This shows pretty plainly where Dr. Barus stands on the question here under discussion. It is evident that he does not draw from his own experiments on glass and glass-hard steel inferences favorable to Mr. Argall's theory of stamp-stem crystal-lization. But M. Osmond is an observer of recognized acuteness and authority if he has really asserted the general proposition, apparently attributed to him by Dr. Barus, his assertion of it has weight, whether Dr. Barus agrees with it or not; and that weight bears unquestionably in favor of Mr. Argall's theory, though the term "strain" may or may not designate the particular kind of strain to which Mr. Argall ascribes a particular kind of molecular change. I have, therefore, exammed with care the statement of M. Osmond; and I find that he speaks exclusively of the two varieties of iron (al/ha and beta iron) which he considers as two "molecular conditions '-not two different arrangements of the mole-cules--and of which he says: (22)

considers as two "molecular conditions '--not two different arrangements of the molecules--and of which he says: (22)
"The *alpha* variety (malleable) predominates in steels slowly cooled from red heat, and the more exclusively, as these metals approach more nearly pure iron.
"The *leta* variety (hard and brittle) is formed:
"a. Artificially, by the action of any mechanical pressure applied below very dark red heat and producing permanent deformation.
"b. Spontaneously, at a certain critical temperature not yet determined."
Clearly enough M. Osmond is announcing no general law, but explaining (upon his own *alpha beta* theory, not by any means universally accepted as yet) the familiar effects of cold-rolling and hammering upon iron and steel. The carefully excludes strains which do not produce permanent deformation, and thus implicitly contradicts Mr. Argall's hypothesis.
4. The researches upon "hysteresis," concerning which Mr. Argall quotes Dr.

Mr. Argall's hypothesis. 4. The researches upon "hysteresis," concerning which Mr. Argall quotes Dr. Barus's reference to Warburg, Ewing, Rowland and Bell, are too abstruse and too little pertinent to repay special analysis and discussion here. If they proved any-thing for his purpose, they would prove too much. The kind of molecular change which these writers call "hysteresis" is something which they can produce by mag-netism as well as by mechanical force; it is evidenced by electrical resistance, wholly or chiefly; it is not shown or asserted or believed to produce a granular structure out of a fibrous one; and it is only called a molecular change, because, on the mole-cular theory of matter, the molecules must be somehow concerned in it. Pure and simple, it is a change in electrical resistance, which is inferred to involve a change in "chemical equilibrium," which is again inferred to be a change in molecular con-dition. dition.

I can easily understand Mr. Argall's frank admission that his citations "do not support a cry-stallization-theory for iron;" but I will leave others to decide whether they prove "that the molecular structure of iron can change and does change under

they prove " that the molecular structure of iron can change and does change under physical conditions and at atmospheric temperatures."
With regard to Mr. Argal's question, "What is electrolysis, but the direction, by means of an electric current, of the movement of molecules in electrolyte, to form new bodies?" I beg to say that I do not pretend to know exactly what electrolysis is, but I strongly suspect, that whatever it is, it is not *that*. I cannot conceive, however, the remotest connection between this question and the one under discussion ; and will therefore abstain from introducing a purely outside and wholly theoretical issue. But a lattle investigation of Mr. Argal's theory itself may not be out of place.

But a little investigation of Mr. Argall's theory itself may not be out of place. It is, if I comprehend it: A. That the iron of new stamp-stems has a fibrous structure. B. That this structure is changed during use, by the effect of repeated blows and vibrations, which cause the molecules previously arranged in fibres to separate and rearrange themselves in crystals. C. That the result of this process is shown by the granular fracture when the stamp-stem breaks.

Stamp-stem breaks. It is process is snown by the granular method when the stamp-stem breaks. It seems to me that any stamp-stem thus fractured in service would break at the beginning, rather than the end, of such a process. The molecules can not be expected to rearrange themselves without separating; and how they are to retain cohesion when they have once separated, so as to resist the breaking-effect of shock until they have got comfortably crystallized, is not clear. The beginnings of separation are incipient fracture ; and the experiments of Wohler and others, cited above, show that shocks producing such slight separation of particles may, by repetition, go on increasing the fracture thus begin; so that at last, the peace breaks by the dissolution of its original, not of a secondary, structure. This conception involves no molecular theory whatever. It rests on the cstablished fact that iron is made up of joined and cemented particles, which can be pulled apart; and that, when they are sufficiently pulled apart, the non breaks. Such a conception explains all the phenomena thus far adduced, and it is searcely necessary to set up an auxiliary and imaginary theory that the particles first separate, then remate, and then break apart agam, under strams which tended to fracture all the time. The whole question of the fibrous structure of wrought-iron and its supposed

which tended to fracture all the time. The whole question of the fibrous structure of wrought-iron and its supposed relation to strength, has received much new light within recent years, especially in connection with the attempt at Avesta to produce fibrous soft steel in the Little-Bes-semer process, by casting some slag with the steel. The peculiar lamination caused in puddled iron by the presence of intermingled cinder was thus reproduced in steel for the benefit of prejudiced consumers; but it was not shown that this structure gave increased strength. However, I will not now pursue that part of the subject.

Let us now examine the testimony of practical experience, adduced by Mr. Argall "apart from abstract theory."

"apart from abstract theory." The opinion quoted from Commander L. A. Beardslee, U.S.N., that the fracture of the 5 inch connecting bar of the Washington Navy Yard testing-machine was "an unmistakable instance of crystallization," might be construed as an assertion that this crystallization was unmistakably due to repeated shocks. Since the statement quoted is part of the report of a committee of which Commander Beardslee was chairman, and was apparently concurred in by the other members, namely, Gen. Q. A. Gill-more, A. L. Holley, William Sooy Smith and David Smith (all experts of recognized ability), the precise language employed is worthy of careful consideration. It will be found in the *Report of the United States Board for Testing Iron and Steel*, Part I., Washington, 1578, pp. 181, 182 : Washington, 1878, pp. 181, 182 :

" The question as to whether crystallization can be produced in iron by stress, or by repetition of stress with alternation of rest, or by vibration, has been very nuch discussed, and very opposte views are entertained by experts; therefore it was con-

discussed, and very opposite views are entertained by experts; therefore it was con-sidered that any data which might be gathered during our tests, bearing upon this point, would possess a value. "We have net with but one unmistakable instance of crystatization which was probably produced by alternations of severe stress, recoils and rest. "The connecting-rod of the chain-prover was 5 inches in diameter, had been in use for forty years, and had, during this period, been frequently subjected to stress up to 250.000 pounds, with recoils produced by rupture of test-pieces. "It was carefully made in the anchor shop, being hammered from the best qual-ity of wrought-iron scrap; it is not probable that any section of it, if broken when first made, would have displayed crystalline structure, but while we were testing, it parted one day at less than 200,000 pounds stress, and the surface of the fractured ends showed well-defined crystallization, the facets being large and bright as mica; the ends having become injured by rust, the bar was again broken by impact, at a

point distant over a foot from the first fracture, and the same appearance was found, which is shown in the illustration, Plate V., Fig. 1, the original of which is now in the cabinet of the Stevens Institute." The illustration here mentioned is a heliotype, reproducing a direct photograph of full natural size; and, while I have not had the opportunity to examine the actual piece said to be at the Stevens Institute, I feel sure that the appearance of the fresh fracture is better shown in the illustration than it could possibly be shown by the piece itself after the lapse of sixteen years. At the same time, the broken piece might still yield, under proper microscopic and other examination, some important further information, although, as I shall point out, its pedigree is not good enough to justify precise conclusions.

Information, annough, as I shar point out, its pengree is not good chough to justify precise conclusions. The photographic illustration plainly shows, I think, the laminated structure due to rolling. Whatever crystallization there is, is clearly subordinate to that general structure, and therefore it ay have existed always, as it existed at the time of fracture, together with the lamination.

together with the lamination. The statement of the committee is, that this is "an unmistakable instance of crystallization," but the opinion as to its cause is much hore cautiously stated as merely "probable." And the degree of this probability is carefully indicated by a statement of all the data upon which the committee's opinion is based. The facts personally known to the committee, or verifable by it beyond reasonable doubt, are, that the piece had been in service for forty years; that it had been frequently under stress up to 250,000 pounds; and that it broke under less than 200,000 pounds. A fact presumably less certainly established, is that it was carefully made, about 1538, by hammering from the best wrought-iron scrap. The committee infers that "it is not probable that any section of it, if broken when first made, would have displayed crystalline structure." And this is the only reason for supposing that such a structure has been since induced. In weighing the force of this conclusion, it must be remembered first that

crystalline structure." And this is the only reason for supposing that such a structure has been since induced. In weighing the force of this conclusion, it must be remembered, first, that wrought-iron has a crystalline structure to begin with, and that this structure can be made clearly visible by cold fracture produced in a certain way; so that, in fact, what the committee means is, that it is not probable that the piece of iron in question, if broken by continued increasing tension, when it was first made, would have failed to show the fibrous fracture due to the elongation of the crystals under such tension. Such an elongation in mass implies that the adhesions of the individual grains in mass is sufficient to resist, for a time, their separation in mass. That a sudden shock or strain might produce separation with little or no elongation is to be expected accord-ing to familiar mechanical principles. Again, the illustration given by the committee represents a fracture *under impact*, which would have been likely to be crystalline in any event. But, considering the character of the observers, we may safely accept their assurance that this fracture presented the same appearance as that produced by tension. The committee's state-ment, then, is substantially that, after forty years of service, a piece of iron, broken by tensile strain smaller than that which it had previously endured without breaking, showed a tension-fracture exactly like its impact-fracture, whereas, if broken when first made, the tension-fracture would *probably* have been more fibrous. Even this *probably* is open to somewhat damaging inquiry. For the committee does not say, and evidently does not know, what heat-treatment this piece of iron received when it was forged forty years before, or whether, during these forty years, it was ever heated, straightened, annealed, or otherwise subjected to heat-treatment. Yet such treatment, as is well-known, might induce a crystalline structure both coarser and less firmly ce

"Now I find nothing here which indicates strongly that any change in crystalli-zation occurs under vibration or shock. The cases of the Washington testing-machine and of the Morgan Iron Works porter-bar may well be due to over-heating under manufacture.

We have, then, as equally "probable," the hypothesis that the crystalline struc-ture, ultimately exhibited upon fracture, had existed in the iron ever since its last heat-treatment; (24) and the only remaining question is, why should the iron break under a smaller strain than it had previously sustained without breaking? The answer to this question is given by Wohler's experiments, and may be summed up in popular phraseology by the statement that repeated stresses, no one of which is sufficient to produce fracture in mass, may, when they individually surpass the limit of elasticity of the weakest elements of the mass, gradually loosen (not transform) the existing structure, and thus by their comulative effect, ultimately pro-duce visible mass-rupture. This is a fact; and it offers a sufficient explanation of all

transform) the existing structure, and thus by their comulative effect, ultimately pro-duce visible mass-rupture. This is a fact ; and it offers a sufficient explanation of all the facts thus far observed with scientific precision. The theory which it suggests may be, either that the looscuing of structure is gradual and uniform, so that, at a given moment during the process, the cohesion of all the granular or crystalline elements under strain which has been equally dimished; or that it is progressive, like the breaking of a wire-cable, wire by wire, so that the final visable mass-fracture is simply the cumulative result of incipient fractures, or minute separations of structural units, which have left fewer and fewer coherent units to endure strain. To my mind, the appearance of all tension-fractures, indicating, as-it does, that the strain upon the mass is not equally sustained by all parts of the section of fracture (*i.e.*, that some parts elongate more than others before breaking), favors the second of these theories, which is, moreover, made plausible by what we now know concorning the unequal internal strains produced (especially by heat-treat-ment) in manufacture. But it is not necessary to maintain either theory. The true ex-planation of the phenomenon may involve them both : and neither the phenomenon nor its theoretical explanation involves any process of re-crystalliation under shock at ordinary temperatures. ordinary temperatures.

Under careful analysis, therefore, the instance presented by the U. S. Board (which is, in my judgment, the strongest that Mr. Argall has adduced) amounts only to a guarded opinion, based upon an incomplete statement of facts, which permits a different explanation.

The declarations of Fairbairn and Greenwood, quoted by Mr. Argall, are simply reiterations of the traditional belier, unsupported by fresh experiment. Like many similar passages in the text-books, they have merely the force of the earlier opinions of which they are echoes.

Rankin's statement that "iron ought to be as little as possible exposed to sharp blows-and rattling vibrations," is not only consistent with the theory of breakage without "crystallization," but immediately follows the intimation of Rankin's doubt of the earlier theory, and a report of experiments made by him on railway axles, which do not confirm the notion of crystallization by vibration.

The only question here at issue is, does the vibration. The only question here at issue is, does the vibration to which stamp-stems are subjected in practice, change the structure of the iron of which they are composed? It is not, "Do stamp-stems break after condinued use?" Nor is it, "Do they show a granular fracture when they break?" A thousand instances of such breakage and fracture will prove nothing. But any one of the following suggested tests would prove a good deal. I.—Let a stamp stem which has been running a long time without breaking be-