MINING.

FAULTING IN VEINS.

Written for the Engineering and Mining Journal by S. F. Emmons.

In your issue of April 9th, Mr. A. Williams, Jr., assumes that fissure veins have a more regular course along their dip than along their strike, and explains the assumed greater irregularity as arising from the fact that the greater number of veins are fault crevices, assuming again that faults are generally up-and-down movements, and that their planes are therefore more regular in an up-and-down direction.

In your issue of April 30th, Mr. Church criticises Mr. Williams' views, substantially, 1st, by denying the first assumption that dip is more regular than strike, and 2nd, by expressing a doubt as to the correctness of his statement that veins are generally fault-fissures, and his disbelief that smooth walls, clays and slickensides are necessary evidences of faulting.

Mr. Williams' first assumption is, as far as I know, not supported by any actual statistics, hence each person's opinions on the subject would be simply the reflex of his individual experience and more or less careful habit of observation. Personally I should be at a great loss to say whether I have found the plane of veins generally more irregular along the dip or along the strike, but I can see a very good reason why irregularities on the strike should appear more prominently to one going through a mine or examining its maps than those on the dip, in that the main drifts or levels are always driven along the strike. In so far I agree with Mr. Church, but while not subscribing to Mr. Williams' statement "that faulting means a movement in an up-and-down direction more than in any other," I entirely disagree with Mr. Church's views, or rather doubts, as to the necessary connection of veins with faulting. In my own experience I have not yet seen a vein which was not originally a rock fracture on which there had been some displacement—in other words, a fault plane. The movement of displacement may have been very slight, and in many cases the evidence of movement that one first looks for, *viz.*, striated surfaces, may be wanting, for all rock materials do not preserve this evidence. But there are many other evidences of fault movement, the principal of which are broken-off and dragged-in fragments of the wall rock, and a sheeting of the country rock parallel to the principal plane of fracture. In my idea a certain slight movement of the walls upon each other is necessary to fully break the cohesion between them and to establish such water channels as would permit of comparative freedom of circulation, and hence tend to concentrate the percolating solutions from the surrounding rocks, and thereby induce a deposition of their contents in and along its walls, which is the ordinary process of deposition.

I am surprised to hear a man of Mr. Church's knowledge and experience say that "no one has given the least proof that the slow movement, which rocks are supposed to have, could produce a polish." Nature so abounds in such proofs that geological experiments, which involve costly apparatus, have not, as far as I know, been applied to demonstrating so self-evident a fact. The striated surfaces produced by the slow movement of the great ice sheet may be seen over half our continent. Whether such surfaces take a polish depends primarily on the character of the rock; this and other conditions influence the preservation, but instances in nature are sufficiently abundant to show that under great pressure a slow and regular movement may produce an even finer polish than Mr. Church's bootblack. The polish is, however, only an incidental, not an essential, part of the phenomenon—it is the striation or scratching which furnishes the evidence of movement and *pressure, for pressure is also necessary, and it must be borne in mind that* phenomena of this kind which we now observe in mines were originally produced at great depths below the surface many times that at which they are now found, hence under pressure greater than it is practical to reproduce experimentally. If some of our 14-story buildings were to be shifted an inch or two on their foundations by the undulatory movement of an earthquake, some striations would doubtless be found on the foundation rocks along the plane of movement. Under the pressure of a weight nearly a hundred-fold greater than that of such buildings, it is easy to conceive that a movement, however slight, within the rock masses where veins were formed would produce molecular deformation and striated surfaces. The instances in nature are so abundant where there is direct evidence that these phenomena are the result of movement and pressure, that it is perfectly justifiable to reverse the reasoning and consider them a proof of movement and pressure, even when it may not be possible to find the direct evidence of movement in discrepancy of structure lines, etc., for there is no other cause known to geological science which will produce them. If, as he seems to imply, Mr. Church is cognizant of another demonstrable cause, he should make it known.

The weak points in the reasoning both of Mr. Williams and Mr. Church appear to be the result of an insufficient knowledge, either theoretical or practical, of what might be called the mechanics of faulting.

While it is true that instances are found of repeated movement along the *same general fault* plane, they are not so frequent as to justify their being qualified as recurrent, still less of being compared to methods of artificial polishing of stones. Repeated movement on the same plane is much less frequent than has been generally supposed, for many of the effects which have been taken as evidence of it may be explained as the result of a torsional strain, as first experimentally demonstrated by Daubrée (Géologie Expérimentale, Dunod, Paris, 1879, pp. 279–384.) For instance, cross-courses which even appear to throw a vein may have been produced contemporaneously with the main fracture, as the result of a torsional pressure. Among the great faults or dislocations, those in stratified rocks, whose planes cross the strata nearly at right angles, are the most readily observed, and probably the most common. The movement on such planes can only be measured by the discrepancy between given bedding planes on either side,