1855. He entered the École Normale in 1873 and obtained his doctorate in 1876. His first published work relates to projective geometry and shows the influence of his teacher, Chasles. His dissertation falls into this period. But beginning with 1877 he, like Picard, devoted himself almost exclusively to the field of analysis. After a brief experience in the provinces, he was recalled to Paris in 1881, where he has remained. He was elected to the Academy of Sciences in 1892. The list of his publications contains 306 titles.

J. W. YOUNG.

Vermeintliche Beweise des Fermatschen Satzes. Besprochen von A. FLECK, PH. MAENNSCHEN, O. PERRON. *Archiv der Mathematik und Physik*, Bände 14–18.

Über das letzte Fermatsche Theorem. Von BENNO LIND. *Abhandlungen zur Geschichte der mathematischen Wissenschaften*, Heft XXVI_2, pp. 21–65, 1910.

THE age of circle squarers had hardly come to an end (one still meets sporadic cases here and there) when a new period, the age of the "Fermatists," arose. This genus suddenly received a tremendous boom through the "Wolfskehlsche Preisstiftung" by which a prize of 100,000 Marks is offered to him who first proves the great theorem of Fermat. And in its wake there have come a host who do not have the least ambition to add anything to mathematical knowledge but merely lust after the prize money. Their number will doubtlessly reach many thousands within the 99 years for which the prize is established.

The editors of the *Archiv* have opened their pages to a discussion of the proofs that might be submitted; and it is the pleasant (?) duty of Messrs. Fleck, Maennschen, and Perron to peruse these and point out to each writer the errors which lurk in his work. Of the 111 attempts at a proof that are discussed to date in the *Archiv*, none is successful and not one adds anything which may be of value in bringing the solution of the problem nearer completion. Fermat stated in a marginal note that *the equation*

$$x^n + y^n = z^n \tag{1}$$

has no solution in integers, for all values of n which are greater than 2, and that he possessed a most wonderful proof of this theorem; but his proof, if such existed, has never been discovered. Each of these latter-day Fermatists believes that he has found a like remarkable proof, and is ready to grasp the prize, only to have his hopes shattered by an exposure of his errors. Some of them, nothing daunted, have made a second and even a third attempt, only to fail each time. What, besides the prize; has driven these engineers, teachers, captains of artillery, building inspectors, doctors, book-keepers, preachers, apothecaries, judges, merchants, cabinet ministers, and representatives of many other vocations and of many nationalities, to waste time, paper, and ink on the worthless stuff they have turned out? The reviewer almost believes it is because they consider the problem as a sort of challenge to their intelligence — it is stated so clearly and in such few words — it sounds so familiar — just a little extension of the Pythagorean theorem — and it asks for a solution in integers, the kind of numbers we played with in our childhood days. Carried away by these impulses, they forget that such mathematicians as Abel, Cauchy, Euler, Legendre, Kummer, and a score of others have attempted the problem before them; they do not consider that they lack the subtlety of mind, the "Scharfsinn" which the subject requires; they do not even think it worth while to find out what progress has been made along this line by previous writers, but each one starts afresh and attempts, by mighty path-breaking methods, to arrive at the goal within the compass of 50 pages or of 2 pages or even on a post-card.

And wherein do these Fermatists fail — what is the nature of their errors? Disregarding completely such puerile algebraic blunders as are usually attributed to our college freshmen, almost all the errors may be classified under one of the following types: 1) Assumption of the converse of a theorem where only the direct theorem has been proved; 2) omission of steps in the proof of a theorem whose proof in turn is either impossible or as difficult as the original theorem; 3) assumption that because certain expressions satisfy an equation, these are necessarily the only ones that do so; 4) reasoning in a circle; 5) drawing conclusions for general values from theorems which have only been proven for a particular value; 6) confusion of algebraic and numerical factoring; 7) ignorance of simple factor theorems or divisibility of numbers; 8) errors in elementary theory of congruences; 9) assumption of particular forms of the unknown quantities; 10) assumption without proof that a certain complicated expression of fractional form which has been set up must be an integer. These types of errors are about equally numerous — the first five are errors in logic, the last five are more closely related to the theory of numbers. May the *Archiv* continue its good work in the exposure of these pseudo-proofs. Let us hope that the editors will not grow tired, within the next 96 years, of their self-imposed task, which does have its compensations on the humorous side if on no other.

Lind, in his monograph, does not attempt to prove Fermat's theorem. It is his aim to give a complete algebraic survey in simplified form of everything that has been done by elementary methods towards a solution of the problem, to point out the directions which might lead to further developments, and to give a historical review of the problem since the time of Fermat. Though not complete, still his collection of the material from so many varied sources and its presentation as a whole is very noteworthy. His work is divided into four parts. Part 1 gives the algebraic analysis of the problem by elementary methods. It contains a host of theorems, with their proof, which have been deduced concerning the relations that must exist between x, y, z, and n if the equation (1) is to hold. We first have the well-known Abel equations expressing x, y, z and the three very important functions of the form

$$\frac{x^n + y^n}{x + y}, \quad \frac{z^n - y^n}{z - y}, \quad \frac{z^n - x^n}{z - x}$$

in terms of the nth powers of 6 quantities $a, b, c, \alpha, \beta, \gamma$. These relations are the starting point of all the later investigations.

Lind includes some original work in this part and in part 4, and here he comes to grief. Fleck discusses this portion of the monograph in the *Archiv*, volume 16, 1st Heft (1910), and in reply to Lind's objections to some of Fleck's criticisms, the latter goes into more detail in volume 18, 1st Heft (1911). And so we have another exhibit in the *Archiv's* gallery of exposures. Several of the theorems which Lind gives as his own in part 1 are false in themselves or their proofs are false. In part 4, he attempts to make a most important addition to the development of the problem, viz., that the equation (1) has no solution in integers x, y, z prime to n, and that in case

$n = 6m + 1$ one of these must be divisible by $3n^2$. Unfortunately, he bases his proof upon two of his theorems dealing only with the most elementary theory of congruences (in particular, if $x \equiv a \pmod 3$, then $x^n \equiv a^n \pmod{3^2}$ for all odd primes n), which Fleck points out are both false, and thus Lind's original work is mostly worthless.

Part 2 contains a bibliography of the subject, citing 183 articles. Part 3 is the historical review. The first attempt to treat Fermat's theorem in general is due to Claude Jaquemets (1651–1729), but he did not advance very far. The very important Abel equations were first set up by Barlow (1811), and then independently by Abel, Kummer, Legendre, Lindemann, F. Lucas, and Stäckel. These men also established a large number of other formulas and theorems. One of the most important theorems that Abel stated without proof was, that if equation (1) is to have integral solutions, then none of the quantities x, y, z, $x + y$, $z - y$, etc., can be a prime. In regard to this theorem, Talbot, in 1857, proved that the two larger of the unknowns cannot be primes. It was not until 1905 that Sauer showed that the smallest of the unknowns cannot be a prime. We should add here that Cauchy, Lebesgue, Liouville, and Gruenert have also arrived at very many interesting results.

As far as the proof of the impossibility of equation (1) in integers for special values of n is concerned, Euler in 1738 gave a proof for $n = 4$, and 22 years later for $n = 3$. The case $n = 5$ is an interesting one, for in 1825 Dirichlet showed that if equation (1) has solutions, then one of the unknowns must be divisible by 5, and that this unknown cannot be odd; Legendre followed this by showing (by the same method) that this unknown cannot be even, and thus completed the case for $n = 5$. In 1840 Lamé proved the case $n = 7$. Many others have given proofs covering these cases. In 1823 Legendre showed that if (1) has solutions, then for all $n < 100$ one of the unknowns must be divisible by n; but it remained for Dickson (*Quarterly Journal*, volume 40 (1908)) to prove that (1) has no integral solutions *all prime to n* for every $n < 6{,}857$, and for the larger primes $< 7{,}000$. But the year 1847 brought the richest harvest for the problem, for then Kummer, with the help of the theory of prime ideals proved that (1) has no integral solutions for *all values of n* which are odd primes and which are contained as factors in the numerators of the first $\frac{1}{2}(n - 3)$

Bernoulli numbers — this includes all odd primes < 100, with the exception of 37, 59, 67. Ten years later he extended this to another series of exponents including these three exceptional numbers, so that the Fermat theorem was proven for all values of $n > 2$ and $\leqq 100$. Kummer received the Paris prize of 1850 for his beautiful work.

In 1909, an award of 1000 Marks was made from the Wolfskehl foundation to G. Wieferich for his proof that Fermat's equation has no solutions all prime to n unless $2^{n-1} \equiv 1 \bmod n^2$. And here the problem rests.

JOSEPH LIPKE.

Solid Geometry. By H. E. SLAUGHT and N. J. LENNES. Allyn and Bacon, Boston, 1911. vi+190 pp.

THIS book follows the plane geometry of the textbook series of the authors. It is divided into seven chapters entitled lines and planes in space, prisms and cylinders, pyramids and cones, regular and similar polyhedrons, the sphere, variable geometric magnitudes, and theory of limits.

The logical phase of the development of solid geometry, as here treated, is a great improvement over that usually found in our textbooks. Many of the more fundamental principles are formally stated as axioms. The first striking example of this is Axiom III: "If two planes have a point in common, then they have at least another point in common." This fundamental theorem of three-dimensional geometry has usually been kept as obscure as possible. In all, ten axioms are thus stated.

A brief treatment of sines, cosines, and tangents, and a few theorems on the projection of lines and of areas are introduced. Some of the theorems on trihedral angles are deferred to the chapter on the sphere, where they are related to the theory of spherical triangles. Euler's theorem is stated without proof, the usual faulty proof being inserted as an exercise in which the error in the proof is to be shown. The definition of polar spherical triangles is made completely. The proof of the theorem that the shortest path between two points on a sphere is the arc of a great circle joining the points is made to depend on the concept of the length of a curve on a sphere as the limit of the sum of the lengths of small arcs of great circles — a somewhat different notion from the limit of the sum of lengths of the chords, which has been previously used in the book for the length of a curve. In the chapter on variable geometric