A Mathematician's Apology

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"I am indebted for many valuable criticisms to Prof. C. D. Board & Dr. C. P. Snow, each of whom read my original manuscript. I have incorporated the substance of nearly all of their suggestions in my text, & have so removed a good many crudities & obscurities.

In 1 case I have dealt with them differently. My §28 is based on a short article which I contributed to *Eureka* (the journal of the Cambridge Archimedean Society) early in the year, & I found it impossible to remodel what I had written so recently & with so much care. Also, if I had tried to meet such important criticisms seriously, I should have had to expand this section so much as to destroy the whole balance of my essay. I have therefore left it unaltered, but have added a short statement of the chief points made by my critics in a note at the end. G. H. H. Jul 18, 1940" – Hardy, 1992, p. 59

$\boxed{1}$ "It is a melancholy experience for a professional mathematician to find himself writing about mathematics. The function of a mathematician is to do something, to prove new theorems, to add to mathematics, & not to talk about what he or other mathematicians have done. Statesmen despise publicists, painters despise art-critics, & physiologists, physicists, or mathematicians have usually similar feelings; there is no scorn more profound, or on the whole more justifiable, than that of the men who make for the *men* who explain. Exposition, criticism, appreciation, is work for 2nd-rate minds.

I can remember arguing this point once in 1 of the few serious conversations that I ever had with Housman. Housman, in his Leslie Stephen lecture *The Name & Nature of Poetry*, had denied very emphatically that he was a 'critic'; but he had denied it in what seemed to me a singularly perverse way, & had expressed an admiration for literary criticism which startled & scandalized me.

He had begun with a quotation from his inaugural lecture, delivered 22 years before–

> Whether the faculty of literary criticism is the best gift that Heaven has in its treasuries, I cannot say; but Heaven seems to think so, for assuredly it is the gift most charily bestowed. Orators & poets . . ., if rare in comparison with blackberries, are commoner than returns of Halley's comet: literary critics are less common . . ..

& he had continued—

> In these 22 years I have improved in some respects & deteriorated in others, but I have not so much improved as to become a literary critic, nor so much deteriorated as to fancy that I have become one.

It had seemed to me deplorable that a great scholar & a fine poet should write like this, &, finding myself next to him in Hall a few weeks later, I plunged in & said so. Did he really mean what he had said to be taken very seriously? Would the life of the best of critics really have seemed to him comparable with that of a scholar & a poet? We argued these questions all through dinner, & I think that finally he agreed with me. I must not seem to claim a dialectical triumph over a man who can no longer contradict me; but 'Perhaps not entirely' was, in the end, his reply to the 1st question, & 'Probably no' to the 2nd.

There may have been some doubt about Housman's feelings, & I do not wish to claim him as on my side; but there is no doubt at all about the feelings of men of science, & I share them fully. If then I find myself writing, not mathematics but

'about' mathematics, it is a confession of weakness, for which I may rightly be scorned or pitied by younger & more vigorous mathematicians. I write about mathematics because, like any other mathematician who has passed 60, I have no longer the freshness of mind, the energy, or the patience to carry on effectively with my proper job." – Hardy, 1992, pp. 61–63

$\boxed{2}$ "I propose to put forward an apology for mathematics; & I may be told that it needs none, since there are now few studies more generally recognized, for good reasons or bad, as profitable & praiseworthy. This may be true; indeed it is probable, since the sensational triumphs of Einstein, that stellar astronomy & atomic physics are the only sciences which stand higher in popular estimation. A mathematician need not now consider himself on the defensive. He does not have to meet the sort of opposition described by Bradley in the admirable defence of metaphysics which forms the introduction to *Appearance & Reality.*

A metaphysician, says Bradley, will be told that 'metaphysical knowledge is wholly impossible', or that 'even if possible to a certain degree, it is practically no knowledge worth the name'. 'The same problems,' he will hear, 'the same disputes, the same sheer failure. Why not abandon it & come out? Is there nothing else more worth your labor?' There is no one so stupid as to use this sort of language about mathematics. The mass of mathematical truth is obvious & imposing; its practical applications, the bridges & steam-engines & dynamos, obtrude themselves on the dullest imagination. The public does not need to be convinced that there is something in mathematics.

All this is in its way very comforting to mathematicians, but it is hardly possible for a genuine mathematician to be content with it. Any genuine mathematician must feel that it is not on these crude achievements that the real case for mathematics rests, that the popular reputation of mathematics is based largely on ignorance & confusion, & that there is room for a more rational defence. At any rate, I am disposed to try to make one. It should be a simpler task than Bradley's difficult apology.

I shall ask, then, *why is it really worth while to make a serious study of mathematics? What is the proper justification of a mathematician's life?* & my answers will be, for the most part, such as are to be expected from a mathematician: I think that it is worth while, that there is ample justification. But I should say at once that my defence of mathematics will be a defence of myself, & that my apology is bound to be to some extent egotistical. I should not think it worth while to apologize for my subject if I regarded myself as 1 of its failures.

Some egotism of this sort is inevitable, & I do not feel that it really needs justification. Good work is not done by 'humble' men. It is 1 of the 1st duties of a professor, e.g., in any subject, to exaggerate a little both the importance of his subject & his own importance it it. A man who is always asking 'Is what I do worth while?' & 'Am I the right person to do it?' will always be ineffective himself & a discouragement to others. He must shut his eyes a little & think a little more of his subject & himself than they deserve. This is not too difficult: it is harder not to make his subject & himself ridiculous by shutting his eyes too tightly." – Hardy, 1992, pp. 63–66

$\boxed{3}$ "A man who sets out to justify his existence & his activities has to distinguish 2 different questions. The 1st is whether the work which he does is worth doing; & the 2nd is why he does it, whatever its value may be. The 1st question is often very difficult, & the answer very discouraging, but most people will find the 2nd easy enough even then. Their answers, if they are honest, will usually take 1 or other of 2 forms; & the 2nd form is merely a humbler variation of the 1st, which is the only answer which we need consider seriously.

(1) 'I do what I do because it is the one & only thing that I can do at all well. I am a lawyer, or a stockbroker, or a professional cricketer, because I have some real talent for that particular job. I am a lawyer because I have a fluent tongue, & am interested in legal subtleties; I am a stockbroker because my judgment of the markets is quick & sound; I am a professional cricketer because I can bat unusually well. I agree that it might be better to be a poet or a mathematician, but unfortunately I have no talent for such pursuits.'

I am not suggesting that this is a defence by which can be made by most people, since most people can do nothing at all well. But it is impregnable when it can be made without absurdity, as it can by a substantial minority: perhaps 5 or even 10% of men can do something rather well. It is a tiny minority who can do anything *really* well, & the number of men who can do 2 things well is negligible. If a man has any genuine talent, he should be ready to make almost any sacrifice in order to cultivate it to the full.

This view was endorsed by Dr Johnson—

> When I told him that I had been to see [his namesake] Johnson ride upon 3 horses, he said 'Such a man, sir, should be encouraged, for his performances show the extent of the human powers . . .'—

& similarly he would have an applauded mountain climbers, channel swimmers, & blindfold chess-players. For my own part, I am entirely in sympathy with all such attempts at remarkable achievement. I feel some sympathy even with conjurors & ventriloquists; & when Alekhine & Bradman set out to beat records, I am quite bitterly disappointed if they fail. & here both Dr Johnson & I find ourselves in agreement with the public. As W. J. Turner has said so truly, it is only the 'highbrows' (in the unpleasant sense) who do not admire the 'real swell'.

We have of course to take account of the differences in value between different activities. I would rather be a novelist or a painter than a statesman of similar rank; & there are many roads to fame which most of us would reject as actively pernicious. Yet it is seldom that such differences of value will turn the scale in a man's choice of a career, which will almost always be dictated by the limitations of his natural abilities. Poetry is more valuable than cricket, but Bradman would be

a fool if he sacrificed his cricket in order to write 2nd-rate minor poetry (& I suppose that it is unlikely that he could do better). If the cricket were a little less supreme, & the poetry better, then the choice might be more difficult: I do not know whether I would rather have been Victor Trumper or Rupert Brooke. It is fortunate that such dilemmas occur so seldom.

I may add that they are particularly unlikely to present themselves to a mathematician. It is usual to exaggerate rather grossly the differences between the mental processes of mathematicians & other people, but it is undeniable that a gift for mathematics is 1 of the most specialized talents, & that mathematicians as a class are not particularly distinguished for general ability or versatility. If a man is in any sense a real mathematician, then it is 100 to 1 that his mathematics will be far better than anything else he can do, & that he would be silly if he surrendered any decent opportunity of exercising his 1 talent in order to do undistinguished work in other fields. Such a sacrifice could be justified only by economic necessity or age." – Hardy, 1992, pp. 66–70

$\boxed{4}$ "I had better say something here about this question of age, since it is particularly important for mathematicians. No mathematician should ever allow himself to forget that mathematics, more than any other art or science, is a young man's game. To take a simple illustration at a comparatively humble level, the average age of election to the Royal Society is lowest in mathematics.

We can naturally find much more striking illustrations. We may consider, e.g., the career of a man who was certainly 1 of the world's 3 greatest mathematicians. Newton gave up mathematics at 50, & had lost his enthusiasm long before; he had recognized no doubt by the time that he was 40 that his great creative days were over. His greatest ideas of all, fluxions & the law of gravitation, came to him about 1666, when he was 24 – 'in those days I was in the prime of my age for invention, & minded mathematics & philosophy more than at any time since.' He made big discoveries until he was nearly 40 (the 'elliptic orbit' at 37), but after that he did little but polish & perfect.

Galois died at 21, Abel at 27, Ramanujan at 33, Riemann at 40. There have been men who have done great work a good deal later; Gauss's great memoir on differential geometry was published when he was 50 (though he had had the fundamental ideas 10 years before). I do not know an instance of a major mathematical advance initiated by a man past 50. If a man of mature age loses interest in & abandons mathematics, the loss is not likely to be very serious either for mathematics or for himself.

On the other hand the gain is no more likely to be substantial; the later records of mathematicians who have left mathematics are not particularly encouraging. Newton made a quite competent Master of the Mint (when he was not quarrelling with anybody). Painlevé was a not very successful Premier of France. Laplace's political career was highly discreditable, but he is hardly a fair instance, since he was dishonest rather than incompetent, & never really 'gave up' mathematics. It is very hard to find an instance of a 1st-rate mathematician who has abandoned mathematics & attained 1st-rate distinction in any other field.[1] There may have been young men who would have been 1st-rate mathematicians if they had stuck to mathematics, but I have never heard of a really plausible example. & all this is fully borne out by my own very limited experience. Every young mathematician of real talent whom I have known has been faithful to mathematics, & not from lack of ambition but from abundance of it; they have all recognized that there, if anywhere, lay the road to a life of any distinction." – Hardy, 1992, pp. 70–73

$\boxed{5}$ "There is also what I called the 'humbler variation' of the standard apology; but I may dismiss this in a very few words.

(2) 'There is *nothing* that I can do particularly well. I do what I do because it came my way. I really never had a chance of doing anything else.' & this apology too I accept as conclusive. It is quite true that most people can do nothing well. if so, it matters very little what career they choose, & there is really nothing more to say about it. It is a conclusively reply, but hardly one likely to be made by a man with any pride; & I may assume that none of us would be content with it." – Hardy, 1992, p. 73

$\boxed{6}$ "It is time to begin thinking about the 1st question which I put in §3, & which is so much more difficult than the 2nd. Is mathematics, what I & other mathematicians mean by mathematics, worth doing; & if so, why?

I have been looking again at the 1st pages of the inaugural lecture which I gave at Oxford in 1920, where there is an outline of an apology for mathematics. It is very inadequate (less than a couple of pages), & it is written in a style (a 1st essay, I suppose, in what I then imagined to be the 'Oxford manner') of which I am not now particularly proud; but I still feel that, however much development it may need, it contains the essentials of the matter. I will resume what I said then, as a preface to a fuller discussion.

(1) I began by laying stress on the *harmlessness* of mathematics – 'the study of mathematics is, if an unprofitable, a perfectly harmless & innocent occupation'. I shall stick to that, but obviously it will need a good deal of expansion & explanation.

Is mathematics 'unprofitable'? In some ways, plainly, it is not; e.g., it gives great pleasure to quite a large number of people. I was thinking of 'profit', however, in a narrower sense. Is mathematics 'useful', *directly* useful, as other sciences such as chemistry & physiology are? This is not an altogether easy or uncontroversial question, & I shall ultimately say No, though some mathematicians, & most outsiders, would no doubt say Yes. & is mathematics 'harmless'? Again the answer is not obvious, & the question is one which I should have in some ways preferred to avoid, since it raises the whole problem

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[1]Pascal seems the best.

of the effect of science on war. Is mathematics harmless, in the sense in which, e.g., chemistry plainly is not? I shall have to come back to both these questions later.

(2) I went on to say that 'the scale of the universe is large & if we are wasting our time, the waste of the lives of a few university dons is no such overwhelming catastrophe': & here I may seem to be adopting, or affecting, the pose of exaggerated humility which I repudiated a moment ago. I am sure that that was not what was really in my mind; I was trying to say in a sentence what I have said at much greater length in §3. I was assuming that we dons really had our little talents, & that we could hardly be wrong if we did our best to cultivate them fully.

(3) Finally (in what seem to me now some rather painfully rhetorical sentences) I emphasized the permanence of mathematical achievement—

> What we do may be small, but it has a certain character of permanence; & to have produced anything of the slightest permanent interest, whether it be a copy of verses or a geometrical theorem, is to have done something utterly beyond the powers of the vast majority of men.

&—

> In these days of conflict between ancient & modern studies, there must surely be something to be said for a study which did not begin with Pythagoras, & will not end with Einstein, but is the oldest & the youngest of all.

All this is 'rhetoric'; but the substance of it seems to me still to ring true, & I can expand it at once without prejudging any of the other questions which I am leaving open." – Hardy, 1992, pp. 74–77

| 7 | "I shall assume that I am writing for readers who are full, or have in the past been full, of a proper spirit of ambition. A man's 1st duty, a young man's at any rate, is to be ambitious. Ambition is a noble passion which may legitimately take many forms; there was *something* noble in the ambition of Attila or Napoleon: but the noblest ambition is that of leaving behind one something of permanent value—

> Here, on the level sand,
> Between the sea & land,
> What shall I build or write
> Against the fall of night?
> Tell me of runes to grave
> That hold the bursting wave
> Or bastions to design
> For longer date than mine.

Ambition has been the driving force behind nearly all the best work of the world. In particular, practically all substantial contributions to human happiness have been made by ambitious men. To take 2 famous examples, were not Lister & Pasteur ambitious? Or, on a humbler level, King Gillette & William Willett; & who in recent times have contributed more to human comfort than they?

Physiology provides particularly good examples just because it is so obviously a 'beneficial' study. We must guard against a fallacy common among apologists of science, the fallacy of supposing that the men whose work most benefits humanity are thinking much of that while they do it, that physiologists, e.g., have particularly noble souls. A physiologist may indeed be glad to remember that his work will benefit mankind, but the motives which provide the force & the inspiration for it are indistinguishable from those of a classical scholar or a mathematician.

There are many highly respectable motives which may lead men to prosecute research, but 3 which are much more important than the rest. The 1st (without which the rest must come to nothing) is intellectual curiosity, desire to know the truth. Then, professional pride, anxiety to be satisfied with one's performance, the shame that overcomes any self-respecting craftsman when his work is unworthy of his talent. Finally, ambition, desire for reputation, & the position, even the power or the money, which it brings. It may be fine to feel, when you have done your work, that you have added to the happiness or alleviated the sufferings of others, but that will not by why you did it. So if a mathematician, or a chemist, or even a physiologist, were to tell me that the driving force in his work had been the desire to benefit humanity, then I should not believe him (nor should I think the better of him if I did). His dominant motives have been those which I have started, & in which, surely, there is nothing of which any decent man need be ashamed." – Hardy, 1992, pp. 77–79

| 8 | "If intellectual curiosity, professional pride, & ambition are the dominant incentives to research, then assuredly no one has a fairer chance of gratifying them than a mathematician. His subject is the most curious of all – there is non in which truth plays such odd pranks. It has the most elaborate & the most fascinating technique, & gives unrivalled openings for the display of sheer professional skill. Finally, as history proves abundantly, mathematical achievement, whatever its intrinsic worth, is the most enduring of all.

We can see this even in semi-historic civilizations. The Babylonian & Assyrian civilizations have perished; Hammurabi, Sargon, & Nebuchadnezzar are empty names; yet Babylonian mathematics is still interesting, & the Babylonian scale of 60 is still used in astronomy. But of course the crucial case is that of the Greeks.

The Greeks were the 1st mathematicians who are still 'real' to us to-day. Oriental mathematics may be an interesting curiosity, but Greek mathematics is the real thing. The Greeks 1st spoke a language which modern mathematicians can understand; as Littlewood said to me once, they are not clever schoolboys or 'scholarship candidates', but 'Fellows of another college'. So Greek mathematics is 'permanent', more permanent even than Greek literature. Archimedes will be remembered when Aeschylus is forgotten, because languages die & mathematical ideas do not. 'Immortality' may be a silly word, but probably a mathematician has the best chance of whatever it may mean.

Nor need he fear very seriously that the future will be unjust to him. Immortality is often ridiculous or cruel: few of us would have chosen to the Og or Ananias or Gallio. Even in mathematics, history sometimes plays strange tricks; Rolle figures in the text-books of elementary calculus as if he had been a mathematician like Newton; Farey is immortal because he failed to understand a theorem which Haros had prove perfectly 14 years before; the names of 5 worthy Norwegians still stand in Abel's *Life*, just fo 1 act of conscientious imbecility, dutifully performed at the expense of their country's greatest man. But on the whole the history of science is far, & this is particularly true in mathematics. No other subject has such clear-cut or unanimously accepted standards, & the men who are remembered are almost always the men who merit it. Mathematical fame, if you have the cash to pay for it, is 1 of the soundest & steadiest of investments." – Hardy, 1992, pp. 80–82

9 "All this is very comforting for dons, & especially for professors of mathematics. It is sometimes suggested, by lawyers or politicians or business men, that an academic career is one sought mainly by cautious & unambitious persons who are primarily for comfort & security. The reproach is quite misplaced. A don surrenders something, & in particular the chance of making large sums of money – it is very hard for a professor to make £2000 a year; & security of tenure is naturally 1 of the considerations which make this particular surrender easy. That is not why Housman would have refused to be Lord Simon or Lord Beaverbrook. He would have rejected their careers because of his ambition, because he would have scorned to be a man to be forgotten in 20 years.

Yet how painful it is to feel that, with all these advantages, one may fail. I can remember Bertrand Russel telling me of a horrible dream. He was in the top floor of the University Library, about A.D. 2100. A library assistant was going round the shelves carrying an enormous bucket, taking down book after book, glancing at them, restoring them to the shelves or dumping them into the bucket. At last he came to 3 large volumes which Russel could recognize as the last surviving copy of *Principia mathematica*. He took down 1 of the volumes, turned over a few pages, seemed puzzled for a moment by the curious symbolism, closed the volume, balanced it in his hand & hesitated …." – Hardy, 1992, pp. 82–83

10 "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 & colors, a poet with words. A painting may embody an 'idea', but the idea is usually commonplace & 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, & could ideas be at once more trite & 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, & 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 colors or the words, must fit together in a harmonious way. Beauty is the 1st test: there is no permanent place in the world for ugly mathematics. & here I must deal with a misconception which is still widespread (though probably much less so now than it was 20 years ago), what Whitehead has called the 'literary superstition' that love of & 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 Prof. 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, & they feel 'coldly' (& 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; & 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; & 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; & every chess-player can recognize & 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), & 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, & 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', & nothing else has quite the kick of mathematics.

I might add that there is nothing in the world which pleases even famous men (& 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 20 (not knowing that it had been proved over 2000 years before by Plato). Prof. Soddy is a more recent & a more striking example (but *his* theorem really is his own)[2]" – Hardy, 1992, pp. 84–88

11 "A chess problem is genuine mathematics, but it is in some way 'trivial' mathematics. However ingenious & intricate, however original & 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, & '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, & 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 & 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 & 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 1 more point which I shall discuss very shortly, not because it is uninteresting but because it is difficult, & 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 2 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, & in this case the ideas have significance & the thesis is sound, so that our emotions are stirred much more deeply. The ideas do matter to the pattern, even in poetry, & much more, naturally, in mathematics; but I must not try to argue the question seriously." – Hardy, 1992, pp. 88–91

12 "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 1st-rate. & here I am very heavily handicapped by the restrictions under which I am writing. On the 1 hand my examples must be very simple, & intelligible to a reader who has no specialized mathematical knowledge; no elaborate preliminary explanations must be needed; & a reader must be able to follow the proofs as well as the enunciations. These conditions exclude, e.g., many of the most beautiful theorems of the theory of numbers, such as Fermat's '2 square' theorem or the law of quadratic reciprocity. & on the other hand my examples should be drawn from 'pukka' mathematics, the mathematics of the working professional mathematician; & this condition excludes a good deal which it would be comparatively easy to make intelligible but which trespasses on logic & mathematical philosophy.

I can hardly do better than go back to the Greeks. I will state & prove 2 of the famous theorems of Greek mathematics. They are 'simple' theorems, simple both in idea & in execution, but there is no doubt at all about their being theorems of the highest class. Each is as fresh & significant as when it was discovered – 2000 years have not written a wrinkle on either of them. Finally, both the statements & the proofs can be mastered in an hour by any intelligent reader, however slender his mathematical equipment.

1. The 1st is Euclid's[3] 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, . . . which cannot be resolved into smaller factors[4] Thus 37 & 317 are prime. The primes are the material out of which all numbers are built up by multiplication:

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

[3]*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.

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

thus $666 = 2 \cdot 3 \cdot 3 \cdot 37$. Every number which is no prime itself is divisible by at least 1 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, & that $2, 3, 5, \ldots, P$ is the complete series (so that P is the largest prime); & let us, on this hypothesis, consider the number Q defined by the formula $Q = 2 \cdot 3 \cdot 5 \cdot \cdots \cdot P + 1$. It is plain that Q is not divisible by any of $2, 3, 5, \ldots, P$; for it leaves the remainder 1 when divided by any 1 of these numbers. But, if not itself prime, it is divisible by *some* prime, & 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; & therefore this hypothesis is false.

The proof is by *reductio ad absurdum*, & *reductio ad absurdum*, which Euclid loved so much, is 1 of a mathematician's finest weapons[5] 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*." – Hardy, 1992, pp. 91–94

13 "My 2nd example is Pythagoras's[6] proof of the 'irrationality' of $\sqrt{2}$.

A 'rational number' is a fraction $\frac{a}{b}$, where $a, b \in \mathbb{Z}$; we may suppose that a & 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 $\left(\frac{a}{b}\right)^2$; & this is the same thing as saying that the equation (B) $a^2 = 2b^2$ cannot be satisfied by integral values of a & 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, b being integers without any common factor. It follows from (B) that a^2 is even (since $2b^2$ is divisible by 2), & therefore that a is even (since the square of an odd number is odd). If a is even than (C) $a = 2c$ for some integral value of c; & therefore $2b^2 = a^2 = (2c)^2 = 4c^2$ or (D) $b^2 = 2c^2$. Hence b^2 is even, & therefore (for the same reason as before) b is even. I.e., a & b are both even, & so have the common factor 2. This contradicts our hypothesis, & 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, & the length of the diagonal is d, then, by a very familiar theorem also ascribed to Pythagoras[7], $d^2 = 1^2 + 1^2 = 2$, 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. E.g., there is what is called 'the fundamental theorem of arithmetic', that any integer can be resolved, *in 1 way only*, into a product of primes. Thus $666 = 2 \cdot 3 \cdot 3 \cdot 37$, & there is no other decomposition; it is impossible that $666 = 2 \cdot 11 \cdot 29$ or that $13 \cdot 89 = 17 \cdot 73$ (& 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 & might be found tedious by an unmathematical reader.

Another famous & beautiful theorem is Fermat's '2 square' theorem. The primes may (if we ignore the special prime 2) be arranged in 2 classes; the primes $5, 13, 17, 29, 37, 41, \ldots$ which leave remainder 1 when divided by 4, & the primes $3, 7, 11, 19, 23, 31, \ldots$ which leave remainder 3. All the primes of the 1st class, & none of the 2nd, can be expressed as the sum of 2 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, & 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 1 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. So I will not try to give more examples. Those which I have given are test cases, & 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, & that beauty & seriousness were the criteria by which his patterns should be judged. I can hardly believe that anyone who has understood the 2 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, & also much more beautiful; can we define, a little more closely, where their superiority lies?" – Hardy, 1992, pp. 94–99

14 "In the 1st place, the superiority of the mathematical theorems in *seriousness* is obvious & 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 & have no external repercussions. We should think in the same way if chess had never been invented, whereas the theorems of Euclid & 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, & Euclid's theorem assures us that we have plenty of material for the task. But the theorem of Pythagoras has wider applications & provides a better text.

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

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

[7]Euclid, *Elements* I 47.

We should observe 1st that Pythagoras's argument is capable of far-reaching extension, & can be applied, with little change of principle, to very wide classes of 'irrationals'. We can prove very similarly (as Theodorus seems to have done) that $\sqrt{3}, \sqrt{5}, \sqrt{7}, \sqrt{11}, \sqrt{13}, \sqrt{17}$ are irrational, or (going beyond Theodorus) that $\sqrt[3]{2}$ & $\sqrt[3]{17}$ are irrational[8].

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 & its extensions tell 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 & 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 recognized 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 2 lengths, e.g., are multiples of some common unit, & they had constructed a theory of proportion based on this assumption. Pythagoras's discovery exposed the unsoundness of this foundation, & led to the construction of the much more profound theory of Eudoxus which is set out in the 5th book of the *Elements*, & which is regarded by many modern mathematicians as the finest achievement of Greek mathematics. This theory is astonishingly modern in spirit, & may be regarded as the beginning of the modern theory of irrational number, which has revolutionized mathematical analysis & 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 & atomic physics deal with 'large' numbers, & 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 10 significant figures. Then 3.14159265 (the value of π to 8 places of decimals) is the ratio $\frac{314159265}{100000000}$ of 2 numbers of 9 digits. The number of primes $< 10^9$ is 50847478: that is enough for an engineer, & he can be perfectly happy without the rest. So much for Euclid's theorem; &, as regards Pythagoras's, it is obvious that irrationals are uninteresting to an engineer, since he is concerned only with approximations, & all approximations are rational." – Hardy, 1992, pp. 99–102

15 "A 'serious' theorem is a theorem which contains 'significant' ideas, & I suppose that I ought to try to analyze a little more closely the qualities which make a mathematical idea significant. This is very difficult, & it is unlikely that any analysis which I can give will be very valuable. We can recognize a 'significant' idea when we see it, as we can those which occur in my 2 standard theorems; but this power of recognition requires a rather high degree of mathematical sophistication, & of that familiarity with mathematical ideas which comes only from many years spent in their company. So I must attempt some sort of analysis; & it should be possible to make one which, however inadequate, is sound & intelligible so far as it goes. There are 2 things at any rate which seem essential, a certain *generality* & a certain *depth*; but neither quality is easy to define at all precisely.

A significant mathematical idea, a serious mathematical theorem, should be 'general' in some such sense as this. The idea should be one which is a constituent in many mathematical constructs, which is used in the proof of theorems of many different kinds. The theorem should be one which, even if stated originally (like Pythagoras's theorem) in a quite special form, is capable of considerable extension & is typical of a whole class of theorems of its kind. The relations revealed by the proof should be such as connect many different mathematical ideas. All this is very vague, & subject to many reservations. But it is easy enough to see that a theorem is unlikely to be serious when it lacks these qualities conspicuously; we have only to take examples from the isolated curiosities in which arithmetic abounds. I take 2, almost at random, from Rouse Ball's *Mathematical Recreations*[9]

(a) 8712 & 9801 are the only 4-figure numbers which are integral multiples of their 'reversals': $8712 = 4 \cdot 2178$, $9801 = 9 \cdot 1089$, & there are no other numbers below 10000 which have this property.

(b) There are just 4 numbers (after 1) which are the sums of the cubes of their digits, viz. $153 = 1^3 + 5^3 + 3^3$, $370 = 3^3 + 7^3 + 0^3$, $371 = 3^3 + 7^3 + 1^3$, $407 = 4^3 + 0^3 + 7^3$.

These are odd facts, very suitable for puzzle columns & likely to amuse amateurs, but there is nothing in them which appeals much to a mathematician. The proofs are neither difficult nor interesting – merely a little tiresome. The theorems are not serious; & it is plain that 1 reason (though perhaps not the most important) is the extreme speciality of both the enunciations & the proofs, which are not capable of any significant generalization." – Hardy, 1992, pp. 103–105

16 "'Generality' is an ambiguous & rather dangerous word, & we must be careful not to allow it to dominate our discussion too much. It is used in various senses both in mathematics & in writings about mathematics, & there is 1 of these in particular, on which logicians have very properly laid great stress, which is entirely irrelevant here. In this sense, which is quite easy to define, *all* mathematical theorems are equally & completely 'general'.

'The certainty of mathematics', says Whitehead[10], 'depends on its complete abstract generality.' When we assert that $2 + 3 = 5$, we are asserting a relation between 3 groups of 'things'; & these 'things' are not apples or pennies, or things of any 1 particular sort or another, but *just* things, 'any old things'. The meaning of the statement is entirely independent of the

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[8]See Ch. IV of Hardy & Wright's *Introduction to the Theory of Numbers*, where there are discussions of different generalizations of Pythagoras's argument, & of a historical puzzle about Theodorus.

[9]11th edition, 1939 (revised by H. S. M. Coxeter).

[10]*Science & Modern World*, p. 33.

individualities of the members of the groups. All mathematical 'objects' or 'entities' or 'relations', such as '2', '3', '5', '+', or '=', & all mathematical propositions in which they occur, are completely general in the sense of being completely abstract. Indeed 1 of Whitehead's words is superfluous, since generality, in this sense, *is* abstractness.

This sense of the word is important, & the logicians are quite right to stress it, since it embodies a truism which a good many people who ought to know better are apt to forget. It is quite common, e.g., for an astronomer or a physicist to claim that he has found a 'mathematical proof' that the physical universe must behave in a particular way. All such claims, if interpreted literally, are strictly nonsense. It *cannot* be possible to prove mathematically that there will be an eclipse to-morrow, because eclipses, & other physical phenomena, do not form part of the abstract world of mathematics; & this, I suppose, all astronomers would admit when pressed, however many eclipses they have predicted correctly.

It is obvious that we are not concerned with this sort of 'generality' now. We are looking for *differences* of generality between 1 mathematical theorem & another, & in Whitehead's sense all are equally general. Thus the 'trivial' theorems (a) & (b) of §15are just as 'abstract' or 'general' as those of Euclid & Pythagoras, & so is a chess problem. It makes no difference to a chess problem whether the pieces are white & black, or red & green, or whether there are physical 'pieces' at all ; it is the *same* problem which an expert carries easily in his head & which we have to reconstruct laboriously with the aid of the board. The board & the pieces are mere devices to stimulate our sluggish imaginations, & are no more essential to the problem than the blackboard & the chalk are to the theorems in a mathematical lecture.

It is not this kind of generality, common to all mathematical theorems, which we are looking for now, but the more subtle & elusive kind of generality which I tried to describe in rough terms in §15. & we must be careful not to lay *too* much stress even on generality of this kind (as I think logicians like Whitehead tend to do). It is not mere 'piling of subtlety of generalization upon subtlety of generalization'[11] which is the outstanding achievement of modern mathematics. Some measure of generality must be present in any high-class theorem, but *too much* tends inevitably to insipidity. 'Everything is what it is, & not another thing', & the differences between things are quite as interesting as their resemblances. We do not choose our friends because they embody all the pleasant qualities of humanity, but because they are the people that they are. & so in mathematics; a property common to too many objects can hardly be very exciting, & mathematical ideas also become dim unless they have plenty of individuality. Here at any rate I can quote Whitehead on my side: 'it is the large generalization, limited by a happy particularity, which is the fruitful conception[12].'' – Hardy, 1992, pp. 105–109

17 "The 2nd quality which I demanded in a significant idea was *depth*, & this is still more difficult to define. It has *something* to do with *difficulty*; the 'deeper' ideas are usually the harder to grasp: but it is not at all the same. The ideas underlying Pythagoras's theorem & its generalizations are quite deep, but no mathematician now would find them difficult. On the other hand a theorem may be essentially superficial & yet quite difficult to prove (as are many 'Diophantine' theorems, i.e. theorems about the solution of equations in integers).

It seems that mathematical ideas are arranged somehow in strata, the ideas in each stratum being linked by a complex of relations both among themselves & with those above & below. The lower the stratum, the deeper (& in general the more difficult) the idea. Thus the idea of an 'irrational' is deeper than that of an integer; & Pythagoras's theorem is, for that reason, deeper than Euclid's.

Let us concentrate our attention on the relations between the integers, or some other group of objects lying in some particular stratum. Then it may happen that 1 of these relations can be comprehended completely, that we can recognize & prove, e.g., some property of the integers, without any knowledge of the contents of lower strata. Thus we proved Euclid's theorem by consideration of properties of integers only. But there are also many theorems about integers which we cannot appreciate properly, & still less prove, without digging deeper & considering what happens below.

It is easy to find examples in the theory of prime numbers. Euclid's theorem is very important, but not very deep: we can prove that there are infinitely many primes without using any notion deeper than that of 'divisibility'. But new questions suggest themselves as soon as we know the answer to this one. There is an infinity of primes, but how is this infinity distributed? Given a large number $\mathcal{N}$, say 10^{80} or $10^{10^{10}}$, [13] about how many primes are there less than $\mathcal{N}$?[14] When we ask *these* questions, we find ourselves in a quite different position. We can answer them, with rather surprising accuracy, but only by boring much deeper, leaving the integers above us for a while, & using the most powerful weapons of the modern theory of functions. Thus the theorem which answers our questions (the so-called 'Prime Number Theorem') is a much deeper theorem than Euclid's or even Pythagoras's.

I could multiply examples, but this notion of 'depth' is an elusive one even for a mathematician who can recognize it, & I can hardly suppose that I could say anything more about it here which would be of much help to other readers." – Hardy, 1992, pp. 109–112

18 "There is still 1 point remaining over from §11, where I started the comparison between 'real mathematics' & chess. We may take it for granted now that in substance, seriousness, significance, the advantage of the real mathematical theorem is overwhelming. It is almost equally obvious, to a trained intelligence, that it has a great advantage in beauty also; but

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[11]*Science & Modern World*, p. 44.

[12]*Science & Modern World*, p. 46.

[13]It is supposed that the number of protons in the universe is about 10^{80}. The number $10^{10^{10}}$, if written at length, would occupy about 50000 volumes of average size.

[14]As I mentioned in §14, there are 50847478 primes less than 10^9; but that is as far as our *exact* knowledge extends.

this advantage is much harder to define or locate, since the *main* defect of the chess problem is plainly its 'triviality', & the contrast in this respect mingles with & disturbs any more purely aesthetic judgment. What 'purely aesthetic' qualities can we distinguish in such theorems as Euclid's & Pythagoras's? I will not risk more than a few disjointed remarks.

In both theorems (& in the theorems, of course I include the proofs) there is a very high degree of *unexpectedness*, combined with *inevitability & economy*. The arguments take so odd & surprising a form; the weapons used seem so childishly simple when compared with the far-reaching results; but there is no escape from the conclusions. There are no complications of detail – 1 link of attack is enough in each case; & this is true too of the proofs of many much more difficult theorems, the full appreciation of which demands quite a high degree of technical proficiency. We do not want many 'variations' in the proof of a mathematical theorem: 'enumeration of cases', indeed, is 1 of the duller forms of mathematical argument. A mathematical proof should resemble a simple & clear-cut constellation, not a scattered cluster in the Milky Way.

A chess problem also has unexpectedness, & a certain economy; it is essential that the moves should be surprising, & that every piece on the board should play its part. But the aesthetic effect is cumulative. It is essential also (unless the problem is too simple to be really amusing) that the key-move should be followed by a good many variations, each requiring its own individual answer. 'If P-B5 then Kt-R6; if ... then ...; if ... then ...' – the effect would be spoilt if there were not a good many different replies. All this is quite genuine mathematics, & has its merits; but it is just that 'proof by enumeration of cases' (& of cases which do not, at bottom, differ at all profoundly[15]) which a real mathematician tends to despise.

I am inclined to think that I could reinforce my argument by appealing to the feelings of chess-players themselves. Surely a chess master, a player of great names & great matches, at bottom scorns a problemist's purely mathematical art. He has much of it in reserve himself, & can produce it in an emergency: 'if he had made such & such a move, then I had such & such a winning combination in mind.' But the 'great game' of chess is primarily psychological, a conflict between 1 trained intelligence & another, & not a mere collection of small mathematical theorems." – Hardy, 1992, pp. 112–115

19 "I must return to my Oxford apology, & examine a little more carefully some of the points which I postponed in §6. It will be obvious by now that I am interested in mathematics only as a creative art. But there are other questions to be considered, & in particular that of the 'utility' (or uselessness) of mathematics, about which there is much confusion of thought. We must also consider whether mathematics is really quite so 'harmless' as I took for granted in my Oxford lecture.

A science or an art may be said to be 'useful' if its development increases, even indirectly, the material well-being & comfort of men, if it promotes happiness, using that word in a crude & commonplace way. Thus medicine & physiology are useful because they relieve suffering, & engineering is useful because it helps us to build houses & bridges, & so to raise the standard of lief (engineering, of course, does harm as well, but that is not the question at the moment). Now some mathematics is certainly useful in this way; the engineers could not do their job without a fair working knowledge of mathematics, & mathematics is beginning to find applications even in physiology. So here we have a possible ground for a defense of mathematics; it may not be the best, or even a particularly strong defence, but it is one which we must examine. The 'nobler' uses of mathematics, if such they be, the uses which it shares with all creative art, will be irrelevant to our examination. Mathematics may, like poetry or music, 'promote & sustain a lofty habit of mind', & so increase the happiness of mathematicians & even of other people; but to defend it on that ground would be merely to elaborate what I have said already. What we have to consider now is the 'crude' utility of mathematics." – Hardy, 1992, pp. 115–117

20 "All this may seem very obvious, but even here there is often a good deal of confusion, since the most 'useful' subjects are quite commonly just those which it is most useless for most of us to learn. It is useful to have an adequate supply of physiologists & engineers; but physiology & engineering are not useful studies for ordinary men (though their study many of course be defended on other grounds). For my own part I have never once found myself in a position where such scientific knowledge as I possess, outside pure mathematics, has brought me the slightest advantage.

It is indeed rather astonishing how little practical value scientific knowledge has for ordinary men, how dull & commonplace such of it as has value is, & how its value seems almost to vary inversely to its reputed utility. It is useful to be tolerably quick at common arithmetic (& that, of course, is pure mathematics). It is useful to know a little French or German, a little history & geography, perhaps even a little economics. But a little chemistry, physics, or physiology has no value at all in ordinary life. We know that the gas will burn without knowing its constitution; when our cars break down we take them to a garage; when our stomach is out of order, we go to a doctor or a drugstore. We live either by rule of thumb or on other people's professional knowledge.

However, this is a side issue, a matter of pedagogy, interesting only to schoolmasters who have to advise parents clamoring for a 'useful' education for their sons. Of course we do not mean, when we say that physiology is useful, that most people ought to study physiology, but that the development of physiology by a handful of experts will increase the comfort of the majority. The questions which are important for us now are, how far mathematics can claim this sort of utility, what kinds of mathematics can make the strongest claims, & how far the intensive study of mathematics, as it is understood by mathematicians, can be justified on this ground alone." – Hardy, 1992, pp. 117–119

21 "It will probably be plain by now to what conclusions I am coming; so I will state them at once dogmatically & then elaborate them a little. It is undeniable that a good deal of elementary mathematics – & I use the word 'elementary' in the sense in which professional mathematicians use it, in which it includes, e.g., a fair working knowledge of the differential &

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[15]I believe that it is now regarded as a *merit* in a problem that there should be many variations of the same type.

integral calculus – has considerable practical utility. These parts of mathematics are, on the whole, rather dull; they are just the parts which have least aesthetic value. The 'real' mathematics of 'real' mathematicians, the mathematics of Fermat & Euler & Gauss & Abel & Riemann, is almost wholly 'useless' (& this is as true of 'applied' as of 'pure' mathematics). It is not possible to justify the life of any genuine professional mathematician on the ground of the 'utility' of his work.

But here I must deal with a misconception. It is sometimes suggested that pure mathematicians glory in the uselessness of their work[16], & make it a boast that it has no practical applications. The imputation is usually based on an incautious saying attributed to Gauss, to the effect that, if mathematics is the queen of the sciences, then the theory of numbers is, because of its supreme uselessness, the queen of mathematics – I have never been able to find an exact quotation. I am sure that Gauss's saying (if indeed it be his) has been rather crudely misinterpreted. If the theory of numbers could be employed for any practical & obviously honorable purpose, if it could be turned directly to the furtherance of human happiness or the relief of human suffering, as physiology & even chemistry can, then surely neither Gauss nor any other mathematician would have been so foolish as to decry or regret such applications. But science works for evil as well as for good (& particularly, of course, in time of war); & both Gauss & lesser mathematicians may be justified in rejoicing that there is 1 science at any rate, & that their own, whose very remoteness from ordinary human activities should keep it gentle & clean." – Hardy, 1992, pp. 119–121

22 "There is another misconception against which we must guard. It is quite natural to suppose that there is a great difference in utility between 'pure' & 'applied' mathematics. This is a delusion: there is a sharp distinction between the 2 kinds of mathematics, which I will explain in a moment, but it hardly affects their utility.

How do pure & applied mathematics differ from one another? This is a question which can be answered definitely & about which there is general agreement among mathematicians. There will be nothing in the least unorthodox about my answer, but it needs a little preface.

My next 2 sections will have a mildly philosophical flavor. The philosophy will not cut deep, or be in any way vital to my main theses; but I shall use words which are used very frequently with definite philosophical implications, & a reader might well become confused if I did not explain how I shall use them.

I have often used the adjective 'real', & as we use it commonly in conversation. I have spoken of 'real mathematics' & 'real mathematicians', as I might have spoken of 'real poetry' or 'real poets', & I shall continue to do so. But I shall also use the word 'reality', & with 2 different connotations.

In the 1st place, I shall speak of 'physical reality', & here again I shall be using the word in the ordinary sense. By physical reality I mean the material world, the world of day & night, earthquakes & eclipses, the world which physical science tries to describe.

I hardly suppose that, up to this point, any reader is likely to find trouble with my language, but now I am near to more difficult ground. For me, & I suppose for most mathematicians, there is another reality, which I will call 'mathematical reality'; & there is no sort of agreement about the nature of mathematical reality among either mathematicians or philosophers. Some hold that it is 'mental' & that in some sense we construct it, others that it is outside & independent of us. A man who could give a convincing account of mathematical reality would have solved very many of the most difficult problems of metaphysics. If he could include physical reality in his account, he would have solved them all.

I should not wish to argue any of these questions here even if I were competent to do so, but I will state my own position dogmatically in order to avoid minor misapprehensions. I believe that mathematical reality lies outside us, that our function is to discover or *observe* it, & that the theorems which we prove, & which we describe grandiloquently as our 'creations', are simply our notes of our observations. This view has been held, in 1 form or another, by many philosophers of high reputation from Plato onwards, & I shall use the language which is natural to a man who holds it. A reader who does not like the philosophy can alter the language: it will make very little difference to my conclusions." – Hardy, 1992, pp. 121–124

23 "The contrast between puer & applied mathematics stands out most clearly, perhaps, in geometry. There is the science of pure geometry[17] in which there are many geometries, projective geometry, Euclidean geometry, non-Euclidean geometry, & so forth. Each of these geometries is a *model*, a pattern of ideas, & is to be judged by the interest & beauty of its particular pattern. It is a *map* or *picture*, the joint product of many hands, a partial & imperfect copy (yet exact so far as it extends) of a section of mathematical reality. But the point which is important to us now is this, that there is 1 thing at any rate of which pure geometries are *not* pictures, & that is the spatio-temporal reality of the physical world. It is obvious, surely, that they cannot be, since earthquakes & eclipses are not mathematical concepts.

This may sound a little paradoxical to an outsider, but it is a truism to a geometer; & 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, & 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, 1st, 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, &, if I can do that, there would be

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[16]I have been accused of taking this view myself. I once said that 'a science is said to be useful if its development tends to accentuate the existing inequalities in the distribution of wealth, or more directly promotes the destruction of human life', & this sentence, written in 1915, has been quoted (for or against me) several times. It was of course a conscious rhetorical flourish, though one perhaps excusable at the time when it was written.

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

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, & has itself a certain pattern. The study of that pattern, & 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 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, & '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, & 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 2 sets of relations, & 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. 1 map, perhaps, will fit the facts better than others, & 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." – Hardy, 1992, pp. 124–128

$\boxed{24}$ "There is another remarks which suggests itself here & which physicists may find paradoxical, though the paradox will probably seem a good deal less than it did 18 years ago. I will express it in much the same words which I used in 1922 in an address to Section A of the British Association. My audience then was composed almost entirely of physicists, & I may have spoken a little provocatively on that account; but I would still stand by the substance of what I said.

I began by saying that there is probably less difference between the positions of a mathematician & of a physicist than is generally supposed, & that the most important seems to me to be this, that the mathematician is in much more direct contact with reality. This may seem a paradox, since it is the physicist who deals with the subject-matter usually described as 'real'; but a very little reflection is enough to show that the physicist's reality, whatever it may be, has few or none of the attributes which common sense ascribes instinctively to reality. A chair may be a collection of whirling electrons, or an idea in the mind of God: each of these accounts of it may have its merits, but neither conforms at all closely to the suggestions of common sense.

I went on to say that neither physicists nor philosophers have ever given any 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 calls 'real'. Thus we cannot be said to know what the subject-matter of physics is; but this need not prevent us from understanding roughly what a physicist is trying to do. It is plain that he is trying to correlate the incoherent body of crude fact confronting him with some definite & orderly scheme of abstract relations, the kind of scheme which he can borrow only from mathematics.

A mathematician, on the other hand, is working with his own mathematical reality. Of this reality, as I explained in §22, I take a 'realistic' & not an 'idealistic' view. At any rate (& this was my main point) this realistic view is much more plausible of mathematical than of physical reality, because mathematical objects are so much more what they seem. A chair or a star is not in the least like what it seems to be; the more we think of it, the fuzzier its outlines become in the haze of sensation which surrounds it; but '2' or '317' has nothing to do with sensation, & its properties stand out the more clearly the more closely we scrutinize it. It may be that modern physics fits best into some framework of idealistic philosophy – I do not believe it, but there are eminent physicists who say so. Pure mathematics, on the other hand, seems to me a rock on which all idealism founders: 317 is a prime, not because we think so, or because our minds are shaped in 1 way rather than another, but *because it is so*, because mathematical reality is built that way." – Hardy, 1992, pp. 128–130

$\boxed{25}$ "These distinctions between pure & applied mathematics are important in themselves, but they have very little bearing on our discussion of the 'usefulness' of mathematics. I spoke in §21 of the 'real' mathematics of Fermat & other great mathematicians, the mathematics which has permanent aesthetic value, as e.g. the best Greek mathematics has, the mathematics which is eternal because the best of it may, like the best literature, continue to cause intense emotional satisfaction to thousands of people after thousands of years. These men were all primarily pure mathematicians (though the distinction was naturally a good deal less sharp in their days than it is now); but I was not thinking only of pure mathematics. I count Maxwell & Einstein, Eddington & Dirac, among 'real' mathematicians. The great modern achievements of applied mathematics have been in relativity & quantum mechanics, & these subjects are, at present at any rate, almost as 'useless' as the theory of numbers. It is the dull & elementary parts of applied mathematics, as it is the dull & elementary parts of pure mathematics, that work for good or ill. Time may change all this. No one foresaw the applications of matrices & groups & other purely mathematical theories to modern physics, & it may be that some of the 'highbrow' applied mathematics will

become 'useful' in as unexpected a way' but the evidence so far points to the conclusion that, in 1 subject as in the other, it is what is commonplace & dull that counts for practical life.

I can remember Eddington giving a happy example of the unattractiveness of 'useful' science. The British Association held a meeting in Leeds, & it was thought that the members might like to hear something of the applications of science to the 'heavy woollen' industry. But the lectures & demonstrations arranged for this purpose were rather a fiasco. It appeared that the members (whether citizens of Leeds or not) wanted to be entertained, & that 'heavy wool' is not at all an entertaining subject. So the attendance at these lectures was very disappointing; but those who lectured on the excavations at Knossos, or on reality, or on the theory of prime numbers, were delighted by the audiences that they drew." – Hardy, 1992, pp. 131–133

26 "What parts of mathematics are useful?

1st, the bulk of school mathematics, arithmetic, elementary algebra, elementary Euclidean geometry, elementary differential & integral calculus. We must accept a certain amount of what is taught to 'specialists', such as projective geometry. In applied mathematics, the elements of mechanics (electricity, as taught in schools, must be classified as physics).

Next, a fair proportion of university mathematics is also useful, that part of it which is really a development of school mathematics with a more finished technique, & a certain amount of the more physical subjects such as electricity & hydromechanics. We must also remember that a reserve of knowledge is always an advantage, & that the most practical of mathematicians may be seriously handicapped if his knowledge is the bare minimum which is essential to him; & for this reason we must add a little under every heading. But our general conclusion must be that such mathematics is useful as is wanted by a superior engineer or a moderate physicist; & that is roughly the same thing as to say, such mathematics as has no particular aesthetic merit. Euclidean geometry, e.g., is useful in so far as it is dull – we do not want the axiomatics of parallels, or the theory of proportion, or the construction of the regular pentagon.

1 rather curious conclusion emerges, that pure mathematics is on the whole distinctly more useful than applied. A pure mathematician seems to have the advantage on the practical as well as on the aesthetic side. For what is useful above all is *technique*, & mathematical technique is taught mainly through pure mathematics.

I hope that I need not say that I am not trying to decry mathematical physics, a splendid subject with tremendous problems where the finest imaginations have run riot. But is not the position of an ordinary applied mathematician in some ways a little pathetic? If the wants to be useful, he must work in a humdrum way, & he cannot give full play to his fancy even when he wishes to rise to the heights. 'Imaginary' universes are so much more beautiful than this stupidly constructed 'real' one; & most of the finest products of an applied mathematician's fancy must be rejected, as soon as they have been created, for the brutal but sufficient reason that they do not fit the facts.

The general conclusion, surely, stands out plainly enough. If useful knowledge is, as we agreed provisionally to say, knowledge which is likely, now or in the comparatively near future, to contribute to the material comfort of mankind, so that mere intellectual satisfaction is irrelevant, then the great bulk of higher mathematics is useless. Modern geometry & algebra, the theory of numbers, the theory of aggregates & functions, relativity, quantum mechanics – no one of them stands the test much better than another, & there is no real mathematician whose life can be justified on this ground. If this be the test, then Abel, Riemann, & Poincaré wasted their lives; their contribution to human comfort was negligible, & the world would have been as happy a place without them." – Hardy, 1992, pp. 133–136

27 "It may be objected that my concept of 'utility' has been too narrow, that I have defined it in terms of 'happiness' or 'comfort' only, & have ignored the general 'social' effects of mathematics on which recent writers, with very different sympathies, have laid so much stress. Thus Whitehead (who has been a mathematician) speaks of 'the tremendous effect of mathematical knowledge on the lives of men, on their daily avocations, on the organization of society'; & Hogben (who is as unsympathetic to what i & other mathematicians call mathematics as Whitehead is sympathetic) says that 'without a knowledge of mathematics, the grammar of size & order, we cannot plan the rational society in which there will be leisure for all & poverty for none' (& much more to the same effect).

I cannot really believe that all this eloquence will do much to comfort mathematicians. The language of both writers is violently exaggerated, & both of them ignore very obvious distinctions. This is very natural in Hogben's case, since he is admittedly not a mathematician; he means by 'mathematics' the mathematics which he can understand, & which I have called 'school' mathematics. *This* mathematics has many uses, which I have admitted, which we can call 'social' if we please, & which Hogben enforces with many interesting appeals to the history of mathematical discovery. It is this which gives his book its merit, since it enables him to make plain, to many readers who never have been & never will be mathematicians, that there is more in mathematics than they thought. But he has hardly any understanding of 'real' mathematics (as any one who reads what he says about Pythagoras's theorem, or about Euclid & Einstein, can tell at once), & still less sympathy with it (as he spares no pains to show). 'Real' mathematics is to him merely an object of contemptuous pity.

It is not lack of understanding or of sympathy which is the trouble in Whitehead's case; but he forgets, in his enthusiasm, distinctions with which he is quite familiar. The mathematics which has this 'tremendous effect' on the 'daily avocations of men' & on 'the organization of society' is not the Whitehead but the Hogben mathematics. The mathematics which can be used 'for ordinary purposes by ordinary men' is negligible, & that which can be used by economists or sociologists hardly rises to 'scholarship standard'. The Whitehead mathematics may affect astronomy or physics profoundly, philosophy very appreciably – high thinking of 1 kind is always likely to affect high thinking of another – but it has extremely little effect on anything else. Its 'tremendous effects' have been, not on men generally, but on men like Whitehead himself." – Hardy, 1992,

pp. 136–138

28 "There are then 2 mathematics. There is the real mathematics of the real mathematicians, & there is what I will call the 'trivial' mathematics, for want of a better word. The trivial mathematics may be justified by arguments which would appeal to Hogben, or other writers of his school, but there is no such defence for the real mathematics, which must be justified as art if it can be justified at all. There is nothing in the least paradoxical or unusual in this view, which is that held commonly by mathematicians.

We have still 1 more question to consider. We have concluded that the trivial mathematics is, on the whole, useful, & that the real mathematics, on the whole, is not; that the trivial mathematics does, & the real mathematics does not, 'do good' in a certain sense; but we have still to ask whether either sort of mathematics does *harm*. It would be paradoxical to suggest that mathematics of any sort does much harm in time of peace, so that we are driven to the consideration of the effects of mathematics on war. It is very difficult to argue such questions at all dispassionately now, & I should have preferred to avoid them; but some sort of discussion seems inevitable. Fortunately, it need not be a long one.

There is 1 comforting conclusion which is easy for a real mathematician. Real mathematics has no effects on war. No one has yet discovered any warlike purpose to be served by the theory of numbers or relativity, & it seems very unlikely that anyone will do so for many years. It is true that there are branches of applied mathematics, such as ballistics & aerodynamics, which have been developed deliberately for war & demand a quite elaborate technique: it is perhaps hard to call them 'trivial', but none of them has any claim to rank as 'real'. They are indeed repulsively ugly & intolerably dull; even Littlewood could not make ballistics respectable, & if he could not who can? So a real mathematician has his conscience clear; there is nothing to be set against any value his work may have; mathematics is, as I said at Oxford, a 'harmless & innocent' occupation.

The trivial mathematics, on the other hand, has many applications in war. The gunnery experts & aeroplane designers, e.g., could not do their work without it. & the general effect of these applications is plain: mathematics facilitates (if not so obviously as physics or chemistry) modern, scientific, 'total' war.

It is not so clear as it might seem that this is to be regretted, since there are 2 sharply contrasted views about modern scientific war. The 1st & the most obvious is that the effect of science on war is merely to magnify its horror, both by increasing the sufferings of the minority who have to fight & by extending to other classes. This is the most natural & the orthodox view. But there is a very different view which seems also quite tenable, & which has been stated with great force by Haldane in *Callinicus*[18] It can be maintained that modern warfare is *less* horrible than the warfare of pre-scientific times; that bombs are probably more merciful than bayonets; that lachrymatory gas & mustard gas are perhaps the most humane weapons yet devised by military science; & that the orthodox view rests solely on loose-thinking sentimentalism[19] It may also be urged (though this was not 1 of Haldane's theses) that the equalization of risks which science was expected to bring would be in the long run salutary; that a civilian's life is not worth more than a soldier's, nor a woman's than a man's; that anything is better than the concentration of savagery on 1 particular class; & that, in short, the sooner war comes 'all out' the better.

I do not know which of these views is nearer to the truth. It is an urgent & a moving question, but I need not argue it here. It concerns only the 'trivial' mathematics, which it would be Hogben's business to defend rather than mine. The case for his mathematics may be rather more than a little soiled; the case for mine is unaffected.

Indeed, there is more to be said, since there is 1 purpose at any rate which the real mathematics may serve in war. When the world is mad, a mathematician may find in mathematics an incomparable anodyne. For mathematics is, of all the arts & sciences, the most austere & the most remote, & a mathematician should be of all men the one who can most easily take refuge where, as Betrand Russell says, "1 at least of our nobler impulses can best escape from the dreary exile of the actual world'. It is a pity that it should be necessary to make 1 very serious reservation – he must not be too old. Mathematics is not a contemplative but a creative subject; no one can draw much consolation from it when he has lost the power or the desire to create; & that is apt to happen to a mathematician rather soon. It is a pity, but in that case he does not matter a great deal anyhow, & it would be silly to bother about him." – Hardy, 1992, pp. 139–143

29 "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 hi subject will find that he is defending himself; & 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, & 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, & 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 & scholarships: I wanted to beat other boys, & this seemed to be the way in which I could do so most decisively.

I was about 15 when (in a rather odd way) my ambitions took a sharper turn. There is a book by 'Alan St Aubyn'[20]

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[18]]. B. S. Haldane, Callinicus: a Defence of Chemical Warfare (1924).

[19]I do not wish to prejudge the question by this much misused word; it may be used quite legitimately to indicate certain types of unbalanced emotion. Many people, of course, use 'sentimentalism' as a term of abuse for other people's decent feelings, & 'realism' as a disguise for their own brutality.

[20]'Alan St Aubyn' was Mrs. Frances Marshall, wife of Matthew Marshall.

called *A Fellow of Trinity*, 1 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 2 heroes, a primary hero called Flowers, who is almost wholly good, & a secondary hero, a much weaker vessel, called Brown. Flowers & Brown find many dangers in university life, but the worst is a gambling saloon in Chesterton[21] run by the Misses Bellenden, 2 fascinating but extremely wicked young ladies. Flowers survives all these troubles, is Second Wrangler & Senior Classic, & 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 in Ordinary Degree, & ultimately becomes a missionary. The friendship is not shattered by these unhappy events, & Flower's thoughts stray to Brown, with affectionate pity, as he drinks port & eats walnuts for the 1st 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 final scene in Combination Room fascinated me completely, & 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; & 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; & I still thought of mathematics as essentially a 'competitive' subject. My eyes were 1st opened by Prof. Love, who taught me for a few terms & gave me my 1st 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*; & I shall never forget the astonishment with which I read that remarkable work, the 1st inspiration for so many mathematicians of my generation, & learnt for the 1st 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 & a genuine passion for mathematics.

I wrote a great deal during the next 10 years, but very little of any importance; there are not more than 4 or 5 papers which I can still remember with some satisfaction. The real crises of my career came 10 or 12 years later, in 1911, when I began my long collaboration with Littlewood, & in 1913, when I discovered Ramanujan. All my best work since then has been bound up with theirs, & it is obvious that my association with them was the decisive event of my life. I still say to myself when I am depressed, & find myself forced to listen to pompous & tiresome people, 'Well, I have done 1 thing *you* could never have done, & that is to have collaborated with both Littlewood & 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 40, when I was a professor at Oxford. Since then I have suffered from that steady deterioration which is the common fate of elderly men & particularly of elderly mathematicians. A mathematician may still be competent enough at 60, 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, & that morning 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 & not less than was due to a man of my particular grade of ability. I have held a series of comfortable & 'dignified' positions. I have had very little trouble with the duller routine of universities. I hate 'teaching', & have had to do very little, such teaching as I have done having been almost entirely supervision of research; I love lecturing, & have lectured a great deal to extremely able classes; & I have always had plenty of leisure for the researchers which have been the 1 great permanent happiness of my life. I have found it easy to work with others, & have collaborated on a large scale with 2 exceptional mathematicians; & 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 or worse when I was 20, I would have accepted without hesitation.

It seems absurd to suppose that I could have 'done better'. I have no linguistic or artistic ability, & very little interest in experimental science. I might have been a tolerable philosopher, but not 1 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 & happy life. But solicitors & stockbrokers & bookmakers often lead comfortable & happy lives, & 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 1 possible answer: yes, perhaps, but, if so, for 1 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, & 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; & outside mathematics it is trivial anyhow. I have just 1 chance of escaping a verdict of complete triviality, that I may be judged to have created something worth creating. & that I

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[21]Actually, Chesterton lacks picturesque features.

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, & helped others to add more; & that these somethings have a value which differs in degree only, & 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." – Hardy, 1992, pp. 144–151

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