Bardwell, State Normal School, Cortland; Dr. Charles W. Dodge, University of Rochester; Principal Thomas B. Lovell, High School, Niagara Falls; Professor, W. C. Peckham, Adelphi College, Brooklyn; Professor J. McKeen Cattell, Columbia University, New York; Professor John F. Woodhull, Teachers College, New York; Professor E. R. Whitney, High School, Binghamton.

FRANKLIN W. BARROWS.

SCIENTIFIC BOOKS.

Urkunden zur Geschichte der nichteuklidischen Geometrie. Von F. ENGEL und P. STAECKEL. *I. Nikolai Ivanovitsch Lobatschefski.* Leipzig, B. G. Teubner. 1899. 8vo. Pp. 476.

The name of Lobachévski is inseparably connected with a scientific advance so fundamental as actually to have changed the accepted conception of the universe.

Yet his first published work and his greatest work have both remained for over sixty years inaccessible, locked up in Russian, and are now for the first time given to the world in this monumental volume by Professor Engel.

As to the precise time at which Lobachévski shook himself free from Euclid's two thousand years of authority there is still room for a most interesting doubt.

The first of the two treatises given in this book, 'On the Elements of Geometry,' was published in 1829, with this note at the foot of the first page:

"Extracted by the author himself from a paper which he read February 12, 1826, in the meeting of the Section for Physico-mathematic Sciences, with the title: 'Exposition succincte des principes de la Géométrie, etc.'"

Again, when the four equations are reached which really contain the essence of the non-Euclidean geometry, Lobachévski subjoins this note: "The equations (17) and all that follows these the author had already appended to the paper which he presented in 1826 to the Section for Physico-mathematic Sciences."

In the introduction to the second of the two treatises here given, the 'New Elements of Geometry,' the author says: "Everyone knows that in geometry the theory of parallels has remained, even to the present day, incomplete.

"The futility of the efforts which have been made since Euclid's time during the lapse of two thousand years to perfect it awoke in me the suspicion that the ideas employed might not contain the truth sought to be demonstrated, and for whose verification, as with other natural laws, only experiments could serve, as, for example, astronomic observations.

"When, finally, I had convinced myself of the correctness of my supposition, and believed myself to have completely solved the difficult question, I wrote a paper on it in the year 1826, 'Exposition succincte des principes de la Géométrie, *avec une démonstration rigoureuse du théorème des parallèles*,' read February 12, 1826, in the séance of the physico-mathematic Faculty of the University of Kazan, but never printed." No part of this French manuscript has ever been found. The latter half of the title is ominous.

For centuries the world had been deluged with rigorous demonstrations of the theorem of parallels. We know that three years later Lobachévski himself proved it absolutely indemonstrable.

Yet the paper said to contain material to stop forever this twenty-centuries-old striving still was headed 'démonstration rigoureuse,' just as Saccheri's book of 1733 containing a coherent treatise on non-Euclidean geometry ended by one more pitiful proof of the parallel-postulate.

If Saccheri had lived three years longer and realized the pearl in his net, with the new meaning, he could have retained his old title: 'Euclides ab omni naevo vindicatus,' since the non-Euclidean geometry is a perfect vindication and explanation of Euclid. But Lobachévski's title is made wholly indefensible.

A new geometry, founded on the contradictory opposite of the theorem of parallels, and so proving every demonstration of that theorem fallacious, could not very well pose under Lobachévski's old title. Least said, soonest mended. He never tells what he meant by it, never tries to explain it.

Yet Engel thinks that under this two thousand years stale title, 'avec une démonstration rigoureuse du théorème des parallèles,' "Lobatschefskij sprach es klipp und klar aus, dass das Euklidische Parallelenaxiom niemals werde bewiesen werden können, weil es unbeweisbar sei."

At the International Mathematical Congress, 1893, I maintained in his presence that Felix Klein was utterly in error where in his 'Nicht-Euklidische Geometrie,' I., p. 174, he says of the letter from Gauss to Bolyai Farkas, 1799, "In this last letter is particularly said that in the hyperbolic geometry there is a maximum for triangle-area;" and again where he says, p. 175, "There can be no doubt that Lobachévski as well as Bolyai owe to Gauss's prompting the initiative of their researches."

Klein's only answer was that his position would be sustained when the public got access to Gauss's correspondence.

Staeckel and Engel have now had complete access to these papers, and this is what Engel says, pp. 428–9: "But at all events in Gauss's letters there is nowhere a support for this tradition; at no point of these letters can be found even the slightest intimation that Gauss connected the discoveries of Lobachévski and J. Bolyai with any direct or roundabout prompting from him.

"On the contrary the letters show (see p. 432 f. and Math. Ann. 49, p. 162, Briefwechsel G. B., p. 109) that Gauss throughout recognized the independence of both, exactly as he recognized that of Schweikart, whose independence of Gauss is subject to no doubt.

"With Staeckel I am at one herein that exactly this circumstance is particularly weighty for the decision of the whole question."

The whole scientific world will breathe a sigh of relief that Klein's ungenerous Göttingen legend, mortally wounded in 1893, is in 1899 annihilated forever.

More inexplicable is Klein's bald misinterpretation of Gauss's letter of 1799 to Bolyai Farkas. I gave this letter in my Bolyai as demonstrative evidence that in 1799 Gauss was still trying to prove Euclid's the only non-contradictory system of geometry, and also the system regnant in the external space of our physical experience. The first is false; the second can never be proven.

Summing up this same letter, Engel, p. 379, instead of finding in it the hypothetical white elephant of Klein's fairy tale, gives the utmost that can be attributed to it in the following sentence: "Hier ist er also ganz nahe daran, an der Richtigkeit der Geometrie, das heisst, des Euklidischen Parallelenaxioms zweifelhaft zu werden."

Five years later, in a letter of November 25, 1804, Gauss speaks of a 'group of rocks' on which his attempts had always been wrecked, and adds: "I have, indeed, still ever the hope that those rocks sometime, and, indeed, before my death, will permit a passage. Meanwhile I have now so many other affairs on hand that at present I cannot think on it, and, believe me, I shall heartily rejoice if you forestall me and if you succeed in surmounting all obstacles." "Surely," says Engel, "that does not sound as if the authority of Euclid had diminished in power since the year 1799; on the contrary, one gets the impression that Gauss in 1804 rather stood more completely under its ban than before."

This was clearly the view of Bolyai János, whose autobiography, after quoting Gauss's letter of 1832, says: "In a previous letter Gauss writes he hopes some time to be able to circumnavigate these rocks—so then he hopes!!"

"These last words," say Staeckel and Engel in the *Mathematische Annalen*, "show a certain suspicion on the part of John against Gauss." But the mention of this earlier letter was highly natural.

János had known of it from boyhood. The joy of his triumph in solving what had baffled all the world for two thousand years was intensified by his knowing that even Gauss had tried and was hoping for the impossible.

His splendid trumpet call of glory announcing his creation of a new universe, scientiam spatii absolute veram exhibens, is answered how? Gauss answers that method and results coincide with his own *meditations* instituted in part since 30–35 years. But of these meditations Gauss had published never a word! How natural then for János to refer to his previous letter, where he still was hoping to prove Euclid's parallel postulate.

The equally complete freedom of Lobachévski from the slightest idea that Gauss had ever meditated anything different from the rest of the world on the matter of parallels is demonstrated most happily.

Bartels, the teacher of Lobachévski, never saw Gauss after 1807, received at Kazan one letter from him in 1808, probably a mere friendly epistle containing nothing mathematical, and not another word during his entire stay there.

But in November, 1808, Schumacher, in Göttingen, writes in his diary that Gauss has reduced the theory of parallels to this, that if the accepted theory were not true there must be a constant *a priori* of length, 'welches absurd ist,' yet that Gauss himself considers this work not yet completed.

Thus in 1808 Gauss still vacillates. The proposition about the *a priori* given unit for length is due to Lambert, 1766, and on the supposed absurdity Legendre in 1794 had founded a pseudo-proof of the parallel postulate.

Thus until after 1808 Gauss had made no advance beyond the ordinary text books.

A most fortunate piece of personal testimony from the distinguished astronomer Otto Struve finishes the whole matter.

When at Dorpat in 1835 and 1836 Struve was attending his lectures, Bartels repeatedly spoke of Lobachévski as one of his first and most gifted scholars in Kazan.

Lobachévski had then already sent his first works on non-Euclidean geometry to Bartels, but, as Struve writes, Bartels looked upon these works 'more as interesting, ingenious speculations than as a work advancing science.'

Struve adds he does not recall that Bartels ever spoke of any accordant ideas of Gauss.

Such misconception of the import of non-Euclidean geometry was due in part to that lack of grit or slip in judgment which let Lobachévski damn this child of his genius with the name 'Imaginary Geometry.'

If Lobachévski had possessed the magnificent Magyar mettle of Bolyai János, and dared to name his creation the Science Absolute of Space, he would not have taught mathematics with ability throughout his life without making a single disciple.

His 'New Elements of Geometry,' here at last made accessible to the world, is such a masterpiece that it remains to-day the completest and most satisfactory text-book of non-Euclidean geometry. Written at the flood of hope and confidence, with ardor still undampened, it is in his 'New Elements' preeminently that the great Russian allows free expression to his profound philosophic insight, which, on the one hand, shatters forever Kant's doctrine of our absolute *a priori* knowledge of all fundamental spatial properties, while, on the other hand, emphasizing the essential relativity of space, and the element of human construction, human creation in it.

Lobachévski's position is still, after sixty years, the necessary philosophy for science. No one has succeeded in finding any escape from its cogency. No one has gone beyond it.

Our hereditary geometry, the Euclidean, is underivable from real experience alone, and can never be proved by experience. Not only can the truth or falsity of Euclid's parallel postulate never be proved *a priori;* not even *a posteriori* can ever its *truth* be proved. Therefore, Euclidean geometry, in so far as Euclidean, must ever remain a creation of the human mind.

The introduction to the 'New Elements' contains a piercing critique of Legendre's attempts on the parallel-postulate.

Here at times Lobachévski almost condescends to be humorous. For example, he says: "Although Legendre designates his demonstration as completely rigorous, he, without doubt, thought otherwise, for he adds the proviso that a difficulty which one would perhaps still find can always be removed. For this he has recourse to calculations founded on the first familiar equations of rectilinear trigonometry, which it would be necessary previously to establish, and which just in this case are useless and lead to no result."

Here for the word *trigonometry* in the Russian of the 'Collected Works,' p. 222, Engel has substituted, p. 70, by some slip, the word *geometry*. Further on Lobachévski continues: "But Legendre has not noticed here that EF may possibly not meet AC. To overcome *this little difficulty* you have only to suppose that EF is the perpendicular from F on BD; but then how can we conclude therefrom that FE = AB and the angle EFC = $\frac{1}{2}\pi$? It is not possible to mend the false deduction, wherein Legendre's inadvertence was so gross that, without remarking this grave error, he considered his demonstration as very simple and perfectly rigorous."

Now for a specimen of Lobachévski's philosophizing: "Strictly we cognize in nature only motion, without which sense impressions are not possible. Consequently all other ideas, for example, geometric, are artificial products of our mind, since they are taken from the properties of motion; and, therefore, space in itself, for itself alone, for us does not exist.

Accordingly it can have nothing contradictory for our mind if we admit that some forces in nature follow the one, others another special geometry.

To illustrate this thought, assume, as many believe, that the attractive forces diminish because their action spreads on a sphere. In the ordinary geometry we find $4\pi r^2$ as magnitude of a sphere of radius r, whence the force must diminish in the squared ratio of the distance.

In the imaginary (sic) geometry I have found the surface of the sphere equal to

$$\pi(e^r - e^{-r})^2,$$

and possibly in such a geometry the molecular forces may follow, whose whole diversity would depend, consequently, on the number e, always very great."

How far Lobachévski was, not only from Riemann's geometry with closed finite straight line, but also from the perspective point of view where the straight is closed by having only one point at infinity, is illustrated by the following sentences of the introduction. "I consider it not necessary to analyze in detail other assumptions, too artificial or too arbitrary. Only one of them yet merits some attention—the passing over of the circle into a straight line. However, the fault is here visible beforehand in the violation of continuity, when a curve which does not cease to be closed, howsoever great it may be, transforms itself directly into the infinite straight, losing in this way an essential property.

In this regard the imaginary geometry fills in the interval much better. In it, if we increase a circle all of whose diameters come together at a point, we finally attain to a line such that its normals approach each other indefinitely, even though they can no longer cut one another. This property, however, does not appertain to the straight, but to the curve which in my paper 'On the Elements of Geometry' I have designated as *circle-limit*."

Lobachévski anticipated in 1835 all that was said not long ago in the columns of SCIENCE on the length of a curve. For example: "In fact, however little may be the parts of a curve, they do not cease to be curves; consequently they can never be measured by the aid of a straight."

"Lagrange takes as foundation the assumption of Archimedes that on a curve one can always take two points so near that the arc between them may be considered greater than its chord, but smaller than the two tangents from its extremities. Such an assumption is actually necessary, but by it is destroyed the primitive idea of measuring curves with straights. Thus the evaluation of the length of a curve represents not at all the rectification of the curvature; but it seeks a wholly different aim—the finding of a limit which the actual measure would approach the more as this measure was made the more exact. But measuring is considered more exact the smaller the links of the chain employed. This is why in geometry one must show that the sum of tangents decreases while the sum of chords increases until the two sums differ indefinitely little from the limit both approach, which geometry assumes as length of the curve."

In the splendid treatise which follows this interesting introduction Lobachévski has given a complete coherent development and exposition of the non-Euclidean geometry. Until I visited Maros-Vásárhely it was not known that Bolyai János had actually commenced and made remarkable progress in an even greater, more masterful treatment of the whole matter. From the mass of John's papers tumbled in a big chest I singled out especially a manuscript in German entitled 'Raumlehre,' and on pointing out to Professor Bedöházi János some of the striking passages in it he promised its publication.

In SCIENCE for September 24, 1897, I mentioned these treasures as 'extended researches anticipating the discoveries of Cayley and Klein.' Engel now says of them, p. 393: "J. Bolyai had also commenced to work out a great and consecutive presentation of geometry, but what he had written down remained entombed in his papers and has never been published.

"Staeckel will before long make generally accessible so much of it as is suitable for publication, and it will then appear that J. Bolyai in his exposition set to work according to principles similar to those Lobachévski actually followed." But though Lobachévski has given his complete message to the ages, yet is perceptible a touch more masterful in even the brief two dozen pages of the young Magyar.

Through a given point to draw a parallel to a given straight; to draw to one side of an acute angle the perpendicular parallel to the other side; to square the circle—these problems would be sought in vain in the two quarto volumes of Lobachévski.

Bolyai János gives solutions of them startling in their elegance. For example (Halsted's Bolyai § 34), "Through D we may Draw DM ∥ AN in the following manner: From D drop DB⊥AN; from any point A of the straight AB erect AC⊥AN (in DBA), and let fall DC⊥AC. A quadrant described from the center A in BAC, with a radius = DC, will have a point B or O in common with ray BD. In the first case the angle of parallelism manifestly is right, but in the second case it equals AOB. If, therefore, we make BDM = AOB, then DM will be ∥ BN."

About 100 pages of Engel's book are devoted to a life of Lobachévski, yet no word is said of his wife, his children, his family life, his home fortunes and misfortunes, nor is mentioned the biography by E. F. Letvenov (St. Petersburg, 1894, pp. 79) containing romantic pictures of these eternal interests.

GEORGE BRUCE HALSTED.
AUSTIN, TEXAS.

The Spirit of Organic Chemistry. An Introduction to the Current Literature of the Subject. By ARTHUR LACHMAN, B.S., PH.D., Professor of Chemistry in the University of Oregon. With an Introduction by PAUL C. FREER, M.D., PH.D., Professor of General Chemistry in the University of Michigan. New York, The Macmillan Company. 1899. Pp. xviii +229. Price, $1.50.

Under the above title an historical account of the development of some of the most important chapters is given. The subjects selected are among those which have exercised the minds and skill of the greatest chemists, and which are to-day before the chemical world. Problems which have been solved in a single masterly research are omitted. In the nine chapters the following subjects are treated: The constitution of rosaniline, Perkins's reaction, the constitution of benzene, the constitution of aceto-acetic ether, the uric-acid group, the constitution of the sugars, the isomerism of fumaric and maleïc acids, the isomerism of the oximes, and the constitution of the diazo compounds.

The author has used excellent judgment in condensing the literature, and has presented the subject in a logical and clear manner. The account is brought up to date, even the most recent work receiving brief mention. The book is, therefore, an introduction to the chemical literature of to-day. On this account it is of special value to the student who has just mastered the text-books of organic chemistry and who desires to go farther. The mass of literature which is summed up in but 225 pages is so great and complex that it is doubtful whether the student would have the time and energy to get as clear a conception of the subject by searching through the journals as he can get by a careful study of this book. After mastering it he would be in a position to follow a paper on any of the subjects treated.

The literature of organic chemistry is so vast that there is room for such critical reviews, for, it seems to the writer, they tend to inspire rather than prevent reading. Professor Lachman's book will make the reading of the current journals easier and is, therefore, helpful. It is a contribution to chemical history and supplements Schlorlemmer's well-known "Rise and Development of Organic Chemistry."

JAMES F. NORRIS.
MASSACHUSETTS INSTITUTE
OF TECHNOLOGY.