pieces tested being some tenths of a grain. Finally, I com bined six specimens, aggregating 8·7 grains." The mean of two different experiments gave (1st) 0 5935; (2d) 0·5956. "It is hardly necessary to insist," adds the author, "upon the extreme importance which attaches to the confirmation of the views of M. Mendeleef concerning the density of the new element."

This, however, is by no means all. Seven years ago, M. Mendeleef said, eka aluminum will have an oxide of the form El_2O_3." The oxide of gallium is Ga_2O_3. "It will be almost fixed, and will melt at a very low temperature This answers exactly to gallium, which melts at 86°. He said, further, that the future element, volatile and taking its place between indium and aluminium, would be discovered by spectral analysis, and so gallium was discovered.

We may agree with *La Nature*, whose editor, M. Tissandier, discussing this same subject, holds that Mendeleef's prediction abstracts nothing from De Boisbaudran's merit as the original discoverer. The French chemist attained his result in no fortuitous manner. He also foresaw the existence of gallium, and he isolated it only after ten years of persevering labor. He compared the spectra of different metals minutely, and thus was led to suspect the intermediate element between aluminum and indium.

The analogy between Mendeleef's discovery in chemistry and that of Leverrier in astronomy is most striking. Leverrier, from the perturbations of Uranus, deduced a hypothetical planet by purely theoretical considerations, treated it as if it were a real world, and then verified his calculations and theories by his magnificent discovery. Mendeleef likewise, by considerations as purely theoretical, conceived a hypothetical element. Had Adams, who discovered Uranus almost at the same time as Leverrier, worked from that astronomer's calculations, the analogy would be without a flaw, for he would then stand as De Boisbaudran now does toward Mendeleef. As it is, the discovery seems to open as wide an horizon in theoretical chemistry as did Leverrier's achievement in theoretical astronomy.

PANICS IN SCHOOL HOUSES.

It seems to us that remedial measures are needed to prevent the occurrence of the panics which, on the breaking out of a fire, real or imaginary, always occur in crowded schools, or at least to obviate the dangers incident to the headlong rush which takes place when the tumult overpowers the means of prevention. Several such scenes of confusion have lately been witnessed in this vicinity, and they are becoming sufficiently frequent to render parents unwilling to permit young and feeble children to attend the crowded public schools. A panic occurred the other day in a large school room, because a steampipe, leaking, discharged into the apartment a cloud of steam, which the children supposed was smoke from a fire. Another was just avoided through the scholars being at recess, when a genuine fire broke out near the recitation rooms, and on one of the stairways which formed a means of egress.

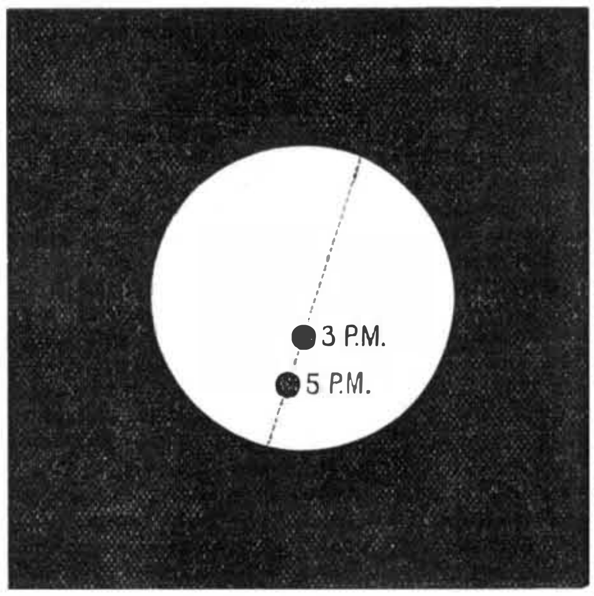
The prevention of disastrous confusion demands the greatest care, especially from those who construct school buildings and those who are responsible for their management. That such care is not exercised, we are persuaded from the frequency with which panics occur. Had the steam pipes been in proper order, or inflammable materials not existed in the school houses, neither of the above examples would have happened; and so, in every instance, some provoking cause can generally be found, which is attributable to a lack of proper vigilance or the absence of proper precautions. School houses should be fireproof and contain no material likely to feed flames. Even the probability of spontaneous combustion should be considered, and no dry or pulverulent material should be allowed to accumulate upon or around the steam heating apparatus. So carefully should risks be avoided that, while considering plans for new structures, or the introduction of new appliances into old buildings, the question whether there is anything in the schemes or projects proposed, which by any possibility might determine conditions sufficient to cause a panic, should be fully weighed. The case is one in which the ounce of prevention is worth a great many pounds of cure, although in the latter respect much can be done in providing ample modes of exit. If some rigid system of inspection of all school buildings, to be made by men thoroughly conversant with all the causes of schoolhouse panics—the principal of which, of course, is fire—were enforced, we probably should hear much less of children killed and injured through the efforts of a frightened crowd to escape.

VULCAN AGAIN.

Still another correspondent, as the subjoined letter shows, informs us that he saw Vulcan, or something on the sun which may answer to the description of that fugitive planet. This time the date is October 24, which is within Leverrier's first predicted period when observations should be made; and singularly enough the hour, 3 P. M., coincides with that of the reported observations of our other correspondents. The motion of the body, according to Mr. Wright, is different from that hitherto reported, as hitherto the spot has appeared moving upon the sun's disk, while now it is traveling off. The observation, in any event, is especially interesting, because it is the only one made since Leverrier's announcements in which anything resembling Vulcan has been seen; and moreover, it is also the only one of late date where the observer was not "taken by surprise," and where he adopted the best measures possible under the circumstances to verify the discovery. The following is the report:

To the Editor of the Scientific American:

I was reading last night in the SCIENTIFIC AMERICAN about the transit of Vulcan, and was so interested that I decided to make search myself during these clear, unclouded days. So this afternoon at 3 o'clock I got out my tube (4 inch lens); and at the very first focus the transit stood before me just as distinctly as it is printed in your paper (page 257). I was so astonished and delighted that I had to look and look again to satisfy my wondering eyes. Then I called my family, and they all saw it as plainly as I had done. Having satisfied myself by a half hour's observation that it had motion, I at once telegraphed to Professor Davidson (of the U.S. coast survey) at San Francisco: "Transit of Vulcan this afternoon; look for it." Then I went to the photograph rooms and used every effort to get a negative of it; but as we had no appliances, no facilities, and no knowledge how to take such a negative, it is not surprising that the one plate exposed is not of much value. The transit, however, was seen by four persons; others I did not summon, as I was more anxious to get a photograph. Had it been earlier in the day, I would have telegraphed to some eastern observers, but at the time I saw it first (3 P. M.) the sun was already set to eastern people; but I did the best I could by telegraphing to San Francisco, and then trying to get a photograph.



The apparent path of the planet, as near as my observations show, for the two hours' time before sunset, was as indicated on this diagram; and the time occupied in its transit I judge to be from about sunrise till 9 in the evening, about fifteen hours. In this diagram, I have drawn Vulcan's appearance too large: the relative size is perfectly shown in your diagram, page 257, this current month. W. G. WRIGHT.

San Bernardino, Cal., October 24, 1876.

If we might pin our faith to M. Leverrier's recent utterances, and assume, as we stated last week, that the supposed planet rotates about the sun once in 33 days, an even number of such periods from July 23, the date of other reported observations, would bring us to October 30, or within a week of Mr. Wright's observation. But M. Leverrier's views on the subject appear at present to be in a transition state, and our French mails each week bring us new statements from him, which of late have invariably failed to accord; in fact they often wholly differ from those enuciated seven days before. The reader will therefore understand that the data we now give, as well as those which we have presented, represent merely stages of progress in M Leverrier's investigation, through which we are endeavoring to follow him. The latest dictum of the eminent astronomer is more logical than some previous announcements, but at the same time seems to contradict flatly his previous results. In lieu of Vulcan swinging in a regular orbit in equal periods about the sun, we are now told that its orbit is highly eccentric, and that the planet behaves like Venus, making two transits within a few years, and then not repeating the passage for a century. This, of course, puts a stop to any such off-hand calculation of future transits as is above referred to.

M. Leverrier's reasoning whereby he reaches this conclusion is very interesting. He starts with the idea of finding a formula which will enable him to predict the Vulcanian transit, and to do this he makes use of Mercury, the theory of the motions of which planet, as is well known, is complete. Taking five good observations of Mercurial transits, dated 1789, 1802, 1832, and 1845, he determines this expression for the Mercurial orbit: $V = 56{\cdot}04° + 4{\cdot}092307°\, j - 7{\cdot}66°$ sin. $v - 9{\cdot}18°$ cos. v, in which j is counted from 1875. From this he calculated the next Mercurial transit, which he found would fall on November 9, 1848. Now this is exactly the date when a transit of Mercury did occur, and it was observed by Hind in London. In other words, had Mercury never before been seen, it would then have been discovered through the calculations.

M. Leverrier applies this method to Vulcan; and assuming the data of previous observers to be correct, he reaches the formula $V = 139{\cdot}94° + 216{\cdot}18°\, k + (10{\cdot}901252° - 1{\cdot}972472°\, k)j$, in which k is unknown, but the values of which are necessarily whole numbers. It is to be noted first that, if the solutions differ in the majority of points on the orbit, they coincide at the node, and this circumstance renders the problem much simpler. Besides, the variation of which k is capable is confined within very narrow limits.

With $k = 0$, the distance to the sun is 0·201, or one fifth of the earth's distance. The elongation is then 10°, that is to say, Vulcan is always so near to the photosphere that it is easy to understand why the planet is so rarely visible. With $k = 1$ almost the same results are obtained. The distance is not more than 0·181, and the rarity of observations is still better justified. But if $k = 2$, the rotation of the star must take place in 24 days, or in a less period than that in which the sun revolves on its axis; and consequently this solution is inadmissible, unless Laplace's cosmogonic hypothesis is rejected. Inversely, if $k = 1$ (or -1) in the above equation, the elongation becomes so great that for this reason the planet could not often be observed.

Now there has always been noted, in the transits of a single planet, periods of frequence and rarity. Venus, as we before stated, crosses twice in ten years, and then a century elapses before another transit occurs. The same is true of Mercury, and, M. Leverrier says, also of Vulcan. The period of the latter planet, he states, is $7\frac{1}{4}$ years, and there should be a transit on March 22, 1877, and not another until 1883. He advises that even the passage next year is not certain, the calculations showing that the trajectory of the planet will be sensibly tangent to the sun's edge; and besides, they do not determine its position with accuracy. But he counsels careful observations on the day noted.

Meanwhile there will be a chance for spectroscopists, as passages will occur frequently in the coronal region. These M. Janssen has already begun to search for.

IRREGULARITY OF THE EARTH'S MOTION.

Professor Simon Newcomb, of the Washington Observatory, is to be credited with a new astronomical discovery, which bids fair to be of some importance. He has found that our planet, instead of rotating regularly about the sun, is pursuing an apparently irregular motion, sometimes running ahead of, sometimes falling behind, the time based upon its own movement at any given period. The consequence is that the motion of the earth becomes no longer an absolutely exact standard for time measurement; and thus our reliance on our globe, already impaired since it has been demonstrated that there is no such thing as *terra firma*, and that its surface is constantly changing, is again weakened, and in a new direction. It is safe to believe that, now the discovery is in the hands of the astronomers, we may look for remarkable deductions

Professor Proctor, who has recently been discussing it, says that for about half a century there has been a doubt among astronomers as to the steadiness of the earth, and that Sir William Herschel suggested the possibility that, if a careful comparison were instituted between the turning motion of the earth and that of other planets, minute changes might be recognized. Accordingly he undertook the study of Mars, and measured the Martial day to a tenth of a second in a day; but this was of no use in testing the errors of our terrestrial time piece, where the same errors have to be measured by hundredths of a second in a year. Besides in Herschel's time the doubt on the earth's motion had been raised by Halley's recognition of the moon's apparent hastening; and this suggested little, because the lunar movements had never been closely analyzed, and the lunar hastening, as it was, indicated too small a change for Herschel to measure by his standards. Still this vague doubt was deemed of sufficient importance to cause Laplace to investigate it; and he showed that, among the various circumstances which affect the moon, there is one whose effect, at present and for many centuries to come, will hasten her motion. Then calculating the amount of such hastening, he concluded that it exactly corresponded with the hastening actually observed. "Perhaps there is not, in the whole history of Science," says Professor Proctor, "a more remarkable circumstance than this seemingly exact solution of a most difficult problem, where in reality the solution was incorrect." There was no forced agreement of figures; the work was placed in all its detail before the scientific world; mathematicians and astronomers recomputed it, and all agreed in its accuracy.

About a quarter of a century after Laplace's death, Adams (the co-discoverer, with Leverrier, of Neptune) re-examined the reasoning and found a flaw. Laplace judged a certain effect might be neglected. Adams thought not, and tested the matter; and then it appeared that it exercised so important an influence that, when due correction was made in Laplace's work, only one half the hastening was accounted for. Then arose a storm in the astronomical world. Leverrier, with all his acumen, failed at first to perceive the nature of the correction, and declared Adams to be mistaken. Pontécoulant sneered at it as "analytical legerdemain;" but the English mathematicians first accepted Adams' result, and then, after Delaunay had verified it, the continental astronomers followed. Delaunay not only admitted a retardation of the earth's motion, but pointed out where and how the same might be affected, namely, by the friction of the great tidal wave, which travels round in a direction opposed to the earth's rotation. This view has been generally accepted; and it can be shown that, if a clock could be made to go at a rate corresponding precisely to the earth's rotation, as indicated now, for 100 years, at the end of that time the earth would be found to have lost 22 seconds.

Now comes in Newcomb's discovery to show that the earth (judging from the moon's movements) undergoes irregular changes. It lost seven seconds between 1850 and 1862, and then, turning too fast between 1862 and 1874, gained eight seconds. Meanwhile smaller changes, some in one direction, others in the other, have taken place, generally lasting about four weeks at a time.

Two theories are suggested to account for these movements, either that the earth's motion is nominally irregular, or that some unseen body passes near enough to the moon to disturb her motion around the earth. Professor Newcomb adheres to the first hypothesis.

UP to the hour of going to press, the list of patents issued during the week ending October 17, and bearing that date, had not arrived from Washington.

SHELL lime, which contains considerable phosphorus, is superior to stone lime for agricultural purposes.