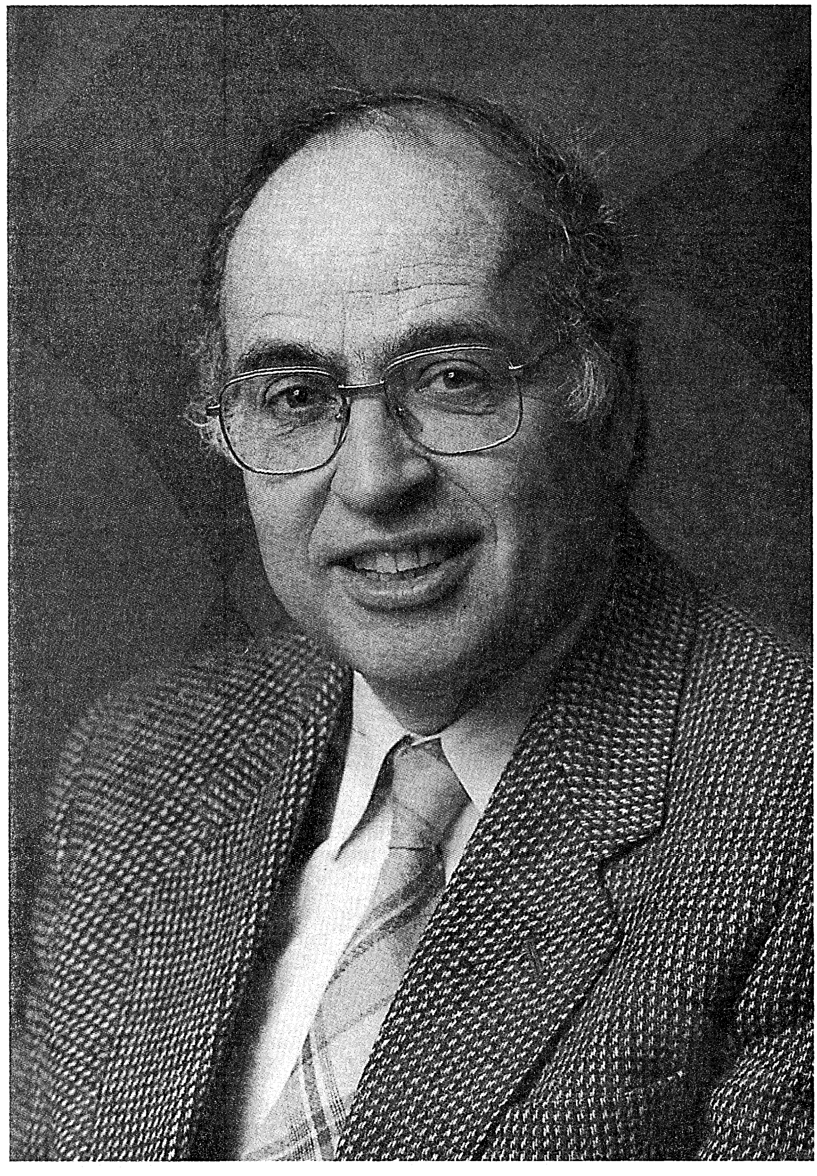


Sir Michael Atiyah

THE ASIAN JOURNAL OF MATHEMATICS

Mathematics in the Asian region has grown tremendously in recent years. The Asian Journal of Mathematics (ISSN 1093-6106), from International Press, provides a forum for these developments, and aims to stimulate mathematical research in the Asian region. It publishes original research papers and survey articles in all areas of pure mathematics, and theoretical applied mathematics. High standards will be applied in evaluating submitted manuscripts, and the entire editorial board must approve the acceptance of any paper. The Journal appears quarterly, in March, June, September and December. Each number is approximately 200 pages, with occasional enlarged issues designed to publish long papers and to clear back-log. Periodicals postage paid at Boston, Massachusetts. Postmaster: Send address changes to International Press; P.O. Box 2872; Cambridge, MA 02238-2872, USA.

PREFACE

Sir Michael Atiyah turns seventy this year. It is an honor for the Asian Journal of Mathematics to dedicate an issue to him on his seventieth birthday. As we know, besides his fundamental and profound contribution to topology, geometry, representation, and partial differential equations; Sir Michael has been the president of Royal Society of England and traveled to most Asian countries to encourage their scientific development.

We had met him many times in Lebanon, in Israel, in India, in Japan, in mainland China, in Taiwan, in Hong Kong and in various other Asian countries. Many Asian mathematicians have been benefited by his presence and his generous help. A very famous example is his friendship with the famous India mathematician Patodi who Sir Atiyah invited to the Institute for Advanced Study and for whom he tried to arrange medical care when Patodi had kidney failure. Sir Atiyah's trips to Tokyo, Kyoto, and other cities in Japan, his trips to Beijing, to Shanghai, and to Hong Kong stimulated great interest in the interfaces between geometry, analysis, topology, and physics. His close association with Asian mathematicians is well-illustrated by the publication of Sir Atiyah's collected works in Chinese through the efforts of Professor Chern. The Chinese version of his collected works resulted in the publication of the English language version by Oxford University Press. Hence it is very appropriate for the newly established Asian Journal of Mathematics to dedicate this special issue to his seventieth birthday.

On this occasion, we think it is appropriate to mention that his friendship with Hong Kong has produce a deep gratitude and admiration at the Chinese University of Hong Kong so that he was awarded an honorary degree from the university in 1998. We reproduce the citation from that ceremony here. We are also happy to have the permission of the Royal Society to print Sir Michael's farewell essay to that distinguished body.

Of course, we are also very grateful to all the distinguished authors who have contributed their significant articles to honor Sir Michael's 70 th birthday. Some of the authors' articles were submitted late, and they will appear in a later issue of the Asian Journal of Mathematics. We are grateful for their support also.

For the great and profound contribution that you made to mathematics and your other activities for mankind, we would like to express our deepest gratitude and wish you a happy birthday at this special occasion!

Editors of AJM

On election as a Research Fellow of Trinity College

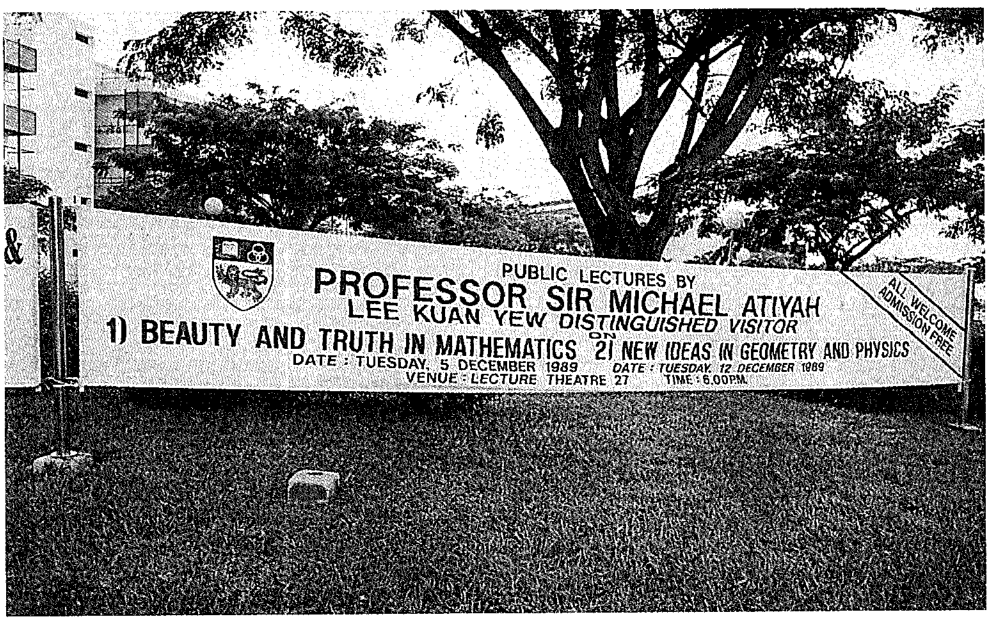
1958, Atiyah with Bolt

1963, Hirzebruch, Eells, and Atiyah

Founding of European Mathematics Society in Poland

In Japan, with Kodaira and Cortau

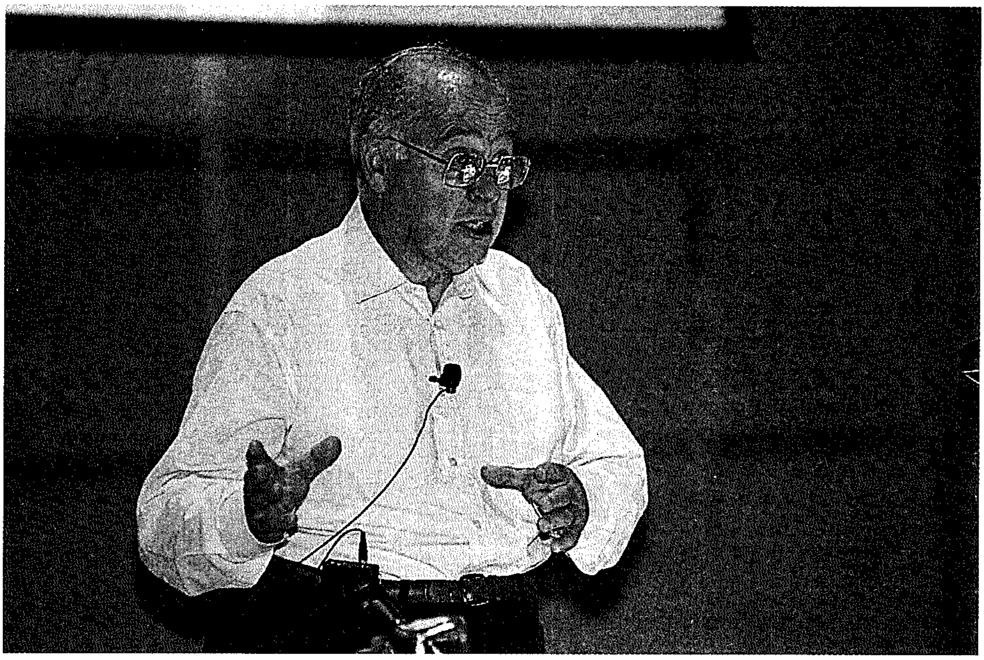
In Singapore, 1989



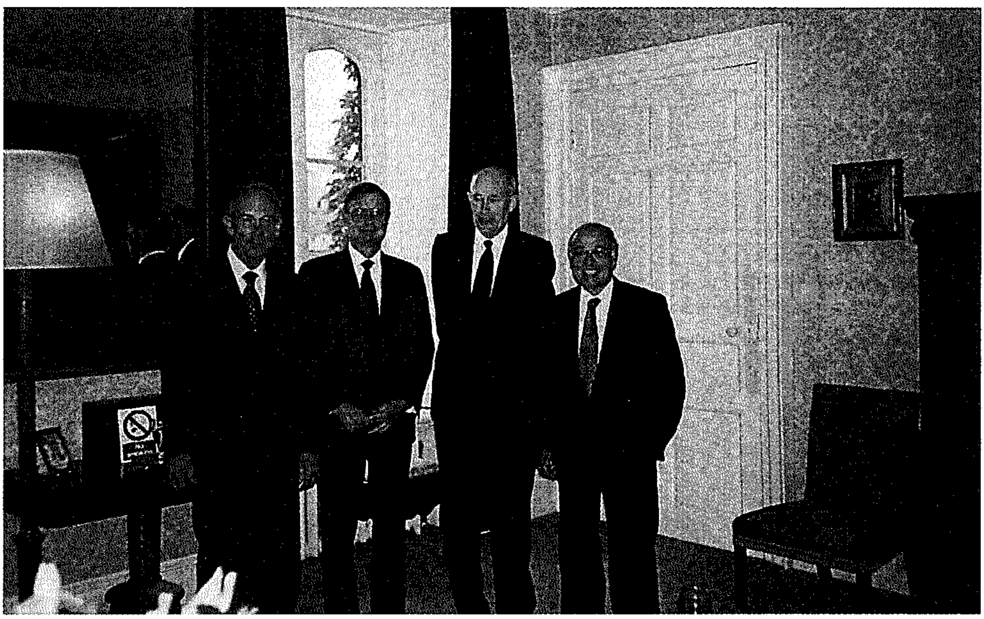
In Singapore, 1989

60th birthday conference
Hitchin, Atiyah, Hirzebruch, Lily Atiyah

1994, Receiving Honorary Doctorate at Brimingham University

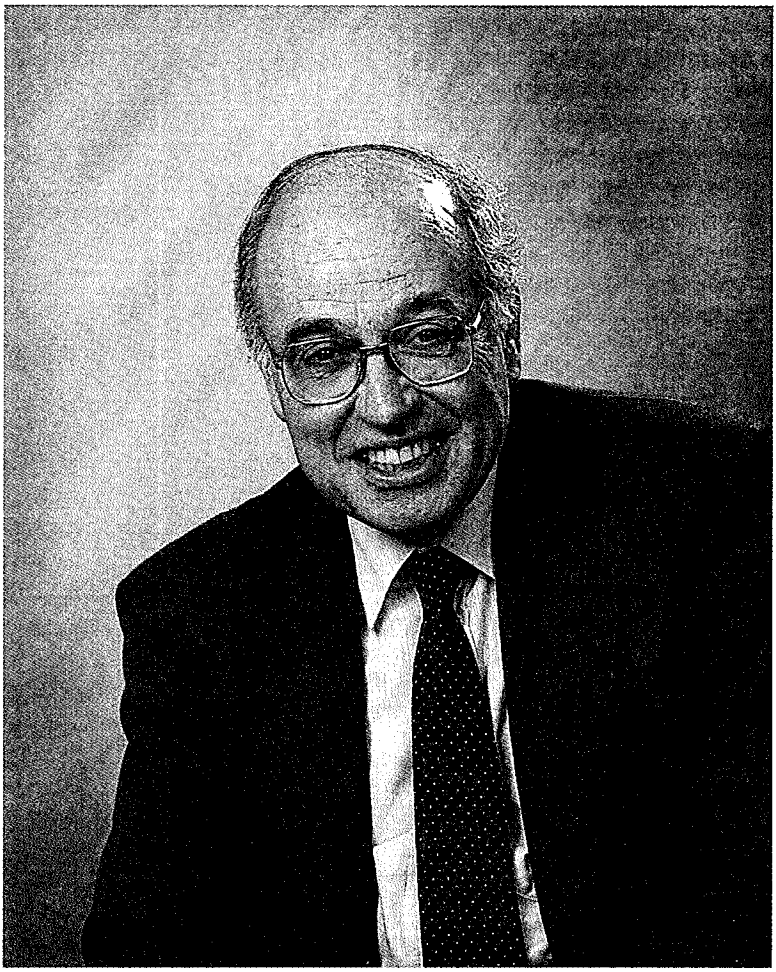


Lecturing in Mexico, 1998



Mathematics Institutes Directors, 1992
Hirzebruch (Bonn); Griffiths (Princeton); Berger (IHES); Atiyah (Newton)
On occasion of opening of Newton Institute

1999, Fields Medalists of the United Kingdom
Gowen, Roth, Donaldson, Atiyah, Baker, Borchards, Quillen
At opening of new headquarters of London Mathematical Society



Sir Michael Atiyah

CITATION FROM CHINESE UNIVERSITY OF HONG KONG ON THE OCCASION OF HIS HONORARY DOCTORATE[1]

Sir Michael Atiyah, OM, FRS

Born in London to an English mother and Lebanese father, thus perhaps inheriting a certain English pragmatism along with the mathematical traditions of the 'middle east' cultures, Micheal Francis Atiyah progressed from schoolboy mathematics in Egypt and then in that famous hothouse for young talent, Manchester Grammar School in England, to a first degree at Trinity College, Cambridge in 1952, a Ph.D. three years later, and, after four decades of mathematical thinking, almost 30 honorary degrees in recognition of his status as one of the great mathematicians of our century.

He has therefore progressed from the basics of addition, subtraction, multiplication and division - deliberately confused with ambition, distraction, uglification and derision, by an Oxford mathematician, Lewis Carroll - to scale the highest peaks of the higher mathematics. He has, in fact, progressed into what must be, for most of us, the mysterious realm of mathematical, particularly geometrical, operations that, being by no means transparent, can only be made plain to an audience of other mathematicians. That audience is one made up of people of many different countries, speaking a host of different languages, who are able to grasp the complexities of this universal language of science. Mathematics, called by Carl Friedrich Gauss 'the queen of the sciences', knows no barriers of race, nationality, culture, or politics. Nor does it recognize frontiers between science and the arts, for it is both science and art. Paradoxically, therefore, it is at once democratic and exclusive, for only those may use this language who have brains, concentration, and imagination enough to follow its cunning intricacies, unanticipated simplicities, its symmetries and sublime asymmetries. For Sir Michael, the beauty of mathematics is an elegance achieved by understanding the complexities of reality and expressing them in simpler, more orderly forms.

Sir Michael's progress as a mathematician might be called a 'geometrical progression' of prizes and honours, the First Smith's Prize being awarded to him the year he become 25 and also research fellow at Trinity College, where he is now Master. In 1955 he was awarded a Commonwealth Fund Fellowship and became a member of Princeton's Institute for Advanced Study for the first time, one of several occasions. Between 1958 and 1961 he was a fellow of Pembroke College, Cambridge, then defected to that other university, Oxford, achieving the rank of reader and professorial fellow of St. Catherine's at the early age of 32. When just 33, he was made fellow of that most prestigious of learned scientific bodies, the Royal Society.

The Nobel Prize committee making no award for mathematics, the highest honour in the field is the Fields Medal. This Atiyah won in 1966, while in his late thirties. The Royal Medal followed in 1968 and in 1988 the Copley Medal of the Royal Society, of which he become a research professor in the early 1970s and president from 1990 to 1995.

A characteristic of mathematics is that many of its brightest stars turn out to be

[1]Reprinted from *the Chinese University Bulletin*, (Spring.Summer, 1997) with the permission of The Chinese University of Hong Kong.

meteors, reaching a zenith while very young, only to vanish into outer darkness. A measure of Sir Michael's genius is that his mathematical ideas led to a new synthesis of seemingly disparate areas of enquiry and thence to hosts of applications, so that his methods could dominate the field for many fecund years.

To think of reality as composed of dimensionless dots or particles is to see it as resembling an incredibly detailed and minute "pointilliste" painting, Sit Michael, however, saw it more as a Jackson Pollock canvas, a web of strings of paints, in a mighty maze, though not without a plan. This view is more easily accounted for by 'topology', the study of objects that bend and stretch, in the so-called 'rubber sheet geometry'. In particular, this has been fashioned into a bridge over the divide between mathematics and physics. Topology also offers ways of stringing or threading together seemingly disparate areas of mathematical thinking. By linking topology to algebraic geometry, a new 'topological invariant' appeared, providing a base for a novel kind of mathematics, K-theory. This in turn was the basis of his collaboration with Isadore Singer that resulted in the 'index theorem' which won Atiyah the Field Medal. Working it all out was 10-year task. The index theorem proved useful in theoretical physics, for when it was found that right-handed and left-handed particles behave differently, the theorem furnished a method of measuring these asymmetries. The applications of topology to quantum mechanics have also been a fruitful development in Atiyah's work. Thus, in his middle years, he was still engaged in some of his most influential work in mathematics. Sir Christopher Zeeman has explained that Atiyah has connected so much in so many different areas that he has remained preeminent in world mathematics for 30 years.

Mathematical research requires neither expensive laboratories nor costly apparatus; nor does it involve painstaking examination of corrupt and corruptible text, or burrowing like a mole onto the learned warrens of great libraries. The mathematician must understand the brief history of previous mathematical innovation and then sit with a pad and pencil and think afresh. It requires intense concentration, laser sharp, to kindle the almost spontaneous combustion of though itself, burning with that which Walter Pater considered desirable in life itself: 'a hard, gem-like flame'. This flame of mathematical thinking lights up the seemingly impenetrable walls of the labyrinth that is the unknown, what has not yet been thought. The product of Atiyah's thinking can be found in numerous papers, and in such works as *K-Theory* (1966), *Collected Works* (5 vol., 1988), *Geometry and Dynamics of Magnetic Monopoles* (1988), and *The Geometry and Physics of Knots* (1990).

Claude LeBrun has called Atiyah 'one of the great mathematical teachers of our time'. Over the years he has generously made himself available to us as an external expert, giving valuable advice to our Department of Mathematics, and conducting seminars on campus in 1992 and 1995.

Here then is a counsellor of the highest value, a great mathematician, and a college administrator of great personal warmth and humanity, He was knighted in 1983 and awarded the Order of Merit in 1992. Made Commander of Lebanon's Order of the Cedars in 1994, he is also an honorary professor of the Chinese Academy of Science.

It is my delighted duty, Mr. Chancellor, to present Sir Michael Atiyah, who entered a dark labyrinth and emerged into the light, holding a thread, to receive the degree of Doctor of Science, *honoris causa.*

FAREWELL ADDRESS BY SIR MICHAEL ATIYAH TO THE ROYAL SOCIETY[2]

The President's Anniversary Address to the Society provides an occasion for reflection on matters of importance to science, particularly those of the previous year. The address is a personal one, not a collective statement of Royal Society policy, but the restraints of office have to be borne in mind. The President has to face his fellow Officers and Council during the next year and he has to regain the confidence of the Fellowship. But these restrictions do not apply to his farewell address, his swan song which terminates his role before the evening is out. In other words, as an outgoing President, I can speak more freely and not weight my words with too much diplomatic tact. This is my last chance to emphasize the things I think are really important and to provide some food for thought for my successors.

Too often we have to react to external events, to short term crises, to financial cuts or to ministerial changes. In this semi-political world in which the scientific community has to operate we are in danger of losing our way and our identity. The scientific ethos becomes increasingly hard to discern. So today I would like to discuss some of the major issues of principle that we face.

As you all know this year is the 50th anniversary of the dropping of the atomic bomb on Japan. No other single event has so profoundly affected the relationship between science and society as the dropping of the atomic bomb. It has cast a very long shadow over the past 50 years. The most immediate effect was to highlight in an awesome way the moral dilemma of scientists in relation to the military application of their discoveries. Many of those most directly involved in the development of the bomb went on to become strong advancers of restraint and responsibility in the nuclear arms race that ensued. This includes those in the Pugwash movement, notably Joseph Rotblat, the recipient of this year's Nobel Peace Prize. I am delighted that this well deserved recognition follows so soon after Professor Rotblat's election as a Fellow of the Royal Society.

The atomic bomb was unique in many respects, particularly in the speed with which a discovery in fundamental physics was put to use. A few short years transformed an abstruse piece of theoretical physics into the most devastating weapon the world had ever seen. No longer would scientists, conducting pure research for its own sake, be ignored on the ground that their work was not relevant to the real world. The ivory tower was no longer a sanctuary.

The scale of the Los Alamos project, as a technological enterprise, brought scientists into the big money league. They were now involved in an operation costing vast sums and this continued, in the postwar years, with the peaceful development of atomic energy.

The days of the scientist as the poor scholar, dependent on a little enlightened philanthropy, were over. From now on science and big money were partners, and, like other partnerships, this has produced tensions and crises. Rich friends are all very well but they can lead one to acquire expensive tastes.

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[2]Reprinted by permission from *Supplement to Royal Society News*, Vol. 8, No. 6, Nov., 1995.

If the technical triumph of the atomic bomb pushed scientists into the military-industrial complex it also initiated a hostile reaction from the general public. Atomic bombs were a menace and the scientists were responsible. Over the past 50 years this anti-science feeling has grown alarmingly, with environmental worries taking over from nuclear weapons as the driving force.

So, as we look back over the 50 years since Hiroshima, we can see that atomic bomb ushered in a new era for the scientific community. Close collaboration with government, both for military and for industrial purposes, has brought substantial material benefits. But this support has been bought at a price and public suspicion is one of the consequences.

We cannot turn the clock back and revert to our ivory towers. Science now occupies too important a position in modern life. The crucial question we scientists face is how to conduct our relations with government and industry so as to regain the confidence of the public. Here we need some humility. It is no use complaining that the public is simply ill-informed and needs re-educating. We have to examine our own position and see whether any of the criticisms levelled against us are valid. Have we sold out to the military-industrial complex? Do we pay sufficient attention to the way science is applied? Have we subverted the international idealism of science for narrow chauvinist aims?

Of course, all these are heavily loaded questions which many of us will feel unjustifiably impugn the integrity of scientists. Behind the scenes, and in the corridors of power, we may constantly be exercising a benign influence. But this will not impress a skeptical public. Scientists are too often thought of as a secretive elite, a sinister part of the establishment, part of "them", not part of "us". The only way to break down this suspicion and distrust is for scientists to speak out openly and freely, to criticize the establishment when necessary and to demonstrate that independence of thought really is the hallmark of a scientist.

So, in that spirit, let me return to Hiroshima and the atomic bomb. The 50th anniversary inevitably raised again the moral dilemma: was it justified, was it necessary? Even with hindsight, there are no easy answers as the extensive correspondence in our daily newspapers so clearly demonstrated. What was important about that public debate, however, was that scientists were not all on one side, some were to be found on the side of the Bishops, if not of the Angels.

Now history, whether factual or mythical, is important: it shapes our attitudes and our thinking. But let me move on to the future, which is more under our control. Although nuclear weapons have not been used in battle since the end of World War II, they have been produced and stockpiled in vast quantities. The bulk of these weapons are held by Russia and the United States, but a number of other countries have significant quantities. China, the UK and France freely acknowledge their nuclear capability, but others are more clandestine.

Even before the collapse of communism in the Soviet Union, the arms race had been reversed and reductions in nuclear stockpiles were agreed between the USA and the USSR. The new political climate offers an excellent opportunity of reducing the nuclear threat even further. The aim of totally eliminating nuclear weapons no longer seems an impossible dream. In working towards this goal, scientists have a unique responsibility, and they can help in various ways.

On the technical side, they can assist with the dismantling of weapons, the disposal of plutonium, and the monitoring of security. On the political side, they can keep reminding the public of the horrific nature of nuclear warfare and so maintain pressure on their Governments to continue along the disarmament route.

It would be good to report that the UK Government is in the forefront of those working for the reduction of nuclear weapons. Regrettably, this is not the case. There seems to be no long-term vision, only a complacent reliance on the status quo.

Leaving aside the need to work for a more stable and secure future, we might well ask questions about British policy over the past 50 years. The development, maintenance and enhancement of a British nuclear deterrent has been the constituent policy of successive governments from both the major parties. Ernest Bevin was as enthusiastic a proponent as Margaret Thatcher, and, at the present time, nuclear policy does not appear to be a matter of political controversy.

This is fortunate, because it means that I, as a scientist, can state my views without becoming embroiled in partisan politics. So, let me venture a prediction. I believe history will show that the insistence on a UK nuclear capability was fundamentally misguided, a total waste of resources, and a significant factor in our relative decline over the past 50 years.

The facts are easy to come by. Comparisons with Germany will show that both countries have devoted approximately the same fraction of their resources to Research and Development. However, the division between civil and military R&D in the two countries is very different. Given this discrepancy, and the acknowledged importance of science and technology for modern industry, it would have required gross incompetence on the part of our German competitors if they had not derived a major economic benefit from this additional investment. Very similar remarks apply to Japan.

It may be argued that this economic sacrifice on the part of the UK was made altruistically in the interest of world peace. Perhaps, but I have yet to see this argument supported outside Britain and France.

The alternative justification, that nuclear weapons have given us extra political clout is equally hard to substantiate. Unless you actually use nuclear weapons as a form of blackmail, they are about as useful politically as an honorary degree is academically. It is economic strength that underpins political influence, and this is precisely what will have been sacrificed.

It has been said that Britain and, to a lesser extent, France, have had difficulty in adjusting to the loss of empire. Nuclear status may have been seen by our Prime Ministers as a substitute, and as reward for being on the winning side in the war; psychologically understandable, but economically disastrous.

Nuclear weapons are just the most conspicuous part of our military arsenal, and by no means the only part which is crucially dependent on science and technology. So, even in the non-nuclear area, the Ministry of Defense (MoD) employs many scientists and engineers who might in other circumstances be creating wealth for the nation.

A few years ago, after the fall of the Berlin Wall and the disappearance of the Russian military threat, there was much talk of the 'Peace Dividend', the conversion of swords into ploughshares. The armed forces would be run down, resources would be saved and diverted into other more productive directions. In particular, the substantial effort that was going into military R&D would decrease with a corresponding increase in the civil expenditure on R&D. Unless I am very inobservant, this does not seem to have happened to any significant extent. True, the MoD bill has gone down, but I have failed to detect any conscious policy on the redistribution of scientific resources.

I can understand the problem. Major changes cannot be expected overnight. The conversion of swords into ploughshares has always been a difficult business, and the conversion of swordsmen into ploughmen may be even trickier. Still, I would like to the 'Peace Dividend' being turned into a reality.

There is at present great emphasis on the economic benefits that should be extracted from our scientific base. It seems quite consistent with this policy that we should be trying to divert some of our scientific resources from military to civilian purposes.

I realize that the manufacture and sale of armaments is part of the economy and considerable effort goes into persuading foreign governments to acquire British weapons. This is good for the balance of payments and provides employment in this country. To criticize our contribution to the arms trade might be deemed naive, unpatriotic and irresponsible. On the other hand, as a scientist, I cannot by my silence condone a policy which uses the scientific skills of this country to export potential death and destruction to poorer parts of the world, where their scarce resources would be better employed on food and health.

For a short while, after the Gulf war, we heard much about a new world order in which the arms trade would be severely curtailed. Unfortunately, the rhetoric has faded, and it seems like business as usual. Our economies thrive by building up the Iraqs and Bosnias of the future.

I hope that a British Government will someday tackle this problem. Of course, it has to be dealt with at the international level and by patient negotiation. But morality and our long-term interests point in the same direction.

Within this large picture one can identify specific problems which can be dealt with directly. At the present time attention is focused on anti-personnel miner and the continuing havoc they are causing in various parts of the world, long after official hostilities cease.

Traditional mines contained enough metal that they could be easily identified and recovered by metal-detectors. Newer mines use little metal and are hard to detect. Presumably, they were developed precisely for this purpose. An asset in military operations becomes an environmental disaster when peace follows. Ironically, scientists are now faced with solving a problem of their own making. This is a technical problem dealing with the legacy of the past, but just as important are the diplomatic efforts now being made to ban the use of anti-personnel mines for the future. I regret that our Government, while supporting weaker steps does not appear to be totally behind such a ban.

As I have made clear, I believe scientists should speak out on matters such as these. It would be immoral not to, but, in addition, it shows the public that scientists are not always part of the official establishment and that they can maintain their independence. I recognize that not all scientists can speak with total freedom. Many are employed in institutions where outspoken comment is either forbidden or strongly discouraged. In addition, many will have genuine and understandable conflicts of interest. Would I be so forthright if I worked for a company? All the more reason, therefore, for those of us who are not inhibited by such constraints to speak freely and stimulate public debate.

I am glad to say that scientists can rise above personal advantage on moral issues. An example is provided by the large number of British scientists who publicly refused to have anything to do with the infamous 'Star wars' research of the Reagan era. The British Government of the time encouraged our scientists to apply for American funds for this purpose, but many refused because they believed the whole project was scientifically doubtful, economically wasteful and politically destabilizing.

The atomic bomb is regretfully not the only weapon of mass annihilation that modern science has produced. Chemical and biological weapons can be just as lethal and terrifying. Fortunately, a combination of public aversion and military doubt have now led to international treaties outlawing the development and use of such weapons. Unfortunately, the verification and enforcement of the agreements is more difficult than with nuclear weapons. Scientists can however assist in this process, and I am glad to say that the Royal Society's Committee on Scientific Aspects of International Security has been actively involved in this area.

Science can be directed and applied to many different purposes. If scientists are unhappy about the worst aspects of military applications, they can console themselves with the thought that medical advances save lives, or that the green revolution averted mass starvation. In between these two extremes are many other applications which may be morally neutral but commercially important. I find it an odd reflection on our society that some of the most sophisticated technology, resting on the contributions of our greatest intellects, finds its ultimate destiny in computer games.

Even in the medical field where the benefits of science are most transparent, there are difficult social problems. From a global perspective, the accusation is sometimes made that research is primarily directed to diseases of the rich. In a world economy dominated by a competitive free market philosophy, it will require special efforts to redress the balance, to see that the health needs of the poor are not ignored.

The role of science as underpinning the industrial development of the future has been a main theme of recent Government policy and is behind the move of the Office of Science and Technology to the Department of Trade and Industry (DTI). While it is too early to predict the practical outcome of this on the support of scientific research, it is hard to pretend that it has been resolved with enthusiasm by the scientific community. Even if there are no disastrous consequences, the message does not seem right. To reduce science to such a subservient role is hardly reassuring. It is not that scientists are averse to their research being made use of by industry for the material benefit of society. It is true that we have a few eccentrics, like the late G.H Hardy, who boasted that he had never done anything which was remotely useful – though he would be discomforted to know that bank security codes now use the prime numbers that he delighted in. No, the unease of scientists at finding themselves in the DTI is that science has a much bigger role, even in a utilitarian sense, than can be encompassed by the DTI. Health and the environment are just two conspicuous examples.

But behind these burenueratic arguments about which Department should be responsible for science lie some bigger issues. Scientists share an ethos which in earlier days would have said that they work for the glory of God and the benefit of all mankind. Even in our more secular age I can think of no better way of describing the scientific enterprise. Starting from this high ground various things follow. Scientists certainly have an interest in seeing that their work is not put to improper or wasteful uses and that its benefits are spread across the whole globe. They should speak up publicly in defense of what they believe, so that the public does not identify them too closely with the vested interests of our society. Medical doctors, because of the Hippocratic oath, have always been held in high regard, and the broader scientific community should aim for similar status.

Work for the glory of God is now translated as blue-skies research: not so grand, though it has the right heavenly flavour. This has the primary loyalty of the scientists and it is the fount of true knowledge, not some kind of minor entertainment designed to keep the workers happy. Turn this off, and we shall no longer attract the creative intellects we need for the future. They will migrate to other lands or to other occupations, perhaps even to politics, with unpredictable consequences. Stalin's Russia tried to shackle the creativity of its artists, and the experiment was not a happy one. Scientists need to be treated with similar consideration.

There is no doubt that getting the balance right between the unfettered pursuit of pure science and the harnessing of science for the benefit of society continues to be a major problem. In times of economic difficulty or financial stringency (and these seem to be perpetually with us) governments have a natural tendency to push, nudge or gajole scientists down the utilitarian path. Interestingly enough, this attitude is not supported by many leaders of industry who see a clear distinction between the role of Government in supporting our basic infrastructure and their role in building on that for industrial application. This view is widely held in other countries, and in the United States, 16 chief executives of major industrial companies recently issued a public statement to this effect.

I began by pointing out that to retain public confidence, scientists should be seem to speak out on controversial issues even when this may involve criticism of or disagreement with the official 'partial time'. I indicated that this might be difficult for those who are supported financially or otherwise by those in authority. I hope the Royal Society, despite the fact that it was founded by a monarch and handles substantial public funds, will never feel unduly intimidated. It is some years since a President was ejected from office by the Crown but other dangers lurk over the horizon.

The recent Labour Party document on Science Policy has many excellent ideas which I welcome. However there is one passage to which I take exception, which constitutes a veiled threat. The document deplores the small proportion of women Fellows of the Society, suggests that these should be increased, and hints that pressure might be put on the Society in view of the fact that it receives public money.

Let me first address the substance, the small percentage of women Fellows. The Society is acutely conscious of this fact though I am glad that during my time as President we elected, in Anne McLaren, our first woman Officer. We have studied the matter in depth and decided that the problem has to be tackled at a fundamental level by encouraging and assisting women scientists at various steps of their careers.

We have instituted a number of schemes for this purpose, including the new Dorothy Hodgkin fellowships. However, we recognize that the process is a slow one, and it will take time to produce a larger flow of women candidates for the Royal Society. What we are all clear about, our women Fellows in particular, in that a separate procedure with different standards for electing women is definitely not the solution. Women scientists should be helped and encouraged to aim for the highest standards and not patronized by second-class status.

Let me now turn to the question of political pressure. The Society is an independent body and it is its very independence that gives it standing and authority. This applies both to its internal procedures, such as the election of its Fellows, and to its public pronouncements on matters of scientific concern. There is no doubt that Government, or potential Governments, may feel inclined to exert pressure. Equally the Society, while being open and responsible about the uses to which it puts public funds, must stoutly resist any improper interference.

It is for this reason that the Society is now seeking to broaden its financial base and thus strengthen its independence. As I have tried to indicate, this independence is essential in order to maintain public confidence in science.

I have already alluded to the internationalism of science and I am glad to say that the Royal Society tries to follow up on the new opportunities for scientific collaboration that recent political changes provide. A few years ago we were very active in Eastern Europe. Now we are taking steps in South Africa and the Middle East. In all these places building up science and technology is an important part of the restructuring of the different counties. It should also be one of the concrete measures that can taken since science is not embroiled in political controversy and there is an international community on which to draw for help. Of course, the Royal Society's own resources are limited but we can use our influence to generate funds from private and public sources. In particular, we work closely with the British Council and the Overseas Development Administration (ODA), and we are grateful to both these bodies for their assistance.

As I look back over five years as President, it is natural to ask what have I achieved? Perhaps, more importantly, you may ask the question. First of all, I have to acknowledge that the Royal Society runs quite well, much of the time, without any contribution from its President. I have been told that in former days the President came in once a week, signed a few letters and then returned to his lab. Things have moved on since those days, but it is still true that, due to the combined efforts of the other Officers, the staff and, of course, the Fellows, a vast amount of business is efficiently and quietly conducted. Fellows are elected, grants are given, journals are published, soirées are organized, lectures are held and medals are awarded. The President can claim little credit for all these multifarious activities.

So what does the President actually do except preside? Essentially be helps to steer the ship, while other provide the power in the engine room. So which direction have we steered in during the past five years?

A high priority has been to strengthen our links with similar organizations, both nationally and internationally. In this country we now work even more closely with the Royal Academy of Engineering, the British Academy and the Medical Royal Colleges. We have complementary interests and expertise and cooperation between us is of great potential benefit. When science was in its ivory tower it could ignore other disciplines, but that is no longer the case and we need friends, partners, and allies.

Internationally we have for many years worked closely with the US National Academy of Sciences. These links continue and are developing into joint activities. They have also led on to initiatives involving academies across the world, exemplified by the Conference on World Population that we held in Delhi. At the specifically European level there is increasing activity all round, through other academies and through the European Science Foundation.

In all these international activities, and many others, the Foreign Secretary, Dr. Anne McLaren, has been a great support. She travels extensively on our behalf and has visited counties that many of us would even have difficulty in identifying on the map.

Link with other organizations, whether at home or abroad, are of course just a means to an end. Through these links I have tried to encourage the Society to take a broader view of his responsibilities, particularly in complex areas where science impinges on public policy.

On the domestic front we have done some modernization and tidying up. The Treasurer, Professor John Horlock, has shouldered the border of refurbishing Carton House Terrace and of reorganizing our administrative source. My own contribution has been more modest, and, as befits a mathematician, purely theoretical. You may have noticed that you are now asked to vote on more things and some of our archaic procedures and statutes have been simplified. I hand over to my successor a more democratic organization: he may not thank me for it!

Throughout my five years the Society has been very well served by Peter Warren and all the staff. As you know many of the staff who have been with us a long time have now retired and I would like to take this opportunity of thanking them all. Sheila Edwards is also to retire shortly and our Library will not be quite the same without her.

The Royal Society is a really unique institution, having played a prominent role in our intellectual history for more than three hundred years. The roll-call of former Presidents, which you can now admire above the marble staircase, is both inspiring and intimidating. The task we all have is how to preserve such a venerable institution while adapting it to the changing needs of new centuries. It is a great pleasure for me that I will be handing over to such a good friend and excellent scientist as Sir Aaron Klug to see you through to the dawn of the next millennium.

Mathematics: Queen and Servant of the Sciences

Sir Michael Atiyah

1. 1. Dichotomies in Mathematics. A 250th Anniversary, especially for a Society such as this, is an appropriate occasion for philosophical talk. It provides an opportunity for perennial questions to be re-examined in the light of modern developments. Moreover, a talk about matematics is much more easily conveyed to a general audience than a mathematical talk per se. For all these reasons I propose to discuss the natureof mathematics. The difficulties and ambivalences in this task are clear when we consider the dichotomies that are present. Is Mathematics an Art or a Science? Universities are uncertain about the answer since some award mathematicians a BA degree while others insist on a BSc. Then there is the traditional divide between Pure and Applied Mathematics, but this is complicated by the ever widening scope of applications of mathematics so that even the purest parts are finding unexpected applications. For example, prime numbers are usually regarded as the purest and most useless form of mathematics but recently they have found application in the construction of security codes for banks and otherorganizations. Algebraic geometry has recently established links with high energy physics and mathematical logic is of increasing importance in computer science.

Lying even deeper is the traditional question: are mathematical theorems inventions or discoveries? Is mathematics a creation of the human mind or a reflection of physical reality?

The title of my lecture: "Queen or Servant of the Sciences" is another variant, presented in more poetic form and playing on our prejudices. Let me begin, in the best traditions of analytical philosophy, by subjecting this title to some textual analysis.

It is perhaps useful to remember that all royal families have (ultimately) humble origins: the queen has evolved from the servant. Moreover their roles are sometimes confused, when the monarch is viewed as the servant of his people. This asoect is amusingly captured by the Gilbert and Sullivan song which concludes with the Gondolier princes singing: "But the privilege and pleasure That we treasure beyond measure Is to run on little errands For the Ministers of State."

In our own time we are left only with constitutional monarchs having ceremonial duties but little authority. So, if we refer to mathematics as "queen of sciences", do we have in mind a decorative symbol or a source of power?

Dichotomies are useful devices to provoke thought. Like paradoxes they highlight the difficulties but they do not provide solutions. A more constructive metaphor is to view mathematics as the language of science, and it is this idea that I would like to develop, beginning with a brief look at natural language.

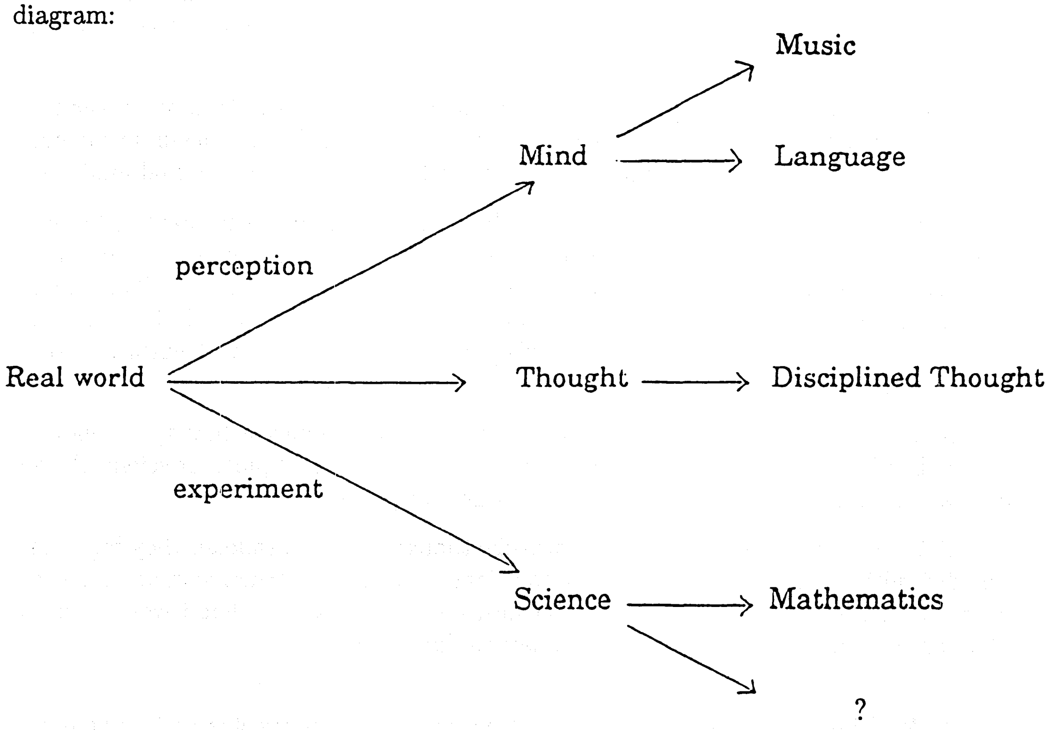
2. 2. Natural Language. How did natural language eveolve and what is its function? Human beings have perceptions of the "real world" (which includes themselves), they reflect on this, and produce descriptions and explanations which they then communicate (to others) via language. Concepts have to be developed, names given and manipulated logically (grammatically) to produce sentences. We might say language is the "externalization of thought". Note that primitive thinking is not verbal but visual; this becomes clear if we consider animals or young children.

But language as it has developed has many different layers and serves many differebt purposes. There is a spectrum, from poetry (and other forms of literature) to the lower forms of information communication as in newspapers. There are also specialized forms of language dealing with fields such as Law. Beyond this there is the study of language itself as in linguistics or philology. We recognize that poetry has an aesthetic and creative component, which frequently transcends the strict rules of grammar, and that it is constantly searching to extend the scope and meaning of language. Moreover, in time, this creative aspect influences the more utilitarian forms of language, so that Shakespearean quotations now abound, even in newspapers.

Language is both grammar and literature. It embodies concepts and meaning and it is hard to separate words from their meaning. Ideas create words and words enable us to formulate new and more complex ideas.

3. 3. Mathematics. Mathematics starts with ordinary language but digs deeper, with greater precision (starting from Numbers) and develops further concepts and rules. It can be viewed as a specialized superlanguage. George Boole explained the relation between grammar and the algebra of symbolic logic (now the foundation of computer science) in which equations play the role of sentences.

We can illustrate the analogy between mathematics and language by the following diagram:



Just as primitive thought (e.g. in animals) is non-verbal, so primitive science (e.g. associating heat and light) is non-mathematical. Moreover, the mind has produced several types of language in which to express itself, music being one example. At present mathematics is by its depth and scope the pre-eminent language of science, but it remains to be seen whether other types of language (indicated by ?) will be needed.

If we accept this analogy then we can begin to understand how mathematics has a creative/aesthetic side, like poetry, where the imagination is being stretched and a more utilitarian side, as used by engineers in routine calculations. Also mathematics is constantly being enlarged by the addition of new concepts in response to the advancing needs of science. This may be compared with the growth and development of language, having to deal with the needs of sophisticated modern society.

As with language, where thought and word interact with one another, so science and mathematics interact with each other. It is difficult to separate contents and framework: each influences the other in a complex symbiosis. It is for thisreason that I have no difficulty in describing mathematics as the language of science. Some of my colleagues might feel that this gives mathematics too humble a status, that of the "servant" and they would prefer the loftier position of the "queen" from whom all authority and beauty emanates. But if we reflecton the power of words, and the role they play in organizing, refining and transmitting ideas, then we see that the role is an honourable one. Ideas without words remain vague and ineffective, and science without mathematics remains similarly handicapped.

4. 4. Mathematics and Physics. Let us consider as a key example the particular relation between Mathematics and Physics. This is the oldest and most intimate. Its most famous embodiment is in Newton's Theory of Gravity. The inverse square law, that all material particles attract each other with a force inversely proportional to the square of their distance apart, proved a spectacular triumph. It explained the planetary system and much else, but the notion of "action at a distance" was philosophically controversial. It was not physically detectable (on small scales) and it was essentially a "mathematical fiction". In due course however it became accepted, attitudes changed and gravitational force is now accepted as a physical phenomenon, with mathematics only acknowledged as a tool when one works out the consequences of the basic laws. In fact historically, philosophically and logically mathematics is there at the beginning in providing the formulationof the basic laws.

In the first half of the 20th century two major new physical theories emerged: Einstein's Theory of General Relativity and Quantum Mechanics. In each case we have very sophisticated mathematical theories which are philosophically difficultto grasp. The only way that they can be understood is in mathematical terms: ordinary language is totally inadequate.

As the 20th century has progressed this process has moved on inexorably. The search for the ultimate building blocks of matter, and the ultimate forces that bind them, delves deeper and deeper into ever more esoteric mathematics. As we get further away from common experience and as experiments become more and more costly, mathematical coherence becomes the predominant criterion. Producing a mathematical theory which embraces all known experimental facts and is internally consistent becomes the real driving force in theoretical physics.

Current ideas involve radical notions which question the fundamental position ofspace-time, postulate quantum "super-strings" instead of particles and conceive of abstract higher-dimensional spaces. "Reality" becomes a purely mathematical structure: the only debate concerns rival mathematical theories. As telescopes probe to outer space and microscopes to minute sizes, so mathematics is our intellectual probe to physical reality.

But the precise type of mathematics needed is itself developed in response to the requirements of physics. At present an interesting mixture is being explored,partly rigorous mathematics, partly infused with physical intuition.

Like mystical poetry, some grammar ignored, part intuition, part language, searching for the deepest truths, a new language is being developed. The aim remainseventually to build a logical structure consistent with experiment. This would represent the final take-over of the physical world by mathematics.

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The Conscience of Science:
Schrödinger Lecture, Imperial College
Sir Michael Atiyah
Tuesday, March 18th, 1997

It is a great pleasure for me to deliver this tenth Schrödinger Lecture. As we all know, Schrödinger was one of the great pioneers of quantum mechanics, but in subsequent years his interests broadened out in many other directions, both scientific and philosophical. This has the great advantage that Schrödinger Lecturers need feel no inhibitions in their choice of material, and I am today taking full advantage of that freedom. I hope that Schrödinger would both have been interested in what I have to say and that he would have approved of it.

In order to prepare myself for this lecture and to put myself in the right mood, I recently read a biography of Schrödinger and realized what an unusual and complex man he was. Like so many of the European scientists of his generation, his life was profoundly affected by the upheaval leading to the war. Although he was not Jewish, his outspoken comments led to his dismissal from the University when the Nazis took over Austria. Unlike the majority of scientific refugees, he did not settle in Britain or the United States, but in Ireland. By a quirk of fate Ireland had, in de Valera, a Prime Minister who was also a mathematician and a great admirer of Schrodinger. De Valera established the Institute for Advanced Studies in Dublin in 1939 specifically for Schrödinger, who stayed happily there until he returned to his native Austria in the last years of his life.

Schrödinger, as a scientist and thinker, had much in common with Einstein. Both made their major discoveries as a result of profound insight and they were not satisfied with the Copenhagen interpretation of quantum mechanics espoused by Niels Bohr and generally accepted by the physics community. Schrödinger and Einstein both searched for unified field theories and though they were not successful in their time, current ideas in physics go some way towards justifying their views.

In the shelter of neutral Ireland, Schrödinger kept away from politics, but he thought deeply about the philosophical meaning of science, and his little book entitled *"What is Life?"*had a remarkable impact on the next generation of biologists. In all, he was a highly individual thinker.

Before I move to the main theme of my lecture, some personal digression is, I think, in order. For most of my life I have been in University research, working out of the limelight on Pure Mathematics, though latterly my interests led me into Theoretical Physics and Quantum Mechanics. However, a major change occurred when unexpectedly I found myself President of the Royal Society, in a very public position, and expected to act as a general spokesman for the whole of science.

Faced with this new challenge I asked myself what was the essential function of the Royal Society and what public issues should I, as President, be addressing? Of course, this question can be answered in various ways; but the answer that I found most appealing, and that has been attributed to one of my predecessors, was that the Royal Society sould act as "the conscience of science."

During my five years as President I had time to reflect on the meaning of this phrase and how it should be interpreted. This lecture today gives me the opportunity, which I welcome, of sharing my views on what I hope to persuade you in an important topic for all of us.

Essentially I want to address the question: Are scientists responsible for the ultimate applications of science, with all its consequences? Should this stir our conscience?

Let me begin by demolishing one possible line of defence used by the 'pure scientist' who says: "I work on basic science, advancing knowledge. It is engineers or applied scientists who have to worry about the consequences. My conscience is clear." In an institution like Imperial College which has always emphasized the intimate links between science and technology, the audience is unlikely to be taken in by this specious argument.

But perhaps it is as well if we take a quick historical look at the evolution of science to convince ourselves of the essential links between basic science and its applications. Euclidean Geometry with its emphasis on axioms and proofs, is often taken as a typical illustration of pure mathematics, but no one doubts that this emerged from earlier practical experience of the real world. A more doubtful example is provided by the famous 16th century astronomer, Tycho Brahe, whose research is reputed tgo have cost the Danish king 10% of the Royal budget. Expenditure on this scale is only justified by practical applications and astronomoers in those days justified their keep by predicting heavenly events which would guarantee success in battle.

But it was Francis Bacon who set out the clearest vision of a scientific community that would seek to understand nature for the practical benefit of mankind. This vision was, in fact, the inspiration that led to the founding of the Royal Society. Incidentally, Bacon thought that some scientific discoveries were too dangerous to be disclosed to the state and should be restricted to the scientific community. This was a far-sighted, if ultimately impracticable, aim.

Some people, have argued that despite this laudable aim of scientists, one should not make exaggerated claims on their behalf and it was really the engineers, the inventors, who led the industrial revolution in the 18th and 19th centuries. There may be some truth in this assertion but only if one ignores the fact that the engineering of one century is made possible by the intellectual climate created by thinkers of previous centuries. I find it difficult to believe that railway locomotives were entirely uninfluenced by the ideas of Newtonian Mechanics.

When we look to the 20th century the pace is speeded up and the practical consequences of basic science are more evident. The vast electrical and communications industry on which modern society is based is crucially dependent on the work of Faraday and Maxwell. Nuclear energy, which is now a major source of power (and to which I will return) emerged from a thorough understanding of the nature of matter. In the biological field the discovery of the double helix is now bearing practical fruit and will have enormous consequences in the next century.

So I hope we can all agree that scientific discoveries lead, in the fullness of time, to practical applications in engineering and medicine, affecting the lives of all mankind. A scientist, pursuing research, may be only dimly aware of, or motivated by, the potential consequences. Moreover, each individual contribution in the jig-saw puzzle may not seem significant. But, taken as a whole, the scientific enterprise has transformed the world and looks set to transform it yet further.

In so far as the practical applications are beneficial, scientists no doubt take collective pride in their contributions. But, what of the downside? Should we not be prepared to accept a share of the blame for misuse or unfortunate consequences of science?

The orthodox reaction is on the following lines. If we cannot put the blame on the applied scientists or engineers, because they are too close to us, then we must put the blame on the politicians who make the decisions. Admittedly, in a democracry, politicians theoretically act on behalf of the people and a scientist, in his private capacity as a citizen, can attempt to influence public affairs.

The attempt to distinguish between the scientist as a creative research worker and the scientist as citizen is, I think, too simplistic and too easy an escape. I believe that scientists have a very particular responsibility, well beyond that of the average citizen, in trying to ensure that science is put to the best use and that any harmful consequences are minimized. Let me try to list the reasons why scientists have a special role and obligation:

1) First there is the argument of moral responsibility. If you create something, you should be concerned with the consequences. This should apply as much to making scientific discoveries as it does to having children.

2) Scientists will understand the technical problems better than the average politican or citizen, and knowledge brings responsibility.

3) Scientists can provide technical advice and assistance for solving the incidental problems that may emerge.

4) Scientists can warn of future dangers that may arise from current discoveries.

5) Scientists form an international fraternity that transcends natural boundaries, so they are well placed to take a global view in the interests of mankind.

6) Finally, there is need to prevent a public back-lash against science. ("We have to stop these mad scientists from ruining the earth, creating monsters, or blowing us all up.") Self-interest requires that scientists must be fully involved in public debate and must not be seen as "enemies of the people."

I put this self-interest argument last because it is, I think, lowest on the ethical scale. However, for those whose conscience is elastic, and who are not swayed by broader ethical issues, self-interest can be compelling. Even if you are not an angel at heart, it is good PR to be seen on the side of the angels.

So I hope I have convinced you that, if only as a matter of self-interest, scientists must acquire a social conscience and concern themselves actively with the political process in so far as this relates to the use and misuse of science. In our present complex technologically-based society, this is a fairly all-embracing agenda.

As a scientist, you are now a concerned citizen in making your contribution to the scientific-political debate. You want to get involved, make use of your expert knowledge, and assist the public. Well, it is great to have got you so enthusiastic, but there are a few obstacles to talking freely. There is such a thing as secrecy.

Educated and trained in the free climate of a University, you will have learnt that the unfettered circulation and publication of ideas is the life-blood of science, that the spread of knowledge is a good thing, and that open discussion is the essential criticism that validates scientific truth. You are in for a rude shock. In the real world, secrecy is the name of the game. It comes in many forms and is widespread, even in democratic countries.

First, there are military secrets, and a substantial portion of our scientists and engineers are involved, directly or indirectly, with military research. If you are in this position, you may be well-informed about matters of public interest but you are effectively debarred from taking part in open debate. Even when you have moved on and are no longer directly involved with military matters, your lips are supposed to be sealed. At least that seems to be the case in the UK where freedom of information is not yet as highly prized as it is in the United States. So we have the odd situation where the only people who have sufficient technical knowledge to inform the public are prevented from doing so. It is then hardly surprising if the public is fed on a mixture of government propaganda and media hysteria– a somewhat indigestible combination.

So, as a dedicated scientist and responsible citizen, you decide to keep away from anything military. This may be more difficult than you think since your research might in some indirect way be supported by, or of interest to, the Ministry of Defence. Still, you persevere and make sure that you are employed by a civilian organization, perhaps a pharmaceutical company which aims to preserve people rather than to kill them. You have lofty humanitarian aims. Alas, you soon discover that secrecy is not the prerogative of the military alone. Commercial secrecy and the struggle for patents is just as potent. "Publish or perish" may be the slogan in the academic world, but in the competitive commercial market it is more likely to be "Publish <u>and</u> perish." You may be a whiz-kid in bio-technology, who could enlighten a much-concerned public about the benefits of dangers of the latest research, but you are severely constrained in what you can divulge. Moreover, your utterances may be biased by commercial considerations and you are unlikely to be trusted as a source of disinterested information.

If the military and commercial worlds are too obsessed with secrecy then how about entering the public service? Surely, as a servant of the people, you will be free to put your knowledge into the public arena? This is an understandable hope but much too naive. Civil servants are only indirectly responsible to 'the people'. In between come Ministers and the Government of the day, whose main concern is usually to prevent embarrassment. Scientific truth may not be helpful to Government policy and if so it is better suppressed. Civil Servants who step out of line do so at their peril and "acting in the public interest" is not usually accepted as a legal defence.

If all these avenues look unpromising for you as a concerned scientist, then perhaps you have no honourable alternative but to stay in academic life. You can tell the Rector of Imperial College that you quite like it here and you want to stay on! Assuming the Rector is co-operative you are at least free to speak your mind. You can rail against nuclear weapons, the patenting of DNA or the dumping of oil-rigs in the North Sea. But you may have to be careful and check who is really funding your research. Universities have to depend on a wide variety of financial sources and the Rector might get a bit worried if his staff was constantly biting the hand that fed them. He might invite you to his office and politely explain how much he sympathized with your views, how determined he was to maintain academic freedom, but perhaps you could be a trifle more circumspect in your public utterances.

Even if we leave out legal inhibitions or financial apron-strings, there are still subtle social pressures that act within the scientific elite. It is most evident in disputes with the environmental movement or with the media and it can be described as the "we know best" syndrome. If there is an official establishment line on a controversial scientific topic, it is regarded as poor form for a scientist to question it openly and side with the opposition. Because protest movements and the popular press, aiming to attract attention, inevitably tend to exaggerate, there is a tendency to write them off as unworthy of serious consideration. A scientist who ventured, however tentatively, to see merit in their case would be seen by his colleagues as letting the side down and providing succour to the rabble.

Now that I have shown you the limits of free speech, perhaps I should turn to the matters of substance. What are the major issues that scientists should be concerned about? Which are the areas where the application of science has been harmful and what threats are there in the future that we should try to avoid?

It is not hard to identify three main areas where science has had a potentially devastating impact and has left us with vast problems that will dominate the next century.

First, there is the enormous military threat posed by the weapons of mass destruction: nuclear, chemical and biological. Not only are these weapons awesome in their destructive power but the scientific contribution is unambiguous. There may not be much science in bows and arrows but there certainly is in the atomic bomb.

The second potential catastrophe that can, indirectly at least, be laid at the door of science us the population explosion of the world. Improving health care and the elimination of many diseases are the benefits of medical research. The reduction in child mortality and the dramatic improvement in life expectancy are great humanitarian triumphs. But the resulting rapid growth of the world population presents us with a major problem. The social, economic and environmental stresses that this has produced are all too evident and we are rapidly approaching the limit that the earth can sustain.

Finally, there is the general degradation of the environment arising out of the improved life style that science and technology have made possible. The motor car is perhaps the most obvious symbol. A great asset to each individual, allowing for mobility and convenience, but collectively an environmental disaster polluting our air and clogging up our streets.

Of course the population explosion accentuates the environmental problem and can be viewed as part of it.

It is hard to deny that these great problems are the major issues facing mankind as we come to the end of the remarkable 20th century. It is also hard to deny the role that science has played in creating them. What is surprising and a little depressing is that they appear to be almost entirely ignored by our politicians in the current election campaign. It is a sad reflection of the democratic process as it operates in this country. Perhaps it is our responsibility as scientists to keep reminding our fellow citizens of the fundamental problems that the world faces, as opposed to the petty parochial problems that attract their attention.

Let me discuss in a little more detail the problem of nuclear weapons since they still represent the greatest threat to all of us. Perhaps it is helpful to review their history over the past fifty-odd years.

As is well known, the first moves came from some of our leading scientists. After the initial work by Otto Hahn in 1939, showing that bombardment by neutrons could split an atom of uranium, Frisch and Peierls in this country and Einstein and Szilard in the United States wrote to their respective governments, pointing out the military implications. Incidentally it is an amusing reflection on British bureaucracy that, as "enemy aliens" Frisch and Peierls were not, at first, allowed to see the top-secret correspondence which their move had generated!

The subsequent history of the "Manhattan Project" at Los Alamos, leading to the atomic bombs over Hiroshima and Nagasaki, is well-known and the moral dilemma of the scientists has also received much attention. As long as there was a significant possibility that the Germans or Japanese might succeed in producing atomic weapons it seemed inevitable that Britain and the United States should press ahead. But by 1944 it was clear that the German effort was too little and too late to affect the course of the war and the Japanese were even further behind. This was the signal that led one of the Los Alamos physicists, Joseph Rotblat, to withdraw from the project and devote himself to more peaceful science. More than 50 years later, after a life-time involved with attempting to limit the dangers of nuclear weapons, Rotblat was fittingly awarded the Nobel Peace Prize.

But let me return to 1945 and its aftermath. For several years after the end of the war there were serious attempts to grapple with the atomic threat, but mutual suspicion at the international level prevented any agreement. This failure triggered the stupendous arms race that followed in which other aspects of science and technology added to the nuclear threat. Missile technology, combined with the power of modern computers and telecommunications, produced the ultimate weapons that could destroy the entire world at the push of a button.

Perhaps the politicians and the generals must take the main blame but many scientists and engineers were eager partners. It was a distinguished physicist, Edward Teller, who was the prime mover behind the Hydrogen Bomb and who constantly urged the military establishment to press ahead with the latest technology. It was scientists who were pressing for the anti-ballistic missile defence system that acquired the notorious title of 'Star Wars'.

I am sure that the scientists involved thought they were acting in the national interest, enhancing security and deterring enemies. But it has to be acknowledged that, to the outsider, the scientific advice and encouragement of more and more sophisticated military programmes could be seen as self-serving. It gave scientists status, prestige and resources. A lot of excellent scientific research was funded through the largesse of the US Defence Department budget, and it required considerable self-denial to turn down such support. I am glad to say that, in this country, many scientists publicly refused to accept US research funding in aid of 'Star Wars', despite considerable pressure and encouragement from the UK Government of the day. This shows that it is possible for scientists to make a stand on moral principles and it is important, for public perception, that they are seen to be doing so.

So the arms race continued with a build-up of nuclear weapons which, a few years ago, had an explosive power equivalent to two tons of TNT for each of the world's inhabitants. If only a small fraction of these weapons were ever used, the destruction of Hiroshima would have paled into insignificance. If the human race survives well into the next millenium, people will look back on the latter part of the 20th century as the time when we came closest to collective suicide.

Fortunately, we seem to be moving in the right direction. As a result of various international agreements, painstakingly negotiated over many years, and proceeding much more rapidly in the past decade, many types of nuclear weapons are being dismantled. In a few years' time the total stock of such weaponry will be one-fifth of what it was at its peak.

Although the political changes in Europe, beginning with Gorbachov and progressing to the break-up of the Soviet Union, have made the task much easier, the initial moves came at a more difficult time when political antagonisms were still deep. I am glad to say that these complex negotiations involved many scientists, some of whom worked through the Pugwash Conferences which fittingly shared the Nobel Peace Prize with its President Joseph Rotblat. One of these, who died just two years ago, was Sir Rudolf Peierls, my colleague for many years at Oxford and one of those who initiated the development of nuclear weapons: a fine example of someone whose conscience stirred him into action.

Because of the progress that has been made in reducing stock-piles of nuclear weapons and because of the changed political climate, there is a tendency to become complacent. Nuclear weapons have disappeared from the headlines and are not seen by most people as an imminent threat. But the nuclear weapons that still exist remain a vast potential danger and those who are more far-sighted are urging further action at the present time, while the political tensions are low. The Canberra Commission, an international group of distinguished and experienced people including politicians, generals and scientists, has produced a report arguing for a substantial programme which should aim at the total elimination of nuclear weapons. I should emphasize that this is not the report of a group of wooly-headed idealists. The Commission contained figures like Robert McNamara (former US Secretary of Defence) and General Lee Butler (former Commander of the US Nuclear Deterrent). Its proposals are measured and realistic and set out a framework which could eventually eliminate the possibility of nuclear catastrophe. I commend it to you as an important document that will, I hope, engage the attention of our political leaders when the election is over and they can turn their minds once more to serious business.

I once spent a sabbatical term as a visitor at the Enrico Fermi Institute in Chicago. As you will know, Fermi was a great physicist who pioneered experiments in nuclear fission. My office in the Institute named after him looked out onto a square which contained the powerful sculpture, by Henry Moore, of the mushroom cloud which depicted and came to symbolize the atom bomb. Those of my generation have lived in the shadow of that cloud most of their lives and we should all do what we can to lift it from the lives of succeeding generations.

When I alluded to weapons of mass destruction, I mentioned chemical and biological weapons as well as nuclear weapons. Clearly all these are based on science, and scientists are heavily involved with them at all stages. Fortunately the world has already stepped back from the brink on chemical and biological weapons. There are now international treaties that ban their use, and countries that possess stock-piles have undertaken to destroy them (although they still await ratification by the US Senate). There are plans for control and verification that are designed to stop any clandestine research or production. Unfortunately these sort of checks are difficult to carry out. The facilities that are required for military purposes are not so different from those for normal commercial use. Unlike nuclear weapons very large scale laboratories are not needed and so external identification is harder. Moreover the dividing line between the kind of research that is needed for peaceful purposes in the chemical or pharmaceutical industry is sometimes difficult to separate from that which may have military applications.

Scientists, as the only ones who thoroughly understand the technicalities, have been closely involved in drawing up the complicated international conventions on chemical and biological weapons. Moreover they will constantly be needed in the future monitoring of these conventions. It is not only as official inspectors that scientists will have to be on the alert. The world is too big a place to be adequately supervised, in the necessary detail, by armies of inspectors. We shall have to rely on individual "whistle blowers", scientists who suspect that conventions are being broken and who bring the matter to international attention. This also applies to nuclear weapons, at least to the small-scale infringements that are difficult to detect.

This "whistle-blowing" role for the individual scientist, in the capacity of a world citizen depends, of course, on a legal and social framework in which such activities are tolerated. As I have already mentioned, not many countries, even democratic ones, allow their citizens the necessary freedom of speech. The leaking of state secrets may incur severe penalties, even if the leak uncovers activities that are contraventions of international obligations.

A citizen's conscience may impel him or her to break the laws of the country if it is in the wider international interest, but not all of us wish to be martyrs and we should press for the necessary protection of those who are trying to get their own governments to abide by international agreements. More generally there should be a clear "public interest" defence for those accused of disclosing information. Scientists who are after all in the business of creating and disseminating knowledge should be in the forefront of those demanding greater freedom of speech.

Although weapons of mass destruction provide the extreme test of the scientific conscience we can hardly turn a blind eye to the role of science in other aspects of warfare. It was presumably a chemist who invented napalm, an efficient device for burning people alive and far more sophisticated than the primitive bonfire at the stake that was used in medieval times. Anti-personnel mines are another of our great inventions, designed to blow the limbs off the rash intruder. Moreover, ingenious scientists have produced mines which traditional devices fail to detect, making them a permanent hazard long after official conflict has ceased. Princess Diana has recently high-lighted this continuing tragedy and her campaign will I hope add pressure on all countries, including our own, to ban the use of or sale of such mines.

These are just two examples of new weapons produced by our scientists which, by their nature, stir our consciences. But the whole arms industry, with its constant search for new and more deadly weapons is one that intimately involves a large part of the scientific community and poses serious moral questions. The countries of the world spend vast amounts on military expenditure diverting resources from more essential purposes. This is bad enough in advanced industrial countries where, as we hear every day, essential services in health and education are under a constant squeeze. But it is infinitely worse in the poorer parts of the world where the bare essentials of life are lacking, where the majority of the population are under-nourished and in bad health, and yet their governments lash out to buy fancy and expensive military hardware.

To my mind it is the international arms trade in which the wealthy countries of the world export their weapons to poor countries that can ill afford them that should trouble our consciences. In fact, the evils of the arms trade and the way this fuels trouble in many parts of the world is so well-known and so frequently brought home to us on our TV screens that you might wonder why it survives. Surely we, the enlightened citizens of the wealthier countries of the world, could collectively ban or control the export of arms?

The trouble is that, as with all real ethical problems, our conscience is subverted by what we see (perhaps wrongly) as our self-interest. Our arms industries, involved in developing hi-tech expensive weapons, need large markets to cover their costs. We, in the UK, can collaborate with our European partners to share the burden, but international competition is still a major factor. We look further afield and try to foist our armaments on former colonies or other countries in our sphere of influence. In this competition it clearly helps to have client states which are tied to us by a combination of factors: historical, economic and political. Many of these countries may be run by cliques or oligarchies whose power and continued existence depend on our support.

I should emphasize that, by referring to 'us' I only mean to show that we are involved in the problem. I do not mean to imply that it is exclusively a British dilemma. Our colleagues in France, Germany, the USA and Russia face the same situation.

From time to time such matters make the newspaper headlines: a major deal to sell hundreds of tanks or aeroplanes is about to be clinched, our Foreign Secretary flies out to help the process and to guarantee jobs for our factories. The ethics of the sale are secondary, the focus is all on the employment prospects and the financial benefits. Occasionally, some minor criticisms appear about the human rights record of the regime we are supporting or its treatment of ethnic minorities, but these are invariably overridden by appeals to our national interest.

The scientist employed, directly or indirectly, by the armaments industry may feel uncomfortable with the ultimate destination of what he or she works on, but as a pawn in the whole process it is difficult to see what can be done. One could resolutely keep away from any military contracts but, in certain fields it is hard to disentangle research into civilian and military compartments. If one is doing research on computers or telecommunications it is unlikely that there are no military ramifications.

All of this highlights the degree to which scientists have been absorbed into the military-industrial complex. It was Eisenhower, a general turned politician, who coined this phrase and he clearly knew what he was talking about. He was identifying the intricate web that links military needs with the industrial infrastructure, and scientists are right at the core of that link.

Of course science benefits enormously from the support it gets from military and industrial quarters. In some areas it may be almost totally dependent on this patronage. Not only does it finance research but it also enhances the status and self-esteem of the scientists concerned. We all like to feel important and there is nothing like a few files marked Top- Secret to raise one's ego.

But there is a price to be paid for this cosy relation between the scientific community and the military-industrial complex. First there is the strain on one's conscience in being in doubtful company and conniving at undesirable practices. Second, and just as important, is the loss of independence entailed and the tarnishing of the scientific image in the eyes of the public. On the one hand we like to say that science is about the search for truth for the benefit of mankind. On the other hand we are seen arm in arm with those who deal with secrecy, death and destruction. We are likely to lose credibility and popularity, making it more difficult for us to play our proper role in society.

I have spent perhaps a disproportionate amount of my time on the military dilemma but, as I indicated at the beginning, there are many other areas where science is involved and where scientists have to examine their conscience. The military case is just the most extreme but the relation between science and industry also produces tensions in other areas.

I identified the environment and pollution problems as another major source of concern. Science has initiated the technology which, as a by-product, has degraded our environment and, to put it bluntly, it is up to us to clear up the mess.

Consider for example nuclear power. In many ways this is an ideal way of dealing with our energy needs on a basis which is sustainable on a long time-scale and also does not threaten us with global warming. Yet, in most countries, plans for nuclear power plants have been drastically cut back in the face of public opposition. Why is this? The official establishment view is that the Green Movement has misled the public by ill-informed criticism, fanning the flames of suspicion. A few nuclear accidents, notably Chernobyl, have been seized on and grossly exaggerated. A great scientific and technological opportunity has been lost because unscrupulous agitators have played on the fears of the public.

There is some truth in this picture but what it ignores is the degree to which the public has lost faith in the scientific community. Because of our involvement with the military, with government and with industry, scientists are not trusted. Past secrecy, or lack of openness, makes us suspect and produces hostility. Assertions of safety are not easily accepted.

It also has to be said that there are technological problems in the disposal of nuclear waste that have not yet been solved and are a major embarrassment for the nuclear industry. As you will know, there is still great controversy about the long-term safety of burying medium level nuclear waste in deep rock deposits and the plans of NIREX (the body responsible for dealing with this problem) are being subjected to careful scrutiny. As you may have heard, these plans were turned down today by John Gummer.

Because of the long life-times of some of the radioactive material, there are concerns about the possible pollution of ground water by seepage from the buried waste, over periods of many thousands of years. This means, for example, that one has to worry about the geological effect of the next ice-age. Given the complex problems of chemistry, geology and fluid flow involved, it is difficult to see how one can have great confidence in predictions over the necessary time-scales. One does not need to be a radical environmentalist to question the long-term outcome.

Although it is difficult to predict physical processes say over 100,000 years, it seems by comparison rather easy to predict that in 100 years' time we shall know a lot more science, and have better ideas on how to dispose of the awkward nuclear waste. On these grounds alone it has always seemed to me that the deep disposal of nuclear waste ought to be in retrievable form so that our successors can extract it easily if they have a better idea. I am glad to say that this now seems to be accepted and is included in the latest document from NIREX.

This seems to be a case where it is better to acknowledge inherent scientific uncertainty and plan accordingly.

Problems of the environment, including those arising from the growth of world population, may have their origins in science, and science has much to contribute to their solution. But the problems are vast and complex involving economic, social and political issues. The scientist can only hope to affect the outcome by participating in the political process. Only the scientist has the relevant technical knowledge to analyze the problems and propose solutions, but it is not easy to operate in the public limelight and under the intense pressures that can be brought to bear.

Perhaps I can illustrate this from my personal experience. A few years ago, while I was President of the Royal Society, I was invited to address the annual lunch of the Parliamentary and Scientific Committee, an event attended by several hundred people including Ministers and leaders of industry. I took the opportunity for arguing the case for a ban on tobacco advertising, in the interests of public health and particularly in the interests of the younger generation who are most at risk. After the lunch one of my more experienced colleagues congratulated me on a "brave speech". I did not consider I had been particularly brave, but that was due to naivety. Over the next few weeks I was subjected to a campaign of vilification by the tobacco industry who claimed that I had demeaned the role of President of the Royal Society by distorting the evidence. In fact I had taken care to consult my statistical colleagues and I was fully aware of the sophistry being put out by the advertising lobby. For many decades the tobacco industry has been spending vast sums advertising its case, ignoring scientific evidence and exerting pressure on those in power. I was simply the latest in a long line of those to be exposed to this force.

For a scientist to be involved in the political process is not easy. There are pressures from many directions and truth is a frequent casualty. But one cannot escape the realities and one has to be prepared to argue one's case robustly.

Let me try, in conclusion, to sum up my message. I have been trying to convince you that, because science has produced such drastic changes in all our lives, scientists have a moral obligation to be concerned. We should try to ensure that science is not misused and we should try to find solutions to the incidental unfortunate by-products of scientific progress.

I may have painted an unduly bleak picture of the difficulties that we face: the secrecy, the malevolent forces, the hysterical media and the ill-informed public. Perhaps I should try to redress the balance by pointing out that the task is not hopeless.

As a top priority I would put freedom of information and the elimination of secrecy. Science is inherently about discovering and disseminating the truth and anything that hinders that should be opposed. Fortunately there are many groups pressing for freedom of information and I hope we can follow the United States in this direction. It is encouraging that the Office of Science and Technology has today issued new guidelines, on the use of scientific advice in policy making, which argue for a more open and transparent consultative process.

Next, I would urge scientists to cultivate the media. There is an increasing number of scientifically trained intelligent journalists and broadcasters who can help to inform the public, acting as a bridge to the scientific community. They have a difficult role to perform since their editors may prefer the controversial head-line to the measured sedate argument, but that is no reason for the journalists to be spurned or vilified by the scientific community. We have a common objective in seeing that scientific issues are properly presented to the public.

In referring to malevolent forces and in my digression on the tobacco industry I may have misled you into believing that all industrialists are villains and our natural enemies. That is not so. There are many enlightened Chief Executives who realize that the public interest is not necessarily incompatible with the company's interest, provided one takes a broad enough point of view. On the other hand the institutional and commercial pressures make their task difficult and they need allies from outside, including the scientists.

Finally we come to the ill-informed public - in other words "the people". There are certainly times when popular movements, fanned by ignorance, mis-information or bigotry, appear hostile to science or to the commercial applications of science. We may deplore this, but we cannot ignore it. We have to counter the ignorance as best we can and to harness public opinion into constructive directions. There are powerful vested interests in government and industry which will only respond to substantial popular pressure. This is entirely appropriate in a democratic society and provides a counterweight to bureaucratic and financial power. Scientists should have the people on their side.

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A LETTER FROM RAOUL BOTT

R. BOTT*

January 20, 1999

Dear Michael,

It is with great pleasure that I salute you on this happy occasion! With my "Bravo" for all your achievements, and best good wishes for the future, I also bring good tidings: There is life after 70, in fact it is a "breeze"! For at our Biblical ages the intense youthful pressures for self-achievement fall away and, granted tolerably good health, we can perceive our subject through more evenhanded grandfatherly eyes. It is also an age in which we can: "Speak when we have Nothing to say," as Serre so charmingly put it, to our hearts content.

But primarily, I would like to take this opportunity to thank you and Lilly for the many wonderful memories that I and my whole family now have of our long and happy collaboration. Apart from the exciting memories of our day-to-day working together: at times in complete harmony, at times with skirmishes on points of view, but always with complete honesty, these memories also abound with purely human moments of camaraderie, fun, adventure, and, at times, trauma.

Preeminent amongst these moments is of course our trip to India in 1960! The impact of this remarkable new culture on us was accentuated by the debacle of my having no visa and all the many, at times, amusing consequences that ensued. For instance, the wonderfully absentminded gentleman whom Chandra Sekharan appointed to put things right, and who instead muddled things up even more after giving us a charming tour of Delhi. But surely most unforgettable of all, was that sight of the Taj Mahal in full moonlight on a clear balmy night!

And then there are the early memories of St. Catherine's, the hard work there combined with the fun and pomp and circumstance of academic life in Oxford. There are the daily walks from our offices across the parks, often in the company of Graeme and or Nigel, to luncheon and St. Catherine's, followed by the give-and-take at the luncheon table regarding American versus British foibles.

All these early memories – now already shrouded in the mystery of having once been young – compete with so many later ones! Our visits to you at the Institute for Advanced Study, your visit at Harvard during the Cuban missile crisis, your later visits to us at Dunster house where I had foolhardily become Master, only to be trumped by our visits to your Masters lodgings at Trinity!

All these personal recollections, and many more, are joyous mementos of our 40-yearlong friendship and collaboration. For me, they also serve as signposts for the problems we worked on. For instance, there is Woods Hole where we worked on our Fixed Point Theorem, and which I now associate with a bright afternoon when we tried to keep up with my son's frisbee throwing. There is the long trip from Trinity back to Oxford in your car, where we hatched our localisation theorem for Equivariant Cohomology, and many of our aforementioned walks to St. Catherine's dealt with K-theory in the 60's and Yang Mills Theory in the later 70's.

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I now also remember with great pleasure your "closing" rituals whenever we would put aside the problem for the time being. Always the optimist, you would invariably single out some ray of hope, however farfetched, in our floundering discussions.

I am writing these remarks in Hong Kong, where I believe I barely missed you on your recent visit here. This, I am sure you will agree, is indeed a good place for all of us over 70. One meets with kindness and courtesy at every step of the way. And I expect it is this fine Chinese tradition of honouring one's elders that inspired Yau to honour you on your 70th birthday also with this coming together, once again, of all four of us old comrades-in-arms. So in conclusion, with my thanks to Yau, let me also thank Fritz and Izz for their contribution to my happy memories. Were I to go into detail on that score also, I am afraid this laudation to you, Michael, would get quite out of hand.

But ultimately our collective thanks are due to Providence for allowing all four of us a glimpse into that realm of which Edna St. Vincent Millay speaks when she said:

> "Euclid alone has looked on beauty bare,
> Let all who prate of beauty hold their peace...."

Happy Birthday Sir Michael! I wish you Godspeed and, foolish old man that I am, yet a few more small joint glimpses into that Euclidean realm.

Raoul

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GEOMETRY IN OXFORD C.1980-85

SIMON DONALDSON†

The first part of this essay comprises some brief reminiscences from my time as a research student of Sir Michael Atiyah: these will be commonplace to my contemporaries, but perhaps younger mathematicians may be less familiar with the research interests of this period. In the second part of the essay I will discuss some current research questions.

The early 1980's was a golden age for geometry in Oxford, or at least it seems so to me and probably to all who were lucky enough to be a part of the group lead by Atiyah at that time. This was a sizeable group—among the Faculty were Graeme Segal, Nigel Hitchin, Brian Steer, Glenys Luke, George Wilson and (somewhat later) Simon Salamon and Dan Quillen. Research students included Frances Kirwan, Michael Murray, Michael Pennington, Jacques Hurtubise, John Roe (all approximate contemporaries of the writer) and, a little later, Yat-Sun Poon, Henrik Pedersen, Peter Kronheimer and Peter Braam—with interweaving research interests. There were also many interactions with the equally large and active group of Mathematical Physicists in Oxford working with Roger Penrose. For us research students the weeks (at least during the short Oxford terms) revolved around Atiyah's "Geometry and Analysis" seminar, which met each Monday at 3pm. This gave a chance to hear many leading mathematicians, no doubt attracted to pass through Oxford by the presence of Atiyah, and the audience was always large. These seminars really shaped our outlook on the mathematical world. We enjoyed lectures from Bott about Witten's renowned "Quantum Mechanics" proof of the Morse inequalities, which was one of the sources for Floer's conception of Floer homology a few years later. Hirzebruch spoke about his application of the Miyaoka-Yau inequality $c_1^2 \leq 3c_2$ to line-arrangements in the plane. Among the other important developments at that time, we had several visits from Connes, who was beginning his theory of noncommutative geometry. After the seminars there would be lively discussions over tea. The most memorable of these seminars were those given by Atiyah himself, which were invariably virtuoso performances—giving, at least to this writer, a standard to aspire to ever since.

The mathematical ambience for our research interests at that time was to a large extent set up by four seminal papers of Atiyah–all related to Yang-Mills theory. In one direction was his solution (with Drinfeld, Hitchin and Manin) of the problem of finding all Yang-Mills instantons on $\mathbf{R}^4$—the ADHM construction [3]. In the early 1980's the analogous theory of "monopoles" on $\mathbf{R}^3$ was a lively topic, with beautiful constructions of Hitchin and Nahm. The spectral curves which featured in these constructions illustrated links with integrable systems, an area in which Segal and Wilson were working at that time. More generally, the whole area of "twistor geometry"; forging a link between 3-dimensional complex geometry and 4-dimensional Riemannian (or Lorentzian) geometry was very much to the fore, and of course a focus of interaction with the Penrose group. In the Riemannian case the fundamental reference is the

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paper of Atiyah, Hitchin and Singer [4] which *inter alia* set up the foundations for the study of the moduli spaces of Yang-Mills instantons. The 4-manifolds which admit twistor spaces are those with "self-dual" structures (the anti-self dual part of the Weyl tensor vanishes), these include the hyperkahler 4-manifolds and in particular the Ricci-flat metrics on K3 surfaces whose existence had been proved at the end of the 1970's by Yau, as a special case of his solution of the Calabi conjecture.

Another major theme stemmed from Atiyah's paper with Bott on the Yang-Mills equations over Riemann surfaces [2]. (This extraordinarily wide-ranging and many-faceted paper could virtually be used as a text book—introducing a student to diverse areas in modern geometry—as also could Atiyah's notes on the ADHM construction [1].) One of the most fruitful themes of this paper was to open up the whole area relating "complex" and "symplectic" quotients, as for example in Frances Kirwan's thesis. More generally, geometrical aspects of group actions in symplectic geometry, for example the Duistermaat-Heckmann theorem, were a great interest of Atiyah's at that time, and the subject of several memorable seminars. (Including one entitled "A generalisation of a theorem of Archimedes"—the prototype case being the action of the circle on the 2-sphere by rotations, which brings Archimedes' formula for the area of a zone in the sphere.) In another direction the Atiyah-Bott stratification of the space of connections was closely related to Segal's stratification of the the loop space of a Lie Group. Lectures by Segal on Loop Groups were another high-point of that period.

A third theme was provided by the paper of Atiyah and Jones "Topological aspects of Yang-Mills theory" [5], discussing the topology of instanton moduli spaces, and in particular intoducing the "Atiyah-Jones conjecture", that the homotopy groups stabilise, as the Pontrayagin class of the bundle increases, to those of the ambient space of all connections modulo gauge equivalence. A parallel case was that of the rational maps from the sphere to itself, which had been studied (for different reasons) at about the same time by Segal. One link between the two discussions was provided by monopoles, since Atiyah conjectured in about 1980 that the moduli spaces of monopoles should be identified with rational functions. There was considerable interest in proving these results using variational methods, and Taubes visited Oxford several times, explaining his work in this direction.

Nearly twenty years have passed since the period I have been recalling, and of course the landscape in this part of mathematics has evolved, although many of the topics mentioned above are still active areas of research and I will not attempt to summarise subsequent developments. (I should also apologise for any omissions from this brief survey.) Looking back, I think that one of the distinctive things that we learnt from Atiyah was his broad view of mathematics. Technical specialisation, as an algebraic geometer, differential geometer, topologist or whatever, was not particularly encouraged; the great thing was to explore the interaction of these different areas. Of course the influence of Mathematical Physics, partly through the Penrose group and partly through the general pre-occupation with Yang-Mills theory, was another distinctive feature—although now it has become much more familiar now, through the immense developments in the years since. Of course other mathematicians, such as Singer, Bott, Taubes—were heavily involved in this initiative, but Atiyah did a huge amount to bring these ideas into the mathematical mainstream.

In the paragraphs above, I have merely tried to give an impression of the way mathematical research appeared to a new research student, beginning in 1980. I have written elsewhere recently about the development of my own research within this ambience [6]. Perhaps it is worth saying here, however, that my own good fortune was to be able to bring some of the tools of nonlinear analysis to these areas; this kind of analysis was something of a gap in the Oxford group before, (and to some extent, this is still an area in which differential geometry in the United Kingdom is comparatively weak). However, I was only able to make much headway in this direction because of the good contacts through Atiyah with Harvard, which meant that Taubes' papers—which I used as a private course on nonlinear analysis—became available at an early stage.

The result of Narasimhan and Seshadri, which lay at the heart of the work of Atiyah and Bott [2], is now very firmly established and many different proofs have been given. Likewise for the generalisations to higher dimensions. Nevertheless there seem to be interesting questions remaining in this area, one of which I will now discuss. These questions are interesting as models for the more difficult problem of constructing constant scalar curvature Kähler metrics, in the direction of work of Lu [6], Luo [7] and Tian – although I will not say more about that problem here. Recall that the Narasimhan-Seshadri theorem says that a holomorphic bundle over a Riemann surface Σ is stable if and only if it admits a compatible connection whose curvature is a constant multiple of the identity. Here we fix an area form μ on the surface to regard the curvature of a connection as an endomorphism of the bundle. Now consider the ADHM construction of instantons in four dimensions. This produces the instanton connections, solutions of a PDE, as pull-backs of the standard connections on the tautological bundles over Grassmannians by a particular class of (real) algebraic maps from the 4-sphere to the Grassmannians. Thus the PDE is reduced to the algebraic problem of studying these special maps. In this vein, we can seek to relate the Narasimhan-Seshadri connection to connections pulled-back under suitable maps to the Grassmannian. The difference is that I shall consider a sequence of maps indexed by a parameter $k >> 0$ and the goal is to obtain the preferred connection in the limit as $k \to \infty$. Consider the Grassmannian $Gr_r(\mathbf{C}^n)$ embedded, as an adjoint orbit, in the Lie algebra $\mathfrak{su}(n)$. If $f : \Sigma \to Gr_r(\mathbf{C}^n)$ is a holomorphic map we can define the centre of mass of f in $\mathfrak{su}(n)$, integrating the push-forward of the measure μ. Let us say that the map is "balanced" if this centre of mass is 0, and that a map f is "b-stable" if its orbit under the natural action of $SL(n, \mathbf{C})$ contains a balanced representative. On the one hand, these definitions are close to familiar ideas in the study of quotient problems in finite-dimensions. We know that if we consider in place of Σ a finite set of points in the Grassmannians, then this set is stable in the algebro-geometric sense if and only if its orbit contains a balanced representative. As one application of the general moment map theory, one can show that the balanced representative in a b-stable orbit is essentially unique. On the other hand, the pull-back of the standard connection on the tautological bundle under the balanced maps form a preferred class of $U(r)$ connections over Σ. Now fix a positive line bundle ξ over Σ, whose curvature form is a multiple of μ, and let E be a rank r holomorphic bundle over Σ. For large k the bundle $E \otimes \xi^k$ is generated by its global sections or in other words its dual is pulled back from the tautological bundle over the Grassmannian. If we start with a bundle

E and fix a basis in $H^0(E \otimes \xi^k)$, we associate to E a map $f_{E,k} : \Sigma \to Gr_r(\mathbf{C}^n)$, for suitable $n = n(k)$. If this map is b-stable, we get a pulled-back connection on $E \otimes \xi^k$, and hence a connection A_k on E. We may then formulate a two-part conjecture:

CONJECTURE.

(1) *The bundle E is stable if and only if the maps $f_{E,k}$ are b-stable for large k.*

(2) *If the maps $f_{E,k}$ are b-stable the connections A_k converge to the Narasimhan-Seshadri connection as $k \to \infty$.*

What I am really thinking of here is that one should try to prove *a priori* that the sequence of connections A_k converges, and thus give yet another approach to proving the Narasimhan-Seshadri theorem. (One can formulate a similar conjecture for bundles over higher-dimensional manifolds. It seems likely that the ADHM construction would be relevant here, in describing sequences of maps associated to "bubbling" phenomena.)

The rationale for this conjecture, beyond its appeal as a natural (and comparatively naive) route between the algebraic and differential geometry, is as follows. Giving a map f to the Grassmannian is the same as giving a bundle V and a basis of sections $s_1, \dots, s_n \in H^0(V)$. Given a fibre metric h on V and this basis of sections, we can define a section $\mathcal{F}_h$ of the endomorphism bundle End V by

$$\mathcal{F}_h = \sum_\alpha s_\alpha^* \otimes s_\alpha,$$

where s_α^* is the section of V^* corresponding to V under h. The fibre-metric on V pulled back from that on the universal bundle is characterised by the fact that $\mathcal{F}_h$ is a constant multiple of the identity, and the map f is balanced if and only if the L^2-inner products $\langle s_\alpha, s_\beta \rangle$ satisfy $\langle s_\alpha, s_\beta \rangle = \lambda \delta_{\alpha\beta}$ for a scalar λ, i.e. if the s_α are *orthonormal* in L^2, up to an overall scalar. Now if we start with a metric h and take any orthonormal basis s_α we get the same bundle endomorphism $\mathcal{F}_h$, that is $\mathcal{F}_h$ is an intrinsic invariant of the Hermitian holomorphic bundle (V, h) (and the fixed measure μ on Σ). In sum, we see that the balanced maps correspond precisely to the fibre metrics h on V which satisfy the equation

$$\mathcal{F}_h = \text{constant}.$$

This should be compared with the Narasimhan-Seshadri equation

$$F_h = \text{constant},$$

where F_h is the curvature of the connection defined by the metric. The motivation for the second part of the conjecture is that in the case when $V = E \otimes \xi^k$ and k is large, one expects the *global* invariant $\mathcal{F}_h$ to be well-approximated by the *local* invariant F_h. For example, if we fix a metric h_0 on E and let $\mathcal{F}_{(k)}$ be the section of End $(E) \equiv$ End $(E \otimes \xi^k)$ obtained from the induced metric on $E \otimes \xi^k$, one can show that

$$\mathcal{F}_{(k)} \sim k + \frac{i}{2\pi} F_{h_0} + C_\Sigma, \tag{1}$$

as $k \to \infty$, where $C_\Sigma = (1 - g(\Sigma))/\mathrm{Area}(\Sigma)$. (In the case when E is a line bundle, or a direct sum of line bundles, this is a corollary of a theorem of Tian [8].)

Finally, it is a great pleasure to have this opportunity to record my thanks to Sir Michael for the immense help he has given me throughout the past 19 years – above all, for bringing into being such a splendid centre for research in Geometry in Oxford – and to send my best wishes for his 70th birthday, and many years to come.

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A HAPPY COLLABORATION

LARS GÅRDING

1. To Michael Atiyah and Raoul Bott. The forward fundamental solution $E(x)$ of a hyperbolic differential operator is a distribution answering to a point impulse at time zero. A lacuna for E is defined as an open set inside the region of propagation in which there is no light or movement. The most famous lacuna of this kind, the inside of the forward light cone of light propagation in an even number of dimensions including time, was very familiar to me since my teacher Marcel Riesz had worked out a new theory of the wave equation (1949) that improves the classical treatise (1932) by Hadamard. Moreover, in a paper of my own (1947) concerned with some very special hyperbolic equations, I had discovered lacunas bounded by manifolds of codimension larger than one. Finally, during the academic year of 1946-47 when I was visiting the mathematics department of Princeton University, my friend Irving Segal once said: "You are interested in lacunas. Well, there are plenty of them in the last issue of the *Matematicheskii Sbornik.*" He had seen Petrovski's paper (1945). I stayed up half the night in the library trying to read it but without understanding anything except for the fact that topology and algebraic geometry were involved.

2. Petrovski's paper. Petrovski considers operators $a(D)$, $D = \partial/i\partial x$ in n variables $x = (x_1, ..., x_n)$ that are homogeneous of degree m and hyperbolic with respect to a time variable $t = x_1$ in the sense that the equation $a(\xi) = 0$ has m real separate zeros in ξ_1 for all real $\xi_2, \ldots, \xi_n$ not all zero. Such an operator has a unique fundamental solution $E(x)$ satisfying $a(D)E(x) = \delta(x)$ and vanishing for $t < 0$. The support of $E(x)$ spans a convex proper conoid C in x-space. The wave front surface W is defined as the intersection of C with the surface generated by $\operatorname{grad} a(\xi)$ when $a(\xi) = 0$. The wave front surface bounds C and splits it into open, connected and conical parts.

Improving on earlier work by Herglotz (1926,1928), Petrovski proves that $E(x)$ is real analytic when $x \in K - W$ where its derivatives of order $> m - n$ can be expressed as rational integrals over a certain cycle $C_{n-3}(x)$ of dimension $n - 3$ in the intersection of the complex hyperplane $X^* : (x.\zeta) = 0$ and the complex hypersurface $A^* : a(\zeta) = 0$. Its homology class is independent of x as long as x stays outside of the wave front surface. When n is even C_{n-3} is is simply the real part of $X^* \cap A^*$, a cycle that Petrovski denoted by $C_{\mathrm{real}}(x)$. When n is odd, C_{n-3} is a differently defined cycle called $C_{\mathrm{imag}}(x)$.

It follows from the above that if the cycle C_{n-3} is homologous to zero in the complex intersection $X^* \cap A^*$ then the fundamental solution $E(x)$ is a polynomial of degree $m - n$ in the corresponding part $\Omega \in K - W$. In particular, if $m < n$, Ω is a lacuna for $a(D)$ in the sense that the fundamental solution vanishes there. That C_{n-3} vanishes will be called the Petrovski condition in the sequel.

In case of the wave equation with $m = 2$ and $n > 2$, the cycle C_{real} is empty in conformity with the wave equation lacunas for even n. In the case $m \geq n$, the simplest example being $m = n = 2$ and a constant $E(x)$, Petrovski found no lacunas inside the propagation cone.

The main point of Petrovski's paper is that the Petrovski condition is necessary for lacunas which are stable under small variations of the polynomial $a(\zeta)$. His proof uses algebraic geometry available at the time, in particular Lefschetz's classical book (1924) on the homology of algebraic surfaces. There it is shown that the homology in middle dimension is spanned by vanishing cycles and, if the dimension is even, an algebraic cycle. The simplest example is the case of hypersurfaces of dimension zero, i.e. a set Z of m points $z_1, \dots z_m$ in the complex plane. Here we may consider cycles $\alpha = \sum \varepsilon_j z_j$ which give a sign ε_j to the point z_j. The cycles of the form $\varepsilon_j z_j - \varepsilon_k z_k$ for which $\varepsilon_j - \varepsilon_k = 0$ are said to be vanishing since they vanish when the two points come together. If $f(z)$ is the polynomial $\prod(z - z_j)$ we may think of an abelian integral over the cycle α as the sum

$$\sum \frac{\varepsilon_j z_j}{f'(z_j)}.$$

If this sum vanishes when any two points z_j come together, α can contain no vanishing cycle and must be of the form $\pm \sum z_k$ which means that it is algebraic.

In his proof that the Petrovski condition is necessary, Petrovski first extended Lefschetz's work to hypersurfaces through a laborious by hand construction of the homology in middle dimension of non-singular hypersurfaces. In a second step it is proved in a way illustrated above that to any non-vanishing such cycle there is rational integral on which it does not vanish.

Petrovski wrote his paper during the war in the early forties when the Soviet government and Moscow University were moved from Moscow to Kuybyshev. The working conditions may be described in Petrovski's own words: only formally a university.

Most of my insight into Petrovski's paper I got from Jean Leray in the early sixties when we tried among other things to understand Petrovski's paper. Leray (1962) could later use Petrovski's cycles for an extension of the transform of Laplace to several variables.

Petrovski used only hyperbolic operators whose characteristic polynomials define non-singular varieties. In the late sixties, I returned to hyperbolic operators with singularities and discovered the usefulness of the local hyperbolicity cones defined below and could start working on a paper (1972) on local hyperbolicity.

3. The collaboration. In the spring of 1966 Michael Atiyah invited me to Oxford to give some lectures on hyperbolic equations. Another guest on that occasion was Raoul Bott. In this very inspiring company it occured to me that I could expect some help with the problem of lacunas which had rested with me since 1947. In an initial step I persuaded Michael to provide me, Raoul, and himself with photocopies of Petrovski's paper from the Bodleian Library. With this step a happy collaboration was initiated.

Already from the beginning it was clear that Petrovski's laborious homology constructions should be replaced by the new and powerful theory of sheaf cohomology. Both my companions were well versed in this field and, in addition, Atiyah was a leading specialist in algebraic geometry. Another basic result had also become available, Hironaka's resolution of singularities (1964). As it turned out, the result that we required was essentially contained in a paper (1966) by Grothendieck written in the form of a letter to Atiyah and extending Atiyah-Hodge (1955). It states that the cohomology of the complement of an affine hypersurface can be realized by rational differential forms with high enough poles on the hypersurface. Returning to the points $Z = (z_1 \dots, z_m)$ and polynomial $f(z) = \prod(z - z_k)$ above, the simplest example is the space of rational differentials $g(z)dz/f(z)$ which vanish at infinity. Any cycle in the complement of Z which is orthogonal to these forms is homologous to zero. The homology of Z is now given by the residues of these differentials. Compared to Lefschetz's analysis of the homology of a hypersurface Z by vanishing cycles we have now moved to the homology and cohomology of its complement.

The result of our collaboration is a two part paper in Acta Mathematica, (1970, 1973) by the three of us. It carries a dedication in Russian: To Ivan Georgievich Petrovski with respect and admiration. I wrote the hyperbolic part and Atiyah the topological part that details and makes precise Grothendieck's paper by specifying lower bounds on the order of the poles. Raoul Bott contributed a vanishing theorem and played the important part of the genial companion and therefore it was decided against his vivid protests that Bott should be the third author. I have dedicated this paper to my two collaborators it is in memory of the good time that they gave me and our happy collaboration.

In what follows I will sketch the main topological results and then the basic applications to the lacuna problem that we reformulated as a condition for sharp wave fronts.

4. The topology. The topological part of the paper proves an algebraic counterpart of de Rham cohomology. One of the first basic results says that if Y is a subvariety of codimension one with normal crossings of a non-singular algebraic variety X, then the cohomology groups of (X, Y) are isomorphic to the de Rham group of of rational differentials γ and $d\gamma$ with only simple poles on the components of Y.

The main result applicable to the lacuna problem concerns the complex cohomology of the complement of a hypersurface $A : a(\xi) = 0$ in projective space P_{n-1}. Let $\omega(\xi) = \xi_1 d\xi_2 \dots d\xi_n + \dots$ be the standard $n-1$-form on $n-1$-spheres. If $a(\xi)$ has degree m, then every cohomology class in $H^{n-1}(P_{n-1} - A)$ is represented by a differential form

$$g(\xi)a(\xi)^{-q}\omega(\xi)$$

where $g(\xi)$ is a homogeneous polynomial whose degree $k = mq - n \geq 0$ makes the differential homogeneous. In addition, q has to be sufficiently large, depending only on m and n. When A is non-singular, it suffices that $q \geq n - 1$.

More generally, if $B : b(\xi) = 0$ is a hypersurface with only normal crossings, then every cohomology class in $H^{n-1}(P_{n-1} - A \cup B)$ can be represented by a differential form

$$\frac{g(\xi)\omega(\xi)}{b(\xi)a(\xi)^q}$$

where g is a homogeneous polynomial of a degree that makes the form homogeneous and, in addition, q is sufficiently large .

5. Hyperbolicity and fundamental solutions. Let $P(D)$ be a polynomial in the derivatives $D_k = \partial/\partial x_k$ in the coordinates $x_1, \dots, x_n$ of real n-dimensional space. A fundamental solution of E is a distribution $E(x)$ such that $P(D)E(x) = \delta(x)$. The operator $P(D)$ and the corresponding characteristic polynomial $P(\xi)$ are said to be be hyperbolic with respect to a real direction $N \neq 0$ or to be in hyp(N) if it has a fundamental solution with support in a closed cone which, apart from the origin, is contained in the half-space $(N, x) > 0$. An equivalent algebraic condition is that $P(\xi - itN) \neq 0$ for all real ξ and for t greater than some real number c. It can be shown that $\mathrm{hyp}(N) = \mathrm{hyp}(-N)$. If a is the principal part of P then $a \in \mathrm{hyp}(N)$ and $a(\xi + iN) \neq 0$ for all real $t \neq 0$. Here a has to be essentially real and we may assume that $a(N) > 0$. In the sequel we limit ourselves to complete polynomials depending on all variables, a property shared by a hyperbolic polynomial and its principal part.

The component of the complement of the real hypersurfacee $A : a(\xi) = 0$ that contains N is an open convex cone $\Gamma(a, N) = \Gamma(P, N)$ called the hyperbolicity cone of P and a. It has the property that $P \in \mathrm{hyp}(M)$ when $M \in \Gamma(P, N)$. For any real $\eta \in R^n$, $a(\xi)$ has a localization $a_\xi(\eta)$ defined as the first non-vanishing term in the Taylor expansion $a(\xi + \eta)$ for small η. These polynomials are in $\mathrm{hyp}(N)$ and have hyperbolicity cones $\Gamma_\xi(P, N) = \Gamma_\xi(a, N)$. When $a(\xi) \neq 0$ such a cone equals all of R^n, if $a(\xi) = 0$ but $\mathrm{grad}\, a(\xi) \neq 0$ it is a half-space. In any case it contains the positive multiples of ξ.

The dual $C(P, N)$ of $\Gamma(P, N)$, defined as all x for which $x.\Gamma(P, N) \geq 0$, is called the propagation cone of P and similarly for the local propagation cones $C_\xi(P, N)$. The reason for these names is that $P(D)$ has a unique fundamental solution $E(P, N, x)$ with support in $C(P, N)$. Moreover, this fundamental solution is real analytic in the interior of $C(P, N)$ outside a certain wave front surface $W(P, N)$ defined as the union of all local propagation cones $C_\xi(P, N)$ for $\xi \neq 0$. The fundamental solution which vanishes outside $C(P, N)$ is the distribution

$$E(P, N, x) = (2\pi)^{-n} \int e^{i(x, \xi - itN)} d\xi / P(\xi - itN) \tag{1}$$

where $t < -c$. When P reduces to its principal part a we get a fundamental solution $E(a, N, x)$ of $a(D)$ which is homogeneous of degree $m - n$. That both are fundamental solutions may be verified by integration with a test function. Letting t tend to infinity, the formula has the immediate consequence that $E(P, N, x)$ vanishes when $(x, N) < 0$. That $E(P, N, x$ vanishes outside the propagation cone follows from the fact that we may use Cauchy's theorem to replace N by any element of $\Gamma(P, N)$.

When P reduces to its principal part a, this process can be made more precise by replacing the vector $N \in \Gamma(a, N$ by a continuous vector field $v(\xi)$ such that, for every ξ, $v(\xi)$ belongs to the local propagation cone $\Gamma_\xi(a, N)$. It turns out that this construction is possible when the real plane $X = \eta : (x.\eta) = 0$ does not meet any local propagation cone, i.e. when $x \in C(a, N)$ is outside the wave front surface $W(a, N)$. Under these circumstances we may also choose $(x.v(\xi)) > 0$ for all $x \neq 0$ and choose $v(\xi)$ so small that $a(\xi + iv(\xi)) \neq 0$. By Cauchy's theorem we may then replace $\xi - itN)$ in (1) by $w(\xi) = \xi + iv(\xi)$. Finally we may also make $v(\xi)$ absolutely homogeneous of degree 1 for large ξ. In this way we have transformed (2) into an absolutely convergent integral which is an infinitely differentiable function of x outside the wave front surface.

In order to express the fundamental solution as a rational integral we have to perform a radial integration amd subtract the opposite fundamental solution $E(a, -N, x)$ which vanishes when x is in the forward propagation cone. The details of this cannot be given here, we can only describe the final result.

Let us first describe a basic chain $U(a, N, X)$ that consists of points $\xi - iv(\xi)$ where $v(\xi) \in \Gamma_\xi(a, N)$ is absolutely homogeneous of degree 1, $a(\xi - itv(\xi)) \neq 0$ when $0 < t \leq 1$, and $(x.v(\xi)) > 0$ for all $\xi \neq 0$. The chain $U(a, N, X)$ with the orientation $(x, \xi)\omega(\xi) > 0$ then determines a basic cycle $\alpha^* = \alpha^*(a, N, x)$ in projective $(n-1)$ space $Z^* = R^{n*}$ relative to the plane X^*. Here the star denotes image in projective space. The basic cycle belongs to the homology group $H_{n-1}(Z^* - A^*, X^*)$ and its boundary $\partial\alpha^*$ to $H_{n-2}(X^* - A^* \cap X^*)$. In the case of strong hyperbolicity $\partial\alpha^*$ is actually a tube around the Petrovski cycle, C_{real} when n is even and C_{imag} when n is odd.

The formulas for the derivatives $D^\nu E(a,x)$ of the fundamental solution $E(a,x) = E(a,N,x)$ when $x \in C(a,x) - W(a,x)$[1] are as follows

$$D^\nu E(a,x) = i(2\pi)^{1-n} \int_{\alpha^*} \chi(i(x.\xi))\xi^\nu a(\xi)^{-1}\omega(\xi) \tag{2}$$

for non-negative homogeneity, $|\nu| \leq m - n$. and

$$D^\nu E(a,x) = (2\pi)^{-n} \int_{t_x \partial\alpha^*} \chi(i(x.\xi))\xi^\nu a(\xi)^{-1}\omega(\xi) \tag{3}$$

for negative homogeneity, $|\nu| > m - n$. Here $\chi_q(t) = t^q/q!$ when $q \geq 0$ and $\chi_q(t) = (d/dt)^{-q} \log t$ when $q < 0$ The operator t_x is the tube operation from X^* to $A^* - X^*$.

6. The Petrovski condition and sharp wave fronts. It is now obvious from the formula (3) that if $\partial\alpha^*$ is homologous to zero in $H_{n-2}(X^* - A^* \cap X^*)$ then all sufficiently high derivatives of $E(a,x)$ vanish in Ω, i.e. $E(a,x)$ is a polynomial in Ω which vanishes if $m < n$ since $E(a,N,x)$ is homogeneous of degree $m - n$. Moreover, since (3) applies to all powers of $a(D)$, our topological result shows that the vanishing of $\partial\alpha^*$ is necessary for this result to apply to the derivatives of sufficiently many fundamental solutions $E(a^j,x)$ of powers of $a(D)$.[2] The formula (2), which applies in particular to $E(a,x)$ itself when $m \geq n$, does not give a lacuna. In fact our paper proves that the cycle α^* is not homologous to zero. This answers a question by Petrovski.

In view of the results above, it is now time to replace the notion of a lacuna by those sharp and diffuse wave fronts or simply fronts. A smooth function $u(x)$ defined in some open set Ω is said to have a sharp front at a point $x \in \partial\Omega$ if, close to x, it is smooth (infinitely differentiable or real analytic) up to the boundary. A diffuse front is just the opposite. In the hyperbolic case, if Ω is a connected part of $C(a,N) - W(a,N)$, the homogeneity of the fundamental solution $E(a,N,x)$ shows that $\partial\alpha^*(x)$, $x \in \Omega$ vanishes if and only if the fundamental solution has a sharp front everywhere at the boundary of Ω. Because of the homogeneity, it suffices that the front is sharp at the origin. This fact extends to from homogeneous to inhomogeneous hyperbolic operators $P(D) = a(D) - b(D)$ with complete principal part $a(D)$. In fact,

$$E(P,N,x) = \sum_0^\infty b(D)^k E(a^{k+1},N,x).$$

The notion of a sharp front and the Petrovski condition has an an obvious extension from the origin to any other point of the wave front surface and then the Petrovski condition assumes a local form. This is the theme of the last part of our paper.

In a footnote we surmised that the wave front surface may also be the singular support of the fundamental solution. But then we did not think of my paper (1947) which deals with the operator $P(D) = \det(\partial/\partial x_{jk})$ where the variable $x = (x_{jk})$ is an $r \times r$ hermitian matrix. This operator is hyperbolic with respect to any positive

[1] We now drop the N from the notations

[2] It is not known if the necessity prevails without recourse to these powers.

definite hermitian matrix and the corresponding propagation cone consists of non-negative matrices and the wave front surface of all hermitian matrices ≥ 0 of rank less than r. But the fundamental solution $E(P, x)$ is supported only on the matrices of rank one and hence only on a small part of the wave front surface. Simpler counterexamples appeared very soon, for instance Andersson (1970). According to Hörmander (1992) our conjecture is true when the wave front surface has at most quadratic singularities.

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RECOLLECTIONS ABOUT MY TEACHER, MICHAEL ATIYAH

G. LUSZTIG*

I am very fortunate to have been able to study with Michael Atiyah for a few months in 1968 and then for two more years (1969-71). His influence on me was especially strong at the beginning of my mathematical career, but his teaching has provided me with tools that I have constantly used throughout the years, even to this day. I am very grateful to him for this. For me he is not only a great mathematician and a great teacher, but also a human being of extraordinary generosity.

As an undergraduate at the University of Bucharest, I was very interested in topology and analysis on manifolds, so naturally, I came in contact with the work of Michael Atiyah. In fact, in 1965 we had a one semester course given by C. Teleman on the recently proved Atiyah-Singer index theorem, and I remember studying Henri Cartan's Paris seminar on the index theorem. I first saw Michael at the ICM in Moscow in 1966. He was sitting between two ladies (one was his wife, Lilly, the other was, he later told me, his mother) while Henri Cartan was talking about Michael's work for which he was just being awarded the Fields Medal. I also heard Michael's lecture at the ICM, which for me, was the high point of the Congress. But I first met Michael only two years later.

In the summer of 1968, I was at a summer school on pseudodifferential operators in Stresa, where Singer was giving one of the courses. There I talked with Singer (I think that he talked in English and I in French, since I didn't know any English) and told him that I was planning to go from Stresa to Warwick to a symposium on dynamical systems, although what I was really interested in was index theory; he then told me that he was in fact going to Oxford to work with Atiyah and why don't I come there too? So after a few weeks at Warwick, I went to Oxford. I remember very well my first meeting with Michael. He was in his office, at 25-29 St. Giles, with Singer. He asked me what problems I was interested in, and a few minutes later he explained to me what he and Singer were discussing: the problem of comparing the semicharacteristics of a $(4k+1)$-manifold with real or modulo 2 coefficients. He and Singer could prove that, if the real semicharacteristic was 0, then one can find two independent vector fields on the manifold, while E. Thomas could prove that if the manifold was spin and the mod 2 semicharacteristic was 0, then one can again find two independent vector fields. They naturally wanted to show that their result was stronger than that of E. Thomas, so they conjectured that the two semicharacteristics coincide with spin manifolds. During the following two months I stayed in Oxford and learned a lot of mathematics from Michael. I also found an answer to the question he asked about the semicharacteristic.

Before the two months were over, I received a letter from Deane Montgomery saying that, at the suggestion of Michael (who was about to move to Princeton), I was invited to spend a year at the IAS. In fact, I stayed at IAS for two years (1969-71). These two years were for me the equivalent of graduate school, with Michael as my teacher. But Michael was not only the most wonderful teacher one could have; he and Lilly were really like family to me during these years. I remember fondly the many times when I had meals in their home; on one occasion, when I developed a

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high fever, Michael drove me to Princeton Hospital so that a doctor can see me.

During my stay at IAS, Quillen gave some lectures on his solution of the Adams conjecture in which a "Brauer lifting" of the standard modular representation of $GL_n(F_q)$ played a key role. After the lecture, I asked Michael, whether this Brauer lifting was explicitly known as a complex (virtual) representation of $GL_n(F_q)$. He told me that it was not known, except at the level of characters, by the work of J. A. Green. Somewhat later he asked me to read and explain to him a paper by S. Gelfand on discrete series representations of $GL_n(F_q)$. These were the seeds for my work (after moving to Warwick) on the Brauer lifting at the representation level, which led to my conversion to representation theory.

Sometime during my first year at IAS, I had the idea of twisting the signature operator on a compact manifold with a local system coming from a variable representation of the fundamental group into U(1). I felt that the resulting family of elliptic operators indexed by a torus must contain some interesting new information about the manifold. When I told Michael about this construction, he immediately said that this should have something to do with Novikov's higher signature. Eventually I proved that Michael's prediction was indeed correct.

Many years later, in may 1990, I met Michael (by that time he was Sir Michael) at a conference in Kyoto. After my second lecture (about canonical bases for quantized enveloping algebras of type A, D, E), Michael told me that quivers (which were used in my work) have also appeared in the work of Kronheimer, with two orientations considered simultaneously. The idea to use two orientations simultaneously turned out to be very useful in my subsequent work on the canonical basis.

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MEMORIES OF SIR MICHAEL ATIYAH

LOUIS NIRENBERG*

I've known Sir Michael Atiyah since about the time he and Iz Singer proved their famous index theorem — a result which has played a fundamental role in geometry and in partial differential equations. I was enormously impressed by the scope and generality of the result.

It's always a real pleasure to meet with Sir Michael — unfortunately we don't do so very often. He has a wonderful knack of making people feel at ease. We talk about everything under the sun: family, politics, history, books, etc.

Over the years he has been a guiding force and an inspiration in the development of mathematics. However, my admiration has grown not only for his mathematical work; but also for his warmth and generosity of spirit.

And what a lecturer he is! For many years he has been my favorite speaker.

Happy birthday, Sir Michael, and many more.

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BEING A GRADUATE STUDENT OF MICHAEL ATIYAH

G. B. SEGAL*

I became Michael Atiyah's graduate student in 1963. My life was immediately transformed, and in retrospect I can hardly believe my good fortune, for at that time I had heard little more about my new supervisor than that he was the only person around likely to take on a floundering late applicant. Michael was then 34, which again is hard for me to imagine, for although he was very benevolent and approachable, and in no way intimidating, he had nevertheless a colossal presence and air of authority, and all the department was in awe of him - it was many, many years before I ventured to call him 'Michael'.

I seem to remember that there were at least six Atiyah students at that time, some official and some de facto. He would direct his abounding energy at each of us in turn. I remember how inspired I felt after each meeting, but on the whole we students used to hide from him, for if he ran into us in the corridor and found that we hadn't made much progress with yesterday's suggestions he would pour forth a torrent of new lines for us to try. At the same time he always left us feeling there was something worthwhile we could do; however wrong were the ideas we came up with, he never crushed us, but made our muddle seem like steps in the right direction. I have often thought about this wonderful ability to be encouraging, and how inimitable it is, when seeing myself having just the opposite effect on my own students. (Another thing I often wondered about was when the Atiyah papers were written: for he seemed to be talking to people all day long.)

The mathematical orientation I learnt from Michael as a graduate student has stayed with me ever since. Its main principle was the primacy of geometry. After that, one was interested in understanding why things were true, was not very interested in details, and was not interested at all in taxonomy. One cannot imagine an Atiyah theorem with complicated hypotheses or conclusions, or one which involves elaborate classification. That was especially true at the time when I was a student, for then Atiyah and Singer were engaged in the search for a truly natural treatment of the index theorem. This pursuit of naturality accorded with the spirit of the new age inaugurated by Grothendieck; but Atiyah differed sharply from Grothendieck in eschewing elaborate abstract machinery.

I have learnt a vast amount of mathematics from Michael, first when I was his student, and then later when we were colleagues at St. Catherine's College (together, successively, with Elmer Rees and Nigel Hitchin). But probably the most fundamental thing I learnt was an attitude: for Michael, no part of mathematics worth knowing was so technical or remote that one could not be put completely in the picture by the right twenty-minute account. He was wonderful at keeping to the high ground and avoiding the mire: talking to him, one always felt a failure if one needed to use a blackboard to explain something. Among the less standard pieces of advice he gave his students in my day was "Never read things. It will only make you depressed. If you need to know something, just ask me."

My whole mathematical life has been under Michael's tutelage, and I owe him more than I can express for his encouragement, his great generosity with his ideas, and many other kindnesses over the years.

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MICHAEL ATIYAH AND THE PHYSICS/GEOMETRY INTERFACE

EDWARD WITTEN*

The first time that I met Michael Atiyah was in the spring of 1977, when he was visiting Roman Jackiw at Harvard. I was a postdoctoral fellow at Harvard, having received my Ph.D. the year before.

It is a bit hard now to recapture the spirit of the time. Theoretical physics had made major advances in the previous decade, with the nonabelian gauge theory revolution, and in a sense had caught up with experiment, though (largely because some of the weak interaction experiments were in an inconclusive state) this was not yet clear. Theoretical physicists had certainly not yet realized that the gauge theory revolution had created a situation in which it would be necessary and worthwhile to develop a greater mathematical sophistication than we were accustomed to. It took a long time to realize this. Michael Atiyah and other mathematicians who became interested in what physicists were doing in quantum gauge theory played an important role in the process.

The first major turning point, out of many, had come in 1976. The so-called $U(1)$ problem, which had been identified by Murray Gell-Mann and Steve Weinberg, among others, as the main remaining flaw in the theory of the strong interactions, was suddenly solved (in work with various contributions by Gerard 't Hooft; Claudio Rebbi and Roman Jackiw; and Roger Dashen, Curt Callan, and David Gross), using instantons. Soon afterwards, Albert Schwarz showed that some of the ingredients in the solution were best understood in terms of the Atiyah-Singer index theorem. Few of us knew what to make of this, as in the theoretical physics environment of those days, the index theorem was way beyond the prevailing level of mathematical sophistication. In fact, it seemed incredibly esoteric and obscure. But things were soon to get much more esoteric.

Much of Atiyah's visit to MIT was devoted to explaining his work with Ward applying the Penrose twistor transform to solve the instanton equations on $\mathbf{R}^4$. Solving those equations was something that many of us had been extremely interested in for the preceding year, largely because of A. M. Polyakov's speculations about the dynamics of gauge theories. The twistor approach, on the other hand, involved things that I and most of my physics colleagues had never heard of – complex manifolds, sheaf cohomology, and fiber bundles.

Atiyah invited me to visit Oxford for a few weeks – perhaps I seemed like a promising student, though I certainly had a lot of catching up to do, as I have just indicated. By the time I arrived (which was in January, 1978), the twistor transform of the instantons had been further elaborated to give the much more precise ADHM construction of instantons. Atiyah lectured on it at the Maths Institute during my visit. I remember him beginning the first lecture explaining that the trouble with working on problems posed by physicists is that once the problem is solved, one might be told that the problem wasn't quite the right one. This must have been at least partly a response to my impatience, at the time, with anything that didn't shed light on *quantum* behavior of gauge theories.

In hindsight, my focus in that period seems shortsightedly narrow to me. I also

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had an impatience for quick results that must have seemed jarring to mathematicians. (I recall one of the Oxford mathematicians commenting on it in January, 1978.) At any rate, I've had no choice over the years but to learn to be a bit more patient. Learning about the ADHM construction has served me well repeatedly – especially in 1995 when it helped in understanding the problem of small instantons in the heterotic string, and the behavior of Type I fivebranes.

Towards the end of this visit, Atiyah showed me a paper by David Olive and Claus Montonen on duality in four-dimensional gauge theories. The paper was new to me, and my initial reaction was skeptical. Their conjecture was very wild, the evidence they offerred was striking but limited, and it was easy to state technical objections to the conjecture in the form in which they originally stated it. I don't know whether he was motivated in part by exhaustion from all our discussions, but at any rate Atiyah urged me to travel down to London to discuss the question with Olive. It turned out to be a very fruitful trip. By the end of the day, Olive and I had understood that the Montonen-Olive conjecture really made most sense in the supersymmetric case, and had formulated a few of the ideas that eventually (fifteen years later) were useful in understanding it better.

My recollections of discussions with Atiyah in the next few years are varied, and I will here mention only a couple of highlights. There was a conference in New Hampshire, just after Simon Donaldson's first breakthrough in four-manifold topology, where I was educated about Donaldson theory for the first time; and a conference in Texas at which Atiyah and Is Singer began to educate us about the topological meaning of perturbative gauge anomalies. That was where we physicists began to learn for the first time that we should think of the determinant of the chiral Dirac operator as a section of a complex line bundle. After 1984, string theory as well as gauge theory was prominent in all the math/physics discussions, and the two subjects have influenced each other very much; but I won't try to describe that side of things here.

In the spring of 1987, Atiyah visited the Institute for Advanced Study and was more excited than I could remember. What he was so excited about was Floer theory, which he felt should be interpreted as the Hamiltonian formulation of a quantum field theory. Atiyah hoped that a quantum field theory with Donaldson polynomials as the correlation functions and Floer groups as the Hilbert spaces could somehow be constructed by physics methods. The idea was clearly tantalizing, but I had a variety of technical objections. For example, the fermionic symmetries in Floer theory were of spin zero, as opposed to the half-integral spin of spacetime supersymmetries as studied by physicists. Even if one were willing to abandon the spin-statistics theorem, the fermions required by Floer theory, if one were to treat it as a Lagrangian field theory, did not seem to form representations of the Lorentz group. Because of these and a few other difficulties, I was skeptical, and though the idea was intriguing, I did not pursue it until I was reminded of the question during another visit by Atiyah to the Institute at the end of 1987. This time I dropped some of my prejudices and had the good luck to notice that a simple twisting of $N = 2$ supersymmetric Yang-Mills theory would give a theory with the properties that Atiyah had wanted.

The other problem that Atiyah recommended for physicists in the years 1987-8 was to understand the Jones knot polynomial via quantum field theory. It was from him that I first heard of the Jones polynomial. There followed other clues in 1987-8 about the Jones polynomial and physics. For example, A. Tsuchiya and Y. Kanie had connected some braid representations that arise in conformal field theory with the ones studied by Vaughn Jones. I didn't understand too much of this paper, which I perhaps had been shown by Dan Friedan and Steve Shenker, but I tried to pay attention to it because of Atiyah's suggestion. The nature and relation to physics of the braid representations was greatly clarified in conformal field theory work in 1987-8 by Erik Verlinde and then Greg Moore and Nathan Seiberg. I was lucky that much of this work was done at the Institute (by Moore and Seiberg) which made it much easier to follow what was going on.

In the summer of 1988, the International Congress of Mathematical Physicists was scheduled in Swansea. I knew that Atiyah, Graeme Segal, and other mathematicians interested in the Jones polynomial would be there, and I knew in particular that Atiyah considered it a major piece of unfinished business to understand the Jones polynomial in terms of quantum field theory. So by way of preparation I sat down in the week before departure with a whole pile of papers on the Jones polynomial and its generalizations that various mathematicians had sent me. It was discouraging, since the papers seemed very deep, and it looked like it would take a lifetime to understand all that. Another paper I saw in the week before the meeting – without connecting it at the time with the papers on the Jones polynomial – was one by Polyakov attempting to use abelian Chern-Simons theory in three dimensions to understand high temperature superconductors.

At any rate, the meeting at Swansea turned out well for me. Atiyah and Segal reminded me of the right clues (in particular Segal reminded me of some points that I think he'd actually explained the year before), and my mind wandered back to Chern-Simons theory during the lecture that Albert Schwarz was supposed to give. (With the Soviet Union nearing collapse, he was the one speaker not permitted to attend the meeting; his lecture was read by Igor Krichever.) Some important points fell in place during a memorable dinner at Annie's restaurant with Atiyah and Segal.

In many ways, this work relating the Jones polynomial to Chern-Simons theory was a turning point in my career. For one thing, I learned that while it might indeed take a lifetime to master all the learnedness in that pile of papers that I had been looking at, the piece of the story that I was suited for personally did not require all that. It required focussing on the right questions and, at times, listening to the right advice.

Going back to Donaldson theory, the quantum field theory formulation of this subject did not lead to any immediate progress. I felt in the years 1988-90 that the Lagrangian representation of the theory would make it possible to perform computations by purely formal, short distance or weak coupling methods. It took me several years to become convinced that this would not work. In fact, Atiyah and other mathematicians helped me on several occasions in understanding that the sort of results I could get that way were more or less along the lines of what mathematicians were anyway doing by more standard (and of course rigorous) mathematical methods. Thus to get somewhere it would be necessary to supply some more physical ingredient.

Though I was extremely reluctant to accept this, it eventually became obvious that the missing ingredient would have to be a knowledge of what physicists call the dynamics of the $N = 2$ quantum field theory. I think that, although he might not have expressed the point in exactly those words, this is essentially what Atiyah was hoping for during those years. Anyway, Seiberg and I had the great good fortune of understanding the $N = 2$ dynamics in the spring of 1994. The most interesting aspects of the dynamics were described in terms of an effective $U(1)$ gauge theory with "monopoles." The monopoles in question are the same ones that star in the book *The Geometry and Dynamics Of Magnetic Monopoles,* by Atiyah and Nigel Hitchin, except that they and other mathematicians (and physicists) have studied monopoles as classical solutions of a nonlinear PDE, while to understand the $N = 2$ dynamics, it is necessary to understand how the monopoles behave in a region of parameters where the quantum effects are big and the classical PDE is not a good approximation. Nevertheless, many features of monopoles described in the Atiyah-Hitchin book are relevant to $N = 2$ dynamics and were later used in checking features of the quantum behavior.

It was fairly evident that the work on $N = 2$ dynamics with Seiberg should lead to a new description of Donaldson theory. To actually elaborate the new description still took some time. Yet another visit to the Institute by Atiyah – in the late spring of 1994 – helped sharpen my ideas about this.

I have tried to recount a few of the highlights of my scientific interactions with Michael Atiyah, and to convey a little of the role he played in encouraging us to study quantum field theory from new points of view. We had to learn a lot of lessons before taking these new perspectives seriously. Atiyah, along with colleagues such as Raoul Bott and and Is Singer, played an important role in teaching some of these lessons to the physics world. Atiyah has always believed intuitively that the study of quantum field theory as a tool in geometry had to be integrated with the study of more "physical" aspects of quantum field theory. This was one of the hardest lessons for me personally to learn.