
The Pursuit of Science: Its Motivations

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Inaugural Lecture delivered by S Chandrasekhar, at the Golden Jubilee Meeting of the Indian Academy of Sciences on 6 February 1985.

I am grateful to President Ramaseshan for assigning to me the privilege of addressing you on this occasion celebrating the Golden Jubilee of this Academy. But for the tragedy which engulfed the country on October 31st of last year, this celebration should have taken place on November 7th, the 96th birthday of the Academy's illustrious founder and its President for its first 35 years. That this occasion is a moving one for my wife - a former student of Professor Raman - and myself must be obvious to you; and I shall not expand on it. That the occasion is also precious to me, on scientific grounds, derives not only from my having been a fellow of this Academy for all of its fifty years, but equally from the purposes to which this Academy is dedicated. Unlike other national academies, it has not sought, nor has it strived, to influence the public policies of the national government; nor has it followed the practice of awarding innumerable prizes, lectureships and medals. The purpose of this Academy, as enunciated by its founder, is to promote the pursuit of science in this country by providing journals and periodicals for the publication of scientific papers of the highest standards, and avoiding the necessity to seek foreign avenues.

During Professor Raman's lifetime, the Proceedings of the Academy, both series A and B, were published without lapse and without delay. Since 1970, when the affairs of the Academy passed on to his successors, this prime objective has so remained. By the efforts, principally of President Ramaseshan, the number of journals which are presently sponsored by the Academy has increased almost ten-fold. And all these journals do indeed maintain the highest standards by strict refereeing. I cannot wish for the future of the Academy any better than to express confidently the hope that the steadily increasing standards of the past ten years will be sustained during the next fifty years.

I

I now turn with some apprehension to the subject of my address, 'The Pursuit of Science: Its Motivations'. This is a difficult subject if one is to avoid the common and the banal. The

From: Patrika – April 1985, No. 10, Newsletter of the Indian Academy of Sciences.



difficulty derives in large measure from the variety and the range of the motives of the individual scientists; they are varied and they are diverse: they are as varied as the tastes, the temperaments, and the attitudes of the scientists themselves. Besides, the motivations are subject to substantial changes during the lifetimes of the scientists. Indeed, it is difficult to discern a common denominator. What is it then, one can usefully say?

I shall skirt the problem and restrict myself to reflections - perhaps, disjointed - on the lives and the accomplishments of some of the great scientists of the past. And I shall, whenever possible, base my remarks on what they have themselves said or written. But reflecting on the motives and the attitudes of great men is beset with grave semantic difficulties of communication: the words and phrases that language allows in these contexts may suggest criticism or judgement. Therefore, let me make it clear from the outset, that my remarks, at no place, are to be construed as having overtones or undertones of criticism or judgement. Indeed, I have no rights to such criticism or judgement in the contexts I shall be speaking. I should also make it clear that during the course of my reflections that I shall present, thoughts derived from my own personal experience have been entirely absent. Since it may not be possible for me to emphasize these points at every stage, I shall begin with a quotation and a narration.

The quotation is from the concluding pages of Turgenev's *On the Eve* in which there is a statement by that silent but indomitable character Insarov:

We are speaking of other people; why bring in yourself?

My narration is of a conversation between Majorana and Fermi in the middle twenties when both of them were also in their middle twenties. (The conversation was reported to me by one who was present on the occasion).

MAJORANA: There are scientists who 'happen' only once in every 500 years, like Archimedes or Newton. And there are scientists who happen only once or twice in a century, like Einstein or Bohr.

FERMI: But where do I come in, Majorana?

MAJORANA: Be reasonable, Enrico! I am not talking about you or me. I am talking about Einstein and Bohr.

Since I shall be talking principally about scientists in the class of Einstein, Bohr, and Fermi, I should indeed be 'reasonable'.



One final reservation. The circumference of my comprehension does not extend beyond a very limited circle of the physical sciences. This is, of course, a most serious limitation when I am addressing myself to so general a theme; but I must abide by my limitation.

II

For a discussion of the motivations which impel one to pursue the goals of science, no example is better than that of Johannes Kepler. Kepler's uniqueness derives from the position he singly occupies at the great crossroads where science shed its enveloping dogmas and the pathway was prepared for Newton. Kepler, in his inquiries, asked questions that none before him, including Copernicus, had asked. Kepler's laws differ qualitatively from earlier assumptions about planetary orbits: the assertion that planetary orbits 'are ellipses' in no way resembles the kind of improvements that his predecessors had sought. In his analysis of the motions of the planets, Kepler was not preoccupied with geometrical questions; he asked, instead, questions such as 'what is the origin of planetary motions?' If the sun is at the centre of the solar system, as it is in the Copernican scheme, should not that fact be discernible in the motions and in the orbits of the planets themselves?'. These are questions in physics; not in some preconceived geometrical framework.

While Kepler's approach to the problem of planetary motions was radically different from that of anyone before him, his work is preeminent for the manner in which he extracted general laws from a careful examination of the observations. His examination was long and it was arduous; it took him twenty and more years of constant and persistent effort; but he never lost sight of his goal. For him, it was a search for the holy grail in a very literal sense.

Before I describe the manner of Kepler's search, I should like to say that I am in no sense a scholar of medieval astronomy. My knowledge of Kepler is in fact mostly derived from Arthur Koestler's *The Sleep-walkers: a History of Man's Changing Vision of the Universe*, some collateral reading, and some discussions with scholars who know very much more. Koestler's sensitive account of Kepler, his life and his achievements, includes numerous quotations from Kepler's own writings. My remarks are largely based on these quotations.

From the outset Kepler realized that a careful study of the orbit of Mars will provide the key to planetary motions because its orbit departs from a circle the most; and it had defeated Copernicus; and further that an analysis of the accurate observations of Tycho Brahe was an essential prerequisite. As Kepler wrote:



Let all keep silence and hark to Tycho who has devoted thirty-five years to his observations...For Tycho alone do I wait; he shall explain to me the order and arrangement of the orbits...

Tycho possesses the best observations, and thus so-to-speak the material for the building of the new edifice...

...I believe it was an act of Divine Providence that I arrived just at the time when Longomontanus was occupied with Mars. For Mars alone enables us to penetrate the secrets of astronomy which otherwise would remain forever hidden from us...

Indeed, Kepler went to extraordinary lengths to acquire the observations of Tycho which he so badly needed. It is not an exaggeration to say that he committed larceny; for, as he confessed:

I confess that when Tycho died, I quickly took advantage of the absence, or lack of circumspection, of the heirs, by taking the observations under my care, or perhaps usurping them...

and as he explained:

The cause of this quarrel lies in the suspicious nature and bad manners of the Brahe family, but on the other hand also in my own passionate and mocking character. It must be admitted that Tengenagel had important reasons for suspecting me. I was in possession of the observations and refused to hand them over to the heirs...

With Tycho's observations thus acquired, the question which Kepler constantly asked himself was: if the sun is indeed the origin and the source of planetary motions, then, how does this fact manifest itself in the motions of the planets themselves? Noticing that Mars moved a little faster when it is nearest the sun than when it is farthest, and 'remembering Archimedes', he determined the area described by the radius vector joining the sun to the instantaneous position of Mars, as we follow it in its orbit. As Kepler wrote:

Since I was aware that there exist an infinite number of points on the orbit and accordingly an infinite number of distances [from the sun] the idea occurred to me that the sum of these distances is contained in the *area* of the orbit. For I remembered that in the same manner Archimedes too divided the area of a circle into an infinite number of triangles.



This was the way Kepler discovered in July 1603 his law of areas. This is the second of his three great laws in Newton's enumeration that has been adopted ever since. The establishment of this result took Kepler some five years; for, already prior to the publication of his *Mysterium Cosmographicum* in 1596, Kepler had sought for such a law in connection with his association of the five regular solids with the existence of the six planets known in his time.

The law of areas determined the variation of the speed along its orbit; but it did not determine the shape of the orbit. A year before he had arrived at his final statement of the law of areas he had in fact discarded circular orbits for the planets: and in October of 1602 he had written:

The conclusion is quite simply that the planet's path is not a circle - it curves inward on both sides and outward again at opposite ends. Such a curve is called an oval. The orbit is not a circle, but an oval figure.

Even after having concluded that the orbit of Mars is an 'oval', it took him an additional three years to establish that the orbit was in fact an ellipse. And when that was established he wrote:

Why should I mince my words? The truth of Nature, which I had rejected and chased away, returned by stealth through the back door, disguising itself to be expected. That is to say, I laid [the original equation] aside, and fell back on ellipses, believing that this was a quite different hypothesis, whereas the two, as I shall prove in the next Chapter, are one and the same...I thought and searched, until I went nearly mad, for a reason, why the planet preferred an elliptical orbit [to mine]...Ah, what a foolish bird I have been!

At long last in 1608 his monumental *Astronomia Nova* was published. As Koestler has written:

It was a beautifully printed volume in folio, of which only a few copies survive. The Emperor [Rudolph] claimed the whole edition as his property and forbade Kepler to sell or give away any copy of it 'without our foreknowledge and consent'. But since his salary was in arrears, Kepler felt at liberty to do as he liked, and sold the whole edition to the printers. Thus the story of the New Astronomy begins and ends with acts of larceny, committed *ad majorem Dei gloriam*.

Ten more years were to elapse before Kepler discovered his third law: that the squares of the periods of revolution of any two planets is in the ratio of the cubes of their mean distances from the sun. The law is stated in his *Harmonice Mundi* completed in 1618. Here is how Kepler describes his discovery:



On 8 March of this present year 1618, if precise dates are wanted, [the solution] turned up in my head. But I had an unlucky hand and when I tested it by computations I rejected it as false. In the end it came back again to me on 15 May, and in a new attack conquered the darkness of my mind; it agreed so perfectly with the data which my seventeen years of labour on Tycho's observations had yielded, that I thought at first I was dreaming.

Thus ended Kepler's long and arduous search for his holy grail.

In his first book, *Mysterium Cosmographicum*, Kepler exclaimed:

Oh! that we could live to see the day when both sets of figures agree with each other.

Twenty-two years later, he added the following footnote to this exclamation in a reprint edition of *Mysterium Cosmographicum* after he had discovered his third law and his poignant cry had been answered:

We have lived to see this day after 22 years and rejoice in it, at least I did; I trust that Maestlin and many other men will share in my joy!

III

In his novel, *The Redemption of Tycho Brahe*, Max Brod, the Czech writer who is known for his publishing, posthumously, the great works of Franz Kafka, portrays and contrasts the characters of Tycho Brahe and Kepler. While Brod's novel is historically, grossly inaccurate, yet the following imagined perception of Kepler by Tycho is an artist's idealization of what a scientist like Kepler might have been:

Kepler now inspired him [Tycho] with a feeling of awe. The tranquillity with which he applied himself to his labours and entirely ignored the warblings of flatterers was to Tycho almost superhuman. There was something incomprehensible in its absence of emotion, like a breath from a distant region of ice...

Is the tranquillity and the absence of emotion which Brod attributes to his imagined Kepler, ever attained by a practising scientist?

May I digress a little at this point to say that Max Brod, when he wrote his novel, *The*



Redemption of Tycho, was one of a small group in Prague that included Einstein and Franz Kafka. It has been said that Brod's portrayal of Kepler was influenced by his association with Einstein. Thus Walter Nernst is reported to have said to Einstein, "You are this man Kepler".

IV

As I have stated the most remarkable aspect of Kepler's pursuit of science is the constancy with which he applied himself to his chosen quest. To use a phrase of Shelley's his 'was a character superior in singleness'. But does the example of Kepler provide any assurance of success for a similar constancy in others? I shall consider two examples.

First, the example of Michelson. His main preoccupation throughout his life was to measure the velocity of light with increasing precision. His interest came about almost by accident, when the Commander of the Naval Academy asked him (then an instructor at the Academy) to prepare some lecture demonstrations to illustrate Foucault's refinement of Cornu's determination of the velocity of light. That was in 1878; and it led to Michelson's first determination of the velocity of light in 1880. On the 7th of May 1931, two days before he died and fifty years later, he dictated the opening sentences of a paper, that was posthumously published and which gave the results of his last measurement. Michelson's efforts resulted in an improvement in our knowledge of the velocity of light from 1 part in 3000 to 1 part in 30,000 i.e. by a factor 10. But by 1973 the accuracy had been improved to 1 part in 10^{10} , a measurement that made obsolete, beforehand, all future measurements. Were Michelson's efforts over 50 years then in vain? Leaving that question aside, one must record that during his long career, Michelson made great discoveries derived from his delight in 'light waves and their uses'. Thus his development of interferometry leading to the first direct determination of the diameter of a star is wondrous. And who does not know the Michelson-Morley experiment which, through Einstein's formulation of the special and the general theory of relativity, changed and changed irrevocably our understanding of the nature of space and time? But it is a curious fact that Michelson himself was never happy with the outcome of his experiment. Indeed, it is recorded that when Einstein visited Michelson in April 1931, Mrs Michelson felt it necessary to warn Einstein "Please don't get him started on the ether."

A second example is Eddington who devoted the last 16 years of his life to developing his 'fundamental theory'. Of this prodigious effort he said, a year before he died:

At no time during the past 16 years have I felt any doubt about the correctness of my theory.



Yet, his efforts have left no trace on subsequent developments.

Is it wise then to pursue science with a single objective and with a singleness of purpose?

V

While Kepler provides the supreme example of sustained scientific effort leading to great and fundamental discoveries, there are instances in which great thoughts have seemingly occurred spontaneously. Thus, Dirac has written that his work on Poisson brackets and on his relativistic wave equation of the electron were consequences of ideas

... which had just come out of the blue. I could not very well say just how it had occurred to me. And I felt that work of this kind was a rather 'undeserved success'.

Dirac's recollection, that his ideas underlying his work on Poisson brackets and his relativistic wave equation of the electron came to him 'out of the blue', is an example of what is apparently not a unique phenomenon: Those who have made great discoveries seem to remember and cherish the occasions on which they made them. Thus, Einstein has recorded that

When in 1907 I was working on a comprehensive paper on the special theory of relativity...there occurred to me the happiest thought of my life...that '*for an observer falling freely from the roof of a house there exists - at least in his immediate surroundings - no gravitational field*'.

This 'happy thought' was, of course, later enshrined in his principle of equivalence that is at the base of his general theory of relativity.

A recollection in a similar vein is that of Fermi. I had once the occasion to ask Fermi, in the context of Hadamard's perceptive '*Essay on the Psychology of Invention in the Mathematical Field*' what the psychology of invention in the realm of physics might be. Fermi responded by narrating the occasion of his discovery of the effect of slow neutrons on induced radioactivity. This is what he said:

I will tell you how I came to make the discovery which I suppose is the most important one I have made. We were working very hard on the neutron-induced radioactivity and the results we were obtaining made no sense. One day, as I came to the laboratory, it occurred to me that I should examine the effect of placing a piece of lead before the incident neutrons. Instead of my usual custom, I took great pains to have the piece of



lead precisely machined. I was clearly dissatisfied with something; I tried every excuse to postpone putting the piece of lead in its place. When finally, with some reluctance, I was going to put it in place, I said to myself: 'No, I do not want this piece of lead here; what I want is a piece of paraffin'. It was just like that with no advance warning, no conscious prior reasoning. I immediately took some odd piece of paraffin and placed it where the piece of lead was to have been.

Perhaps the most moving statement in this general context is that of Heisenberg relating the moment when the laws of quantum mechanics came to a sharp focus in his mind. He has written,

One evening I reached the point where I was ready to determine the individual terms in the energy table, or, as we put it today, in the energy matrix, by what would now be considered an extremely clumsy series of calculations. When the first terms seemed to accord with the energy principle, I became rather excited, and I began to make countless arithmetical errors. As a result, it was almost three o'clock in the morning before the final result of my computations lay before me. The energy principle had held for all terms, and I could no longer doubt the mathematical consistency and coherence of the kind of quantum mechanics to which my calculations pointed. At first, I was deeply alarmed. I had the feeling that, through the surface of atomic phenomena, I was looking at a strangely beautiful interior, and felt almost giddy at the thought that I now had to probe this wealth of mathematical structures nature had so generously spread out before me. I was far too excited to sleep, and so, as a new day dawned, I made for the southern tip of the island, where I had been longing to climb a rock jutting out into the sea. I now did so without too much trouble, and waited for the sun to rise.

There is no difficulty for any of us to share in Heisenberg's exhilaration of that supreme moment: we all know of the difficulties and paradoxes that beset the 'old' Bohr-Sommerfeld quantum-theory of the time; and we also know of Heisenberg's long puzzlement over these difficulties and paradoxes with Sommerfeld, Bohr, and Pauli. And he had already published at that time his paper with Kramers on the dispersion theory - a theory which in many ways was the precursor to the developments that were to follow.

But what is our reaction to Heisenberg's account of his ideas on the theory of elementary particles that he developed some thirty years later, after his tragic experiences during the war and his disappointments and frustrations of the post-war years? Mrs. Heisenberg, in her book on her husband, has written,



One moonlight night we walked all over the Hainberg Mountain, and he was completely enthralled by the visions he had, trying to explain his newest discovery to me. He talked about the miracle of symmetry as the original archetype of creation, about harmony, about the beauty of simplicity, and its inner truth.

And she quotes from one of Heisenberg's letters to her sister at this time:

In fact, the last few weeks were full of excitement for me. And perhaps I can best illustrate what I have experienced through the analogy that I have attempted an as yet unknown ascent to the fundamental peak of atomic theory, with great efforts during the past five years. And now, with the peak directly ahead of me, the whole terrain of interrelationships in atomic theory is suddenly and clearly spread out before my eyes. That these interrelationships display, in all their mathematical abstraction, an incredible degree of simplicity, is a gift we can only accept humbly. Not even Plato could have believed them to be so beautiful. For these interrelationships cannot be invented; they have been there since the creation of the world.

You will notice the remarkable similarity in the language and in the phraseology of this description with the description of his discovery of the basic rules of quantum mechanics some thirty years earlier. But do we share in his second vision in the same way? In the earlier case, his ideas won immediate acceptance. In contrast, his ideas on particle physics were rejected and repudiated even by his long time critic and friend Pauli. But it is moving to read what Mrs Heisenberg writes towards the end of her biography:

With smiling certainty, he once said to me: 'I was lucky enough to look over the good Lord's shoulder while He was at work.' That was enough for him, more than enough! It gave him great joy, and the strength to meet the hostilities and misunderstandings he was subjected to in the world time and again with equanimity, and not be led astray.

VI

A different aspect of the effect a great discovery can have on its author is provided by the autobiography entitled *The Traveler* by Hideki Yukawa. The book was written when Yukawa was past fifty. One would normally have expected that an autobiography entitled *The Traveler* by one whose life, at least as seen from the outside, had been rich and fruitful, would be an account of his entire life. But Yukawa's account of his 'travels' ends with the publication of his 1934 paper describing his great discovery with the sombre note:



I do not want to write beyond this point, because those days when I studied relentlessly are nostalgic to me; and on the other hand, I am sad when I think how I have become increasingly preoccupied with matters other than study.

VII

While all of us can share in the joy of the discoveries of the great men of science, we may be puzzled by what those many, very many, less perceptive and less fortunate, are to cherish and remember. Are they, like Vladimir and Estragon, destined to wait for Godot as in Samuel Beckett's play; or, are they to console themselves with Milton's thought 'they also serve who stand and wait'?

VIII

I now turn to the role which approbation and approval play in one's pursuit of science. The example of Newton 'voyaging through strange seas of thought alone' is not one that any of us can follow.

I have referred to Eddington's lonely efforts in pursuing his fundamental theory. In spite of his expressed confidence in the correctness of his theory, Eddington must have been deeply frustrated by the neglect of his work by his contemporaries. This frustration is evident in his plaintive letter to Dingle written a few months before he died:

I am continually trying to find out why people find the procedure obscure. But I would point out that even Einstein was considered obscure, and hundreds of people have thought it necessary to explain him. I cannot seriously believe that I ever attain the obscurity that Dirac does. But in the case of Einstein and Dirac people have thought it worthwhile to penetrate the obscurity. I believe they will understand me all right when they realize they have got to do so – and when it becomes the fashion 'to explain Eddington'.

The lack of approval by one's contemporaries can have tragic consequences when they are expressed in the form of sharp and violent criticisms. Thus, Ludwig Boltzmann, greatly depressed by the violence of the attacks directed against his ideas by Ostwald and Mach, committed suicide 'as a martyr to his ideas', as his grandson Flamm has written. And George Cantor, the originator of the modern theory of sets of points and of the orders of infinity, lost his mind because of the hatred and the animosity against him and his ideas by his teacher



Leopold Kronecker, and was confined to a mental hospital during the last many years of his life.

IX

A case very different from the ones I have considered so far is that of Rutherford.

Consider his record. In 1897 he analyzed radioactive radiations into three types: α -particles, β -rays and γ rays, a nomenclature that has survived to this day. In 1902 he formulated the laws of radioactive disintegration: the first time a physical law was formulated in terms of probability and not certainty: a forerunner of the probability interpretation of quantum mechanics that was to become universal some 25 years later. In 1905-1907 he formulated, together with Soddy, the laws of radioactive displacement and identified the α -particle as the nucleus of the helium atom; and, together with Boltwood, initiated the determination of the ages of rocks and minerals by their radioactivity. In 1909-1910, there were the experiments of Geiger and Marsden, the discovery of the large angle scattering of α -rays and Rutherford's formulation of his law of scattering and the nuclear model of the atom. Then in 1917 he effected the first laboratory transformation of atoms: that of nitrogen-14 into oxygen-17 and a proton by α -ray bombardment. In the twenties, he was associated with the clarification of the relationship between the α -ray and the γ -ray spectra. And 1932 – the *annus mirabilis* as R H Fowler called it – saw the discovery of the artificial disintegration of Li into two α -particles by Cockroft and Walton, of positrons in cosmic-ray showers by Blackett, and the neutron by Chadwick - all of them in Rutherford's Cavendish. In the following year Rutherford, together with Oliphant, himself discovered hydrogen-3 and helium - 3. Altogether, then, an accomplishment unparalleled in this century.

Rutherford's attitude to his own discoveries is illustrated by his response to a remark of one who was present at the moment of one of his great discoveries: 'Rutherford, you are always on the crest of the wave.' To which Rutherford responded: 'I made the wave, didn't I?' Somehow from Rutherford's vantage point everything he said seems right, even including his remark, 'I do not let my boys waste their time' when he was asked if he encouraged his students to study relativity!

Rutherford was a happy warrior if ever there was one.



X

So far, I have tried to illustrate some facets of the pursuit of science by drawing on incidents in the lives of some great men of science. I shall turn now to some more general matters.

I shall start with an example. It has been reported that when Michelson was asked towards the end of his life, why he had devoted such a large fraction of his time, to the measurement of the velocity of light, he replied 'it was so much fun'. There is no denying that 'fun' does play a role in the pursuit of science. But the word 'fun' suggests a lack of seriousness. Indeed, the Oxford Dictionary gives to 'fun' the meaning 'drollery'. We can be certain that Michelson did not have that meaning in his mind when he described his life's main interest as 'fun'. If not, what precisely is the meaning we are to attach to 'fun' in the context in which Michelson used it? More generally, what is the role of pleasure and enjoyment?

While 'pleasure' and 'enjoyment' are often used to characterize one's efforts in science, failures, frustrations, and disappointments are equally, if not more, the common ingredients of scientific experience. Overcoming difficulties, undoubtedly, contributes to one's final enjoyment of success. Is failure, then, a purely negative aspect of the pursuit of science?

A remark of Dirac's describing the rapid development of physics following the founding of the principles of quantum mechanics in the middle and the late twenties is apposite in this connection.

It was a good description to say that it was a game, a very interesting game one could play. Whenever one solved one of the little problems, one could write a paper about it. It was very easy in those days for any second-rate physicist to do first-rate work. There has not been such a glorious time since then.

Consider in the context of these remarks, J J Thomson's assessment of Lord Rayleigh in his memorial address given in Westminster Abbey:

There are some great men of science whose charm consists in having said the first word on a subject, in having introduced some new idea which has proved fruitful; there are others whose charm consists perhaps in having said the last word on the subject, and who have reduced the subject to logical consistency and clearness. I think by temperament Lord Rayleigh belonged to the second group.

This assessment of Rayleigh by J J Thomson has sometimes been described as double-edged.



But could one not conclude, instead, that Rayleigh by temperament chose to address himself to difficult problems and was not content to play the kind of games that Dirac describes in his characterization of the 'glorious time' in physics as a time 'when second-rate physicists could do first-rate work'?

The last question concerning Rayleigh's temperament raises the further question: after a scientist has reached maturity, what are his criteria for his continued pursuit of science? To what extent are they personal? And to what extent are aesthetic criteria like the perception of order and pattern, form and substance, relevant?

Are such personal criteria exclusive? Has a sense of obligation a role? I do not mean obligation with the common meaning of obligation to one's students, one's colleagues, and one's community. I mean, rather, obligation to science itself. And what, indeed, is the content of obligation in the pursuit of science *for* science?

Let me finally turn to a different aspect. G H Hardy concludes his *A Mathematician's Apology* with the following illuminating statement:

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

Hardy's statement is made relative to mathematicians; but it is equally applicable to all scientists. I want to draw your attention particularly to Hardy's reference to one's wanting to leave behind some kind of a memorial *i.e.*, something that posterity may judge. To what extent, then, is the judgement of posterity (which one can never know) a conscious motivation in the pursuit of science?

X I

The pursuit of science has often been compared to the scaling of mountains, high and not so high. But who amongst us can hope, even in imagination, to scale the Everest and reach its summit when the sky is blue and the air is still: and in the stillness of the air survey the entire Himalayan range in the dazzling white of the snow stretching to infinity? None of us can hope for a comparable vision of nature and of the universe around us. But there is nothing mean or lowly in standing in the valley below and awaiting the sun to rise over the Kunchenjunga.

