line faces, but where pulled apart longitudinally, the same iron shows a fibrous structure. In other words, the stamp stem may have been weakened and finally broken off by successive shocks, and short kinks or bends, operating transversely, as the result of striking uneven surfaces in the mortar, etc.

2. The iron in a bar may be crystalline at one point, but fibrous at another.

3. Iron may have been crystalline at the point tested, but assumed a fibrous appearance at the tensile fracture, due to the flow of metals.

4. Mr. Kreuzpointner not only gives his own opinions, but quotes eminent German authorities in support of the idea that changes in the component elements of iron are necessary for changes in its crystallization, and that these changes cannot occur at low temperatures.

5. The results of Dr. Wedding's researches are given to show, also, that repeated stresses cannot produce crystallization.

While, therefore, there is a strong weight of argument against the crystallization of iron in service, Wohler and Spangenberg agree that alternate and intermittent stresses tend to deteriorate and fatigue metals; and Mr. Kreuzpointner says:—

"If we consider how, with insufficient dimensions and impaired cohesion, sudden shock will produce sudden fracture, then we have all the elements necessary to produce the well-known crystalline appearance of the fractured surfaces.

"The fractures will thus appear crystalline, even if the iron were ever so fibrous, because of the suddenness of rupture which did not allow the metal time enough to flow, giving, consequently, a clear transverse break of the fibers, which, as already explained, are nothing but elongated crystals, the transverse sections of which are the measure of their sizes."

Wohler declares, as the result of his experiments, that "the members of structures which are subject to alternating strains, pulling and pushing, or bending and twisting, ought to be made larger in the proportion of 9 to 5."

Pieces of iron, planed, polished, and etched, are said to give "undoubted evidence of the crystalline conditions existing before the iron was ever subjected to any strain."

The foregoing seems to establish that, though there may be the weakening of stamp stems by repeated shocks, which finally may cause them to break suddenly, *thereby showing the crystalline faces of the iron to great advantage, there has been no* enlargement in service of such crystalline faces in the iron.

H. M. HOWE, Boston, Mass., (communication to the Secretary)—Will Dr Raymond let me modify the statement, which he gives, *Trans.*, xxiii, 560, of my position in regard to the crystallization theory of rupture under repeated stress and vibration? My argument on page 196, *et seq.*, of my Metallurgy of Steel, was that, though it was quite conceivable on *a priori* grounds that vibration might make iron crystallize, yet there was no evidence that it ever does. My summing up was that we have "every reason to believe that the granulation and crystallization of iron under vibration and shock is a myth."

We seem to be at cross-purposes with Mr. Argall. He seems to think that people have denied that iron under certain sets of conditions, some of which include shock and vibration, breaks with a crystalline fracture: whereas, so far as I know, nobody has ever denied this. It is not the occurrence of a crystalline fracture but its explanation that is in dispute. I suppose that he must have fallen in to this confusion; for I see no other way of accounting for his setting forth the undisputed crystalline fracture of stamp-stems in such a way as to imply that it answers the question at issue.

Let me try to sum up briefly the condition of our knowledge. Repetitions of stress, wholly unaccompanied by vibration and shock, are well known to induce some kind of deterioration which eventually breaks iron. Vibration and shock, unaccompanied by great stress, or at least by prolonged repetition of considerable stress, have never, so far as I know, been known to break it. This points to repetition of stress, and not to the vibration and shock which only in certain cases accompany or cause it, as the real cause of such breakage.

Examination of the fragments of pieces thus broken by repeated stress, even when accompanied by vibration and shock, has indicated that the injury was local;* and careful microscopic examination of the fragments close to the fracture has detected no crystalline change, but at most a shattering and incipient separation of the pre-existing particles, grains or crystals whichever you call them.† All the evidence has been thus against the theory that vibration caused even a local crystallization.

The crystallization-theory thus was a discredited one. Fresh evidence might indeed rehabilitate it. But I fail to see that Mr. Argall has given us the faintest ray of evidence or of reasoning in favor of that theory.

We know that iron, if nicked on one side and bent backwards, yields a fibrous fracture, but that the same bar, if nicked all around and broken with a sharp blow, yields a crystalline one. The two different modes of causing rupture induce it to follow different paths, and yield different fractures; for the fracture is nothing more than the path of rupture. In this case nobody supposed that nicking all around and breaking with a single sharp blow has crystallised the iron; it has simply developed a new path for rupture. Thus a crystalline fracture is shown to be no proof, but at most only a suggestion, of crystallization. The planes along which the rupture of the nicked bar travelled existed before rupture followed them, just as the cleavages in a feldspar crystal exist before I cleave the crystal with my knife, and as the image exists in the exposed but undeveloped photographic plate.

Mr. Argall vainly attempts to escape from the fact that "iron when fractured suddenly presents invariably a crystalline appearance, when fractured slowly its appearance is invariably fibrous," by his unqualified assertion that "In the first case the fibers are not given time to stretch, but are broken off at right angles to their longer axis, whence the apparant fine crystallization; while, in the latter case, actual crystals are developed in the iron, some reaching as muce as 0.25 inches in diameter."

Let us see how true this theory is. First so far as our present evidence goes, there probably are no fibers in iron such as Mr. Argall supposes, prior to rupture. Its particles apparently are nearly equiaxed.

Next, when a crystalline fracture forms in suddenly breaking iron, its faces are not as Mr. Argall asserts, at right angles to the imaginary fibers, or to the axis of the fibers which would actually have formed during fiber-favoring rupture. They are in general approximately at an angle of 45° with those axes.

Finally, it is not the suddenness of breaking, as such, that gives us a crystalline instead of a fiberous fracture; for in certain extremely rapid breakages, as for instance when a bar is torn apart longitudinally by an explosion of gun-cotton, we get invariably a silky fibrous fracture.*

The simple truth is that each new mode of causing rupture seems to direct it along a special peculiar path, and causes a special fracture. The fracture thus depends jointly on the properties of the material broken, and the conditions under which breakage occurs. *Why rupture follows this or that special path under special conditions,* is for the clastician and mathematician to determine with great care.

Even for them the question is no easy one; and it certainly cannot be brushed aside off-hand or answered at random by those who run.

With these facts before us, shall we wonder if the special set of conditions under which breakage occurs in stamp-stems directs rupture along still a new special path, and thus yields a special kind of fracture? Is this special kind of fracture really any stronger evidence of crystallization than the other kind of crystalline fracture which we had long known that we could cause by r sing all round?

The defenders of any discarded theory, of this one as of the corpuscular theory of light, need not trouble themselves to show that their theory is conceivable; that it does not violate "any law of modern physics or of the molecular theory of matter." What we need is evidence which this theory explains, and which other theories cannot explain. We have no room for theories which are simply conceivable or even possible. We want those which are probable through evidence. But evidence, like the fracture of stamp stems, which accords equally well with either theory, really helps the accredited theory but does not help the discredited one.

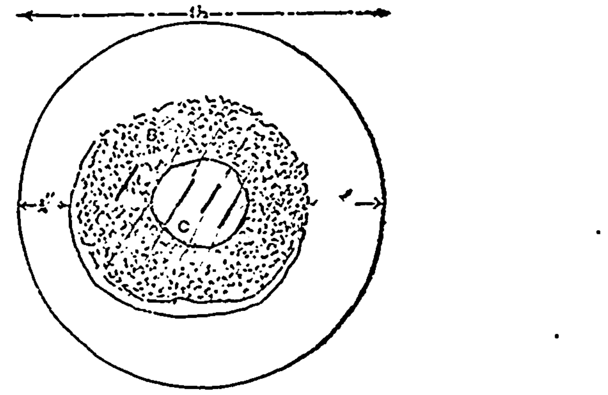
If Mr. Argall or Mr. Wilkes will send me a piece of broken stamp stem containing the fracture, I will gladly try to procure some evidence which will count, whether it be for or against the crystallization-theory.

It seems to me that the chief teaching of this discussion is care in the use of words. Had Mr. Argall contented himself with saying (*Trans.*, xxiii., p. 557), not "vibration under all conditions will crystallize iron," an assertion certainly *wholly* unjustified and probably very far from the truth, but "severe shock will eventually weaken or destroy iron," he would have asserted all that was necessary for his purpose. By going beyond this, and needlessly asserting that *all vibrations* injure iron, and by specifying that the particular way in which they injure it is by causing a crystalline change within it, he gave criticism a most pressing invitation.

His calling those whose opinions he attacks, "dogmatic theorists" seems unfortunate. *If by theorists he means those who habitually study the causes of the pheno-*mena, or "theories," he simply says that their habits should qualify them to form trustworthy opinions as to the cause of this pheaonmenon. If he means that they are ignorant of the conditions under which metals fail in practice, he is simply mistaken. And as to dogmatism, those whom he attacks have not *denied*, but *questioned* and *doubted* crystallization by vibration; while he positively asserted at first that *vibration under all conditions* will crystallize iron; and his later modification merely limits the proposition to certain conditions, *without changing its character as positive assertion.* It is bad enough for the sceptic to be excommunicated, but to be called *dogmatic* to boot, and by the Pope at that, would be rather bewildering.

DR. RAYMOND—Since the foregoing discussion took place, I have received from Mr. Argall, in a private letter, the following statement, which seems to me worthy of preservation in the record as a pertinent observation. He writes that on the 24th of July last, he was delayed for some time near Hill City, South Dakota, by *reason of the fracture of an axle under the tender of Burlington engine No. 256:* "The axle broke off close to the wheel; an old and rusty crack, varying in depth from three-quarters to one inch, ran completely round the journal; next came coarsely crystalline iron, while in the centre the iron was beautifully fibrous, and showed the bars from which the axle had been forged. These, by the way, as indicated by heavy lines in the drawing, were not properly welded."

The accompanying figure made from a pencil sketch in Mr. Argall's letter, illustrated his statement. I will only observe as to the conclusions to be drawn from this case, that the facts seem to me consistent with the theory of progressive fracture, and with the well-known relation between the nature of the stress causing fracture and the appearance of the fracture-surface.



Fracture of Railway-Axle.

The indications of imperfect welding observed by Mr. Argall may fairly be taken as evidence of improper heat-treatment for the process of forging; and this, as has been emphasized in the present discussion, is a source of crystalline structure (or, more precisely, of that condition which yields a crystalline or granular fracture under circumstances in which a fibrous fracture would otherwise be expected). The existence of the old crack round the outside seems to indicate that this part of the mass was in such a condition as to break without such elongation as might have held the whole axle together, until a fibrous fracture of the whole had been effected. In other words, improper heat-treatment may have over heated the outside and under heated the centre of the forging, so that the former became "crystalline," while the latter, not hot enough to weld perfectly, retained the capacity of elongation before fracture, which is called "fibrous structure."

On this hypothesis, the axle, if broken at any time after manufacture, would have shown on the surfaces of fracture a difference of quality between the outside and the inside. But it should not be forgotten that such a fracture would not fairly represent the process of repeated shock and stress undergone by the axle in practice. Even if the material were uniform throughout, the peculiar nature of the stresses to which it was subjected might well develop differences in the successive fractures of different concentric parts. Recent experiments have proved the somewhat surprising fact that locomotive wheels advance not in constant contact with the rails, but by a series of jumps. If I remember correctly, these experiments were confined to driving wheels; but it seems to me that the same proposition must be true in some degree of all railway wheels, especially those which are nearest to the drivers, and thus receive most directly *the effect of the successive jumps of the latter.* We have to consider, in that case, the effect of trasverse blows, repeated at the rate of 1,000 to 2,000 times per minute. Considering this rate of rapidity, and the weight supported by a railway-wheel, I think I am justified in saying that the test is more severe than that to which stamp-mill practice subjects the stem of a stamp. But the effect of this series of blows is doubtless somewhat different. Each shock exerts a tensile strain upon the lower, and a corresponding strain of compression upon the upper half, of the axle. It is obvious that, *by virtue of the revolution of the axle, every part of the circumference* experiences these strains in rapid alternation, and that every part of the interior exper-

* Baker, Trans. Am. Soc. Mech. Eng., viii., p. 163, 1887. Howe, The Metallurgy of Steel, p. 199, Column 1. Sorby, Journ. Iron and Steel Inst., 1887, i., p. 265.

† Martens, Stahl und Eisen, vii., p. 238, 1887. Sorby, Journ. Iron and Steel Inst., 1887, i., p. 265.

* Maitland, "The Treatment of Gun-Steel," Proc. Inst. Civ. Eng., lxxxix., pp. 120, 121, 1887.