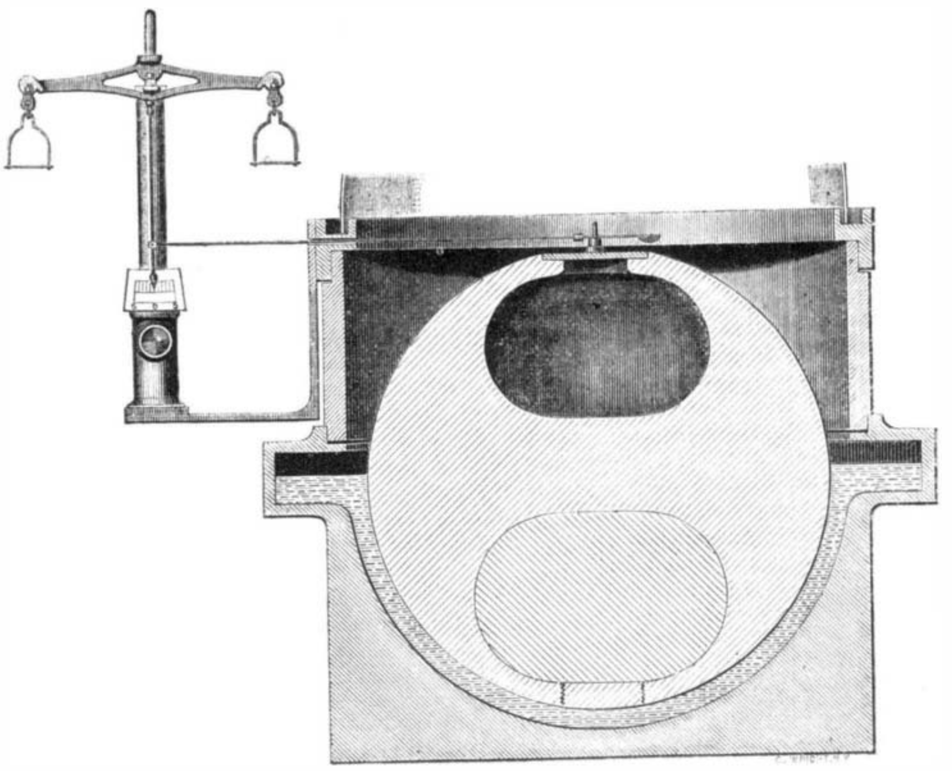
Correspondence.

SOLAR ATTRACTION AND THE EARTH'S ORBITAL CENTRIFUGAL FORCE.

To the Editor of the Scientific American:

The accompanying illustration represents an instrument constructed for the purpose of proving, by actual practical test, that the sun's attractive energy is balanced by the centrifugal force called forth by the earth's orbital motion round the sun. I desire it to be distinctly understood, however, that my intention is not to demonstrate what astronomers proved centuries ago, namely, that solar attraction is counteracted by the centrifugal force resulting from the orbital motion of the earth round the luminary. Léon Foucault, in his celebrated experiment of swinging a pendulum from the dome of the Panthéon in Paris, simply intended to furnish ocular proof of the correctness of the assumption that our planet rotates round an axis at right angles to the equator. So with regard to the instrument under consideration: the object is simply to furnish ocular proof of the correctness of the assumption that the sun's attractive energy is counteracted by the centrifugal force developed by the orbital motion of the earth round the sun. The reader is aware, from previous statements in these columns, that my scheme consists in presenting a highly polished iron globe, floating on the surface of mercury, to the sun at the moment of rising or setting, the terrestrial attraction being then exerted at right angles to the line of solar attraction, hence incapable of interfering with its action. From previous statements the reader is also aware that experiments, conducted with the new instrument at sunrise and sunset, have established the fact that, although a tractive force of a few grains is capable of moving the polished iron globe over the surface of the mercury, yet no movement whatever takes place when it is subjected to the pull exerted by the attraction of the sun as stated. A brief description will suffice to explain the nature of the instrument. The illustration represents a section through the center of the iron globe and the circular cistern which contains the mercury. Two spheroidal cavities, it will be seen, are formed in the globe, the upper cavity being empty while the lower one is filled with a metal of much greater specific gravity than iron, the object being to retain the vertical axis of the floating globe in a fixed position. A movable ring is applied at the upper part of the mercurial cistern, admitting of a free rotary motion while the cistern remains stationary. To the said ring an angular bracket is secured, supporting the central column of a delicate chemical balance. Obviously this arrangement admits of the scale beam being turned in such a direction that it points toward the rising or setting sun, without disturbing the mercurial cistern or its contents. The lower end of the vertical index of the scale beam is connected with the floating iron globe by means of a straight steel wire, as shown in the illustration; this wire extending beyond the vertical axis of the globe, a small counter weight being applied at the extreme end of the extension in order to relieve the balance from disturbing influence. To prevent dust from lodging on the mercury, a glass shade covers the cistern, resting in a groove at the upper part of the rotating ring, the shade also preventing currents of air from agitating the sensitive globe during experiments. Such is the nature of the instrument constructed for comparing the energy of solar attraction and orbital centrifugal force, which Dr. Vander Weyde says he has "disposed of" by his discovery that a "floating object is identical with a lever scale, as the liquid balances the floating body," and because (see his communication inserted May 23) he understands the instrument "only too well, so well indeed as to know that even the attraction of the rising and setting moon can never affect such an arrangement." I will not detain the reader by demonstrating the absurdity of mixing up questions concerning lunar attraction with a question relating solely to the comparative energy of the earth's orbital centrifugal force and solar attraction. I deem it necessary, however, to point out briefly the utter fallacy of Dr. Vander Weyde's stated objections. It requires but a slight acquaintance with dynamics to perceive that his first objection has absolutely no bearing on the question. Of course, the weight of the floating iron globe is balanced by the weight of the liquid metal which supports it; but how can the pull exerted by the rising sun on the iron globe be affected by the earth's attraction because the weight of the globe is balanced by the weight of the fluid mass which it displaces? The second objection urged by Dr. Vander Weyde, that my instrument is incapable of showing that solar attraction balances the earth's orbital centrifugal force because the instrument is not affected by the rising and setting moon, scarcely needs refutation. It will suffice to state that, when the floating iron globe is presented to the rising sun, the mercury which supports the globe remains perfectly level, because the centrifugal force which acts on the fluid metal exactly balances the sun's attractive energy. But, in presenting the instrument to the rising moon, the unbalanced pull exerted by its attraction on the mercury will produce an inclination of the surface of the latter in a direction opposite to the satellite. Obviously, that inclination will bring the floating globe under the influence of terrestrial attraction to an extent exactly balancing the lunar attraction. Having called the reader's attention to Dr. Vander Weyde's objections, it would be inconsistent not to notice the communication from Mr. Hugo Bilgram, published in the SCIENTIFIC AMERICAN of May 23, concerning my demonstration on page 291, current volume. Mr. Bilgram says: "Though Captain Ericsson in his communication of March 14 proved to be master of the subject, he evidently overlooked one point." This "overlooked" point your correspondent thus adverts to: "Though solar attraction does balance the orbital centrifugal force while the sun is rising, it will not do so three hours afterwards." Now, the sole object of my demonstration was to prove that such *is* the fact, my figures showing that, although solar attraction exactly balances orbital centrifugal force at sunrise, the energy of solar attraction gradually overcomes the orbital centrifugal force during the diurnal revolution, until at noon the difference amounts to 0·0001312. My demonstration also proved that a weight of 20,000 pounds suffers a diminution of 0·001546 of a pound during six hours of diurnal rotation, owing to the very cause which Mr. Bilgram asserts that I have overlooked!



Referring to the experiments which have been instituted with my solar attraction instrument, it will be well to observe that, although the energy of lunar attraction is practically imperceptible, it has been deemed best to conduct the observations when the moon is in the first quarter, its attraction being then exerted at right angles to the line of solar pull. Let us now consider whether the observations have been conducted on a sufficiently large scale to warrant definite conclusions. The weight of the iron globe employed being 181·47 pounds, calculations based on the relative mass of the sun and the earth and other known data show that the pull of the sun amounts to 748 grains. The startling fact that the floating iron globe, while subjected to such a considerable direct horizontal pull, remains stationary, at once suggests the following question: Is the surface of the mercury in the cistern perfectly level in a line pointing east and west,—does not solar attraction raise the surface of the fluid metal at the eastern edge of the cistern, thereby producing an inclined plane which solar energy is incapable of causing the iron globe to mount? This important question the writer has disposed of by the following device: Two open cisterns containing mercury, connected by a horizontal tube, are placed twenty feet apart on a level stone foundation. Above the center of each cistern a micrometric mechanism is applied, by means of which the hight of the mercury may be measured with the utmost precision. The two cisterns with their connecting tube being placed east and west, and time allowed for the mercury to come to a state of perfect equilibrium, the micrometers are adjusted.

This adjustment, it should be particularly observed, is made when the sun is in the zenith, at which time its attraction evidently cannot disturb the equilibrium of the fluid metal in the connected cisterns. The contact of the micrometers and the mercury is then examined from time to time during the diurnal revolution, the final observation being made when, near sunset, the two cisterns point towards the luminary, at which moment the attractive force, tending to disturb the equilibrium of the fluid metal, is at its maximum. Regarding the result of the observations conducted P. M., it may be briefly stated that, when the micrometers are properly adjusted, not the least excess of elevation of the level of the mercury in the western cistern is produced by solar attraction, at the moment when the attractive energy is exerted in the direct line of the two cisterns. Persons familiar with cosmical questions will say that, in case the sun and moon should be nearly in conjunction when the observation is made, lunar attraction will sensibly affect the equilibrium of the mercury in the cisterns. The relative energy of terrestrial and lunar attraction at the earth's surface being in the mean ratio of 320,602 to 1, a difference of level in the cisterns amounting to 0·000748 of an inch takes place under the stated conditions. Consequently this difference calls for a correction, after the adjustment at noon, readily effected by turning one of the micrometric screws through an arc of 8° 40′, the pitch being thirty-two threads per inch. The perfectly level state of the mercury in the cistern of the solar attraction instrument having been established by such accurate means, the absence of any motion of the floating globe when subjected to the pull of the rising and setting sun furnishes positive ocular demonstration of the fact that the sun's attractive energy exerted on the mass of the iron globe is exactly balanced by the centrifugal force resulting from its orbital motion round the luminary. No reflecting observer, aware of the actual amount of the solar pull (748 grains), can witness the perfect repose of the floating iron globe on the level surface of the mercury, at the moment when the sun is rising, without being impressed with the importance of what he beholds. Again, if he has previously calculated the curvature of the orbit in which the instrument is moving, he can assert that the velocity of the floating iron globe round the sun must exceed 18 miles per second, in order to develop, by centrifugal force, an energy capable of counteracting the pull which he *knows* the globe is subjected to while he is watching its repose on the surface of the fluid metal.

J. ERICSSON.

The Planet Mars.

To the Editor of the Scientific American:

A few particulars relating to the future movements of Mars may be of interest to your readers:

At the present time this planet is badly situated for observation, being nearly at its greatest distance from the earth and but a few degrees east of the sun. The next opposition of Mars will not occur until the 20th of June, 1875. The planet will then be seen near the well known Milk Dipper of *Sagittarius*. This opposition will not be a very favorable one, however. The low altitude which the planet will attain in our northern latitudes will render it difficult to obtain good views. Moreover, on account of the ellipticity of the orbits of Mars and the earth (especially that of Mars), the planet is much farther from the earth at some oppositions than at others; and on this occasion, it will not be as well situated in this respect as is sometimes the case.

At the next following opposition, however, which will take place in the first part of September, 1877, Mars will be very favorably situated for observation. The planet will, on this occasion, arrive nearly at its minimum distance from our globe; and as it will be situated but a few degrees south of the equinoctial, it will, when on the meridian, be at a convenient altitude for observation in these latitudes.

It happens, in 1877, that Saturn will arrive in opposition to the sun nearly at the same time as Mars. Both planets will be seen, near the time of their opposition, close together, in the constellation *Aquarius*, near the line which separates that constellation from *Pisces*.

At the next opposition, in November, 1879, Mars will not be well situated, but a favorable opposition will occur again in 1892.

At present, the perihelion point of the orbit of Mars is in heliocentric longitude 333° 45′, and the aphelion is in heliocentric longitude 153° 45′. Mars is therefore most favorably situated when its opposition occurs in the latter part of August, while the most unfavorable oppositions take place in the latter part of February. In the former case the apparent diameter of the planet reaches 23·5″, and in the latter case it is only about 13″.

St. Catherine's, Ontario. J. M. BARR.

Laying Out Railroad Curves and Gear Wheels.

To the Editor of the Scientific American:

In your issue of April 11, 1874, I notice an article from the pen of H. C. Parsons, concerning the laying out of railroad curves. Having felt the need of some simple mode for this operation, I discovered the following method, which I find sufficiently correct and easy of application. I append a sketch, the rule for its application, and tables of coefficients with which to ascertain the chords. These tables are calculated especially for laying out gear wheels, by using the angular or chordial pitch instead of the arc; therefore it must always be borne in mind that the pitch mentioned is the chord of the arc.

RULE.—Divide the circle into a convenient number of equal parts of degrees and minutes, then use one half of the same for the changes on the instrument, in establishing points. Then apply rule 2 of my table of coefficients for gears, which will give the chord of the arc of each division of the circle.

Example: What will be the angle for the instrument and the length of the chords for a circle of 600 feet radius, divided into 36 parts of 10° each? Answer: The angle will be 5°, and the chord of the arc will be 104·58 feet.

By this method at least one third of the circle can be laid without moving the instrument, or the latter can be shifted to any point of the circle, whenever any obstructions or irregularities of the land make it requisite to do so. By dividing the circle into many parts, the chords can be brought down to any desirable length.

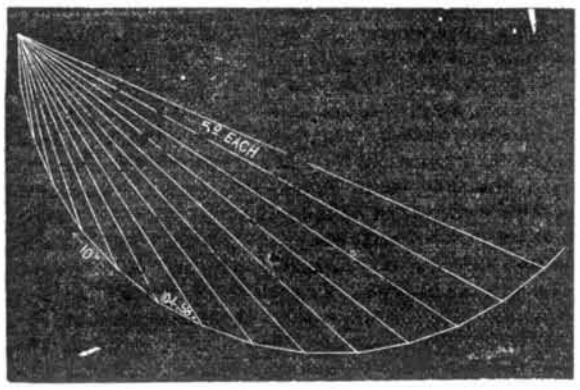


TABLE OF COEFFICIENTS.

Nc. of Teeth.	Coefficient.	No. of Teeth.	Coefficient.	No. of Teeth.	Coefficient.	No. of Teeth.	Coefficient.
		26	8·29616	51	16 24500	76	24·20000
		27	8 61370	52	18 56313	77	24·51824
		28	8·92131	53	16 88127	78	24 83650
4	1·41421	29	9 24898	54	17·19942	79	25·15476
5	1·70111	30	9·55671	55	17·51757	80	25·47302
6	2·00000	31	9 88450	56	17 83573	81	25·79128
7	2·30480	32	10 20233	57	18·15390	82	26·10955
8	2·61316	33	10·52021	58	18·47207	83	26·42782
9	2·92380	34	10·83811	59	18·79024	84	26·74610
10	3·23607	35	11·15604	60	19·10842	85	27·06437
11	3 54980	36	11·47400	61	19 42661	86	27·38266
12	3·86403	37	11·79198	62	19·74481	87	27·70094
13	4 17876	38	12·10998	63	20·06300	88	28·01922
14	4·49399	39	12·42800	64	20 38121	89	28 33751
15	4·80942	40	12·74600	65	20·69943	90	28 65580
16	5·12609	41	13 06406	66	21 01764	91	28·97409
17	5·44247	42	13·38211	67	21·33585	92	29·29238
18	5·75906	43	13 70017	68	21·65407	93	29·61067
19	6 07581	44	14,01824	69	21·27230	94	29·92896
20	6·39270	45	14 33631	70	22.29053	95	30 24725
21	6 70971	46	14·65441	71	22·60876	96	30·56555
22	7·02681	47	14 97251	72	22·92700	97	30·88385
23	7 34400	48	15 29062	73	23·24524	98	31 20214
24	7·66129	49	15·60874	74	23·56350	99	31·52044
25	7·97866	50	15·92686	75	23·88174	100	31·83874

Rule 1: To find the diameter of a wheel when the pitch and number of the teeth are known: Multiply the coefficients in the table, corresponding to the number of teeth, by the given pitch, in inches and hundredths; the product will be in inches and hundredths.

Rule 2: To find the pitch of a wheel, when the diameter and number of teeth are known: Divide the given diameter by the coefficient in the table corresponding to the number of teeth, and the quotient will be the pitch.

Rule 3: To find the number of teeth in a wheel where the pitch and diameter are known: Divide the given diameter by the given pitch, and the number in the table corresponding to the quotient will be number of teeth.

These tables were computed by two distinct processes, at seven places of decimals, and are warranted not to vary more than $\frac{1}{100}$ of an inch in the diameter of a wheel of 200 teeth and 3 inch pitch.

New Bedford, Mass. H. C. CRANDALL.

Professor Mayer's Discoveries in Acoustics.—A Note from the Author.

To the Editor of the Scientific American:

Will you permit me to correct two erroneous statements in the accounts you published of my discoveries in acoustics, recently read before the National Academy of Sciences?

Under the heading "The Duration of the Sensation of Sound," for "he concludes that the whole ear vibrates as one mass," etc., read as follows: The following table gives the notes, the number of their vibrations, and the duration of their residual sensations, (the French notation, used by König, is adopted):

Note	No. of vibrations per second.	Duration of residual sensation of the sound.
C_1	64	$\frac{1}{16}$ sec.
C_2	128	$\frac{1}{26}$ "
C_3	256	$\frac{1}{47}$ "
G_3	384	$\frac{1}{60}$ "
C_4	512	$\frac{1}{76}$ "
E_4	640	$\frac{1}{95}$ "
G_4	768	$\frac{1}{109}$ "
C_5	1024	$\frac{1}{135}$ "

Calling D, the duration of the residual sensation, and N, the number of vibrations per second of the note, we have:

$$D = \left(\frac{53248}{N+23}\right) \cdot 0001$$

Now carrying this law (which we discovered by means of vigorous experimental measures) downwards and upwards, through the range of audible sounds, we have, for 40 vibrations per second, the residual sensation lasting $\frac{1}{11}$ of a second after the vibrations which caused the sound have ceased; while for 40,000 vibrations per second, we have a residual sensation of only $\frac{1}{500}$ of a second. If we apply the law to vibrations below 40, where they produce, not a continuous sound, but explosive sensations in the ear, we reach a remarkable result, thus: 30 vibrations per second give a residual sensation of $\frac{1}{10}$ of a second; but if the residual sensation is $\frac{1}{10}$ of a second, why is it that 30 impacts on the ear, in one second, do not blend? For they follow one another at each $\frac{1}{30}$ of a second. This is explained by the fact that co-vibrating parts of the ear, corresponding to sounds produced by vibrations fewer than 40 per second, do not exist, and therefore there are no bodies to co-vibrate and keep up their oscillations after the cause which set them in motion, has ceased to exist, it follows that in other cases the ear is vibrated only as one mass, and the duration of these oscillations of the whole ear are far too short to remain the $\frac{1}{30}$ of a second. This supposition also explains why the higher notes, far beyond those used for musical sounds, produce continuous sensations, though we have every reason to believe that no co-vibrating parts of the ear correspond to them; with these high notes, the ear vibrates as a mass, but the duration of this vibration is sufficient to keep up sonorous vibrations, following one other at each $\frac{1}{5000}$ of a second; but for notes thus perceived without the intervention of corresponding co-vibrating parts in the inner ear, the pitch should be difficult to distinguish, and this we find is actually the case.

The heading "The Reflection of Sound from Flames and Heated Glasses" should read "The Reflection of Sound from Flames and Heated and Cold Gases." Under this head, for "among other curious results, Professor Mayer has ascertained that there is an absorption of sound in the bat's wing flame; that the flame is heated by the sonorous vibrations which enter it as such, and issue as heat vibrations," please substitute the following:

"The contemplation of these experiments naturally calls up the question:

Is the action of the flame due entirely to reflection? May it not also absorb part of the sonorous vibration, as in the analogous phenomena of the reflection of light?

If the intensity of the sonorous vibrations which have traversed the flame equal the intensity of the vibration which impinged on the flame, *minus* the intensity of those which were reflected from the flame, then there is no absorption of these vibrations by the flame; but if this equality does not exist, then there is absorption in the flame; and this means that the flame is heated by the sonorous vibrations which enter the flame as heat vibrations. It thus at first appears that the absorption of the sonorous vibrations might be detected by their production of an increase in the temperature of the flame, just as sonorous vibrations are absorbed by caoutchouc, and reappear in this substance.

In the following manner I have recently made experiments in the direction of determining the equivalent of a given sonorous aerial vibration, in fraction of a Joule's unit of 772 foot pounds. I stretched between the prongs of an Ut^3 tuning fork a piece of sheet caoutchouc, 100th of an inch in thickness, and about ½ inch broad. The effect of this rubber on the vibrating fork is rapidly to extinguish its vibrations, with which the rubber itself is heated; and if a fork be vibrated continuously, by one and the same force, when the rubber is stretched on it and then when it is taken off, the aerial vibrations produced by the fork are far more intense in the latter circumstances than in the former. By a method described by me in the *American Journal of Science*, February, 1871, I now measured the relative intensities of the aerial vibrations, in these two conditions of vibration. The sheet of caoutchouc was now enclosed in a compound thermobattery, and the fork vibrated during a known interval; the rubber was heated by the vibrations, which would have appeared as sonorous vibrations, if the rubber had been removed from the fork. The amount of heat given to the caoutchouc was accurately determined, by the deflection of a Thomson reflecting galvanometer, connected with the thermo battery; and by knowing the interval during which the fork vibrated, and the amount of heat given by the caoutchouc during this interval, and the equivalent of the heated rubber in water, I calculated the intensity of the sonorous vibration in terms of a thermal unit, from which I at once obtained the value of the sonorous aerial vibrations, when the fork was not heating the rubber, in other words, when it vibrates freely. I thus found that the sonorous aerial vibrations, during ten seconds, of an Ut^3 fork placed in front of its resonator, equaled about the 100,000th part of a Joule's unit; that is, they can be expressed in the work done in lifting 54 grains one foot high. This quantity of heat, which is equal to the heating of 1 pound of water one 100,000th of a degree Fah., expressed the amount by which the gas flame would be heated, if it absorbed all of the sonorous vibrations issuing from the Ut^3 resonator. But this is such a small fraction of the entire heat in the flame that it is far within the actual fluctuations in temperature in the flame; and, even if the flame were constant in temperature, this small increase could not be detected by any known thermometric method. We cannot therefore determine the amount of absorptive power of a flame, or sheet of heated air, for sonorous vibrations, by experiments on their increased temperature, when sonorous vibrations impinge on these bodies."

ALFRED M. MAYER.

Stevens Institute of Technology, Hoboken, N. J.

Turbine Water Wheels.

To the Editor of the Scientific American:

In our experience, if we have a flood of water with reasonable head, almost any kind of wheel, if it be large enough, will do; but when we come to substitute a turbine for an overshot wheel, on light streams, we find that it is a nice matter to decide on the size the wheel should be to give sufficient power and to use the water economically. We venture to say here that there have been more failures in turbines on light streams on account of using too large wheels than from all other causes combined; and we set it down as a well established fact, without having reference to any water wheel pamphlets, that there are now in use and have been for some years several different makes of turbine wheels that will give from seventy-five to eighty per cent. when working with seven eighths to full gate; and persons interested can inform themselves more satisfactorily by corresponding with parties having wheels in use than by consulting pamphlets on the subject.

It is said that the best wheels afford almost all their power at five eighths gate or under. Now this is entirely at variance with our experience. Putting in a turbine wheel, on a light stream, that would be large enough to drive the machinery at half gate would be a failure simply because of the small percentage yielded, and consequently the use of too much water for the amount of power given.

Substituting large wheels operating at from one quarter to one half gate, for small wheels requiring seven eighths gates, results in the use of much less water for a given effect, and is also at variance with our experience and can only be based on the idea that the wheels give a better percentage at one quarter than at three quarter gate, which is not the case with any wheels we are acquainted with; but there is ample room for improvement in turbine wheels in that direction.

There is one advantage in using large wheels, and it is that when there is a flush of water it can be utilized, which is the only offset to the loss of power in running at ordinary stages of the water. J. BROOMELL.

Christiana, Pa.

New Steamboat Law. Authorized Increase of Steam Pressure on the Mississippi.

"AN ACT relating to the limitation of steam pressure of vessels used exclusively for towing and carrying freight on the Mississippi river and its tributaries:

Be it enacted by the Senate and House of Representatives of the United States of America in Congress Assembled: That the provisions of an act entitled " An act to provide for the better security of life on vessels propelled in whole or in part by steam," etc., approved February twenty-eighth, eighteen hundred and seventy-one, so far as they relate to the limitation of steam pressure of steamboats used exclusively for towing and carrying freight on the Mississippi river and its tributaries, are hereby so far modified as to substitute for such boats one hundred and fifty pounds of steam pressure in place of one hundred and ten pounds, as provided in said act for the standard pressure upon standard boilers of forty-two inches diameter, and of plates of one quarter of an inch in thickness; and such boats may, on the written permit of the supervising inspector of the district in which such boats shall carry on their business, be permitted to carry steam above the standard pressure of one hundred and ten pounds, but not exceeding the standard pressure of one hundred and fifty pounds to the square inch.

Approved January 6, 1874."

To the Editor of the Scientific American:

A recent act of Congress, regulating the management of steam vessels, authorizes tow and freight boats on the Mississippi river to carry a steam pressure of 150 lbs. to the square inch, instead of 110 lbs. as heretofore, in standard boilers of 42 inches diameter and one quarter of an inch thick. I presume that, by standard boilers, is meant such as are ordinarily well made of good average material and single riveted. However this may be, the pressure stated is clearly in excess, and very dangerously so, of that allowed by the rules generally adopted by first class engineers. A boiler 42 inches in diameter and one quarter of an inch thick, with 150 lbs. to the square inch, is subjected to a strain of 12,600 lbs. to each square inch of sectional area of the solid plate, or fully one quarter of the ultimate tensile strength of good boiler iron. According to Fairbairn, in single riveted work the strength is reduced to 0·52 and in double riveted work to 0·7, of that of the solid plate. Under the above circumstances, therefore, a good new boiler, if single riveted, would be subjected to a working pressure equal to nearly one half of that at which it might be expected to tear asunder, or, if double riveted, to more than one third of the breaking strain. The rule given by Bourne for the thickness of locomotive boilers is to multiply the diameter in inches by the pressure per square inch and divide by 8,900, which, in this instance, would require the shell to be about seven tenths of an inch thick. For marine boilers he allows 3,000 lbs. per square inch of sectional area of plates. Now it does not appear that there is any legitimate reason why the owners of boats, used simply for freight or towing, should be allowed to subject their employees to imminent danger from explosion, while persons merely passengers are protected by law from such danger. That the pressure stated is really known to be dangerous needs no further proof than the fact that it is confined to that class of boats; and since it must be admitted that all citizens are entitled to equal protection, why not the officers and men serving in these vessels? It may be said that they voluntarily expose themselves with full understanding of the circumstance, but this is not always the case, and, if it were, would not be a good argument. An explosion of one of the above mentioned boats occurred in March last, causing the loss of sixteen lives. Dare we say that those lives were less precious because they belonged to engineers, firemen, deck hands, or others forced by the necessity of providing for themselves and families to work under constant dread of danger and death? JOHN LEPPER.

Washington, D. C.

OF all solid substances found upon the earth, carbon is both the hardest and the softest. In the form of diamond, it is the hardest. In the form of graphite, it is the softest. Both diamond and graphite are the same in chemical composition.