BREAD.

Messrs. Editors:—On page 52 of the present volume of the Scientific American, a correspondent (L. K.) has some judicious remarks upon the common methods of making bread; I therefore submit the following communication, trusting it may be deemed worthy of publication.

The term "bread" may be considered as a generic word, including in its signification biscuit, cakes and pastry. People of all countries, with few exceptions, prefer "raised," or light and porous bread, to the unleavened kind. Bread may be raised by three means: by the use of leaven, dough or yeast, in a state of fermentation; by the mechanical introduction of carbonic acid; and by the chemical liberation in the dough of carbonic acid from some substance with which is is combined. If the first method is properly conducted, it is quite unobjectionable; but if, as is often the case, fermentation is allowed to proceed too long, acetic and lactic acids are formed, and some of the complex nitrogenous substances arising from the decomposition of the plastic bodies of the flour. Saleratus or soda, to sweeten the sour sponge, is now the resort of the cook; and the result is an unpalatable and unwholesome loaf, unworthy the name of bread, much less of food. The second method is impracticable in the family, where the large amount of bread consumed is and must be made. The third plan is to introduce carbonic acid, in combination with soda, bi-carbonate of soda and an acid—such as tartaric—which, combining with the soda to produce a neutral salt, liberates the carbonic acid, and thus renders the bread light and porous. Instead of tartaric acid, cream-of-tartar (a bitartrate of potassa) is commonly used to decompose the soda, and the resulting compounds left in the bread are tartrate of soda and tartrate of potassa. Whatever may be said of the wholesomeness of these two bodies, they are, to say the least, quite as palatable and as desirable as the acetic, lactate or butyrate of soda or potash, which would be formed by the attempt to sweeten a sour sponge raised by the first method—by the use of soda or saleratus. But "cream-of-tartar and soda bread," as it is called, is dry and tasteless, especially when cold; so is fermented or leavened bread, unless the fermentation is arrested by baking at *just the right time.* Ordinarily, as is well known to those acquainted with the philosophy and practice of making good bread, this "*right time*" is a period of short duration, and I presume it is within the bounds of truth to declare that not one loaf in one hundred is raised and baked when it should be. The circumstances which modify the time in which the fermentation may take place are so varying that it may occur in thirty minutes or twelve hours. The sponge requires constant watching, and this, in the multitudinous duties of the kitchen, it is not always possible to secure. The difficulty of always securing good bread by this method is so great, among the masses, that dietetists and housekeepers have, for the most part, come to the conclusion that, could any substance be devised for combination with soda, in bread-making, that would be free from the objectionable features of cream-of-tartar and at the same time supply the desirable and essential elements of nutrition, a great benefaction would be conferred upon the human family. Such a discovery seems to have been made by Professor Horsford, and I think that if what your correspondent (L. K.) says of the want of "phosphates in the blood" and of "thin bones and rotten teeth," is not clearly shadowed forth in the following extracts from a circular of the professor, it is at least clearly shown that the article devised by him will supply what L. K. considers (and what is, in fact) so much needed:—

My attention was called, five years since, to the necessity of a substitute for cream-of-tartar, as an article of domestic consumption. It was represented to me by extensive dealers, that the production of cream-of-tartar was no longer equal to the demand, and that the greatly increased consumption in the arts and for culinary purposes, had caused its price to rise, until it seemed possible that for some important purposes its further use must be given up. It was also stated, that its high price had led to frequent adulterations, some of them of more than questionable character in their relations to health. Upon these representations, I undertook the solution of the problem as one of great public importance.

Among the essential qualities of a substitute for cream-of-tartar, in the preparations of all forms of light bread, cakes and pastry, are, that the article should be at least as unobjectionable as cream-of-tartar in its relations to the animal economy—that it should be pulverulent—and that when mixed with bicarbonate of soda and flour, it should, on the addition of moisture or application of heat, yield a neutral salt, and set free carbonic acid. If, in addition to these qualities, an article could be devised which should possess, in the form in which it is used, unquestionable excellence as an element of food, its value would be placed beyond doubt.

I tried in a great variety of ways, as numerous others have tried, without success, to find some form of muriatic acid which could be mixed with bicarbonate of soda, so as, after raising the dough or paste, common salt should be found in the product. To this most desirable end, insuperable difficulties presented themselves. I sought some form of harmless organic acid, suited to all the conditions of the problem, but this effort and many others were alike fruitless. At length it occurred to me, to find, if possible, an acid constituent present in all the cereals and healthful food, and place this in the necessary conditions to fulfill the wants of the problem—and at the same time, in such form, that when taken into the system, it would be suited to the agencies there in action, to be absorbed, if needed, or readily and healthfully removed if not required. Of all such constituents no one is so important as phosphoric acid. Physiological and chemical research have shown, that wherever in the body there is an organ of important functions, there nature has provided a store of phosphates. They are present in the juices, the tissues, the muscles, and in large measure in all the brain and nervous matter, and in larger measure still, in the bones. The grains we consume contain them. The flesh we eat contains them. The bones we boil and dissolve contain them. The French army was formerly supplied with rations of dissolved bone, prepared at high temperatures in Papin's digester, in the form of small cakes, which a little hot water resolved into soup. The bran which we withdraw from our wheat contains fourteen times as much phosphoric acid as the flour which we convert into bread. The natural provision in the animal economy for the removal of surplus phosphates, as in the waste and renewal of the bones, is well-known.

All these considerations led me to the conviction that, if it were possible to prepare phosphoric acid in some form of acid phosphate of lime, such that, after its action with moist carbonate of soda, it would leave phosphate of soda (a constituent of the blood) and phosphate of lime (an essential constituent of food), and confer upon it the necessary qualities of a dry, pulverulent acid, the end would be so far attained as to justify a practical experiment in domestic use.

I succeeded in producing the article in condition to meet the wants of the problem. I then introduced it into my family for use in all forms, as a substitute for cream-of-tartar for culinary purposes. When many months of daily use had assured me that my theoretical views were sustained by practical application, I gave it into the hands of friends, whose prolonged experience fully confirmed my own. It has been in constant use in my family now for more than four years; and in the form of yeast powder, during this time, it has been produced and consumed in all parts of the country to a very large extent, settling, in the most satisfactory manner, all questions as to its serviceability and healthfulness.

The article is prepared according to instructions furnished by myself, as the result of long-continued experiment, and it will be produced of invariable purity and strength equal to that of cream-of-tartar.

E. N. Horsford.

Of the same purport, and having a direct reference to this case, are the views of Dr. Samuel Jackson, professor of the institute of medicine in the University of Pennsylvania:—

Your substitute for cream-of-tartar for the raising of bread is a decided improvement. The tartaric acid is not a constituent of the grains from which flour is made; it is not a nutritive principle, and often disagrees with the alimentary organs. The phosphate of lime, which is the principal ingredient of your preparation, is an essential constituent of all grains. It is further an important nutritive principle; and recent experiments have proved it is an indispensable element in the construction, not of bones only, but of all the animal tissues. A deficiency of the phosphate of lime in food is a common cause of ill health, of defective development and retarded growth in children. In the conversion of wheat into flour, the phosphate of lime is rejected with the bran; and, in consequence, this necessary element of nutrition, contrary to the arrangement of nature, is not obtained from our fine wheat bread. Your preparation, while it makes a light, sweet and palatable bread, restores to it the phosphate of lime which has been separated from the flour, and thus adapts it as an aliment for the maintenance of a healthy state of the organization.

Samuel Jackson,
No. 224 South Eighth-street, Philadelphia.

Of a like import are the expressed opinions of chemists and physicians of acknowledged high character and standing, which might be continued at length.

If these facts were properly placed before the public, there would seem to be no longer any excuse for having bad and unwholesome bread. G. F. W.

Mr. Prentice, of Mt. Hope, near Albany, N. Y., lost a number of cattle with *pleuro-pneumonia* in 1858.

CRITICISMS ON THE EXPERIMENTS WITH TURBINE WHEELS AT PHILADELPHIA.

Messrs. Editors:—As conductors of a journal which is looked-to and depended upon as the index by which to ascertain what is useful and worthy of recommendation, and to detect what is questionable and of no value in actual practice, there is an importance attached to your pen and publications probably far exceeding the influence which *you* suppose yourselves to exercise by the course you pursue. As constant readers of the Scientific American, we are convinced of your steady aim to advance mechanical and scientific pursuits, to guide them in the right direction and to increase their usefulness; and we perceive that, in pursuing this object, you have not hesitated to censure so influential a body of scientific men as the Polytechnic Association of the American Institute. Therefore we feel certain that, if we can satisfactorily show that the public has been misled by printed reports, you will make such correction or mention of the subject as your judgment may dictate to be just and due to the public interest. We have reference to the several articles which appeared from time to time in your journal, in regard to the test of water wheels which has taken place at Fairmount Waterworks, near Philadelphia. Said articles (covering in all some five columns of your paper) are certain to appear to the unitiated as *perfectly reliable* and *impartially-reported* results of said experiments. Now, we aver that the results of the tests, as reported, *cannot* be relied upon, and that the report published on page 22 of the present volume of your journal, when viewed in connection with the facts, gives evidence of a want of practical science and a lack of impartiality, sufficient to exclude its introduction into any record of useful and correct experiment referred to, and depended upon by the public. We admit that we are forced into the position we occupy in this communication by the injustice done to the Littlepage wheel; aside of that, however, every one, engaged in a kindred pursuit, certainly feels a desire to see correct and just information spread abroad in a matter as important in a general view as this is. We also admit that we feel unable to pursue the subject without, in the proper place, making mention of said wheel. And the position we take, we are sure, will be supported and found correct by other testimony and future experiments.

In the first place, the public are led to believe that, to procure a power of 250 horses, with 8 feet of fall and the most economical wheels, requires a sum of from $23,000 to $29,000. The Stevenson wheel is presented to the public as giving-out—with a discharge of water of only 200 cubic feet per minute—nearly 91 per cent of useful effect. It was first tested in November last, and produced 73.36 per cent; a result which was then received with satisfaction, and no mention was made of a new trial or that anything was not operating as it ought. The reported test is dated March 9, 1860, and is given at 87.77 per cent; no mention is made how many intermediate tests took place. Three per cent is added, as a guess of the loss by friction of the apparatus and wheel. No mention is made of the fact that the Jonval wheels tested had proportionally very small steps, being built expressly for a short race, without regard to durability; whereas, a Jonval wheel built for *use* requires an enormously large step, which must necessarily reduce its effect considerably; the step of the Geyelin wheel at Fairmount, although very large, was perfectly *charred* by friction! In the Kalbach and Littlepage wheels, this difference in the steps of working wheels and *race* wheels does not exist, as the water comes into the wheels from below, and more than supports the weight of wheel and gearing.

The Parker wheel was tried in November, and gave out 58 per cent; and it is reported, under a test dated February 21, 1860, as the *Smith* wheel, with 75.69 per cent—a difference of 18 per cent. The report also shows that it had a test on a horizontal shaft, giving 67 per cent; making three separate tests known to us.

The Rich wheel is reported as giving out 61 per cent, under date of October 20, 1859; while in your paper (Vol. II., page 297) it is reported as giving out 74 per cent.

The Geyelin wheel was tested in November, and gave out 68 per cent; and it is reported, under a test dated February 29, 1860, as giving out 82 per cent.

The difference made in the Rich wheel is 13 per cent; in the Geyelin, 14 per cent; and in the Stevenson, 14 per cent—a remarkable coincidence of increase of power in the last tests. In the Geyelin wheel, it is explained as the result of changing the wheel from a vertical into a horizontal position; but in the Stevenson and Rich wheels, no such change was made; and yet there is claimed the same increase of 13 and 14 per cent. In the Parker wheel, 8 per cent is claimed to be gained by the same change; still leaving 10 per cent unexplained.

No cause was shown why any of the wheels should have more than one test (meaning by "one test" a consecutive series of trials, selecting the best as the result), except in the tests of the Collins and Littlepage wheels. The Collins wheel gave out 50 per cent only in November; his packing, where the shaft passed out of the draft pipe, was imperfect; and the Littlepage wheel was locked in its solid, unyielding bearings and in the bevel gearing. Collins had a test reported, under date of February 9, 1860, with an increase of nearly 27 per cent. Littlepage was refused another test; while some of the other wheels, without showing cause for a new trial, had continued tests during the term of six months.

Of the Kalbach wheel, Mr. Birkenbine states:—"It is remarkable for its simplicity; and, had it been constructed with the same amount of care and finish as that of some of the others, it is believed that the co-efficient of useful effect would not have been surpassed by any." To this should be added that this wheel, as also the Littlepage wheel (as before remarked), has the advantage over the Jonval wheels of using the same step for a short *race*, as for common daily work; while the Jonval wheels require immense large steps to support the wheel, gearing and water. Mr. Kalbach was no doubt convinced that the Littlepage wheel surpassed his in simplicity and easy construction, though fully as durable; from the similarity of the wheels, he must have expected them to be similar in their capacities; he therefore proposed to Littlepage, reciprocally, to make common cause in the sale of the wheel, as proving which was the best of the two. The report states that a letter was addressed to Mr. Kalbach, as well as to the other parties named; but his proposal is not stated. He would no doubt have furnished the required power for a much smaller sum than those named—probably for one-half the amount. His wheel is rejected for the following reasons, which the public are expected to receive as sound, scientific and conclusive, as they ought to be from the chief-engineer and associates of one of the largest cities in the Union:—

1st. The report says:—"Our minimum head and fall is but 8 feet. To produce a power of 125-horses, by two wheels, would require each of them to be 50 inches in diameter, and *they would occupy so large a proportion of the head and fall that the co-efficient of useful results would of necessity be low.*" Such an assertion, coming from the source it does, is really astounding. We understand it to mean that the actual useful fall of 8 feet would be reduced by 50 inches, leaving a fall of nearly 4 feet; when, in fact, with the draft pipes used, if the wheels were *ten* feet high, the full fall of 8 feet would be utilized, just the same as if only 10 inches high—the difference, *if any*, is caused *not by a reduction of fall*, but by a different mechanical arrangement.

2d. It is asserted that a wheel upon a vertical shaft gives out a better per-centage than two wheels upon a horizontal shaft. There may be a difference, but it is a small one. At all events, the data furnished by Mr. Birkenbine cannot be recorded as the difference of useful effect of identically similar wheels in the two positions named. In the Parker wheel, the difference is given at 8 per cent; and with the Geyelin wheel at 14 per cent. This does not agree by 6 per cent; whereas, had the wheels not been otherwise changed, the amount of difference in the two ought to be very nearly proportional. At the same time, it is very remarkable that this additional 14 per cent here added to the Geyelin wheel, ostensibly caused by the said change from a vertical motion of the wheel into a horizontal one, has also been arrived at with the Stevenson wheel without any such change—to wit: 14 per cent difference between the first and last test. The same applies to the Rich wheel, which also acquired 13 per cent by the second test, without any change in the direction of the shaft.

3d. The last objection to the Kalbach wheel is that "the velocity would be so great, and the reduction of the speed, by means of gearing, to the speed of the pumps would therefore involve much greater loss by friction than could, in possibility, be the result of the plan adopted for the gearing of the Jonval wheel as proposed." Please to compare the last objection with the one of the reasons given for adopting the Geyelin wheel, to wit: "and, as regards the bevel gearing or reducing the velocity for the proper speed of the pumps by two or four wheels, there is only an *apparent additional loss by friction, but none in reality, as a little reflection will demonstrate!*" Gearing down to a less speed in the Geyelin wheel is declared to be *no* loss of power; while the same thing in the Kalbach wheel *is* declared a loss of power!

Mr. Birkenbine says:—"The department will adopt the Jonval turbine, arranged and geared similar to the one now in use at Fairmount."

We would suggest that, if the fly-wheel is placed on the shaft to which the crank of the pump is attached, as now in use, then the fly-wheel, if effectual, will require a weight of 200,000 lbs., with 44 feet circumference and 16 revolutions, for a power of 125 horses. Whereas, if the fly-wheel is placed on the shaft having the same velocity of the water wheel, to wit: 40 revolutions, then 32,000 lbs. will be as effectual as the 200,000 lbs. Had the two wheels upon a horizontal shaft been adopted with 80 revolutions, a fly-wheel of 8,000 lbs. upon the same shaft, or one of the same velocity, would be as effectual as the one of 200,000 lbs., as the latter is now applied; all having a circumference of 44 feet. The rule is to place the fly near the working point, when intended to accumulate force, as in this case. But rules have exceptions; and the more imperative rule ought to be adhered to in this case, to wit: a fly should always be made to move rapidly. This gives a difference, in favor of the horizontal shaft wheels, of 80 revolutions, in comparison with the Geyelin wheel, as arranged and now approved of at the Fairmount Waterworks; also, of 192,000 lbs. of metal saved in the fly-wheel, provided both be made to produce an equally smooth and even motion; and these 192,000 lbs., at 3 cents per lb., give the neat little sum of $5,760 in favor of the two wheels on a horizontal shaft. In addition, there would be no necessity for bevel gearing, nor for a step of which the surface is measured by the square foot, to sustain tuns of water. The bearings of the horizontal shaft wheels would only have to sustain the weight of the wheel *proper*, and they would be easy of access for lubrication.

LITTLEPAGE & CREUZBAUR.

Austin City, Texas, August 2, 1860.

REFORM IN WEIGHTS AND MEASURES.

MESSRS. EDITORS:—Having recently noticed that you were calling public attention to the propriety of using an extra degree of effort to bring the subject of a decimal system of weights and measures before the next Congress, and being much interested in that subject, I trust you will permit a British subject to offer a few remarks upon this very momentous question. I certainly must endorse your views as to the propriety and necessity of the measure; but may not a more comprehensive and direct course be adopted—a course that shall arouse attention not only in the United States of America, but in every civilized part of the world, or, at least, so far as the influence of American, English and French commerce extends? It may be asked, how can such a comprehensive measure be brought about? I reply that circumstances will sometimes transpire which will render the greatest difficulty apparently easy, and I consider this to be the case in the present instance. Now is the time, and the SCIENTIFIC AMERICAN is the instrument; and, in doing so, you will be accomplishing one of the greatest and most useful reforms of the age, not even excepting the Atlantic telegraph. I think this advocacy can be made a *paying* business, of itself, as it will undoubtedly extend the circulation of your very interesting journal. Being myself an Englishman, I candidly confess that, if the facilities which you possess existed in Canada, I should avail myself of that medium; but, as it does not, I deem it my duty, as a cosmopolitan, to communicate with you or any parties who will interest themselves in bringing about such a laudable measure. The time of action is the present season—the period of the visit of the Prince of Wales to this country.

The programme I suggest is as follows:—Let one number of the SCIENTIFIC AMERICAN be got-up in the best possible style, or in such a manner as will make it worthy of the subject and object; let its principal feature throughout be *union*; let its emblems be peace, commerce and literature; let the motto of this number be "Universality of Weights, Measures, Currency and Decimal-arithmetical Education for all Nations—or, at least, in France, Great Britain and the United States;" let those three great powers unite in the object, and become the theater of enterprise for the remainder of the world (for whatever they agree upon would be followed by the other nations, not only as a matter of choice, but of necessity); let there be a suitable (short) preface to the subject; let an address follow, proving the disadvantages of the present system and the advantages of the new one; let *fac-simile* representations be made of the several necessary silver coins, from a five-cent piece to a dollar—say 3, 5, 10, 20, 50 and 100-cent pieces. A page or two (or even more, if necessary), may be devoted to silver coins. The face side to contain the suitable insignia, with its proper mottoes, which can easily be obtained from any of the "coin manuals" of the day, with any improvement which may suggest itself; the reverse side, in all cases, to bear the value universally applied, as 3 cents, 5 cents and 10 cents, &c. Preceding this, however, should be printed a table of the values of various coins, unless it should be deemed expedient to put all the tables together; in either case, I recommend the American terms to be used, as now understood. Silver coins may be distinguished from gold by the face side having the *bare* head, as in the Canada silver coins of 1858. Gold coins of monarchies to be represented (as usual) by a crown or eagle. Most of the characters and inscriptions can be copied from a coin manual, or arranged in a somewhat similar manner. I apprehend that the circulating medium of the world, or all that is actually necessary, could be thus represented. The reverse sides of the coins of every nation would agree in the denomination according to the value of each coin, but the face sides would differ according to the latest insignia represented on their coins; pretty and interesting pictures would be thus produced, independent of utility.

Other pages may contain tables of weights. This may be rendered very simple, especially if we use French terms for weights; and I prefer those for two reasons. One is, we have chosen the American terms for money; let us therefore, out of courtesy, take the French terms for weight. Now, as we have but one standard of weights, we can reduce them to the lowest possible fraction, say a pound avoirdupois shall be the unit reduced to, or rather composed of, 10,000 parts; while terms of the smallest parts are in beautiful unison with the object they represent; hence, we should say 10 atoms make 1 Partical (French *particule*); 10 Particals, 1 Grain; 10 Grains, 1 Ounce; 10 Ounces, 1 Pound or 1 Livre (French); 10 Livres, 1 Stone; 10 Stones, 1 Quintal; 10 Quintals, 1 tun of 1,000 lbs. avoirdupois.

Of measures, there are some excellent delineations in the United States, which only want to be properly matured. For lineal measure, I would simply recommend a *commercial* Foot; this should govern cords, yards and fathoms, and be determined on by a committee or congress of nations, who should settle what species of measures should be adopted; but I may suggest, as a rule, say 10 Lines make 1 Mark; 10 Marks, 1 Inch; 10 Inches, 1 Foot; 10 Feet, 1 Rod, &c.

Let the opinions of the several Boards of Trade be previously obtained: and, if favorable, insert them in the "presentation copies" of the SCIENTIFIC AMERICAN; get (it possible) the co-operation of consuls and plenipotentiaries: and then, when your arrangements are all completed, present a splendid copy to the Prince of Wales when he visits your city; having first secured a patent or copyright for publishing it in the United States, France and Great Britain; and first sending a suitable number of magnificent copies to the Emperor of the French, the Queen of England, the President of the United States, the Governor-general of Canada, and such others as you may think would further the end in view. I am inclined to believe that if the price was not too high, there would be a greater demand for this one