Some famous problems of the theory of numbers and in particular Waring's problem. An inaugural lecture delivered before the University of Oxford by G. H. Hardy. Oxford, Clarendon Press, 1920. 8vo. 34 pp.

The particular problems with which this lecture is concerned belong to the additive theory of numbers. The general problem of the latter is stated by Hardy as follows: "Suppose that n is any positive integer, and $\alpha_1, \alpha_2, \alpha_3, \cdots$ positive integers of some special kind, squares, for example, or cubes, or perfect kth powers, or primes. We consider all possible expressions of n in the form $n = \alpha_1 + \alpha_2 + \cdots + \alpha_s$, where s may be fixed or unrestricted, the α's may or may not be necessarily distinct, and order may or may not be relevant, according to the particular problem on which we are engaged. We denote by $r(n)$ the number of representations which satisfy the conditions of the problem. Then *what can we say about* $r(n)$? Can we find an exact formula for $r(n)$, or an approximate formula valid for large values of n? In particular, is $r(n)$ *always positive*? Is it always possible, that is to say, to find at least *one* representation of n of the type required? Or, if this is not so, is it at any rate always possible when n is sufficiently large?"

The number $p(n)$ of unrestricted partitions of n into positive integral summands has been studied by many authors; the principal result of the investigation of this function by Hardy and Ramanujan has been the discovery of an approximate formula for $p(n)$ which enables them to approximate to $p(n)$ with an accuracy which is almost uncanny. Of $p(200)$, for example, the value 3,972,999,029,388 is obtained with an (additive) error of .004 by employing eight terms of their series; and the result has been verified by MacMahon, without the use of their formula, by a direct computation which occupied over a month.

The principal object of the lecture is a discussion of the problem of Waring of determining the number of representations of an integer n as a sum of s positive kth powers of integers and particularly of the (more usual) restricted form of this problem in which one seeks to show that for fixed k there exists a finite s_k independent of n such that every integer n has at least one representation as a sum of s_k non-negative kth powers. In connection with this problem there are two functions of fundamental importance, whose existence has been proved in recent years; they may be defined as follows: The number $g(k)$ is defined to be the least number for which it is true that every positive integer is the sum of $g(k)$ non-negative kth powers of integers; the number $G(k)$ is defined to be the least number for which it is true that every positive integer from a certain point onwards is the sum of $G(k)$ non-negative kth powers of integers. The existence of either of the functions $G(k)$ and $g(k)$ obviously implies that of the other. The existence of $g(k)$ was first proved by Hilbert in 1909, after an interval of 139* years from the time of its enunciation by Waring,† who gave the theorem without proof.

For a long time it has been known that $g(2) = G(2) = 4$. In 1859 Liouville proved that $g(4)$ exists and does not exceed 53; it was shown by Wieferich in 1909 that $g(4) \leqq 37$, the most that is known at present. The number $79 = 4 \cdot 2^4 + 15 \cdot 1^4$ needs 19 biquadrates, and no number is known which needs more. There is still therefore a wide margin of uncertainty as to the actual value of $g(4)$. The existence of $g(3)$ was first established in 1895 when Maillet proved that $g(3) \leqq 17$; Wieferich proved in 1909 that $g(3) \leqq 9$. As 23 and 239 require exactly 9 cubes, the value of $g(3)$ is exactly 9. [Hardy remarks that it is "no doubt true" that 23 and 239 are the only integers requiring 9 cubes for their expression.] In 1909 Landau proved the "singularly beautiful theorem" that the number of integers requiring 9 cubes each for its expression is finite. It was in view of this fact that the number $G(k)$ was introduced. It is known that $4 \leqq G(3) \leqq 8$; and Hardy is

* Corrected from Hardy's "127" on page 17, in accordance with the information indicated in the next footnote.

† At the time when Hardy wrote his address he was under the impression that Waring first stated his theorem [that every positive integer is a sum of at most 4 positive squares, 9 positive cubes, 19 positive biquadrates, and so on] in the third edition (1782) of his *Meditationes Algebraïcae* [pp. 349–350], but in a letter of Jan. 4, 1921, he writes me that a correspondent has called his attention to its appearance in an earlier edition. On examining the three editions I fail to find it in the first (1762), but find it in the second (1770) [pp. 204–205], and in the third (1782), as indicated. These references are given also in Dickson's *History of the Theory of Numbers*, vol. II, pp. xviii and 717 in connection with his elaborate history of Waring's problem. [Am I right in supposing (as I have done in numbering the editions above) that Waring, when he came to publish the third edition, treated the first part (pp. 1–65) of his *Miscellanea Analytica* (1762) as the first edition of his *Meditationes Algebraicae*, its material being reproduced in the editions of 1770 and 1782; or was there another edition of the latter between those of 1770 and 1782? The view which I have taken agrees with a statement given in an old manuscript note on the fly-leaf of a copy of the 1770 edition of the *Meditationes Algebraicae* in the library of the University of Illinois.]

disposed to conjecture that $G(3)$ has the value 4 or 5, and he seems to lean towards the former (p. 23). A simple argument shows that $G(4) \geqq 16$ and Hardy and Littlewood have proved that $G(4) \leqq 21$, so that $G(4)$ lies in a comparatively small known range.

On pages 27–34 we have one of the most fascinating accounts in our literature of the fundamental idea which has guided a mathematical investigation, the account of that which directed Hardy and Littlewood in their investigation of the properties of $G(k)$. The method seems to be very powerful. It has brought them for the first time into relation with the series on which the solution in the last resort depends; it gives numerical results which, as soon as k exceeds 3, are far in advance of any known before; and it gives a definite upper bound to $G(k)$, namely,

$$G(k) \leqq (k - 2)2^{k-1} + 5.$$

[On the other side it is known that $G(k) \geqq k + 1$ and that $G(2^\alpha) \geqq 2^{\alpha+2}$ if α is a positive integer.] Hardy adds: "It is beyond question that our numbers are still very much too large; and there is no sort of finality about our researches, for which the best that we can claim is that they embody a method which opens the door for more."

Concerning Goldbach's assertion that every even number is the sum of two primes we have the following (p. 34): "Our method is applicable in principle to this problem also. We cannot solve the problem, but we can open the first serious attack upon it, and bring it into relation with the established prime number theory. The most which we can accomplish at present is as follows. We have to assume the truth of the notorious Riemann hypothesis concerning the zeros of the zeta function, and indeed in a generalized and extended form. If we do this we can prove, not Goldbach's theorem indeed, but the next best theorem of the kind, viz. that *every odd number*, at any rate from a certain point onwards, *is the sum of three odd primes*. It is an imperfect and provisional result, but it is the first serious contribution to the solution of the problem."

It is with genuine regret that the reviewer has to point out one or two historical errors in an address which is otherwise so charming. (We have already mentioned one of these.) On page 18 he refers to Fermat's "notorious assertion concerning Mersenne's numbers"; a letter to the reviewer indicates that this error probably arose through referring to Fermat a statement which was in fact made by Mersenne (and stated by W. W. Rouse Ball to be "probably due to Fermat").*

From page 18 of Hardy's lecture I quote as follows: "No very laborious computations would be necessary to lead Waring to a highly plausible speculation, which is all I take his contribution to the theory to be; and in the theory of numbers it is singularly easy to speculate, though often terribly difficult to prove; and it is only proof that counts." It is hard to see in what sense the author can say that "it is only proof that counts" when he has before him a conjecture like that of Waring which has certainly influenced for good the development of a very fascinating chapter in the modern theory of numbers. Probably the same feeling that induced this statement led to Hardy's calling by the name "theorem of Lagrange" the theorem that every integer is a sum of four non-negative squares, whereas Fermat had stated that he had a proof of the theorem (both Fermat and Bachet ascribing the theorem to Diophantus) and Euler had made repeated efforts for forty years to prove it before Lagrange through the aid of Euler's work succeeded in giving the first proof in 1772. [See Dickson's *History*, vol. II, pp. ix, x, 275–303.] It appears to me to be unfortunate to have this theorem called by the name of Lagrange; it certainly represents one extreme of judgment concerning the question of attaching names of mathematicians to specific theorems.

The opposite extreme of the same thing recently came to my attention in another connection; curiously enough, it is again a case of a "theorem of Lagrange." The theorem that the order of a subgroup is a factor of the order of the group containing it has been called the "theorem of Lagrange" by at least two authors of high repute [see Pascal's *Repertorium* (in German), vol. I, 2d edition, 1910, p. 194, and Miller, Blichfeldt and Dickson's *Finite Groups*, 1916, p. 23 (in the part written by G. A. Miller)]. Now the facts seem to be that

* If any one of the many mathematical propositions stated by Fermat is incorrect, with perhaps a single exception, I am unaware of it. The case of exception is that concerning the prime character of the so-called Fermat numbers $2^k + 1$ where $k = 2^n$; and this incorrect statement he first made several times as a conjecture and finally (after the lapse of several years) implied that he knew a proof of it. This would seem therefore to be a lapse of memory rather than an error in reasoning. All his other theorems have been proved with the one famous exception. See Dickson's *History*, vol. I, p. 375, and vol. II, p. xviii.

Lagrange knew the theorem only for the case of the subgroups of the symmetric group and that even for this case he had no satisfactory proof. Abbati (in 1803) completed the proof for subgroups of the symmetric group and also proved the theorem for cyclic subgroups of any group; but it was apparently more than seventy-five years after the publication of Lagrange's memoir (in 1770–1771) before the completed theorem became current (though it had appeared earlier in a paper by Galois in 1832). In this case we have attributed to Lagrange a theorem which he probably never knew or conjectured, on the ground (it would seem) that he knew a certain special case of it. In Hardy's paper we have a theorem referred to Lagrange apparently on the ground that he first published a proof of it though it had been in the literature long before. Somewhere between these two extremes lies the golden mean of proper practice in attaching the names of mathematicians to specific theorems; and this mean, in the opinion of the reviewer, is rather far removed from each of the extremes indicated.

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Statics, including Hydrostatics and the Elements of the Theory of Elasticity. By Horace Lamb. Cambridge, University Press, 1916. xii + 341 pp.

Mathematics as ordinarily taught in our colleges and mathematics as used in this work-a-day world are birds of entirely different feather, and they do not flock together. This may perhaps be illustrated by a simple problem (No. 20, p. 178) from Lamb's *Statics*:

"Water is poured into a vessel of any shape. Prove that at the instant when the center of gravity of the vessel and the contained water is lowest it is at the level of the water surface."

Let us imagine a well trained sophomore attacking this problem. It is clearly a minimum problem involving integration. We measure h vertically upward from the bottom of the inside of the container, take the density as unity (or shall we keep it as ρ?), and let $A(h)$ be the area of the cross-section of the vessel. Then the center of gravity of the water is at a height

$$h_1 = \int_0^h \rho h A dh \div \int^h \rho A dh.$$

Let the mass of the vessel be denoted by M, and let its center