importance. The space devoted to the other "Cataracts" of the world is small, though most of the important ones are mentioned. The illustrations are good, and on the whole the book is interesting.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Priestley and Lavoisier

If Mr. Rodwell had anything new to tell us about Lavoisier, there would have been a sufficient motive for his writing; but I do not see what useful purpose is gained by telling us what was already known, namely, that a century ago Lavoisier rendered many important services to science; or, what was not so well known, namely, that chemistry is a French science; or, that Lavoisier was "the most generous of men," "incapable of any meanness." The real question Mr. Rodwell himself asks:—"Upon what authority does Dr. Thomson assert that Dr. Priestley informs us that he prepared the gas in M. Lavoisier's house in Paris, and showed him the method of procuring it in the year 1774?"

Mr. Rodwell quotes from Thomson's notice of Priestley; had he turned to that of Lavoisier (p. 105, vol. ii. 1831, not 1830), he would have found an answer:—"Dr. Priestley discovered oxygen in August, 1774, and he informs us in his life [this ought to be "Life," *i.e.* autobiography] that in the autumn of that year he went to Paris and exhibited to Lavoisier, in his own laboratory, the mode of obtaining oxygen gas by heating red oxide of mercury in a gun-barrel, and the properties by which this gas is distinguished; indeed, the very properties which Lavoisier himself enumerates in his paper. [*Mem. Acad.* 1775, pub. 1778.] There can therefore be no doubt that Lavoisier was acquainted with oxygen gas in 1774, and that he owed his knowledge of it to Dr. Priestley."

Dr. Black complained of the publication of Lavoisier's papers without any allusion whatever to what he himself had previously done on the same subject. Cavendish complained of something more than a similar neglect. The facts, as stated in Dr. George Wilson's "Life of Cavendish," are briefly these:—Blagden went to Paris in June, 1783, and informed Lavoisier of the discovery of the composition of water. Lavoisier was incredulous, expressing his opinion that the union of the two gases (O and H) would produce, not water, but an acid. Nevertheless he repeated Cavendish's experiment on a large scale; and in his account of it to the Academy on June 25, stated that the conclusion as to the compound nature of water was drawn by Laplace and himself. The charge brought against Lavoisier by Cavendish, Blagden, and Watt, was summed up by Watt to this effect, that after Lavoisier had had the theory of the composition of water explained to him, "he invented it himself."

Mr. Rodwell "utterly denies" that the acceptance of Lavoisier's doctrine was mainly due to Cavendish's discovery. A strong objection to the oxygen theory was advanced by Berthollet and others, founded on the observation that in the action of dilute acids on metals inflammable air is produced. [The inflammable air of Cavendish, in 1766, was referred not to water, but to the metals]. Whence came this element? The discovery of the composition of water answered the objection, and converted it, as Dr. Whewell remarks, into an argument in favour of the theory.

My statement that "the compound is always equal to the sum of its elements" was already known, was elicited by a remark of Lavoisier's, quoted by Mr. Rodwell:—"I am obliged in this reasoning to suppose that the weight of the bodies employed was the same after the observation as before." My statement is new to Mr. Rodwell, and he calls for references. Many of the old writers on the idea of substance acknowledged the proposition, and its best application was Wenzel's doctrine of definite and reciprocal proportions, although its full significance did not become apparent until the aeriform elements were also taken into account.

But to return to Priestley, I am bound to admit that 1744 is a mistake into which I was misled by Whewell ("His. Ind. Sci., 1857, iii. 110), who gives that date.

Priestley was presented with the Royal Society's Copley medal, as an honourable testimony to his numerous scientific discoveries, which, considering the crude state of chemistry in his time, must be regarded as admirable. He was afterwards driven from the Royal Society and from his country, his house was pillaged, and his library, manuscripts, and apparatus destroyed, and all this persecution was on account of certain opinions which happily are now widely spread. The statue at Birmingham is a less impressive tribute to his memory than the maintenance of respect for his fame; and it is with no unfriendly feeling towards Mr. Rodwell that I express an opinion that this old quarrel between Lavoisier, Priestley, and Cavendish had better be left to repose in the history of science, where it has been discussed with sufficient fulness and fairness by such writers as Thomson, Brande, Whewell, and George Wilson.

Highgate, N., December 4 C. TOMLINSON

The Forth Bridge

In some remarks made in NATURE, vol. xxvii. p. 101, by Mr. Charles Shaler Smith, the following passage occurs:—"The tests of the last few years show conclusively, that iron exposed to compression within its buckling limit is compacted in texture and strengthened by such use while, if subjected to continuous tension beyond two-thirds of its elastic limit, it is attenuated and weakened." As I think that the words above quoted may perhaps to a certain extent mislead those who have not themselves made experiments on the elasticity of iron and steel, and on the alteration of density which can be produced by compression or extension, I would observe:—

1. That the increase of *density* which can be produced by compressing within the buckling-limit such rods as may be employed in the construction of bridges, would certainly not account for the strengthening of iron exposed to continuous compression. I have examined carefully the alteration of density which can be effected in iron and steel wires by longitudinal extension, &c., and even in cases where the wire was strained to breakage, and the permanent extension exceeded 20 per cent., there was no diminution of density equal to 1 per cent. Of course the words "compacted texture" may not mean that the density is increased, but the idea seems to be not uncommon among engineers, that increase of strength *necessarily* implies increase of density. Though I cannot at this moment lay hands upon it, I remember reading an account of some theories advanced respecting the hardening of steel, from which it was evident that the author of these theories assumed that the hardening is attended with *increase of density*, whereas the density of steel can be more *diminished* by this process than by any mechanical means with which I am acquainted.

2. It is quite true that iron, if subjected to continuous tension beyond two-thirds of its elastic limit, is attenuated, but whether the attenuation is attended with weakening or not depends largely upon the manner in which the tension is applied. If the latter is increased by small amounts at a time, and each amount allowed to act for a few hours before any increase of stress is made, not only is there comparatively small permanent extension, but there may be an actual increase of strength as far as resistance to extension is concerned. The fact is that whether we subject iron and steel to *long-continued* compression or extension, we increase the resistance to compression and extension respectively, mainly for the same reason, namely, that we *give time* for the molecules of the metal to take up such positions as will enable them to offer the maximum resistance. Thus I have proved that the value of "Young's Modulus" is considerably increased in the case of an iron wire which has suffered permanent extension, by allowing the wire to rest for some hours either loaded or unloaded; this increase of elasticity is not attended with any appreciable increase of density.

As I feel that too much precaution cannot be taken in a question of this kind, where life is at stake, I would venture to make the following suggestion:—That bars or rods of steel and iron which run the *slightest* risk of having at any time to undergo a considerable extending or compressing stress should before use be subjected, if possible, to the same kind of stress gradually increased in amount with intervals of some hours between each increase until a stress equal to at least three-fourths of the breaking-stress be reached. Three or four days would suffice to bring the metal to its maximum strength, both as regards resistance to permanent and to temporary strain.