inches scantling. There is one joint for each panel, which joint is always over a floor-beam. The three inch plank that composes the flooring, is laid upon these stringers and spiked to them. *The sway or lateral bracing* is formed of $1\frac{1}{8}$ inch rods in four pieces to each panel, starting from the upper series of bolts that fasten the floor-beams and the posts together, to which they are attached by means of an eye, and through which the above described bolts pass. At the point where they would intersect they are cut off, passing into a ring of half-inch flat iron, against which the ends are screwed up and adjusted. *The truss rods* are introduced as shown upon the Plate, passing between the pairs of suspension posts, and are all of one inch round iron. For three panels each way from the towers, the tops and bottoms of the suspension posts are connected directly with the bases and tops of towers. The rods of the last panels (those that incline downwards) are secured to the bottoms of towers on their opposite sides; in other words, they pass through the nearest legs of the towers, and are screwed up against the legs on the further side. They thus act as an additional tie and stiffener for the bases of the towers. The ends of the truss rods are screwed up against oak blocks, bearing against the floor-beams and the feet of the suspension posts. In order to allow for the stretching of the cables, a camber of 12 inches for each span was framed into the bridge, and adds very much to the general appearance. It is intended eventually to put the foot-walk on, to pass through the towers and provided for in construction in the elongation of the floor-beams. The railing will be a substantial Howe truss 4 feet high. At present the bridge is subjected to a heavy amount of traffic, such as stone, lumber, cattle, &c., and it gives evidence to a surprising degree of stiffness due entirely to the peculiar system of trussing, in making the verticals act by thrust, and the diagonals by tension. This general arrangement for suspension bridges was first practised by John Murphy, Esq., C.E., over the Gauley, in West Virginia, and also at Franklin, Pennsylvania. But he used his diagonals to truss each panel separately, instead of trussing panels by attaching the diagonals to fixed points, as is the case in the above-described bridge, where the diagonals are secured to the *towers* for *six* panels out of ten in each span. *Without* the aid of the trussed railing, the effect of a heavy team passing over the bridge can hardly be felt until it strikes the span that we may be standing on, further than the slight tremor inseparable from all bridge superstructure, especially those of the suspension class. Without considerable modifications, the above construction for suspension bridges is inapplicable to spans much above 200 feet.

For the Journal of the Franklin Institute.

The Bourdon Pressure Gauge. By JOHN D. VAN BUREN, JR., U.S.N.

MR. EDITOR: I observe in the *Journal* for this month a very ingenious and plausible attempt to prove me in error as regards a note appended to my article on the Bourdon Pressure Gauge, in which I attempt to show that the principle involved in the straightening of the tube under an increased pressure does *not* depend upon the fact that the outer is greater than the inner circumference of the bend of the tube. This attempt I now proceed to prove has failed.

The writer makes his grand *faux pas* when he assumes as established the following two *remarkable* points:

1. That in the same system of forces, when estimating their effects upon the same body, we are at liberty to take a different centre of reference for each force.

2 That the *components* of two forces will produce different effects from those of the *original* forces.

Let us refer to the figure, Fig. 1. Here we have a bent tube of circular cross-section of *invariable* size or diameter, and we may therefore assume that the points *a* and *b* are *rigidly* connected by the line *a b*. Now, he proceeds to resolve the forces p_1 and p_2 into components—an entirely unnecessary operation, tending to confusion, referring the components of p_1 to the center *c*, and the components of p_2 to the centre *g*, which, by first principles of mechanics, he has no right to do, since the forces are of the same system acting upon points of the same *rigid* body, viz: the line *a b*. If the line *a b* (or, which is the same thing, the points *a* and *b* connected by this rigid line *a b*) swerve from its original position under the effect of the forces p_1 and p_2, it must do so around a *single point* or centre, kept as it is in one plane; for there are but two possible motions, *one* of rotation and *one* of translation, and these must be estimated from a *single* point of reference, when considering the *resultant* effect of all the forces. Thus, Fig. 3, let us suppose *a b* to be a rigid line connecting the two arms *a c* and *b d*, which are pivoted respectively at the points *c* and *d*, *a c* being 20 feet in length and *b d* 5 feet in length; and that a force $p_1 = 10$ lbs. acts at *a* in the line *a b* and a force $p_2 = 10$ lbs. acts at *b* opposite to p_1. Now, no one will pretend to say that p_1 will preponderate over p_2, simply because the *moment* of p_1 is greater than that of p_2, *when estimated after the above peculiar method*—from a distinct centre? or, making $p_1 = 5$ lbs., that p_2 will *not* preponderate? But the line *a b*, Fig. 3, is under precisely the same circumstances as the line *a b*, Fig. 1; for the line is rigid, and the section *a b*, being a *circular* one, is that of equilibrium, making the elementary forces

Fig. 1

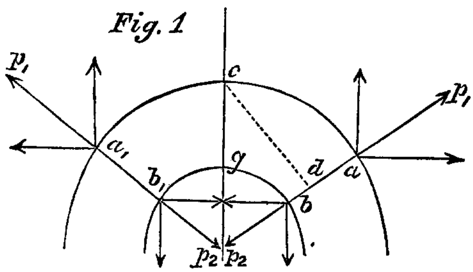
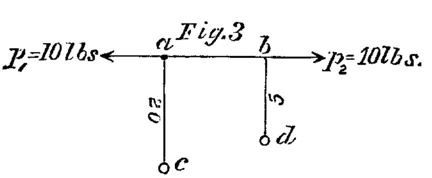


Fig. 3



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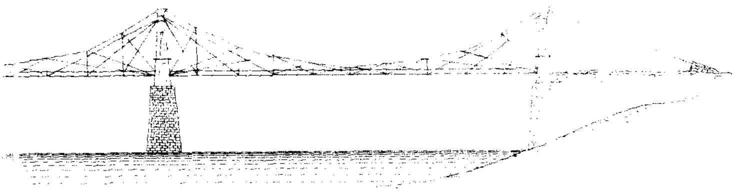
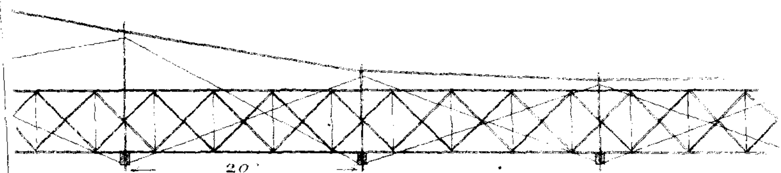
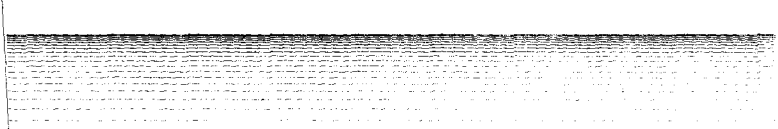


Scale 100' to 1"



Elevation of Half Span



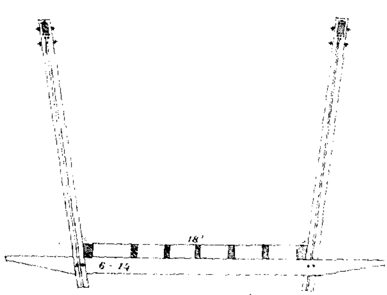
WILLIAMSPORT SUSPENSION BRIDGE

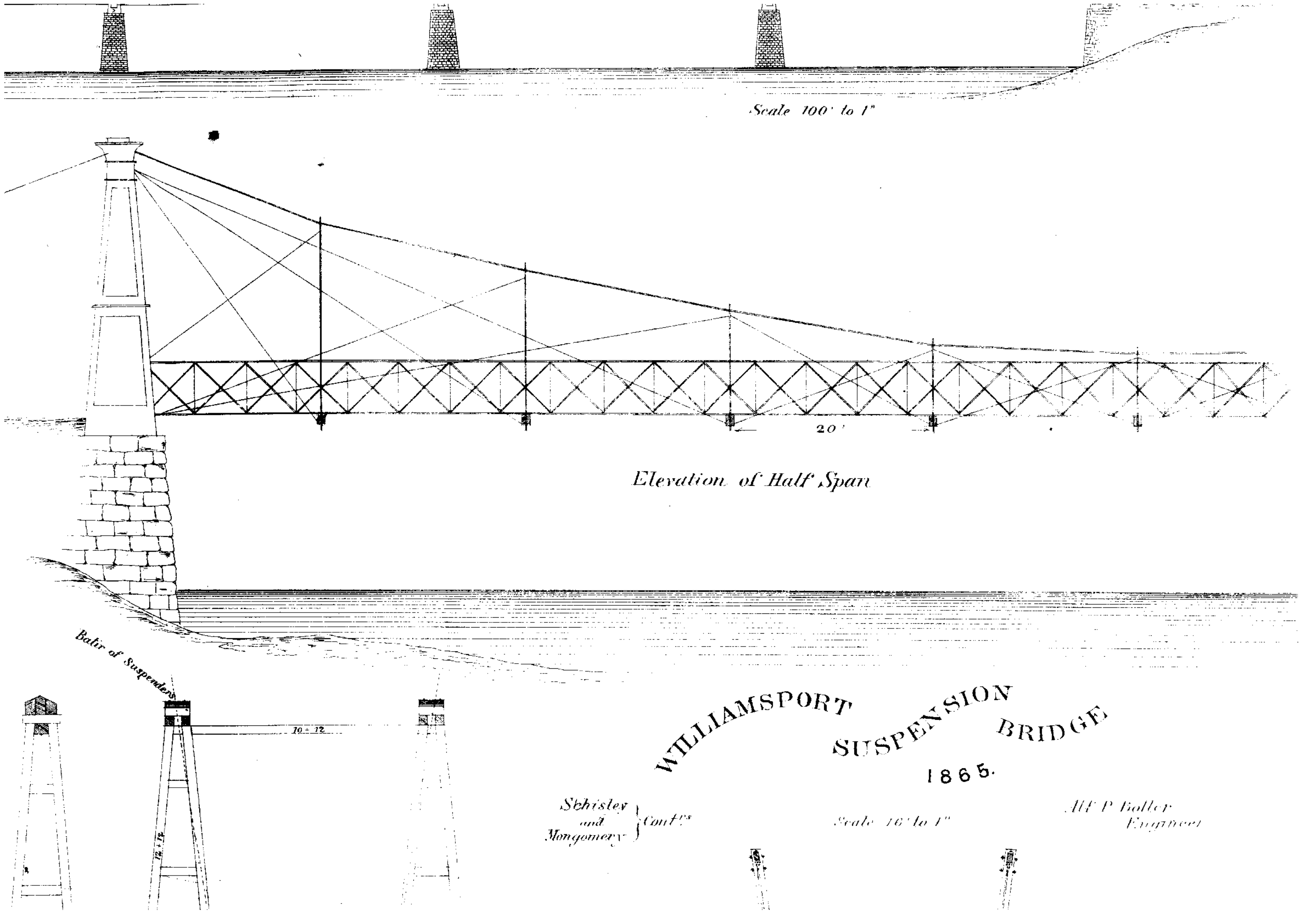
1865.

Cont^rs

Scale 16' to 1"

Alf P. Boller
Engineer



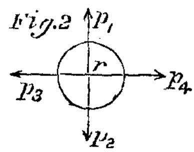


Scale 100' to 1"
20'
Elevation of Half Span
10 + 12
12 + 12
WILLIAMSPORT SUSPENSION BRIDGE
1865.
and
Mongomery
Cont'rs
Scale 16' to 1"
Alf P Boller
Engineer

diametrically opposite *equal.* Such an absurd conclusion as the above is not yet established by mechanics.

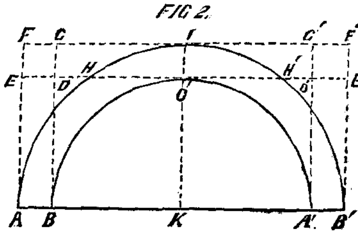
But, without following the writer further, we now proceed to prove that there can be no tendency in the tube with *invariable circular cross-section* to straighten under an increased pressure; and thus to refute his conclusion in reference to the influence of the excess of outer circumference upon the tendency in the tube to straighten. If an elementary ring (a cross-section) of the tube be considered, as the ring r, Fig. 2, it is evident that, having the circular form, it is in perfect equilibrium in reference to an uniform pressure exerted centrifugally or centripetally upon its inner or outer surface; and therefore any two elementary forces which are diametrically opposite, as p_1 and p_2, must be *equal.* This being so, the force acting at a, Fig. 1, equilibrates the force acting at b; and hence the *resultant* force is *zero*, and no motion of any kind can ensue. Thus, taking as the centre the point c, we see that the equal and opposite forces p_1 and p_2 have a common arm $c\,d$, leaving no unbalanced force or moment to cause motion around it.

Fig.2



In his second figure, while considering the meridian arcs A I B′ and A′ O′ B, the writer loses sight entirely of all *intermediate* arcs of metal. Thus, Fig. 2, he asserts that there is nothing (save the resistance to *deflection* as a beam, of course,) to prevent the arcs H I and H′ I from spreading apart and thus causing the tube to straighten, forgetting that every point of the arc H I is connected with every point of the arc H′ I in precisely the same manner as every point of the arc A H is connected with every point of the arc B O′, *by a ring of metal*—as from H to H′, and this metal is not supposed to stretch between any of these points.

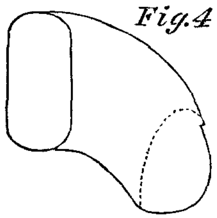
FIG 2.



There is no tendency in the ring of metal connecting any two corresponding points of the arcs H I and H′ I to elongate, thus allowing these points to *separate.* On the contrary, if any such ring be considered independent (as he supposes each filament to be, wrongly, of course,) and free, then the *horizontal* pressure on its interior must give it a tendency *to draw together* these points; for being *elliptical* the pressure would cause it to assume the circular form. Hence, the influence of the excess of outer circumference H I H′ would cause the tube to *coil up*, and not to straighten. He also proves (?) that all points below the line H H′ are in equilibrium in respect to *two* rectangular directions; *hence* they are in equilibrium in respect to any direction, *but* by taking the point I in any other position, we may also *thus* prove, that some of these points are *not* in equilibrium. Why the particular arc H I H′ should thus suffer does not appear.

But, again :* With the same *invariable circular cross-section the capacity of the tube cannot be increased by bending the tube ; for the present normal capacity is its maximum.* The capacity of the tube is found by the centro-baric theorem, by multiplying the *central* arc, Fig. 1, by the area of the cross-section, *and in no form could we place this metal, preserving its circular section invariable as well as its surface,* so as *to increase its capacity.* In fact, by bending the tube the capacity must be diminished, since the *circular section* will be destroyed. Now, the steam will force itself into the greatest possible space, thus preventing any change of shape in the tube tending to diminish its volume. Thus we prove that the tube of *invariable circular cross-section* will *not* tend to straighten under an increased pressure. If, however, the circular section be enlarged by the *stretching* of the metal, *then* our theory shows there will be a tendency to straighten under an increased pressure.

Fig. 4



Finally, it is shown, experimentally, that the tube of cross-section represented by Fig. 4 will tend to *coil up* under an increased pressure, and that the tube of cross-section represented in Fig. 5 will tend to *straighten* under the same circumstances, corroborating the theory advanced by me, which predicts these results.

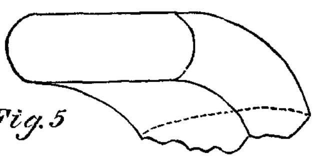


Fig. 5

In practice we often meet with very large and very long steam pipes of copper, with circular cross-sections, having a short bend at one end or both, Fig. 6 ; but these pipes never tend to assume any such position, as is shown by the dotted lines in Fig. 6, which would be the case if the short bends had any tendency to straighten. The rigidity being small could not prevent such action if it were exerted. So the small pipes of the hydrostatic press are never straightened under enormous pressure, unless their cross-section be other than a circle.

In my paper I concluded that the tube could not move until the unbalanced and concerting forces of extension and compression are excited in the metal of the tube by a change of section. These are the forces which directly act to move the tube. The explanation given in my paper is not only the "good and sufficient reason," but I think the *only* true explanation of the action of this gauge.

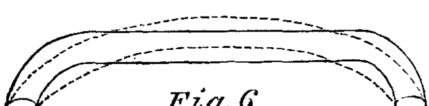


Fig. 6

P. S.—Since the above was written, a friend,—a Chief Engineer in

* In considering the effect of excess of outer circumference, the tube should be considered perfectly *flexible* and *inelastic*, and therefore liable to *wrinkle* if deflected. The tube of maximum capacity *consistent with equilibrium* is meant.

the Navy,—has made an experiment to illustrate the above principles. The apparatus used consisted of two rubber tubes of equal lengths, with *circular sections* curving from a common rubber bulb between them. When subjected to the hydrostatic pressure produced by filling the bulb with water, and placing weights upon it, the tubes showed no perceptible tendency to straighten ; although, according to my paper, if the cross-section had enlarged by the stretching of the material, *then* a slight straightening of the tubes should have resulted. This crucial test is conclusive of the correctness of the theory I have mathematically demonstrated.

For the Journal of the Franklin Institute.

On the Steam and Exhaust Ports of Steam Cylinders—The practicability of a material reduction in their dimensions suggested. By D. M. Greene, U. S. Naval Engineers.

The rules employed by designers of steam machinery in determining the areas of steam ports and passages, seem to be generally, if not always, entirely arbitrary; indeed, the "*rule of thumb*" appears to be not only the principal one used, but to find advocates in quarters and under circumstances where they would be least expected. The ever-varying conditions of steam pressure and velocity of piston appear to be ignored, and the unwarrantable assumption made, that the relation between the areas of the ports and passages, and of the piston once determined, for an engine working under a given steam pressure, and with a given velocity of piston, furnishes a reliable precedent in designing all engines whatever may be the conditions under which they are to perform their functions.

In engines of great power, having large cylinders, and using steam of more than ordinary high pressure, great difficulty has been experienced in securing a proper action of the valves. This has been owing to the excessive weight of the valve, together with the enormous steam pressure upon its back, causing a rapid destruction of its face by friction. Much ingenuity has been expended in providing remedies for this evil, and as the results we have, among other contrivances, the "vacuum ring," the "Wadell plate," and the steel roller, all of which possess merit; but which, while affording partial relief, are objectionable on account of the difficulty attending their proper fitting up and adjustment, and especially on account of their liability to derangement after having been once adjusted.

It seems strange that, while so much has been done to mitigate the *effect* of the evil of large and heavy valves, nothing appears to have been done to reduce the *evil itself* to its minimum practicable proportions. Why has not some one suggested that the ports, and consequently the valves, as now proportioned, may be twice or even three times as large as necessity requires?

The writer, in constructing a formula for his own use in determining the proper areas of opening for steam ports, has had his attention forcibly directed to this matter by the result obtained; and, feeling