above named, yet in one very near to it, in which the cinder films chance to be of greater thickness than those in that plane, and, as a matter of fact, fractures in such bars are usually within a few inches of the point where the bar enters its head, as at G-H, Fig. 1.

The particular point in the circumference of such a hammer bar where the imminent fracture first appears is often determined by the manual peculiarity of the "hammer-man." A left-handed man will incline his work to the left, and a man who is right-handed will be likely to use the right side of the anvil more than the left. In the latter case, the work B, Fig. 1, will tend (whenever it is in the position shown), to produce a tensile strain at the point G, which, as the work is shifted to the center or occasionally to the left side of the anvil, becomes a compressive strain. We should, therefore, expect (as is, in fact, the case), that the initial manifestation of the fracture would be found at that point, and that it would gradually extend towards H, until the bar was finally "jarred" asunder. The separation would take place through films of cinder between the ends of the elongated compound crystals of the bar, thus exposing those ends, and exhibiting what is called a crystalline fracture.

The belief in the so-called crystallization of wrought iron, as the result of prolonged use, is, I think, altogether a mistake; and I am clearly of the opinion that the crystallization observed in the case of any particular fracture existed, just as we see it, at the time the metal was given the shape in which it was ruptured. After a bar of distinctly fibrous wrought iron has been subjected to multitudes of sudden jerks of extension or jars of percussive compression, the cinder in some cross section of it (in which this impurity is slightly thicker than elsewhere), gets broken up, cohesion is destroyed, and the bar breaks with a crystalline fracture.

I have had a specimen prepared for the purpose of making the foregoing explanation of the apparent crystallization of fibrous iron more evident. It is a short piece of square bar of wrought iron. One end is decidedly crystalline in its fracture, showing distinctly that the bar was originally built up of five flat bars. The other end is, for more than one half of its area, as decidedly fibrous as wrought iron can well be; and this end would have been uniformly fibrous in appearance had the workman who made the specimen exercised the requisite care. Thus, in a sample not over two inches in length, we have an instance of a fracture which most observers would call very bad, and another which as certainly would be called good.

It is a well known fact that wrought iron is improved in strength by repeated working. This may be accounted for thus: In the initial heating and shaping of the metal, its crystals were left with a comparatively thick film of cinder between them; but by each successive re-working, the crystals of metal are driven into closer order, some of the intervening cinder is expelled, and what remains is very much reduced in thickness, so that the cohesive attraction (whatever that may be) between these crystals, having less space to act through, acts with augmented intensity. It is well to remember when we speak of "less space" in a matter of this kind, that we are dealing with a very small quantity indeed—one that is a near neighbour to the infinitesimal.

W. H. SHOCKLEY, San Francisco, Cal.—With regard to the use of mine water in the battery, the custom on the Pacific coast is the same as Mr. Wilkes states it to be in North Carolina. Mine water is not used in the battery when it can be avoided.

I do not think the water causes the stamps to break, as suggested by Mr. Rothwell; for they last longer in a wet-crushing mill than they do in a dry-crushing one, where no water at all is used.

From my own observation, I do not think the vibration causes the stamp-stems to crystallize and hence to break. The chief strain on a stamp-stem is a bending-strain, caused by pieces of rock tilting the stamp when struck by the circumference of the shoe. This gives a strain, nearly all the effect of which is concentrated at the place where the stem enters the boss; and it usually breaks at this place. I have noticed on shafts that have been broken flattened places on the fractured surface, showing that the bending caused enough motion to wear the surfaces smooth.

The pieces of metal mentioned by Dr. Ledoux and Mr. Durfee prove conclusively that the appearance of the fracture does not show what the internal structure of the metal was before it was broken.

If the vibration causes stamp-stems to crystallize and break, it certainly requires a very long time to produce that effect; for I have known stamps to be in use for four years, dropping 95 times per minute throughout that period, which would give something over 200,000,000 blows.

As a matter of interest, however, I remark that all the blacksmiths and manual workers of iron with whom I have talked believe that iron will crystallize under shock.

MR. WEBSTER—Some years ago, an inspector of bridge-material, after making thorough tests of double-refined iron bars for eye-bars, was so well satisfied with the tension-, bending- and nicking-tests that he made a special report to the rolling mill, saying that it was the best material he had ever inspected. After these bars had been manufactured and shipped to the mill-site, a test of the full-sized bars was received, which showed very poor results, the bars having been broken through the head and in the neck with bad crystalline fractures and low ultimate strength. The bars were all condemned and taken out of the structure. A thorough investigation was made of this material by nicking tests, starting in the centre of the bar and going towards the end; and in all cases good results were obtained from the body of the bar, and crystalline fractures in the neck. Bending-tests without nicking showed the same difference. In many cases the bars did not bend 10 degrees in the neck, but even at the first stroke or two of the small hydraulic jack that we were using, the sharp, snapping sounds were heard and the material gave way all at once. The crystals were very large. Additional tension-tests of the full-sized bars were made, and some of them broke in the neck with as low an ultimate strength as 42,000 pounds per square inch, the fractures being all crystalline. Had these bars been in use several years, when this trouble was discovered, it no doubt would have been cited as another instance of crystallization of the material, caused by the vibration.

I cite this to show the importance of knowing the heat-treatment to which iron has been subjected before we attempt to theorize on the change of structure due to vibration.

In 1884, Mr. Peck, superintendent of bridges for the Missouri Pacific Railroad Company, called my attention to the fracture of some eye-bars, taken from the wreck of one of their bridges, which had been knocked down by a derailed train. These bars, he claimed, were made from good material by one of the leading bridge companies of the country, and yet they broke off short like pot-metal. Upon thoroughly investigating, we found that the bars had broken through the neck, with a coarse, crystalline fracture. I called his attention to the trouble often caused in that portion of the bar in the course of manufacture; and he embodied in his new specifications a clause which called for "eye-bars to withstand bending to a curve of 90 degrees in the neck." This test was carried out by subjecting to a welding-heat a piece of the bar about 16 inches long, allowing it to cool slowly without putting work upon it, and then bending it under a press. Several lots of material were condemned as not meeting this test.

In 1881, while we were making bending-tests of double-refined bar-iron under a small hydraulic press, the work was interrupted after several pieces had been bent about 170 degrees over a 2-inch round. Twelve hours afterwards, these pieces were put on end under the hydraulic press and we attempted to close them down further. Much to our surprise, they broke off short, the fracture being 100 per cent. granular. At first it was thought that the cold might have had something to do with this, as it was in the winter season and the pieces had been left out over night; but upon repeating the experiment and keeping the pieces indoors all night, at a temperature of about 70° Fahr., we got the same short fractures as obtained before. The same bars, when broken in the ordinary way, that is, without any interruption of the test, gave fibrous fractures and were satisfactory in every respect. This experiment was repeated on different sizes and makes of iron; and sometimes the fracture was changed and sometimes not. (I refer to the fractures as granular, as they were entirely different from the crystalline fractures cited above as being produced by the heating of the bars.) It would be interesting to follow up a set of experiments on this line and carefully note all the conditions, including chemical composition, in order to get at the cause of this apparent change of structure.

I believe I have still a piece of one of these bars, about four inches long, one end of which is entirely granular and the other end fibrous.

DR. RAYMOND—On page 12 of the pamphlet discussion of this subject already issued, in the last paragraph but one, allusion is made to the photograph of the broken connecting-bar of the Washington navy-yard, as showing "the laminated structure due to rolling." As the bar was made under the hammer, I should have written "forging." This error will be corrected in the *Transactions*.

This discussion illustrates forcibly the importance of attaching definite meaning to the terms employed in describing observed facts. "Molecular change" and similar phrases—even "change of structure"—may be (and, I fancy, have been, in this discussion) employed as signifying no more than incipient fracture, or the progressive separation of the units of structure in the line of stress, or the gradual diminution of tensile strength under repeated stresses. Strictly speaking, not one of these phenomena necessarily involves *molecular* change, such as is involved in the re-arrangement of the molecules, to form crystals. That they do indicate, in a certain sense, a structural change, is not denied. But this change may be the same in kind as that produced by any kind of fracture. When any two continuous elements of structure are pulled apart, whether gently or violently, gradually or suddenly, there is a change of structure, if we choose to call it so. But it is useless to confound that change with one that is supposed to take place prior to any rupture between the elements. When Mr. Kent speaks of "molecular disintegration," I understand him to mean a loosening of the existing structure, not the formation of a new one; and, in that sense, I conceive that he is stating exactly the position assumed by modern investigators, who fail to find any proof of a radical change of structure preceding fracture.

Mr. Kent does "not hold that iron once fibrous becomes crystallized," yet declares that "the statements that no crystallization happens are all theories." I must repeat my protest on the latter point. The advocates of the crystallization-theory have no right to call simple disbelief in this proposition a "theory." It is incumbent on them to prove their position; they cannot demand that doubters should prove a negative. As to the only theory here under discussion, it is perhaps not fairly represented by the proposition that "iron once fibrous becomes crystallized." If that be the theory, then it suffers under a double lack of proof; for there is no evidence that any iron is fibrous prior to rupture. We produce a fibrous or a non-fibrous fracture at will, according to the method of breaking.

Mr. Durfee's explanation of the process of fracture in wrought-iron seems to me to satisfy the observed facts, although I do not think that the presence of films of cinder between the elements of structure is absolutely necessary to an explanation. Planes of small cohesion might suffice. What Mr. Durfee has pointed out concerning the breaking of steam-hammers is, to my mind, pertinent and conclusive; and I deem it highly significant that he has directly observed, in such cases, incipient fracture.

It seems to me also significant that Mr. Wilkes's stamp-stems break only just above the head and not under the tappet, while Mr. Austin reports that in western mills the stamps break in both places. I am inclined to infer that the North Carolina arrangement of tappets and cams is superior; and I may go further and say that possibly some better connection between stem and head might reduce the amount of breakage in both types of mills. I venture to believe that if a stamp were composed, for instance, of one solid cylinder of iron, of equal diameter throughout, there would be no sign of "crystallization" in it if it ran fifty years. In other words, I think there is no proof of an inevitable destruction of the material, by the operation of a universal law, which cannot be largely prevented by strengthening the parts now exposed, without special protection to nicking- and bending-strains.

In this connection I would call attention to two very able and thorough articles by Mr. Paul Kreuzpointner, of Altoona, Pa., entitled, "Do Iron and Steel Crystallize in Service?" and published in the *Iron Age* of July 5th and September 27, 1894. Mr. Kreuspointner is the accomplished assistant of Dr. Dudley in the Altoona laboratory of the Pennsylvania Railroad Co. His discussion of this subject ought to convince any one who still inclines to the "crystallization theory" of the baseless and untenable character of that theory. I will quote but one sentence from his second article, which gives a new reason for disputing the traditional error. He says:

"It would hardly be worth while to take the old superstition about the crystallization of iron under shock seriously at this late day, if it were not for the fact that this superstition is being transferred to steel. This is really a misfortune to the constructing engineer who may happen to believe in it, and to the consumer of steel in general."

MR. WILKES—Referring to what Dr. Raymond has said concerning breakage under the tappet, I have no doubt that the best shapes for cams and tappets should be used, so that, when the lift begins, the blow may be as light as possible, and the friction between cam and tappet during the whole lift to be as small as possible. This shape we have secured in our practice by adopting true curves at first, and modifying them as observation of their behavior in actual work suggested. While we were using iron, we succeeded in this way in reducing vibration, wear and tear to a minimum. Since we have adopted steel for the parts referred to, a great further reduction in wear and breakage has been secured, as the result, in my opinion, of the retention of the original form of the cams and tappets, and consequently, the more certain keeping of the stem in its proper place during the lift. This permits a fairer blow, and more effective work. The stamp mill is often regarded as a rough machine that can be taken care of by anybody. But it needs, like any other machine, to be kept in order, if it is to do good work. Suitable care bestowed upon it will effect improved results as important as those to be got from any other kind of machinery used about a mine. A properly constructed and properly handled stamp mill is, by reason of its simplicity and its economy in metal consumed per ton crushed, still the favorite appliance for reducing ores for amalgamation and concentration.

MR. OLCOTT (later communication to the Secretary)—The result of a little study on the subject shows the weight of scientific argument to be against the crystallization of iron from shock or vibration at ordinary temperature. I have read the two able articles on the subject by Mr. Kreuzpointner, to which Dr. Raymond has called attention. The salient points of these papers, as affecting the stamp stem discussion, seem to be:—

1. That the crystalline appearance on the fracture is caused by the manner of breaking. That is, where fibers are broken transversely they show granular or crystal-