perature being recovered as well as the volume. The sole result of the cycle is that heat is raised from a lower to a higher temperature. Since this is assumed to be impossible, the supposition that the operations can be performed without external work is to be rejected—in other words, we must regard the radiation as exercising a pressure upon the moving piston. Carnot's principle and the absence of a pressure are incompatible.

For a further discussion it is, of course, desirable to employ the general formulation of Carnot's principle, as in a former paper.[3] If p be the pressure, θ the absolute temperature,

$$\theta\frac{dp}{d\theta}=M \quad . \quad . \quad . \quad . \quad . \quad . \quad (29),$$

where Mdv represents the heat that must be communicated, while the volume alters by dv and $d\theta=0$. In the application to radiation M cannot vanish, and therefore p cannot. In this case clearly

$$M=U+p \quad . \quad . \quad . \quad . \quad . \quad . \quad (30),$$

where U denotes the volume-density of the energy—a function of θ only. Hence—

$$\theta\frac{dp}{d\theta}=U+p \quad . \quad . \quad . \quad . \quad . \quad (31).$$

If we assume from electromagnetic theory that

$$p=\tfrac{1}{3}U \quad . \quad . \quad . \quad . \quad . \quad . \quad (32),$$

it follows at once that

$$U\propto\theta^4 \quad . \quad . \quad . \quad . \quad . \quad . \quad (33),$$

the well-known law of Stefan.

In (31) if p be known as a function of θ, U as a function of θ follows immediately. If, on the other hand, U be known, we have

$$d\left(\frac{p}{\theta}\right)=\frac{U}{\theta^2}d\theta,$$

and thence

$$\frac{p}{\theta}=\int_0^\theta\frac{U}{\theta^2}d\theta+C \quad . \quad . \quad . \quad . \quad . \quad (34).$$

RAYLEIGH.

[3] "On the Pressure of Vibrations," *Phil. Mag.*, iii., p. 338, 1902; "Scientific Papers," v., p. 47.

"Atmospherics" in Wireless Telegraphy.

THE greatest difficulty in wireless telegraphy is due to atmospherics. I believe that every attempt to prevent these sudden shocks from entering the receiving apparatus in important stations has failed. Now Mr. S. G. Brown has wires stretched horizontally from his house to his stables in Kensington at about 40 ft. from the ground; he receives all the ordinary messages and time signals with practically no sign of atmospherics. Of course, lessening the height of high antennæ lessens the energy received, but it seems that the diminution of the blow is much greater than the diminution of ordinary signals. One of Brown's latest relays magnifies the currents in the receiving apparatus one hundred times, and he expected that the signals would be well received, in spite of the lowness of his wires, but he was surprised to find that the blow, the atmospheric, had almost altogether disappeared. In fact, there was no blow to magnify. I believe that the Salcombe Hill Observatory arrangement for receiving time signals is also free from atmospherics, its antennæ being quite low, and a Brown relay being used.

If the following explanation of this curious phenomenon is correct, it ought to be easy to destroy atmospherics however high the antennæ may be.

An antenna is affected by rays of all frequencies because its vibrations are damped by resistance, although it is, of course, most sensitive to rays of its own frequency. An atmospheric is of the nature of a sudden shock; it consists of rays of all frequencies, and particularly of rays of all sorts of very high frequencies. Suppose the frequency of the antenna to be anything from 50,000 to 300,000 per second; let us say 100,000. I take it that houses and trees are very imperfect antennæ the frequencies of which are probably much greater than 100,000 generally, although sometimes less. When rays are proceeding horizontally the æther in the neighbourhood of trees and houses is therefore greatly robbed of all energies which accompany waves of high frequency. In fact, all rays of frequencies corresponding to the frequencies of trees and houses are absorbed, and a low antenna of frequency 100,000 receives but little energy of other frequencies than its own, and therefore little of the "atmospheric" blow. If this explanation is correct, it is only necessary to surround a receiving antenna by numerous others of all sorts of high frequency. If I am right it is scarcely possible to receive atmospherics in the middle of a large city unless the ground is much higher than neighbouring ground, just as we know that an ordinary house in the middle of a city is never struck by lightning.

My explanation cannot be complete, for the man in charge of a coast station in the Mediterranean states that he has difficulty in receiving signals because disturbing atmospherics are so numerous, whereas ships in the neighbourhood, or even five miles away, are comparatively undisturbed in their signalling. Now these ships are far away from trees and houses.

Again, Mr. Brown tells me that although he receives no atmospherics from great distances, his signals are certainly disturbed by local thunderstorms. In fact, he can predict the coming of a thunderstorm when it is probably twenty miles away. My explanation may be defended by saying that the fronts of the Maxwell waves are not vertical in such cases. Again, I have been told that without altering the antenna at a receiving station, if we tune it to a lower frequency, there is more disturbance from atmospherics. It is possible that this is not generally true, but only true for certain stations, and, if so, my explanation may escape censure. JOHN PERRY.

December 30, 1913.

Columbium versus Niobium.

AT a meeting of the council of the International Association of Chemical Societies in Brussels, last September, a committee on inorganic nomenclature, among other recommendations, endorsed the name and symbol "niobium" and "Nb," for the element which was originally named columbium. As this recommendation is historically erroneous, a brief statement of the facts appears to be desirable.

In 1801 Hatchett, an English chemist, analysed a strange American mineral, and in it found a new metallic acid, the oxide of an element which he named columbium. A year later, Ekeberg, in Sweden, analysed a similar mineral from Finland, and discovered another element, which he called tantalum. Wollaston, in 1809, undertook a new investigation of these elements, and concluded that they were identical, a conclusion which, if it were true, would have involved the rejection of the later name, and the retention of the earlier columbium. The accepted rules of scientific nomenclature make this point clear.

For more than forty years after Hatchett's discovery both names were in current use; for although Wollaston's views were accepted by many chemists, there were others unconvinced. In 1844, however, Heinrich Rose, after an elaborate study of columbite and tantalite from many localities, announced the discovery of