Correspondence.

The Editors are not responsible for the Opinions expressed by their Correspondents.

The Interference of Vibrating Pendulums.

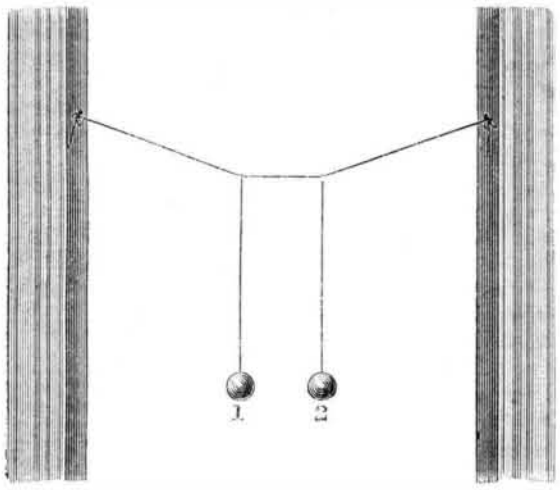
[MESSRS. EDITORS :—In compliance with your request I send a sketch of the experiment with the interfering balls. I am very truly yours. E. N. HORSFORD.]

We often hear the word "interference" used to explain certain phenomena of light, heat, and sound. To the popular mind it is not easy to present an intelligible illustration of the process to which reference is made in the use of this word. We can understand that if a violin string be twanged at one end of a lead tube of, say, a thousand feet in length, the note peculiar to the tension or taughtness of the string will be heard by the ear applied at the other end of the tube. So would it be heard at the end of another tube a few yards longer than the first. Now it will not be so readily understood, why, if the two tubes be placed side by side to receive the vibrations of the violin string, and the longer tube be made to curve and enter the shorter, at a very sharp angle, just before its distant terminus, that the ear now placed at the end of the shorter tube will hear nothing. Let us see if we cannot present a mechanical conception of what takes place.

This is a case of interference. The sound is due to what may be called currents of alternate compression and dilation in the air, falling on the drum of the ear, throwing the membrane into vibration, the compressed air forcing the membrane inward and the dilated air permitting it to return. These alternate layers of compressed and dilated air are caused by the sudden backward and forward movement of the violin string. They produce the same effect on the ear when propagated through either tube. But when the two tubes—one so much longer than the other that the air at the distant terminus of the long tube is dilated, while that at the exit of the shorter is compressed—are joined, the stratum of compression in one will mingle with that of dilation in the other, and the air at the end of the shorter tube will have its normal condition of uniform density, and no oscillation of the drum of the ear be produced, and of course, no sound heard.

Thisillustration will at least give an idea of one kind of interference, and will open the way to the presentation of an interesting experiment illustrating interference of another kind, which was brought to the attention of the public at the meeting of the American Association for the Advancement of Science, at Burlington in 1867, by Mr. Henry Waterman, of Hudson, New York.

The experiment may be made by any one. Take two apples, or balls, or spools of about the same size. Attach to each a slender cord or strong thread. Suspend a string across a doorway, from tacks at equal hight on the opposite sides of the doorway. Let the string be a quarter longer than the width of the door so as to hang slack. At a distance of about three inches from the middle point of the slack string, suspend one ball, giving its string about two feet of length. At the same distance on the other side of the center, suspend the other ball, giving its string the same length as the first. The apparatus is now complete and this diagram will illustrate it.



Now taking hold of the ball 2, draw it from the doorway about a foot and let it swing. It will cause the other ball to commence swinging. After a few oscillations ball 2 will gradually come to rest, when ball 1 will have attained a maximum sweep nearly or quite equal to the original sweep of ball 2. Ball 2 will however immediately resume its oscillations and the other will gradually come to rest; in its turn, however, starting, rising to a maximum of sweep and subsiding to momentary repose. Thus the two balls will continue to interfere with each other for a long time.

If the string attached to one ball be shortened the phenomena will be modified: neither ball will come to absolute rest, but both will have alternately maxima and minima of sweep.

If both balls be drawn from the perpendicular on the same side, but one farther than the other, and both be released at the same instant, the effect will be the same as if one string had been shortened.

If, instead of starting the vibration through the doorway, it be instituted across it, that is, in the direction from one jam to the other, the same phenomena of alternate momentary rest and renewed oscillation will be observed.

If the vibration be commenced obliquely across the doorway the resulting phenomena will be wonderfully interesting, but difficult to detail, involving two sets of maxima and minima of effect, and a very complex system of alternate motions and rest.

Additional balls will modify the results, and the length and approximate taughtness of the string, as well as the distance of the support of the balls from the points of suspension of the slack string, will, in a great variety of ways, influence the phenomena. As a source of entertainment and a theme for investigation it will not be likely soon to exhaust itself.

Two or three points borne in mind will perhaps be of service in pursuing the subject as a problem of mechanics.

1st, When the two balls swing strictly together through the doorway the whole will be a pendulum, the length of which is the perpendicular distance of the balls, from a straight line joining the ends of the slack string.

2d, When one ball is at rest and the other swinging, the length of the pendulum is less, than when both are swinging evenly together.

3d, The time required for the sweep of a pendulum is greater as its length is greater.

Let me now give an application of the truth of this kind of interference.

Most persons are familiar with the fact that clocks are sometimes brought to rest, or their rate modified by the jarring of neighboring machinery. It is related that two clocks have been so placed on a common shelf that they have not only modified each other's rate, but have alternately brought each other to rest, and caused after each pause, the motion to be resumed. The action of two pendulums on each other, where the motion of the point of support of one may influence the motion of the point of support of the other, is shown in the experiment above detailed.

Persons fond of material conceptions of psychological phenomena, will find much of suggestive interest in experiments that show how vibrations may awaken or strengthen, or weaken or arrest their fellows; and I am persuaded that the simple apparatus I have described will afford lasting entertainment and food for thought to all, old as well as young, who will be at the trouble to put it in operation.

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Peat.

[Concluded from page 230.]

ON HARVESTING AND MANIPULATING PEAT FOR HEATING USES.

Now sketch, as near as you can on paper, the shape of your bed, and measure it by pacing across it; or, if you cannot do that, take, instead, a parallel line on the adjoining land, and another at right angles with it, ascertaining approximate lengths each way, and obtain the area as near as convenient of the deposit in square yards, of which, there being 4,840 to an acre, it will be near enough to compute the measurement of each side of the acre at 69½ yards. Now, with a long, strong, slender rod marked, in feet, sound the bed at intervals, noting the measurements in their proper places on your sketch: Your soundings may vary considerably, this record, therefore, should be kept for subsequent reference. To avoid penetrating the underlying strata, cut a "nick" in the end of your rod, and the clay-like substance of the bottom will be detected therein, and you can proceed accordingly.

Having now the superficial area of your bed, find the average depth by taking the mean of the sum of your soundings, this multiplied into the area will give the cubical contents in peat. It is difficult to calculate accurately the quantity of useful material in a bed of peat. I find the published estimates differ considerably, and my own investigations greatly vary. These differences arise from the different proportions of water and foreign matter mixed with the peats, and also from their own different densities.

For most practical purposes, in estimating quantity by the cubic yard, peat, as ordinarily in the bed, will weigh from 2,100 to 2,400 pounds, and if drained in the bed, 1,340 to 1,490 pounds, and air-dried 320 to 380 pounds, when it will be found to be reduced to about one-fourth to one-sixth the original bulk.

Peat, saturated with salt water, is generally unfit for heating purposes.

The fine clay-like substance found underlying peat beds is sometimes marly, particularly where the saturating water holds lime in solution, as at the vast beds under the Cayuga marshes in the State of New York. But generally it is of a very different nature, an impalpably fine powder, varying in color from white to dingy slate, and from yellowish white to brown, and composed of infusorial shields of animalculæ—little shells which, under a microscope, are resolved into the most exquisite shapes and forms, yet not composed of carbonate of lime but of pure silex. It forms a superior powder for polishing metals.

In working a bed of peat, the first step will be to ascertain if drainage is necessary; and, secondly, how it can be effected and at the least cost. Generally the material removed in this process will be available for future treatment. If the bed cannot be economically drained, resort must be had to mechanical excavation, which I should only use from compulsion, as I doubt, all points considered, any practical advantages of mechanical over hand labor for that purpose. If, however, it is necessary, a most excellent and economical apparatus has been made and successfully used therefor. One man raising fifty lbs. of peat a minute, will lift and place fifteen tuns in ten hours, whereas some men will do more. It is best not to drain a bed below the level to which you can effectually work out in a season, unless you can close the outlet drain to allow it to fill again with water for the winter, for the reason that drained peat that has been frozen is apt to disintegrate after thawing, and become impoverished for a solid homogeneous fuel.

For economic heating purposes and rendering the peat compact, the substance must be kneaded to break up the sponginess reducing the mass to a smooth, paste-like, homogeneous consistency, which will dry hard and solid, without pressure. To effect this result, cheaply and rapidly, has been the great problem, a solving of which has occasioned many costly failures and annoying disappointment.

Many expensive essays have been made also to dry peat artificially, but always resulting, as trial for this end always must do, in final abandonment.

Many contrivances have also been invented for pressing the water from peat, and also to mold it under pressure, but never in either case with economic success; and I believe all systems for these purposes must eventuate in failure to produce a good fuel at remunerative cost; indeed, I know almost every conceivable plan has been exhausted for these objects and all failed alike; beside, all peat, from which water has been expelled by pressure, requires subsequent drying.

All that is needful to be done with excavated peat is to manipulate the mass properly, and expose it directly to the air until it is dried or fit for removal; and as its cost for fuel is almost entirely due to manual labor, the complete process should be accomplished by the least possible number of handlings, in the shortest possible time, occupying the least possible area of ground therefor, and in a given time producing the largest possible quantity in tuns of prepared fuel.

Various machines have been made embracing several distinct mechanical systems, and modifications of each. The most satisfactory being on the general principle of the "pug mill," for working brick clay, where a vertical shaft with working arms and kneaders is used. And those various patented contrivances have failed from their liability to derangement, or from breakage of some of the parts, or from insufficiency of product in a given time, or from inferiority of fuel made, or what is perhaps the shortest road to the trouble, the ultimate expense in producing 100 tuns of satisfactory fuel.

Aside from the necessity to turn off day by day a required amount of good work, the machine must possess within itself protection from injury against stones, bones, and even roots, which must pass unseen into the machine with the peat, and be separated and forced away from the working parts, while the general flow of the useful matter continues on unobstructed in its proper course, and such a machine has been made, tried, and proven to embrace all those essential elements of success.

Peat that has been well manipulated and dried for fuel rarely holds more than ten per cent of moisture, and it will not afterwards become saturated with water, even by immersion for an entire winter.

A cubic yard of closely packed peat fuel will weigh from 1,620 to 2,180 pounds, and the heating value of one pound of such peat is equal to even one and a-half pounds of wood. One cord of good wood will weigh almost ,4200 pounds, and one cord of peat fuel will weigh about 3,750 pounds, showing a gain in space as well as greater heating power.

J. B. HYDE.

New York city.

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Do We Measure Horse-power Correctly?

MESSRS. EDITORS:—On page 197, current volume of the SCIENTIFIC AMERICAN, I noticed an article signed "Mathematician," in which the author says: "When we wish to find the actual horse-power of a steam engine, and compute the same by multiplying area of cylinder by stroke of piston, pounds of steam, and number of strokes per minute, without other qualification, the result is erroneous. As for instance, apply the foregoing rule to a steam engine furnishing power for a machine shop, and running at the rate of seventy-five revolutions per minute, and let the result in horse-power be thirty; then disconnect, throw the belting off the power wheel, use the some amount and pressure of steam, and the number of revolutions will be doubled on account of outside resistance being removed. Now measure the horse-power by same rule, and the result will be sixty-horse power, which is evidently absurd; for it is equal to saying that the engine uses most horse-power when doing least work, and least horse-power when doing most work."

The above method of computing power, or "horse-power," as the author styles it, is not correct or not correctly stated. He says, "multiplying *area of cylinder* by stroke of piston, *pounds of steam*, and number of strokes per minute, without other qualification, the result is erroneous."

I believe it would necessarily be so, since the above data, are not required for computing power. All that is necessary is the mean total pressure of steam on piston and its velocity.

EXAMPLE.—Suppose the total mean pressure of steam on the piston to be 1,000 lbs., and the velocity of the same 300 feet per minute, then by the above rule we have 300 feet × 1000 lbs. = 300,000 foot-pounds, indicated power of engine. For useful effect, exclusive of friction, see table of friction and make the necessary deductions, or apply a dynamometer.

To prove the accuracy of the preceding rule, let us take the experiment the gentleman proposes on an engine of thirty-horse power, driving a machine shop, and throw off the main belt. Of course he would have to disconnect the regulator, for if not it would close the valve. He says, use the same amount and pressure of steam, and the number of revolutions will be doubled. To use the same amount and pressure of steam would be impossible, for since the velocity is double, the cylinder will be filled twice as often and consequently if you use the same pressure the quantity would be double.

This would be an interesting experiment which I would like to see tried, provided I were out of the reach of the flying fragments. Suppose the engine consumed two-horse power when working seventy-five revolutions per minute to overcome the friction, and suppose the ports of the cylinder were of sufficient dimensions to allow a free passage to the steam; then thirty-horse power would drive the engine fifteen times as fast. For we have two-horse power × 15 = 30-horse power consumed in friction giving a velocity of 15 × 75 = 1125 revolutions per minute.

Consequently the engine would do what the gentleman considers absurd, viz.—Use most horse-power when doing least work, and least horse-power when doing most work.

The method he gives us for computing the power of an engine exclusive of friction is equally erroneous, as he supposes the friction to be the same, whether the engine is doing work or not, which is evidently wrong.

Again he says, "It must be admitted that a better test of the superior economy of one man's make of engine over another could scarcely be had than that of the amount of steam consumed in running any engine alone." I believe that the gentleman is also mistaken on this point. The fact is that a toy engine, like the one of which you give an illustration on the same page on which the gentleman's article appeared, would run with less steam alone than the most perfect engine yet made, on account of the simplicity of parts. I think it capable of demonstration that the poorest engine would run alone with the least steam, and also that a very bad engine may show a good card by indicator. B.

Newark, N. J.

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Large Centrifugal Pumps.

MESSRS. EDITORS:—In a recent number of your paper you published an extract, from the *Colliery Guardian*, about two large centrifugal pumps which had lately been made in England, and which were said to be the largest in the world. The writer of the article in question cannot have been very well "posted" as to the dimensions of some of the large pumps at present in operation—as I know of two (and there are probably others), each of which exceeds in size those described in the article referred to. These pumps are at present at work on the sugar estate of Messrs. Ewing, of Glasgow, in Demarara, and were made from the designs of Prof. James Thomson, C. E., Belfast, Ireland. The larger of the two was constructed under my supervision by Messrs. Harland & Wolff, Belfast, and as some of your readers may desire to know some of the particulars I give you the principal dimensions. Diameter over all, 15 feet 6 inches; diameter of wheel, 7 feet 9 inches; breadth of wheel at periphery, 2 feet 7½ inches; diameter of shaft, 7½ inches; diameter of suction pipes (2), 4 feet 9 inches.

St. Louis, Mo. JAS. SIMPSON.

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THE LAW OF STEAM.

BY PROF. JULIEN M. DEBY.

Regnault, the celebrated chemist and natural philosopher, in the published results of his admirable researches on steam, undertaken at the requisition of the French Government, while speaking of the intimate relation existing between the pressure and the temperature of steam, says: "The question we are at present studying is probably one of the least complex of the theory of heat, and if the law which governs it has not been made manifest by our experiments, this depends probably on the empirical definition given of *temperature*, which definition, in all likelihood, does not establish any simple relation between various temperatures and absolute quantities of heat."

He further says: "We are at present totally unacquainted with the theoretical law which connects the elastic forces of vapors with their temperatures."

Dalton, long before Regnault, propounded a law, stating that, while the pressures increased in geometrical ratio, the temperatures did so in an arithmetical one; and Faraday, to a certain extent, corroborated Dalton's theory during his investigations on the expansion of gases. More recent observations have, however, proved the fallacy of this supposed law, especially when applied to long ranges of pressures or to great differences in temperature.

Neither the researches of Arago and Dulong, nor those of the Franklin Institute, nor of other modern physicists have, to our knowledge, been able to solve the mystery, and we have, to this day, been reduced either to direct experiment or to the use of empirical formulæ in order to determine the temperature of any given pressure of steam, or, *vice versa*, to determine the pressure from the temperature.

The formulæ for this purpose are quite numerous; but as I have said before, they are, without exception, purely empirical; and their results must be considered only as rough approximations to practical results. Many of these formulæ are complex, involving quantities to be raised to the fifth or sixth power or require the extraction of the fifth or sixth root, and combine the use of various constants and coefficients with multitudinous rows of decimals attached to them.

How much more simple the matter really is, I shall now proceed to show, leaving those who take interest in the subject to judge for themselves, whether or not Dame Nature has long mystified the mathematicians in this special case.

While reflecting on the theory which regards heat as a mode of motion, it occurred to me to think of the cause of the well-ascertained fact, that the latent heat of steam decreases as the tension increases, and this naturally led me to the conclusion, that, in all probability, as the pressure of steam increases so is a portion of the latent heat really converted into this pressure itself, or, more properly speaking, the tension is in reality itself only modified latent heat.

Expressed mathematically, if such be the case, no matter what the tension is, we have: Tension of steam (a certain amount of motion) + latent heat of same steam (a certain amount of motion) = total amount of heat (total motion) in steam.

In order to ascertain if I was right in my supposition, I took up—not any of the tables calculated by the formulæ of various authors, but the results of direct experiments made by the most reliable scientific authorities—and I soon had the satisfaction of discovering that I had, to all appearance, solved the gordian knot.

The tension of steam, or its elastic force, does not present any natural simple relation to either thermometric temperature or to the total units of heat supposed to be contained in steam, but is most intimately related to its latent heat, a portion of which, in fact, it really is. According to my views, the simple law reads as follows:

While the pressure of steam increases in a geometrical progression, the latent heat decreases in an arithmetical progression, and vice versa.

If the pressure in atmospheres be as 1, 2, 4, 8, 16, 32, etc., the corresponding diminution in latent heat will be, respectively, as 1, 2, 3, 4, 5, 6, etc. The same would occur with the series 3, 6, 12, 24, 48, 96, etc., or 5, 10, 20, 40, 80, etc., or any other

If we take 537 C. units of caloric as the quantity of "latent heat" in steam, indicating 100° C. on the thermometer under atmospheric pressure, we find that the difference between the terms of the above arithmetical progression is 17, or a number which approximates to it within a very minute fraction.

This number of 17 units of heat is an average of the differences found by me to exist between a large number of the carefully *observed* temperatures, noted by Arago, Dulong, and Regnault, as corresponding to *observed* pressures.

It gives us:

Pressure in atmosphere.	Latent caloric.
1	537 units.
2	537—17
4	537—17 × 2
8	537—17 × 3
16	537—17 × 4, etc.

By interpolation, I have formed the following table, showing the latent heat (which may always be readily calculated from the thermometric indications, by means of Regnault's formula T ·305 + 506·5 for Centigrade degrees, or (T—32)·305 + 911·7 for Fahrenheit degrees, and the corresponding pressures of steam in atmospheres, from 1 to 16. The temperature is also readily calculated from the latent heat by the formula T=606—L÷·695, in which L represents the units of latent heat.

The letter A indicates the units of latent heat of steam of 100° C., or 212 Fah. or of atmospheric pressure, and *b* indicates the number corresponding to the difference between two terms of the arithmetical progression. I shall here only exhibit the Centigrade series in numerals.

Pressures in atmospheres.	Corresponding units of latent caloric.	In general.
1	537	$A - o$
2	537—17	$A - b$
3	$537 - (17 + \frac{17}{2})$	$A - (b + \frac{b}{2})$
4	$537 - (17 \times 2)$	$A - (b + b)$
5	$537 - [(17 \times 2) + \frac{17}{4}]$	$A - (b + b + \frac{b}{4})$
6	$537 - [(17 \times 2) + (2 \times \frac{17}{4})]$	$A - (b + b + 2\frac{b}{4})$
7	$537 - [(17 \times 2) + (3 \times \frac{17}{4})]$	$A - (b + b + 3\frac{b}{4})$
8	$537 - (17 \times 3)$	$A - (b + b + b)$
9	$537 - [(17 \times 3) + \frac{17}{8}]$	$A - (3b + \frac{b}{8})$
10	$537 - [(3 \times 17) + (2 \times \frac{17}{8})]$	$A - (3b + 2\frac{b}{8})$
11	$537 - [(3 \times 17) + (3 \times \frac{17}{8})]$	$A - (3b + 3\frac{b}{8})$
12	$537 - [(3 \times 17) + (4 \times \frac{17}{8})]$	$A - (3b + 4\frac{b}{8})$
13	$537 - [(3 \times 17) + (5 \times \frac{17}{8})]$	$A - (3b + 5\frac{b}{8})$
14	$537 - [(3 \times 17) + (6 \times \frac{17}{8})]$	$A - (3b + 6\frac{b}{8})$
15	$537 - [(3 \times 17) + (7 \times \frac{17}{8})]$	$A - (3b + 7\frac{b}{8})$
16	$537 - [(3 \times 17) + (8 \times \frac{17}{8})]$	$A - 4b.$

I am at present occupied in computing the latent heat of all pressures, from 1 to 16 atmospheres and up to 1,000ths parts, which will furnish more complete data than any extant.

In order to facilitate at once to others the verification of my statements, I will limit myself to showing how the 10ths, 100ths, and 1,000ths are interpolated by an example.

PRESSURE FROM ONE TO TWO ATMOSPHERES.

TENTHS.

Atmospheres		Units.
Atmospheres	1	537
"	1·1	$537 - \frac{17}{10}$
"	1·2	$537 - 2 \times \frac{17}{10}$
·	·	·
"	1·9	$537 - 9 \times \frac{17}{10}$
"	2	537—17

HUNDREDTHS.

Atmospheres		Units.
Atmospheres	1	537
"	1·10	$537 - \frac{17}{10}$
"	1·11	$537 - (\frac{17}{10} + \frac{17}{100})$
"	1·12	$537 - (\frac{17}{10} + 2 \times \frac{17}{100})$
·	·	·
"	1·99	$537 - (9 \times \frac{17}{10} + 9 \times \frac{17}{100})$

THOUSANDTHS.

Atmospheres		Units.
Atmospheres	1	537
"	1·101	$537 - (\frac{17}{10} + \frac{17}{1000})$
"	1·102	$537 - (\frac{17}{10} + 2 \times \frac{17}{1000})$
·	·	·
"	1·999	$537 - (9 \times \frac{17}{10} + 9 \times \frac{17}{100} + 9 \times \frac{17}{1000})$

I have applied my formula to most of Regnault's practical observations, taken high and low in the scale, and find the discrepancies to be really insignificant.

He gives, for instance, pressure 1·905 atmospheres; observed temperature, 119·16; latent heat, 523; I find 521,615, or a difference of only 1·385 units. Another is T = 119·16; pressure, 1·924 atmospheres; latent heat, 522·2; I find 521·292 units, or a difference of 1·008 units.

Among the higher pressures, we find: Pressure, 13·344 atmospheres; temperature, C., 193·8; latent heat, 472·2. We here, by our theory, have 473·662, a difference of 1·42 only; and again, P = 13·625; T = 194·8; latent heat, 471·2, when I find 474·047, a difference of 2·847 units.

The above are only a few examples, taken at random from among many, to serve as a verification of my law, but all those I have tried have approximated as closely to the practical results of experiment as those we have just quoted.

I have rapidly penned the present notice for the purpose of eliciting the opinion of others upon this important and interesting subject.

In a future article I may furnish various practical formulæ in connection with it, and will enter into the discussion of the relation existing between, so-called, latent heat and the volume of steam, as also its connection with the present theory of expansion and condensation, all of which we hope to show, have the most intimate dependence on its amount.

Let us conclude by reminding the reader, that we are, in all probability, fast approaching the day when it will be admitted by all sound philosophers, that only one law exists in nature, MOTION, the modes of which are familiarly known as heat, light, electricity, chemical affinity, molecular forces, gravitation, innervation, etc., all of which will be found to be perfectly convertible into one another. This will constitute a sufficient proof of their identity.

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THE SEWING MACHINE---ITS ORIGIN AND SUGGESTIONS FOR IMPROVEMENT.

In the year 1825, there lived in the city of Saint Etienne, in France, a poor and obscure tailor whose patrons were few and far between. His carelessness about the work intrusted to him, joined to his eccentric habits, obtained for him throughout the neighborhood an unenviable reputation, the natural consequences of which were that his business declined from day to day and he ended by becoming a veritable pauper. In 1827, he was considered as laboring under the constant influence of hallucinations, and in 1829, he was unanimously regarded by the gossips of his precinct as insane.

This madman was no other than Barthlemy Thimonnier, the inventor of the first sewing machine. He was born at Abreste in the year 1793, and was the son of a dyer of Lyons.

It is an old custom with many manufacturers of the south of France, to give out large quantities of needle-work and embroidery to the country girls residing around their establishments. This attracted the notice of Thimonnier and originated in his mind the first idea of a sewing machine. On its construction he worked without help or money during four successive years, at the expiration of which, in 1830, he obtained his letters patent.

A government engineer by the name of Beaunier, living at Saint Etienne at the time, examined the machine, and appreciating at a glance the value of the invention, took the tailor with him to Paris, where a firm was soon started under the title of "Ferrand, Thimonnier, Germain, Petit & Co., with a view to the profitable working of the patent.

In 1841, in the Rue de Sevres, might have been seen a workshop, in which eighty wooden sewing machines were constantly employed in making army clothing.

That same year, however, the tide of a fierce revolutionary outbreak swept over France, and the laboring men of the capital, in their blind and ignorant fury, saw in this new substitution of machinery for manual labor, nothing but a means of robbing their wives and daughters of their daily bread. The consequence was exactly the same as in the case of the canal boatmen of Münden, who destroyed the first steamboat started there in the year 1707, and of the Belgian weavers, who some years ago broke up the first flax-spinning machinery imported from England into the city of Ghent. An armed and infuriated mob smashed all of Thimonnier's machines, and he himself had to flee for his life.

Soon after this Beaunier died, and the firm of Germain, Petit & Co., was dissolved leaving our poor tailor out in the cold.

In the year 1834, Thimonnier returned to Paris, and having improved his machine, attempted to make a living by taking in sewing. In this, however, he failed, and was at length obliged to walk all the way back to his native home with his machine upon his back, exhibiting it as a curiosity along the road in order to enable him to purchase his daily meals.

After this sad experience it would be thought Thimonnier would have given up the matter in despair, but, on the contrary, he went to work and constructed several new machines which he disposed of with the greatest difficulty.

In the year 1845, the date of Howe's patent in America, the French machine was already making two hundred stitches a minute

M. Magnin, of Villefranca, at this crisis joined our inventor, and furnishing the necessary funds, the construction of ten-dollar machines was at once begun by them, with a fair prospect of pecuniary reward. In 1848, these machines made three hundred stitches per minute, and could sew and embroider any material from muslin to leather inclusive. The woodwork had now also been replaced by metal.

In the memorable month of February, 1848, another convulsion of the people took place in France, and for the second and last time were Thimonnier's hopes of success entirely blighted, himself and his partner being completely ruined by it.

He sold his English patent to a Manchester company for a trifle, sent his best machine in 1851 to the great London Exhibition, but too late to be noticed; and, finally, after thirty years of a life of incessant struggle and adversity, he died at the age of 64, in the greatest poverty, on the 5th day of August, 1857, at a place called Amplepuis.

While our poor tailor was starving in Europe, the sewing machine was being perfected on a new principle, in the United States, and in 1845, Elias Howe, Jr., obtained his patent out of which he eventually made quite a large sum of money.

Since 1852, American sewing machines by various makers