be in accordance with those derived from the U.S. maps. At the same time some of the theoretical remarks made by Prof. Loomis will not, I think, be generally endorsed by meteorologists. The statement that "it needs no argument to prove that when the wind is flowing from all quarters inwards upon a central area, there is a rapid accumulation of air, which can only escape by an upward motion," is incorrect; the depression of the barometer in the centre showing that there is no accumulation, but a rarefaction, produced in part, as Prof. Loomis has himself previously shown, by precipitation, and which is itself the cause of the influx.

Under the present conditions of anemometry all endeavours to calculate the upward movement in a storm from anemometrical data should also be accepted with much reserve. Still more hazardous (considering the inclination of depression-axes and the frequent difference of direction between currents at small and those at great elevations) is the attempt, in such an inquiry, to correct the observed velocities at sea-level by those on the summit of Mount Washington. With a depression in Eastern Canada a west wind not uncommonly blows on Mount Washington while more southerly airs are felt at the three nearest stations. If in such a case we calculate the amount of influx towards the depression-centre simply from the ratio between the velocity at sea-level and that on Mount Washington, it is obvious that the result will be the reverse of accurate.

Aug. 25 W. CLEMENT LEY

Aurora

ON Sept. 11 I was at Kyle Akin (Skye). The day had been wet and stormy, but towards evening the wind fell and the sky became clear. About 10 P.M. my attention was drawn to a beautiful auroral display. No crimson or rose tint was to be seen, but a long low-lying arc of the purest white light wa