COMMENTARY ON

G. H. HARDY

G. H. HARDY was a pure mathematician. The boundaries of this subject cannot be precisely defined but for Hardy the word "pure" as applied to mathematics had a clear, though negative, meaning. To qualify as pure, Hardy said, a mathematical topic had to be useless; if useless, it was not only pure, but beautiful. If useful—which is to say impure—it was ugly, and the more useful, the more ugly. These opinions were not always well received. The noted chemist Frederick Soddy, reviewing the book from which the following excerpts are taken, pronounced as scandalous Hardy's expressed contempt for useful mathematics or indeed for any applied science. "From such cloistral clowning," wrote Soddy, "the world sickens." [1] Hardy was a strange, original and enigmatic man. He was also a fine mathematician and a charming writer.

Godfrey Harold Hardy was born in Surrey in February 1877. His parents were teachers and "mathematically minded." He was educated first at Winchester—which he hated—and then at Cambridge, where he taught the greater part of his life. From 1919 to 1931 he held the Savilian chair of geometry at Oxford; in 1931 he was elected to the Sadlerian chair of pure mathematics at Cambridge and resumed the Fellowship at Trinity College which he had held from 1898 to 1919.

Hardy's main work was in analysis and arithmetic. He is known to students for his classic text, *A Course of Pure Mathematics*, which set a new standard for English mathematical education. But his reputation as the leader of pure mathematicians in Great Britain rests on his original and advanced researches. He wrote profound and masterly papers on such topics as the convergence and summability of series, inequalities and the analytic theory of numbers. The problems of number theory are often very easily stated (e.g., to prove that every even number is the sum of two prime numbers) "but all the resources of analysis are required to make any impression on them." [2] The problem quoted, and others of equally innocent appearance, are still unsolved "but they are not now—as they were in 1910—unapproachable." [3] This advance is due mainly to the joint work of Hardy and the British mathematician J. E. Littlewood. Their collaboration was exceptionally long and immensely fruitful; it is considered the most remarkable of all mathematical partnerships. An equally brilliant but unhappily brief partnership existed between Hardy and the

[1] *Nature*, Vol. 147, January 4, 1941.
[2] Obituary of G. H. Hardy, *Nature*, Vol. 161, May 22, 1948, pp. 797–98.
[3] *Ibid.*

self-taught Indian genius Ramanujan (see p. 368). It is hard to imagine two men further apart in training and background, yet Hardy was one of the first to discern what he termed Ramanujan's "profound and invincible originality." Ramanujan "called out Hardy's equal but quite different powers." "I owe more to him," Hardy said, "than to anyone else in the world with one exception, and my association with him is the one romantic incident of my life." [4]

I once encountered Hardy in the early 1930s at the subway entrance near Columbia University in New York City. It was a raw, wet winter day, but he was bareheaded, had no overcoat and wore a white cable-stitched turtle-necked sweater and a baggy pair of tennis slacks. I recall his delicately cut but strong features, his high coloring and the hair that fell in irregular bangs over his forehead. He was a strikingly handsome and graceful man who would have drawn attention even in more conventional dress. Hardy had strong opinions and vehement prejudices; some were admirable, some merely eccentric, and, I cannot help thinking, deliberately assumed. In political opinion as well as in his mathematical philosophy, he shared Bertrand Russell's views. His hatred of war was one reason why he regarded applied mathematics (ballistics or aerodynamics, for example) as "repulsively ugly and intolerably dull." [5] Hardy "always referred to God as his personal enemy. This was of course a joke but there was something real behind it. . . . He would not enter a religious building, even for such purpose as the election of a Warden of New College." [6] A special exemption clause had to be written into the by-laws of the college to enable him to discharge certain duties by proxy which otherwise would have required him to attend Chapel.

His love of mathematics was almost equaled by his passion for ball games: cricket, tennis and even baseball.[7] Justice Frankfurter tells the

[4] Obituary of G. H. Hardy by E. C. Titchmarsh, *The Journal of the London Mathematical Society*, April 1950, pp. 81–88. One of the joint papers of Hardy and Ramanujan is worth noting briefly. "Denote by $p(n)$ the number of ways of denoting the positive integer n as the sum of integers. For example, 5 can be expressed as $1+1+1+1+1$ or $1+1+1+2$ or $1+2+2$ or $1+1+3$ or $2+3$ or $1+4$ or 5, and so $p(5)=7$. It is plain that $p(n)$ increases rapidly with n; and $p(200)=3972999029388$ (as was shown by a computation which took a month). Hardy and Ramanujan's achievement was to establish an explicit formula for $p(n)$, of which the leading term is:

$$\frac{1}{2\pi\sqrt{2}}\,\frac{d}{dn}\,\frac{\exp\left\{\dfrac{2\pi}{\sqrt{6}}\sqrt{(n-1/24)}\right\}}{\sqrt{(n-1/24)}}$$

Five terms of the formula give the correct value of $p(200)$.

[5] Titchmarsh, *op. cit.*, p. 84.

[6] *Ibid.*, p. 86.

[7] Hardy frequently enlivened his discussions of philosophy or mathematics by illustrations taken from cricket. One of his papers "A maximal theorem with function-theoretic applications" contains the sentence "The problem is most easily grasped

story of Hardy's visit to Boston in 1936 when he delivered his Ramanujan lectures at the Harvard Tercentenary. He was to be the house guest of a well-known lawyer, later a United States Senator, and was terrified that he would find little to talk about with his host. The host was similarly alarmed, but the visit turned out to be easy and pleasant for both. For while the lawyer was no better prepared to discuss Zeta functions than the mathematician to comment upon the rule in Shelley's case, they discovered a common enthusiasm for baseball. The Red Sox were playing a home stand at the time and Hardy could barely spare the time for his lectures.

"I have never done anything 'useful.' No discovery of mine has made, or is likely to make, directly or indirectly, for good or ill, the least difference to the amenity of the world." These lines appear in Hardy's half-defiant, half-ironical apology for his misspent life as a pure mathematician. The statement is nonsense. Hardy, I do not doubt, knew it was nonsense. Contributions such as his are certain to be useful; unexpectedly, and considering the world of today, perhaps even disagreeably useful. To make matters worse by his standards, it appears that Hardy once made a contribution to genetics. Writing a letter to *Science* in 1908 on a problem involving the transmission of dominant and recessive Mendelian characters in a mixed population, he established a principle known as Hardy's Law. This law (though he attached "little weight to it") turns out to be of "central importance in the study of Rh-blood groups and the treatment of haemolytic disease of the newborn." [8]

Hardy received many degrees and honors, including of course election to a Fellowship in the Royal Society in 1910. He died on December 1, 1947, the day the Copley Medal of the Royal Society, its highest award, was to have been presented to him.

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when stated in the language of cricket. . . . Suppose that a batsman plays, in a given season, a given 'stock' of innings." This paper, published in *Acta Mathematica* (54) and "presumably addressed to European mathematicians in general" must not have been very helpful to the Hungarians, say, who may not have appreciated all the fine points of the example.

[8] Titchmarsh, *op. cit.*, p. 83. J. B. S. Haldane gives another example of the useful, if unintentional, consequences of Hardy's work. There is a function called Riemann's Zeta function "which was devised, and its properties investigated, to find an expression for the number of prime numbers less than a given number. Hardy loved it. But it has been used in the theory of pyrometry, that is to say the investigation of the temperature of furnaces." *Everything Has a History*, London, 1951, p. 240.

Mark all Mathematical heads which be wholly and only bent on these sciences, how solitary they be themselves, how unfit to live with others, how unapt to serve the world.

—ROGER ASCHAM (*ca. 1550*) (*Quoted in E. G. R. Taylor, "The Mathematical Practitioners of Tudor and Stuart England"*)

I admit that mathematical science is a good thing. But excessive devotion to it is a bad thing. —ALDOUS HUXLEY (*Interview, J. W. N. Sullivan*)

1 A Mathematician's Apology

By G. H. HARDY

A MATHEMATICIAN, like a painter or a poet, is a maker of patterns. If his patterns are more permanent than theirs, it is because they are made with *ideas*. A painter makes patterns with shapes and colours, a poet with words. A painting may embody an 'idea,' but the idea is usually commonplace and unimportant. In poetry, ideas count for a good deal more; but, as Housman insisted, the importance of ideas in poetry is habitually exaggerated: 'I cannot satisfy myself that there are any such things as poetical ideas. . . . Poetry is not the thing said but a way of saying it.'

Not all the water in the rough rude sea
Can wash the balm from an anointed King.

Could lines be better, and could ideas be at once more trite and more false? The poverty of the ideas seems hardly to affect the beauty of the verbal pattern. A mathematician, on the other hand, has no material to work with but ideas, and so his patterns are likely to last longer, since ideas wear less with time than words.

The mathematician's patterns, like the painter's or the poet's, must be *beautiful*; the ideas, like the colours or the words, must fit together in a harmonious way. Beauty is the first test: there is no permanent place in the world for ugly mathematics. And here I must deal with a misconception which is still widespread (though probably much less so now than it was twenty years ago), what Whitehead has called the 'literary superstition' that love of and aesthetic appreciation of mathematics is 'a monomania confined to a few eccentrics in each generation.'

It would be difficult now to find an educated man quite insensitive to the aesthetic appeal of mathematics. It may be very hard to *define* mathematical beauty, but that is just as true of beauty of any kind—we may not know quite what we mean by a beautiful poem, but that does not prevent us from recognizing one when we read it. Even Professor Hogben, who is out to minimize at all costs the importance of the aesthetic element in mathematics, does not venture to deny its reality. 'There are, to be sure, individuals for whom mathematics exercises a coldly impersonal attraction. . . . The aesthetic appeal of mathematics may be very real for a chosen few.' But they are 'few,' he suggests, and they feel 'coldly' (and are really rather ridiculous people, who live in silly little university towns sheltered from the fresh breezes of the wide open spaces). In this he is merely echoing Whitehead's 'literary superstition.'

The fact is that there are few more 'popular' subjects than mathematics. Most people have some appreciation of mathematics, just as most people can enjoy a pleasant tune; and there are probably more people really interested in mathematics than in music. Appearances may suggest the contrary, but there are easy explanations. Music can be used to stimulate mass emotion, while mathematics cannot; and musical incapacity is recognized (no doubt rightly) as mildly discreditable, whereas most people are so frightened of the name of mathematics that they are ready, quite unaffectedly, to exaggerate their own mathematical stupidity.

A very little reflection is enough to expose the absurdity of the 'literary superstition.' There are masses of chess-players in every civilized country —in Russia, almost the whole educated population; and every chess-player can recognize and appreciate a 'beautiful' game or problem. Yet a chess problem is *simply* an exercise in pure mathematics (a game not entirely, since psychology also plays a part), and everyone who calls a problem 'beautiful' is applauding mathematical beauty, even if it is beauty of a comparatively lowly kind. Chess problems are the hymn-tunes of mathematics.

We may learn the same lesson, at a lower level but for a wider public, from bridge, or descending further, from the puzzle columns of the popular newspapers. Nearly all their immense popularity is a tribute to the drawing power of rudimentary mathematics, and the better makers of puzzles, such as Dudeney or 'Caliban,' use very little else. They know their business: what the public wants is a little intellectual 'kick,' and nothing else has quite the kick of mathematics.

I might add that there is nothing in the world which pleases even famous men (and men who have used disparaging language about mathematics) quite so much as to discover, or rediscover, a genuine mathematical theorem. Herbert Spencer republished in his autobiography a theorem about circles which he proved when he was twenty (not knowing that it had been proved over two thousand years before by Plato). Professor Soddy is a more recent and a more striking example (but *his* theorem really is his own).[1]

[1] See his letters on the 'Hexlet' in *Nature*, vols. 137–9 (1936–7).

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A chess problem is genuine mathematics, but it is in some way 'trivial' mathematics. However ingenious and intricate, however original and surprising the moves, there is something essential lacking. Chess problems are *unimportant*. The best mathematics is *serious* as well as beautiful—'important' if you like, but the word is very ambiguous, and 'serious' expresses what I mean much better.

I am not thinking of the 'practical' consequences of mathematics. I have to return to that point later: at present I will say only that if a chess problem is, in the crude sense, 'useless,' then that is equally true of most of the best mathematics; that very little of mathematics is useful practically, and that that little is comparatively dull. The 'seriousness' of a mathematical theorem lies, not in its practical consequences, which are usually negligible, but in the *significance* of the mathematical ideas which it connects. We may say, roughly, that a mathematical idea is 'significant' if it can be connected, in a natural and illuminating way, with a large complex of other mathematical ideas. Thus a serious mathematical theorem, a theorem which connects significant ideas, is likely to lead to important advances in mathematics itself and even in other sciences. No chess problem has ever affected the general development of scientific thought: Pythagoras, Newton, Einstein have in their times changed its whole direction.

The seriousness of a theorem, of course, does not *lie in* its consequences, which are merely the *evidence* for its seriousness. Shakespeare had an enormous influence on the development of the English language, Otway next to none, but that is not why Shakespeare was the better poet. He was the better poet because he wrote much better poetry. The inferiority of the chess problem, like that of Otway's poetry, lies not in its consequences but in its content.

There is one more point which I shall dismiss very shortly, not because it is uninteresting but because it is difficult, and because I have no qualifications for any serious discussion in aesthetics. The beauty of a mathematical theorem *depends* a great deal on its seriousness, as even in poetry the beauty of a line may depend to some extent on the significance of the ideas which it contains. I quoted two lines of Shakespeare as an example of the sheer beauty of a verbal pattern; but

After life's fitful fever he sleeps well

seems still more beautiful. The pattern is just as fine, and in this case the ideas have significance and the thesis is sound, so that our emotions are stirred much more deeply. The ideas do matter to the pattern, even in poetry, and much more, naturally, in mathematics; but I must not try to argue the question seriously.

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It will be clear by now that, if we are to have any chance of making progress, I must produce examples of 'real' mathematical theorems, theorems which every mathematician will admit to be first-rate. And here I am very heavily handicapped by the restrictions under which I am writing. On the one hand my examples must be very simple, and intelligible to a reader who has no specialized mathematical knowledge; no elaborate preliminary explanations must be needed; and a reader must be able to follow the proofs as well as the enunciations. These conditions exclude, for instance, many of the most beautiful theorems of the theory of numbers, such as Fermat's 'two square' theorem or the law of quadratic reciprocity. And on the other hand my examples should be drawn from 'pukka' mathematics, the mathematics of the working professional mathematician; and this condition excludes a good deal which it would be comparatively easy to make intelligible but which trespasses on logic and mathematical philosophy.

I can hardly do better than go back to the Greeks. I will state and prove two of the famous theorems of Greek mathematics. They are 'simple' theorems, simple both in idea and in execution, but there is no doubt at all about their being theorems of the highest class. Each is as fresh and significant as when it was discovered—two thousand years have not written a wrinkle on either of them. Finally, both the statements and the proofs can be mastered in an hour by any intelligent reader, however slender his mathematical equipment.

1. The first is Euclid's [2] proof of the existence of an infinity of prime numbers.

The *prime numbers* or *primes* are the numbers

(A) $$2, 3, 5, 7, 11, 13, 17, 19, 23, 29, \ldots$$

which cannot be resolved into smaller factors.[3] Thus 37 and 317 are prime. The primes are the material out of which all numbers are built up by multiplication: thus $666 = 2.3.3.37$. Every number which is not prime itself is divisible by at least one prime (usually, of course, by several). We have to prove that there are infinitely many primes, i.e., that the series (A) never comes to an end.

Let us suppose that it does, and that

$$2, 3, 5, \ldots, P$$

is the complete series (so that P is the largest prime); and let us, on this hypothesis, consider the number Q defined by the formula

$$Q = (2.3.5. \ldots .P) + 1.$$

[2] *Elements* IX 20. The real origin of many theorems in the *Elements* is obscure, but there seems to be no particular reason for supposing that this one is not Euclid's own.

[3] There are technical reasons for not counting 1 as a prime.

It is plain that Q is not divisible by any of 2, 3, 5, ..., P; for it leaves the remainder 1 when divided by any one of these numbers. But, if not itself prime, it is divisible by *some* prime, and therefore there is a prime (which may be Q itself) greater than any of them. This contradicts our hypothesis, that there is no prime greater than P; and therefore this hypothesis is false.

The proof is by *reductio ad absurdum*, and *reductio ad absurdum*, which Euclid loved so much, is one of a mathematician's finest weapons.[4] It is a far finer gambit than any chess gambit: a chess player may offer the sacrifice of a pawn or even a piece, but a mathematician offers *the game*.

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2. My second example is Pythagoras's[5] proof of the 'irrationality' of $\sqrt{2}$.

A 'rational number' is a fraction $\dfrac{a}{b}$, where a and b are integers: we may suppose that a and b have no common factor, since if they had we could remove it. To say that '$\sqrt{2}$ is irrational' is merely another way of saying that 2 cannot be expressed in the form $\left(\dfrac{a}{b}\right)^2$; and this is the same thing as saying that the equation

$$a^2 = 2b^2 \tag{B}$$

cannot be satisfied by integral values of a and b which have no common factor. This is a theorem of pure arithmetic, which does not demand any knowledge of 'irrational numbers' or depend on any theory about their nature.

We argue again by *reductio ad absurdum*; we suppose that (B) is true, a and b being integers without any common factor. It follows from (B) that a^2 is even (since $2b^2$ is divisible by 2), and therefore that a is even (since the square of an odd number is odd). If a is even then

$$a = 2c \tag{C}$$

for some integral value of c; and therefore

$$2b^2 = a^2 = (2c)^2 = 4c^2$$

or

$$b^2 = 2c^2. \tag{D}$$

[4] The proof can be arranged so as to avoid a *reductio*, and logicians of some schools would prefer that it should be.

[5] The proof traditionally ascribed to Pythagoras, and certainly a product of his school. The theorem occurs, in a much more general form, in Euclid (*Elements* x 9).

Hence b^2 is even, and therefore (for the same reason as before) b is even. That is to say, a and b are both even, and so have the common factor 2. This contradicts our hypothesis, and therefore the hypothesis is false.

It follows from Pythagoras's theorem that the diagonal of a square is incommensurable with the side (that their ratio is not a rational number, that there is no unit of which both are integral multiples). For if we take the side as our unit of length, and the length of the diagonal is d, then, by a very familiar theorem also ascribed to Pythagoras,[6]

$$d^2 = 1^2 + 1^2 = 2,$$

so that d cannot be a rational number.

I could quote any number of fine theorems from the theory of numbers whose *meaning* anyone can understand. For example, there is what is called 'the fundamental theorem of arithmetic,' that any integer can be resolved, *in one way only*, into a product of primes. Thus $666 = 2.3.3.37$, and htere is no other decomposition; it is impossible that $666 = 2.11.29$ or that $13.89 = 17.73$ (and we can see so without working out the products). This theorem is, as its name implies, the foundation of higher arithmetic; but the proof, although not 'difficult,' requires a certain amount of preface and might be found tedious by an unmathematical reader.

Another famous and beautiful theorem is Fermat's 'two square' theorem. The primes may (if we ignore the special prime 2) be arranged in two classes; the primes

$$5, 13, 17, 29, 37, 41, \ldots$$

which leave remainder 1 when divided by 4, and the primes

$$3, 7, 11, 19, 23, 31, \ldots$$

which leave remainder 3. All the primes of the first class, and none of the second, can be expressed as the sum of two integral squares: thus

$$5 = 1^2 + 2^2,\ 13 = 2^2 + 3^2,$$
$$17 = 1^2 + 4^2,\ 29 = 2^2 + 5^2;$$

but 3, 7, 11, and 19 are not expressible in this way (as the reader may check by trial). This is Fermat's theorem, which is ranked, very justly, as one of the finest of arithmetic. Unfortunately there is no proof within the comprehension of anybody but a fairly expert mathematician.

There are also beautiful theorems in the 'theory of aggregates' (*Mengenlehre*), such as Cantor's theorem of the 'non-enumerability' of the continuum. Here there is just the opposite difficulty. The proof is easy enough, when once the language has been mastered, but considerable explanation is necessary before the *meaning* of the theorem becomes clear.

[6] Euclid, *Elements* I 47.

So I will not try to give more examples. Those which I have given are test cases, and a reader who cannot appreciate them is unlikely to appreciate anything in mathematics.

I said that a mathematician was a maker of patterns of ideas, and that beauty and seriousness were the criteria by which his patterns should be judged. I can hardly believe that anyone who has understood the two theorems will dispute that they pass these tests. If we compare them with Dudeney's most ingenious puzzles, or the finest chess problems that masters of that art have composed, their superiority in both respects stands out: there is an unmistakable difference of class. They are much more serious, and also much more beautiful: can we define, a little more closely, where their superiority lies?

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In the first place, the superiority of the mathematical theorems in *seriousness* is obvious and overwhelming. The chess problem is the product of an ingenious but very limited complex of ideas, which do not differ from one another very fundamentally and have no external repercussions. We should think in the same way if chess had never been invented, whereas the theorems of Euclid and Pythagoras have influenced thought profoundly, even outside mathematics.

Thus Euclid's theorem is vital for the whole structure of arithmetic. The primes are the raw material out of which we have to build arithmetic, and Euclid's theorem assures us that we have plenty of material for the task. But the theorem of Pythagoras has wider applications and provides a better text.

We should observe first that Pythagoras's argument is capable of far-reaching extension, and can be applied, with little change of principle, to very wide classes of 'irrationals.' We can prove very similarly (as Theaetetus seems to have done) that

$$\sqrt{3},\ \sqrt{5},\ \sqrt{7},\ \sqrt{11},\ \sqrt{13},\ \sqrt{17}$$

are irrational, or (going beyond Theaetetus) that $\sqrt[3]{2}$ and $\sqrt[3]{17}$ are irrational.[7]

Euclid's theorem tells us that we have a good supply of material for the construction of a coherent arithmetic of the integers. Pythagoras's theorem and its extensions tells us that, when we have constructed this arithmetic, it will not prove sufficient for our needs, since there will be many magnitudes which obtrude themselves upon our attention and which it will be unable to measure: the diagonal of the square is merely the most obvious example. The profound importance of this discovery was recog-

7 See Ch. IV of Hardy and Wright's *Introduction to the Theory of Numbers*, where there are discussions of different generalizations of Pythagoras's argument, and of a historical puzzle about Theaetetus.

nized at once by the Greek mathematicians. They had begun by assuming (in accordance, I suppose, with the 'natural' dictates of 'common sense') that all magnitudes of the same kind are commensurable, that any two lengths, for example, are multiples of some common unit, and they had constructed a theory of proportion based on this assumption. Pythagoras's discovery exposed the unsoundness of this foundation, and led to the construction of the much more profound theory of Eudoxus which is set out in the fifth book of the *Elements*, and which is regarded by many modern mathematicians as the finest achievement of Greek mathematics. This theory is astonishingly modern in spirit, and may be regarded as the beginning of the modern theory of irrational number, which has revolutionized mathematical analysis and had much influence on recent philosophy.

There is no doubt at all, then, of the 'seriousness' of either theorem. It is therefore the better worth remarking that neither theorem has the slightest 'practical' importance. In practical applications we are concerned only with comparatively small numbers; only stellar astronomy and atomic physics deal with 'large' numbers, and they have very little more practical importance, as yet, than the most abstract pure mathematics. I do not know what is the highest degree of accuracy which is ever useful to an engineer—we shall be very generous if we say ten significant figures. Then

$$3 \cdot 141592654$$

(the value of π to nine places of decimals) is the ratio

$$\frac{3141592654}{1000000000}$$

of two numbers of ten digits. The number of primes less than 1,000,000,000 is 50,847,478: that is enough for an engineer, and he can be perfectly happy without the rest. So much for Euclid's theorem; and, as regards Pythagoras's, it is obvious that irrationals are uninteresting to an engineer, since he is concerned only with approximations, and all approximations are rational.

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The contrast between pure and applied mathematics stands out most clearly, perhaps, in geometry. There is the science of pure geometry,[8] in which there are many geometries, projective geometry, Euclidean geometry, non-Euclidean geometry, and so forth. Each of these geometries is a *model*, a pattern of ideas, and is to be judged by the interest and beauty of its particular pattern. It is a *map* or *picture*, the joint product of many hands, a partial and imperfect copy (yet exact so far as it extends) of a section of mathematical reality. But the point which is important to us

[8] We must of course, for the purposes of this discussion, count as pure geometry what mathematicians call 'analytical' geometry.

now is this, that there is one thing at any rate of which pure geometries are *not* pictures, and that is the spatio-temporal reality of the physical world. It is obvious, surely, that they cannot be, since earthquakes and eclipses are not mathematical concepts.

This may sound a little paradoxical to an outsider, but it is a truism to a geometer; and I may perhaps be able to make it clearer by an illustration. Let us suppose that I am giving a lecture on some system of geometry, such as ordinary Euclidean geometry, and that I draw figures on the blackboard to stimulate the imagination of my audience, rough drawings of straight lines or circles or ellipses. It is plain, first, that the truth of the theorems which I prove is in no way affected by the quality of my drawings. Their function is merely to bring home my meaning to my hearers, and, if I can do that, there would be no gain in having them redrawn by the most skilful draughtsman. They are pedagogical illustrations, not part of the real subject-matter of the lecture.

Now let us go a stage further. The room in which I am lecturing is part of the physical world, and has itself a certain pattern. The study of that pattern, and of the general pattern of physical reality, is a science in itself, which we may call 'physical geometry.' Suppose now that a violent dynamo, or a massive gravitating body, is introduced into the room. Then the physicists tell us that the geometry of the room is changed, its whole physical pattern slightly but definitely distorted. Do the theorems which I have proved become false? Surely it would be nonsense to suppose that the proofs of them which I have given are affected in any way. It would be like supposing that a play of Shakespeare is changed when a reader spills his tea over a page. The play is independent of the pages on which it is printed, and 'pure geometries' are independent of lecture rooms, or of any other detail of the physical world.

This is the point of view of a pure mathematician. Applied mathematicians, mathematical physicists, naturally take a different view, since they are preoccupied with the physical world itself, which also has its structure or pattern. We cannot describe this pattern exactly, as we can that of a pure geometry, but we can say something significant about it. We can describe, sometimes fairly accurately, sometimes very roughly, the relations which hold between some of its constituents, and compare them with the exact relations holding between constituents of some system of pure geometry. We may be able to trace a certain resemblance between the two sets of relations, and then the pure geometry will become interesting to physicists; it will give us, to that extent, a map which 'fits the facts' of the physical world. The geometer offers to the physicist a whole set of maps from which to choose. One map, perhaps, will fit the facts better than others, and then the geometry which provides that particular map will be the geometry most important for applied mathematics. I may add that even a pure mathematician may find his appreciation of this geometry quickened, since there is no mathematician so pure that he feels no interest at all in the physical world; but, in so far as he succumbs to this temptation, he will be abandoning his purely mathematical position.

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I will end with a summary of my conclusions, but putting them in a more personal way. I said at the beginning that anyone who defends his subject will find that he is defending himself; and my justification of the life of a professional mathematician is bound to be, at bottom, a justification of my own. Thus this concluding section will be in its substance a fragment of autobiography.

I cannot remember ever having wanted to be anything but a mathematician. I suppose that it was always clear that my specific abilities lay that way, and it never occurred to me to question the verdict of my elders. I do not remember having felt, as a boy, any *passion* for mathematics, and such notions as I may have had of the career of a mathematician were far from noble. I thought of mathematics in terms of examinations and scholarships: I wanted to beat other boys, and this seemed to be the way in which I could do so most decisively.

I was about fifteen when (in a rather odd way) my ambitions took a sharper turn. There is a book by 'Alan St. Aubyn' [9] called *A Fellow of Trinity*, one of a series dealing with what is supposed to be Cambridge college life. I suppose that it is a worse book than most of Marie Corelli's; but a book can hardly be entirely bad if it fires a clever boy's imagination. There are two heroes, a primary hero called Flowers, who is almost wholly good, and a secondary hero, a much weaker vessel, called Brown. Flowers and Brown find many dangers in university life, but the worst is a gambling saloon in Chesterton [10] run by the Misses Bellenden, two fascinating but extremely wicked young ladies. Flowers survives all these troubles, is Second Wrangler and Senior Classic, and succeeds automatically to a Fellowship (as I suppose he would have done then). Brown succumbs, ruins his parents, takes to drink, is saved from delirium tremens during a thunderstorm only by the prayers of the Junior Dean, has much difficulty in obtaining even an Ordinary Degree, and ultimately becomes a missionary. The friendship is not shattered by these unhappy events, and Flowers's thoughts stray to Brown, with affectionate pity, as he drinks port and eats walnuts for the first time in Senior Combination Room.

Now Flowers was a decent enough fellow (so far as 'Alan St. Aubyn' could draw one), but even my unsophisticated mind refused to accept him as clever. If he could do these things, why not I? In particular, the

[9] 'Alan St. Aubyn' was Mrs. Frances Marshall, wife of Matthew Marshall.
[10] Actually, Chesterton lacks picturesque features.

final scene in Combination Room fascinated me completely, and from that time, until I obtained one, mathematics meant to me primarily a Fellowship of Trinity.

I found at once, when I came to Cambridge, that a Fellowship implied 'original work,' but it was a long time before I formed any definite idea of research. I had of course found at school, as every future mathematician does, that I could often do things much better than my teachers; and even at Cambridge I found, though naturally much less frequently, that I could sometimes do things better than the College lecturers. But I was really quite ignorant, even when I took the Tripos, of the subjects on which I have spent the rest of my life; and I still thought of mathematics as essentially a 'competitive' subject. My eyes were first opened by Professor Love, who taught me for a few terms and gave me my first serious conception of analysis. But the great debt which I owe to him—he was, after all, primarily an applied mathematician—was his advice to read Jordan's famous *Cours d'analyse*; and I shall never forget the astonishment with which I read that remarkable work, the first inspiration for so many mathematicians of my generation, and learnt for the first time as I read it what mathematics really meant. From that time onwards I was in my way a real mathematician, with sound mathematical ambitions and a genuine passion for mathematics.

I wrote a great deal during the next ten years, but very little of any importance; there are not more than four or five papers which I can still remember with some satisfaction. The real crises of my career came ten or twelve years later, in 1911, when I began my long collaboration with Littlewood, and in 1913, when I discovered Ramanujan. All my best work since then has been bound up with theirs, and it is obvious that my association with them was the decisive event of my life. I still say to myself when I am depressed, and find myself forced to listen to pompous and tiresome people, 'Well, I have done one thing *you* could never have done, and that is to have collaborated with both Littlewood and Ramanujan on something like equal terms.' It is to them that I owe an unusually late maturity: I was at my best at a little past forty, when I was a professor at Oxford. Since then I have suffered from that steady deterioration which is the common fate of elderly men and particularly of elderly mathematicians. A mathematician may still be competent enough at sixty, but it is useless to expect him to have original ideas.

It is plain now that my life, for what it is worth, is finished, and that nothing I can do can perceptibly increase or diminish its value. It is very difficult to be dispassionate, but I count it a 'success'; I have had more reward and not less than was due to a man of my particular grade of ability. I have held a series of comfortable and 'dignified' positions. I have had very little trouble with the duller routine of universities. I hate 'teaching,' and have had to do very little, such teaching as I have done having been almost entirely supervision of research; I love lecturing, and have lectured a great deal to extremely able classes; and I have always had plenty of leisure for the researches which have been the one great permanent happiness of my life. I have found it easy to work with others, and have collaborated on a large scale with two exceptional mathematicians; and this has enabled me to add to mathematics a good deal more than I could reasonably have expected. I have had my disappointments, like any other mathematician, but none of them has been too serious or has made me particularly unhappy. If I had been offered a life neither better nor worse when I was twenty, I would have accepted without hesitation.

It seems absurd to suppose that I could have 'done better.' I have no linguistic or artistic ability, and very little interest in experimental science. I might have been a tolerable philosopher, but not one of a very original kind. I think that I might have made a good lawyer; but journalism is the only profession, outside academic life, in which I should have felt really confident of my chances. There is no doubt that I was right to be a mathematician, if the criterion is to be what is commonly called success.

My choice was right, then, if what I wanted was a reasonably comfortable and happy life. But solicitors and stockbrokers and bookmakers often lead comfortable and happy lives, and it is very difficult to see how the world is the richer for their existence. Is there any sense in which I can claim that my life has been less futile than theirs? It seems to me again that there is only one possible answer: yes, perhaps, but, if so, for one reason only.

I have never done anything 'useful.' No discovery of mine has made, or is likely to make, directly or indirectly, for good or ill, the least difference to the amenity of the world. I have helped to train other mathematicians, but mathematicians of the same kind as myself, and their work has been, so far at any rate as I have helped them to it, as useless as my own. Judged by all practical standards, the value of my mathematical life is nil; and outside mathematics it is trivial anyhow. I have just one chance of escaping a verdict of completely triviality, that I may be judged to have created something worth creating. And that I have created something is undeniable: the question is about its value.

The case for my life, then, or for that of any one else who has been a mathematician in the same sense in which I have been one, is this: that I have added something to knowledge, and helped others to add more; and that these somethings have a value which differs in degree only, and not in kind, from that of the creations of the great mathematicians, or of any of the other artists, great or small, who have left some kind of memorial behind them.