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THE THEORY OF NUMBERS[1]

There is probably less difference between the methods of a physicist and a mathematician than is generally supposed. The most striking among them seems to me to be this, that the mathematician is in much more direct contact with reality. This may perhaps seem to you a paradox, since it is the physicist who deals with the subject-matter to which the epithet "real" is commonly applied. But a very little reflection will show that the "reality" of the physicist, whatever it may be (and it is extraordinarily difficult to say), has few or none of the attributes which common-sense instinctively marks as real. A chair may be a collection of whirling atoms, or an idea in the mind of God. It is not my business to suggest that one account of it is obviously more plausible than the other. Whatever the merits of either of them may be, neither draws its inspiration from the suggestions of common-sense.

Neither the philosophers, nor the physicists themselves, have ever put forward any very convincing account of what physical reality is, or of how the physicist passes, from the confused mass of fact or sensation with which he starts, to the construction of the objects which he classifies as real. We can not be said, therefore, to know what the subject-matter of physics is; but this need not prevent us from understanding the task which a physicist is trying to perform. That, clearly, is to correlate the incoherent body of facts confronting him with some definite and orderly scheme of abstract relations, the kind of scheme, in short, which he can borrow only from mathematics.

A mathematician, on the other hand, fortunately for him, is not concerned with this

[1] From the address of the president of the Section of Mathematics and Physics, given at the Hull meeting of the British Association for the Advancement of Science.

physical reality at all. It is impossible to prove, by mathematical reasoning, any proposition whatsoever concerning the physical world, and only a mathematical crank would be likely now to imagine it his function to do so. There is plainly one way only of ascertaining the facts of experience, and that is by observation. It is not the business of a mathematician to suggest one view of the universe or another, but merely to supply the physicists with a collection of abstract schemes, which it is for them to select from, and to adopt or discard at their pleasure.

The most obvious example is to be found in the science of geometry. Mathematicians have constructed a very large number of different systems of geometry, Euclidean or non-Euclidean, of one, two, three, or any number of dimensions. All these systems are of complete and equal validity. They embody the results of mathematicians' observations of *their* reality, a reality far more intense and far more rigid than the dubious and elusive reality of physics. The old-fashioned geometry of Euclid, the entertaining seven-point geometry of Veblen, the space-times of Minkowski and Einstein, are all absolutely and equally real. When a mathematician has constructed, or, to be more accurate, when he has observed them, his professional interest in the matter ends. It may be the seven-point geometry that fits the facts the best, for anything that mathematicians have to say. There may be three dimensions in this room and five next door. As a professional mathematician, I have no idea; I can only ask some competent physicist to instruct me in the facts.

The function of a mathematician, then, is simply to observe the facts about his own intricate system of reality, that astonishingly beautiful complex of logical relations which forms the subject-matter of his science, as if he were an explorer looking at a distant range of mountains, and to record the results of his observations in a series of maps, each of which is a branch of pure mathematics. Many of these maps have been completed, while in others, and these, naturally, are the most interesting, there are vast uncharted regions. Some, it seems, have some relevance to the structure of the physical world, while others have no such tangible application. Among them there is perhaps none quite so fascinating, with quite the same astonishing contrasts of sharp outline and mysterious shade, as that which constitutes the theory of numbers.

The number system of arithmetic is, as we know too well, not without its applications to the sensible world. The currency systems of Europe, for example, conform to it approximately; west of the Vistula, two and two make something approaching four. The practical applications of arithmetic, however, are tedious beyond words. One must probe a little deeper into the subject if one wishes to interest the ordinary man, whose taste in such matters is astonishingly correct, and who turns with joy from the routine of common life to anything strange and odd, like the fourth dimension, or imaginary time, or the theory of the representation of integers by sums of squares or cubes.

It is impossible for me to give you, in the time at my command, any general account of the problems of the theory of numbers, or of the progress that has been made towards their solution even during the last twenty years. I must adopt a much simpler method. I will merely state to you, with a few words of comment, three or four isolated questions, selected in a haphazard way. They are seemingly simple questions, and it is not necessary to be anything of a mathematician to understand them; and I have chosen them for no better reason than that I happen to be interested in them myself. There is no one of them to which I know the answer, nor, so far as I know, does any mathematician in the world; and there is no one of them, with one exception which I have included deliberately, the answer to which any one of us would not make almost any sacrifice to know.

1. *When is a number the sum of two cubes, and what is the number of its representations?* This is my first question, and first of all I will elucidate it by some examples. The numbers $2 = 1^3 + 1^3$ and $9 = 2^3 + 1^3$ are sums of two cubes, while 3 and 4 are not: it is exceptional for a number to be of this particular form. The number of cubes up to 1,000,000 is 100, and the number of numbers, up to this limit

and of the form required, can not exceed 10,000, one hundredth of the whole. The density of the distribution of such numbers tends to zero as the numbers tend to infinity. Is there, I am asking, any simple criterion by which such numbers can be distinguished?

Again, 2 and 9 are sums of two cubes, and can be expressed in this form in one way only. There are numbers so expressible in a variety of different ways. The least such number is 1729, which is $12^3 + 1^3$ and also $10^3 + 9^3$. It is more difficult to find a number with *three* representations; the least such number is $175,959,000 = 560^3 + 70^3 = 552^3 + 198^3 = 525^3 + 315^3$. One number at any rate is known with *four* representations, namely, 19×363510^3 (a number of 18 digits), but I am not prepared to assert that it is the least. No number has been calculated, so far as I know, with more than four, but theory, running ahead of computation, shows that numbers exist with five representations, or six, or any number.

A distinguished physicist has argued that the possible number of isotopes of an element is probably limited because, among the ninety or so elements at present under observation, there is none which has more isotopes than six. I dare not criticise a physicist in his own field; but the figures I have quoted may suggest to you that an arithmetical generalization, based on a corresponding volume of evidence, would be more than a little rash.

There are similar questions, of course, for squares, but the answers to these were found long ago by Euler and by Gauss, and belong to the classical mathematics. Suppose, for simplicity of statement, that the number in question is *prime*. Then, if it is of the form $4m + 1$, it is a sum of squares, and in one way only, while if it is of the form $4m + 3$ it is not so expressible; and this simple rule may readily be generalized so as to apply to numbers of any form. But there is no similar solution for our actual problem, nor, I need scarcely say, for the analogous problems for fourth, fifth or higher powers. The smallest number known to be expressible in two ways by two biquadrates is $635318657 = 158^4 + 59^4 = 134^4 + 133^4$; and I do not believe that any number is known expressible in three. Nor, to my knowledge, has the bare existence of such a number yet been proved. When we come to fifth powers, nothing is known at all. The field for future research is unlimited and practically untrodden.

2. I pass to another question, again about cubes, but of a somewhat different kind. *Is every large number* (every number, that is to say, from a definite point onwards) *the sum of five cubes?* This is another exceptionally difficult problem. It is known that every number, without exception, is the sum of nine cubes; two numbers, 23 (which is $2.2^3 + 7.1^3$) and 239, actually require so many. It seems that there are just fifteen numbers, the largest being 454, which need eight, and 121 numbers, the largest being 8042, which need seven; and the evidence suggests forcibly that the six-cube numbers also ultimately disappear. In a lecture which I delivered on this subject at Oxford I stated, on the authority of Dr. Ruckle, that there were two numbers, in the immediate neighborhood of 1,000,000, which could not be resolved into fewer cubes than six; but Dr. A. E. Western has refuted this assertion by resolving each of them into five, and is of opinion, I believe, that the six-cube numbers have disappeared entirely considerably before this point. It is conceivable that the five-cube numbers also disappear, but this, if it be so, is probably in depths where computation is helpless. The four-cube numbers must certainly persist for ever, for it is impossible that a number $9n + 4$ or $9n + 5$ should be the sum of three.

I need scarcely add that there is a similar problem for every higher power. For fourth powers the critical number is 16. There is no case, except the simple case of squares, in which the solution is in any sense complete. About the squares there is no mystery; every number is the sum of four squares, and there are infinitely many numbers which can not be expressed by fewer.

3. I will next raise the question *whether the number* $2^{137} - 1$ *is prime.* I said that I would include one question which does not interest me particularly; and I should like to explain to you the kind of reasons which damp down

my interest in this one. I do not know the answer, and I do not care greatly what it is.

The problem belongs to the theory of the so-called "perfect" numbers, which has exercised mathematicians since the times of the Greeks. A number is perfect if, like 6 or 28, it is the sum of all its divisors, unity included. Euclid proved that the number $2^m(2^{m+1} - 1)$ is perfect if the second factor is prime; and Euler, 2,000 years later, that all *even* perfect numbers are of Euclid's form. It is still unknown whether a perfect number can be odd.

It would obviously be most interesting to know generally in what circumstances a number $2^n - 1$ is prime. It is plain that this can be so only if n itself is prime, as otherwise the number has obvious factors; and the 137 of my question happens to be the least value of n for which the answer is still in doubt. You may perhaps be surprised that a question apparently so fascinating should fail to arouse me more.

It was asserted by Mersenne in 1644 that the only values of n, up to 257, for which $2^n - 1$ is prime are 2, 3, 5, 7, 13, 17, 19, 31, 67, 127, 257; and an enormous amount of labor has been expended on attempts to verify this assertion. There are no simple general tests by which the primality of a number chosen at random can be determined, and the amount of computation required in any particular case may be appalling. It has, however, been imagined that Mersenne perhaps knew something which later mathematicians have failed to rediscover. The idea is a little fantastic, but there is no doubt that, so long as the possibility remained, arithmeticians were justified in their determination to ascertain the facts at all costs. "The riddle as to how Mersenne's numbers were discovered remains unsolved," wrote Mr. Rouse Ball in 1891. Mersenne, he observes, was a good mathematician, but not an Euler or a Gauss, and he inclines to attribute the discovery to the exceptional genius of Fermat, the only mathematician of the age whom any one could suspect of being hundreds of years ahead of his time.

These speculations appear extremely fanciful now, for the bubble has at last been pricked. It seems now that Mersenne's assertion, so far from hiding unplumbed depths of mathematical profundity, was a conjecture based on inadequate empirical evidence, and a somewhat unhappy one at that. It is now known that there are at least four numbers about which Mersenne is definitely wrong; he should have included at any rate 61, 89 and 107, and he should have left out 67. The mistake as regards 61 and 67 was discovered so long ago as 1886, but could be explained with some plausibility, so long as it stood alone, as a merely clerical error. But when Mr. R. E. Powers, in 1911 and 1914, proved that Mersenne was also wrong about 89 and 107, this line of defence collapsed, and it ceased to be possible to take Mersenne's assertion seriously.

The fact may be summed up as follows. Mersenne makes fifty-five assertions, for the fifty-five primes from 2 to 257. Of these assertions forty are true, four false, and eleven still doubtful. Not a bad result, you may think; but there is more to be said. Of the forty correct assertions many, half at least, are trivial, either because the numbers in question are comparatively small, or because they possess quite small and easily detected divisors. The test cases are those in which the numbers are prime, or Mersenne asserts that they are so; there are only four of these cases which are difficult and in which the truth is known; and in these Mersenne is wrong in every case but one.

It seems to me, then, that we must regard Mersenne's assertion as exploded; and for my part it interests me no longer. If he is wrong about 89 and 107, I do not care greatly whether he is wrong about 137 as well, and I should regard the computations necessary to decide as very largely wasted. There are so many much more profitable calculations which a computer could undertake.

I hope that you will not infer that I regard the problem of perfect numbers as uninteresting in itself; that would be very far from the truth. There are at least two intensely interesting problems. The first is the old problem, which so many mathematicians have failed to solve, whether a perfect number can be odd. The second is whether the number of perfect numbers is infinite or not. If we assume that all perfect numbers are even, we can state this problem in a still more arresting form. *Are there infinitely many primes of the form* $2^n - 1$? I find it difficult to imagine a problem more fascinatiing or more intricate than that. It is plain, though, that this is a question which computation can never decide, and it is very unlikely that it can ever give us any data of serious value. . . . .

There is a great deal of mathematics the purport of which is quite impossible for any amateur to grasp, and which, however beautiful and important it may be, must always remain the possession of a narrow circle of experts. It is the peculiarity of the theory of numbers that much of it could be published broadcast, and would win new readers for the *Daily Mail*. The positive integers do not lie, like the logical foundations of mathematics, in the scarcely visible distance, nor in the uncomfortably tangled foreground, like the immediate data of the physical world, but at a decent middle distance, where the outlines are clear and yet some element of mystery remains. There is no one so blind that he does not see them, and no one so sharp-sighted that his vision does not fail; they stand there a continual and inevitable challenge to the curiosity of every healthy mind. I have merely directed your attention for a moment to a few of the less immediately conspicuous features of the landscape, in the hope that I may sharpen your curiosity a little, and that some may feel tempted to walk a little nearer and take a closer view.

G. H. Hardy

WHITHER?[1]

I

Whether one enters a group of socially minded thinkers or a group of doctors in private conference or in public assembly, one soon becomes conscious of a restlessness regarding the profession of medicine. What does one think of membership in the American "Royal" College of Surgeons or Physicians, of medicine practiced under the ægis of a "group," of higher education for nurses, of chiropractors, of Christian Sciencers, of medical societies going to the public with their wares? Is the patient still the doctor's, or does he belong to a hospital? Should "industrial" medicine be developed? Should hospitals be standardized? Should the medical educational requirements of six years be lengthened to seven or eight or nine? Where ought one to stand on "state" medicine; should medicine have a portfolio in the cabinet; should clinical teachers be forbidden private practice? Should hospitals be open only to staffs or to all licentiates in medicine?

Are the answers to these problems really hard to find?

The medical profession has been caught in the swirl of the times. In the press of the moment it has forgotten its origins. Lost sight of are the circumstances, the principles and the ideas which in all times have made medicine what it is. Cause and effect are being mixed up. The present day shows too much of the form and too little of the spirit of that which has given the doctor his place and power.

II

It is no new discovery that the tyranny of a crowd is no better than the tyranny of an individual and that both lead to death. In spite of our cry that we are democratic we are almost exactly the reverse. We certainly dress alike; it has been said that we look alike; the corollary is that we think alike. Tersely put, we work in crowds and think in gangs and when applied to medicine we forget why anything smacking of such forms has prospered.

A case in point is offered by the diagnostic and operating "groups" in medicine which today infest us. Blinded by the success of one or two prototypes, medical men have concluded that their form accounts for their popularity. The fact is that none such has prospered—save as any business which is not bankrupt may be said to be prospering—except as the old substance of medical practice has been kept alive in the group by one or two dominating personalities. Without such vital souls there is left only a paper organization—all, it is safe to predict, that will survive when the present day medical or surgical leaders of these groups are gone.

[1] Remarks made at the banquet of the Ohio State Medical Association meeting, May 3, 1922.