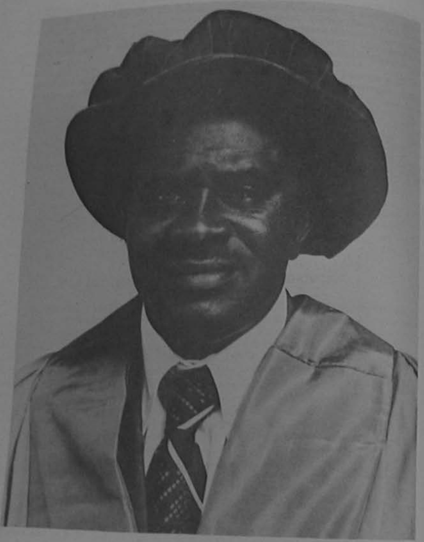

Professeur Victor NGU

THE SCIENTIFIC METHOD
LA METHODE SCIENTIFIQUE

The Vice-chancellor takes a Stand
Prise de position du Vice Chancelier

"SCIENCE AND SCIENTIFIC METHOD"

AN ADDRESS BY PROF. V. ANOMAH NGU, VICE CHANCELLOR ON THE OCCASION OF THE FORMAL OPENING OF THE ACADEMIC YEAR 27TH OCTOBER 1976.

It is my privilege once again to welcome you all to the university. To our distinguished guests and friends, I would like to express our deep gratitude for the honour which you have done us by your presence here today. It has given solemnity and dignity to this formal opening of our academic year. To our first year students and to the new members of our staff, I extend a particularly warm welcome and sincerely hope that your new career in the university will be both interesting and fruitful.

As on previous occasions, I should like to address a few words to the students and the staff and the subject I have chosen as my topic is «Science and the Scientific Method». I hasten to add that this brief address concerns not only those in the purely physical sciences but those in the classical, literary and the social sciences. If it seems like a lecture. I hope our guest will not be too bored by it.

The word science is of latin origin and means quite simply knowledge. Hence all forms of knowledge constitute science in the true meaning of the word. Since the university is the place, par excellence, where knowledge is the 'stock in trade' it is good and proper, especially on an occasion like this when all members of the university are assembled together, to remind ourselves of our principlal functions : that of acquiring and transmitting knowledge, in short, to remind ourselves about Science and the Scientific Method.

Before embarking on this topic, I should like to call your special attention to an excellent treatise or the subject written and published by Dr. Bernard Fonlon. It is entiled *To Every African Freshman or the Nature, End and Purpose of University Studies*. I have been assured that a French edition is soon to be published for the benefit of our francophone compatriots. Don't let the title of the booklet fool you! Dr. Fonlon usually address important and weighty matters to the general public by pretending to address them to a particular class of the public. So although the book bears on its title *Freshman*, its contents will instruct, enthral and edify all educated people students and professors alike. It is a real masterpiece and I very strongly recommend it to anyone who wants to understand more clearly and more deeply what science and the scientific method are about. For my part, I must state that although I have borrowed freely from Dr. Fonlon, any errors which may be evident in my presentation today are entirely my own.

As knowledge or science is our principal function in the university, it is appropriate that we deal on this occasion with science and the scientific method. The second reason for treating this subject at this time is that as you know, the Minister of Edu-

cation has signed an Arrêté which legalizes the start of the Doctorate Programme in the university this October. At last, our university in its turn, is about to embark *officially* on a programme that will contribute to that stock of knowledge destined to become the universal heritage of mankind. It is thus appropriate that on the eve of such an historic event, we should spend a few minutes to ponder on the nature of science and the scientific method.

Science, as I stated earlier, means knowledge. Common usage however has tended to reserve the term to the physical sciences such as physics, chemistry, biology, botany etc. which deal with physical matter. The physicists, the chemists, the physiologists, the botanists are all interested in *iron*, for example the physicist is interested in the properties of iron such as its behavour under various temperatures, its electromagnetic properties etc. The chemist is interested in chemicals derived from iron while the physiologist in the oxygen binding properties of haemoglobin, a complex protein which contains iron. They are all viewing and studying the same matter iron from slightly different angles and with slightly different tools. For that reason, I hope one can see clearly that there is a certain unity in these 3 aspects of the physical sciences since their subject matter is one matter.

When we turn to the social sciences, such as psychology, sociology, economics, etc. there is also a certain unity in that they deal with the behaviour of individuals, of peoples in groups or societies and the relation-ships of material things such as goods, services, labour, prices etc. Literature, philosophy, the so-called fine arts, the dramatic or performing arts, music and the all embracing study of culture deal in each case with a body of knowledge that relates to that particular discipline. In keeping with the definition of the term science, these various bodies of knoweldge are also science. As their subject matter - man as a unit, is one, one can see that there is also among these various disciplines a certain unity.

These various sciences - the physical and the non physical, those that deal with matters and those that deal with ideas, are all really only different disciplines of a single science. This is so because they are all concerned with the one subject - the universe made up of matter, man and ideas. They see that universe and study it from different angles and the methods used for its study should have, if not an identity of method, at least certain common features. This is one justification for the title of this address *Sciences and the Scientific Method*. The second justification for assuming a common approach to science will be evident later when we examine the method themselves.

Partly for convenience and partly because I am myself a biologist, I shall from now on take some of my illustrations and arguments from the physical sciences. Nevertheless, I trust that they will at least illustrate principles that can be seen or applied to the social and non-physical sciences.

As you all know, we live in the so-called scientific or technological era. The term technology conjures up for many people a picture of machines of all kinds, computers, television, radiotelephones, satellites, space ships, etc. On the darker side, are the atomic bombs, nuclear missiles, the threat of nuclear war-fare and the total extermination of mankind, and possibly our planet in one great explosion. There is also the possibility of the slow extermination of man by air pollution and harmful ecological changes of the kind that produce permanent damage to our life systems or again there is the simple running out of our life resources because of their excessive, wasteful or greedy consumption ! This is only one side of the coin. Spectacular as some of it may seem, it is, in its essence, of less importance than the second side of the same coin. This second side consists in the fact that these achievements, both good and bad, both spectacular and modest, are the outcome of the exploitation, of man's *discoveries made through* research.

Our technological era will thus be remembered partly for its technological achievements but more for the knowledge explosion on research activities and discoveries that preceded or accompanied these great and fearsome technological feats. Whilst the dust of the atomic explosion may have settled, that of the "knowledge explosion" is still very thick and waxing strong. The most lasting aspect of our era will be the fact that man has *discovered* that by the deliberate process of research, he can make discoveries and so find out not only solutions to the many problems that beset his life but also raise the quality of that life. Knowledge or science, research and discovery are inseparable concepts that together form the hallmark of our times and should form the motto or the guiding principle of any university worthy of the name.

Let me therefore sieze this occasion to remind my colleagues – teachers and students alike as I did on a previous occasion, of the great importance of research to their mission in the university and their obligations to the nation. In the same spirit, I wish to remind the government and the general public especially industrial and businessmen that, if they wish to see *tomorrow* a permanent improvement in the quality of life for all, they must support research in the universities *today*. Research is the golden key that unlocks the richest treasurehouse of them all, the treasurehouse of nature who in her boundless bounty, will reward a billion fold anyone who pays her court !

Scientific knowledge, in the words of Dr. Fonlon, is the knowledge of things through their causes. This definition is based on the axion that all things that exist must have a cause and that the cause and its effects are inseparably linked together. Thus, we can pass from a given cause and by deduction arrive at its effects or starting from the observable effect, we can work back to or induce its cause. *The purpose of science is not only to understand this cause / effect relationship well, but also if possible to establish the rules, laws or principles that regulate them.* It is important to stress that the scientist only exposes the cause effect relationship or the ground rules for this relationship. He cannot create a new one or alter any of them. The relationship between a cause and its effects existed from the beginning of time itself, although it might only have been last year or 10 years ago that a scientist called our attention to it by his discoveries.

Another important feature of scientific knowledge is that although it can often tell us a great deal about the cause effect relationship, how they are linked and the rules that regulate these linkages *science usually tells us nothing about why a particular relationship exist and not another one.* A scientist will tell you how heat changes water to steam but he cannot say why it happens. *Why things are as they are is, I believe, a mystery !*

It might be asked of what use is the establishment of a general principle or laws that regulate or link a cause to its effects. If we knew these principles well for a given cause, we could predict with a certain degree of certainty which

effects may be expected. For example, in physics, it is a known law or property of matter that heat causes the expansion of matter. Iron rods, water, gases etc., all expand when heated. Although the amount and the rate of expansion varies according to the physical characteristics of the matter under study, yet all of them expand and are predictable. The fact that trinitroglycerine expands explosively does not in any way detract from the fact that, being matter, its expansion when heated is predictable. The knowledge of the principle that matter expands when heated has enable scientists not only to predict what will happen but also exploit or harness its effects, for example, in steam engines from heated water, internal combustion engines from heated hydrocarbons, and even the use of explosives such as TNT or nitroglycerina for military or peaceful purposes. There is thus behind every research undertaking the implied or explicit hope of finding something that is useful to man.

To summarize thus far, we can say, science is concerned with understanding the general principles which regulate or link a cause to its effects. This understanding enables the scientist to predict and to exploit, to our profit, the cause effect relationships. Although science tells us how cause and effect are linked, it does not tell us anything on why they are so linked.

I had said earlier that knowledge or science, research and discovery are inseparable concepts. So they truly are for research is the long road the scientist must travel before he can reach the point of a discovery which, as it were, is the window through which he can take a brief, fragmentary, and often only a momentary glimpse into the rich and secret store-house of nature !

It was also stated that if a scientist knew the cause and the ground rules that linked it to its effects, he could predict or deduce other effects that could be linked to that cause. Research that follows such a principle is called applied research since it is concerned with the application of an established fact or facts. It is oriented towards a given or specific goal. It does not *generally* establish any new rules or principles. It does not therefore generate new research once the goal has been achieved. In contrast, the scientist may have made an isolated observation in the course of studies that may not have had a precise goal in the mind or the observation might have been made in the course of goal oriented or applied research. In the latter case, the new observation may or may not have contributed anything to the goal of the project. In either case if he can link the observation with its cause and by further work establish links between similar observations and a common cause, then he may be in position to establish a general principle or law. Sometimes isolated observations remain isolated until one crucial one permits the other preceding observations to fall into place, as it were, and permit their rational exploitation ! Research of the type where isolated phenomena or the crucial final observation that permits principle or law to be established are called basic or fundamental research. Newton's laws of motion were the outcome of basic or fundamental research started several centuries before Sir Isaac Newton himself was born !

It characteristics of great basic discoveries in Hans Selye's words *that they possess to a high degree and simultaneously three qualities : they are generalizable and they are surprising in the light of what was known at the time of the discovery.* The basic scientist, as I stated above, may not have a clear goal in mind and indeed some cynics have described basic research as *what you do when you don't know what* you are doing. Yet it is true to say that the results of basic research, once clearly and firmly established, generate new research in all sorts of unexpected ways. Newton's law of motion gave birth to the science of machinics etc. Einstein's formula $E = Mc^2$ or energy is the product of the mass multiplied by the square of the speed of light. This formula has given birth to space sciences and its full exploitation is only just beginning.

It would be misleading however to imply by these definitions that the distinction between basic and applied research is as clear cut as I have indicated. A bit of the one is sometimes to be found in the other ; even applied research sometimes lead as an offshoot, to new basic discoveries and vis versa. In any case most research centres have both as each compliments and stimulates the other. I emphasized this distinction deliberately in order to underline a fallacy prevalent in some quarters which says that as we in Africa are in a hurry and do not have the resources we must cut out basic research and concentrate only on applied research. True basic research may not yield immediate results. It is a fallacy nevertheless because in order to say that we cannot do or afford this or that basic research we must assume that we knew exactly what is involved in the project, that we already knew what we are looking for. If that were already, so, there would be hardly any need to carry out the research since we would already have the information needed.

History has shown that some basic discoveries were made at a time and place when the prevailing level of development was as low as, if not lower than, that which prevails in many African countries today ! The individual scientist carried out his work with the same problems and frustrations that we face in underdeveloped countries. History has shown also that the time lag between the basic discovery and its exploitation has nothing to do with the state of physical development of the country as such. In any case, underdevelopment is not due to a lack of basic discoveries that could be applied for the benefit of the people but rather to a lack of the means of applying such discoveries coupled with the natural time lag common to all basic discoveries. It occasionally happens that some applied research even in developed countries has yielded no results for years where as a basic discovery has been exploitable immediately. The real error therefore is to generalize or to take a rigid stance vis-a-vis basic or applied research.

Dr. Fonlon has indicated that the research method follows a certain order – *observation, classification, experiment, measurement and hypothesis,* all of these to be repeated again and again as often as scientists are interested in the problem, for there is no final truth. I will not bore you any further with an individual examination of these items. Instead I shall tell you the story of a medical discovery, one of many which can be told and which I hope, illustrates all of these processes in practice as well as underline many imponderable qualities that go into the making of a great discovery. The story will show I hope that applied and basic research are usually closely linked and that it is futile to draw too sharp a line between the two. The history of science is full of similar examples taken from the other disciplines and all of them serve as great landmarks or milestones punctuating as well as lighting up the dark road of a man's upward march towards the truth.

My only claim for telling this particular story is that I met the man who made this great medical discovery. I was privileged to be among his students at St. Mary's Medical School London where he worked and died. Those who knew him well and

worked with him learnt, at first-hand, something about the scientific method. Those of us who met him only towards the end of his career were inspired by his quiet example.

My story concerns Dr. Alexander Fleming who discovered penicillin in 1929, received the Nobel Prize for it in 1954 and died in 1956. Some of you present here today probably know the story well, in which case I ask your indulgence. I hope that it may in its telling, also inspire our younger colleagues and students, as he inspired us, if they realise that discoveries can be made in spite of physical difficulties, lack of equipment, money, frustrations of all kinds etc., and that these difficulties should indeed serve only to strengthen the determination of the researcher.

In the twenties, there were no effective drugs for treating infections Sulphonamides were only later discovered by a German Professor, Dr. Domagk in 1936. In 1929 Alexandre Fleming was already a young surgeon on the staff of St. Mary's Hospital in London and in those days wound infections were often fatal. Antisepsis with carbolic sprays was still in practice. Asepsis was in its early days. Death from surgical infections were common place occurrences and the surgical fraternity, including obstetricians and gynaecologists, were rightly depressed by the state of affairs. So young Fleming set himself to try to find a possible solution to the problem. It was a problem which at the time must have seemed impossible and required someone with a herculian determination and intellect to solve. Some of Fleming's colleagues must have thought him a little mad or inordinately ambitious to want to tackle such a problem. It was in short, a simple case of a David before a Goliath !

Let me pause for a moment to underline an important quality which must have motivated Fleming at this time. To put it quite simply, he must have dreamt of the day that he could kill those microbes that caused wound infections and killed his patients. The ability to dream is a requisite, perhaps the first requisite, of all great researchers, for all great ideas are dreams first conceived of in the mind. It is of little importance that the dream is achieved exactly as it was conceived. The essential point is that he must be a dreamer, a great dreamer, otherwise he will become only an average scientist or a high grade technician. A great dream provides the scientist with the motive force necessary to see him through some difficult times ahead.

There was no room – or laboratory available for young Fleming in Wright Institute, a laboratory built in honour of another famous microbiologist of St. Mary's Hospital – Sir Almoth Wright. Fleming finally managed to secure a basement room – for a laboratory below the main hospital. Central heating was uncommon in those days and the better homes and hospital wards had only fireplaces where such heat as was available, was provided by coal fires. It was therefore not surprising that Fleming's room in the basement of the hospital was cold, damp, mouldy smelling from a lack of heating or proper ventilation. It was moreover a room where junk and other unwanted items such as old mattresses, old discarded splints, and other items were stored away. As St. Mary's Hospital was, and is still, less than 100 metres from Paddington Station, the second largest railway station of Metropolitan London, coal smoke, coal dust and fumes were common in the hospital wards, corridors as well as in Fleming's little basement laboratory. All trains in those days used only coal !

Fleming was a fulltime physician and was therefore obliged to carry out his researches in his spare time at night or at weekends etc. There was no money to support full-time research. This was, as some of you may remember, the period of the great depression ! I have painted this somewhat gloomy picture to underline the difficulties which Fleming faced at the start of his researches and also to undefline an important factor of scientific research namely, the firm, unshakable, granite-like determination to carry on no matter the difficulties! Fleming had had a great dream. In order that his dream should come true he must stand resolute and unshaken before all difficulties ! That is the second requisite of research - a great dream transformed into a firm conviction coupled to an unwavering determination to carry through, no matter what the difficulties.

Fleming had set out to find an antimicrobial agent that could kill the staphylococcus aureus and the haemolytic streptococcus, 2 dangerous germs ! His basic hypothesis was that tears contained an enzyme, lyzozyme, that killed bacteria, for were it not so, we would all be blind from uncontrollable infections in the eyes. Perhaps if he could isolate this enzyme and study it, he might be able to help treat his patients. His plan of work or *experimental method* was to produce tears and test its action on the germs that caused infections. It must have been a bizzare sight to see Fleming at weekends or in his sparetime seated in his laboratory shedding tears induced by the chemical irritation of his own eyes, for he could not, nor can we for that matter, send for or order a gallon of tears from a firm or company ! He had to produce his own. This item illustrates yet another point about basic discoveries. The scientist must, in many cases, not only come up with an original idea born of a dream but he must also fashion some of his own equipment. Research where you can buy all the equipment require ready-made is usually of the applied type or the development or a slight modification of what is already known. More than ninetenths of all research projects and publications in scientific journals are of this kind. As the results reported frequently do not permit any firm new conclusions *the authors customarily protect themselves by stating self righteously that they draw no conclusions from their observations* (Hans Selyle).

Thus it was that Fleming not only produced the tears he required but also made himself other important pieces of equipment such as incubators for his cultures etc as he could not in any case afford to buy them.

Fleming had prepared culture plates on which he grew the germs that caused wound infection. As the germs grew he would add some of his tears to one corner of the plate to see if the germs were killed. Sometimes a few of the germs would be killed and this gave young Fleming's heart a great thump of joy. Most times, the germs were not killed at all. One day as he prepared his culture plate of germs, he either forgot to cover it up properly to keep out the many stray germs that floated about in his damp cold laboratory or perhaps he was called away before he could finish properly. He could not himself be sure of what had happened exactly. The fact is that when he came to examine his plate, to his horror, he noticed that a bread mould had settled and grown in one corner of the plate, thus ruining that plate for his experiments. His first reaction, was to discard the plate and to start all over again. But then something *totally unexpected caught his attention*. In the area around the mould, the germs appeared to have died but grew very well elsewhere on the plate. That was curious he thought ! Why had the germs failed to grow around the mould ? What was it that linked the mould to the dead germs ? Instead of throwing away his spoiled plate he carefully preserved it and now repeated the experiment which an accident of nature had displayed before him. He transferred some of the mould

on to his new culture plates and was gratified to notice that the germs were again killed by the mould. He repeated the *experiment* several times in different ways with the same and different germs. There was no doubt about it, the bread mould of the species penicilliun had produced a substance that killed bacteria or germs. Penicillin was born and Fleming reported his findings in a medical journal of 1929.

It must be evident that Fleming's discovery of penicillin was the outcome of an accident or chance. But the same accident had occurred to thousands of research and routine workers in microbiology before Fleming. Yet all of his predecessors' to man, threw out the contaminated culture plates and cursed the mould for ruining their work ! While chance played a part in the discovery of penicillin and in other discoveries, we can assert with Hans Selyle that "chance is a lady who only smiles upon those few who know how to make her smile".

Fleming more than anyone else in his field was able to make "Lady Chance" smile because he had a great dream, an audacious dream, an unshakable conviction or faith in that dream, a resolute determination to make that dream come true in spite of the many and crushing difficulties that beset him. Is chance not like the true lady who will yield only to those whose audacity is matched by their determination and perseverance ? Fleming was also a man of *keen observation* whith the curiosity of a child. A curious child, as you must all remember, sees the world with a sense of wonderment. Everything seems new, wonderful and interesting. A child also lacks prejudice and this permits its imagination to play with the most unlikely possibilities. It was these childlike qualities of curiosity, wonderment coupled to an imagination freed from prejudice which permitted Fleming to see and interprete correctly something that had been seen and ignored from centruies before ! These are the imponderables that go into the making of great discoveries.

Some one has rightly said that research is 99 % sweat and only 1 % imagination. Most scientific discoveries belong to this category. It is as well because advancements in knowledge are made a little a time, a small new fact here, another over there, and they all fit like the pieces of a mosaic to make a great picture. However those who make great contribution to that picture and stand out or climb up from the foothills to the summit must display some of the qualities which Fleming showed. Their contribution to science are not only their scientific discoveries but by their example, they inspire others who may have started out on the same long and difficult road.

The epilogue to Fleming's story is well known - not only penicillin was discovered but thousands of other antibiotics came into being. Fleming had shown that the moulds which generally cause disease and ruin culture plates could also produce powerful and useful drugs to fight all kinds or germs including disease causing moulds and even cancer etc. It should be admitted that Fleming's discovery remained ignored by the scientific and general public until the great need for treating war injuries and the pressures of the second world war itself forced the authorities to re-examine and exploit penicillin.

In summary, I will repeat that the scientific method follows the classical route starting either with a hypothetis or an observation, passing by classification, experimentation and measurement and finishing with a new hypothesis which in its turn leads to the whole process again. The scientific method is thus a continuous and dynamic process, never standing still but pushing upwards and forwards always. Those who can bring to this task those imponderable qualities of imagination, curiosity, audacity a firm and resolute determination and perseverance, will *occasionally* be rewarded with a glittering piece of knowledge, the exploitation of which could profoundly affect the entire course of history and the life of man !

Scientific research is sometimes indeed frequently, pursued with the objective of exploiting its results to satisfy man's material needs. However behind all research, whether applied or basic there is at the subsonscious level a basic need to know for its own sake. We want to penetrate the mystery that we are and that surrounds us in nature and the universe. If man begins to know and to understand the laws that regulate matter starting with the tiniest atom with its protons, neutrons and electrons and passing on to the visible matter of this planet, earth, to the stars and the galaxies beyond the stars etc. which make up the universe one must inevitably be impressed by *the order, the logic, the rationality, the beauty and even majesty,* which pervade and manifest themselves in all of these systems. The atomic or particulate physicist with his streak on photographic emulsions produced by high velocity particles and the radio astronomer scanning the dark impenetrable reaches of outer space are both really looking at different aspects of the same thing. Matter and the Universe. Neither the physicist nor the astronomer will tell you that he ever saw the face of a Grey Old Man on his photographic plate or at the end of his telescope. Yet if they looked long enough and really understood only a little of what they are looking at, if a scientist really saw and understood the particular corner of the universe that interests him, then I believe that behind the tiniest particle of the atom, behind the viruses that kill us, behind the planets, the stars, the galaxies and the dark outer space beyond, *behind them all,* he will see the face of a venerable *Grey Old Man* ; he will see *the face of GOD.*

I thank you for your patience and your attention.

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