Reflections on Tradition, Centre and Periphery and the Universal Validity of Science: The Significance of the Life of S. Ramanujan

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Source: *Minerva*, December 1991, Vol. 29, No. 4 (December 1991), pp. 393-419

Published by: Springer

Stable URL: https://www.jstor.org/stable/41820850

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EDWARD SHILS

"THERE ARE MANY highly respectable motives which can lead men to prosecute research. . . . The first (without which the rest must come to nothing) is intellectual curiosity, the desire to know the truth."[1]

I recall an incident at the Graduate Center of the City University of New York more than ten years ago. I was addressing a large audience in the main lecture hall and referred to the obligation of universities and of university teachers to remember their primary responsibility to truth in their teaching and research and in their public activities as advisers, consultants and publicists. Professors I.I. Rabi and Ernest Nagel were in the audience. When I mentioned "truth", they turned to each other, grimacing in disapproval of my use of such an archaic term. I then addressed them directly by name—I was personally fairly well acquainted with both of them and had deep respect for their intellectual achievements and their moral probity. I first asked them with what ends they acted in the conduct of research. I then asked them by what criteria they decided in the course of any particular piece of work that they had attained their objective sufficiently to submit it for publication. I asked them if they did not regard the truthfulness of their results as the criterion by which they wanted their publications to be judged? They both nodded in an affirmative answer to my query. I told them that their affirmative nods were sounder than their reluctance to be linked with such an old fashioned term as truth, with its quasi-religious overtones. Again they nodded, this time with a friendly smile.

The word "truth" might sound to some persons in this secular, antinomian age as carrying with it an odour of sanctity which they would avoid if they could. In fact, however, they cannot avoid it.

In the present age, with its ecumenical aspirations, many scientists and scholars shrink from the use of the term truth in discussions about the great achievements of scientific research over the past centuries. Quite apart from the desire to be "tough-minded" and to escape from the

[1] Hardy, G.H., *A Mathematician's Apology* (Cambridge: Cambridge University Press, 1967), p. 79.

danger of being accused of idealistic pretentiousness and quasi-theological sentimentality by the use of the word "truth", there is also a trace of modesty in this avoidance. The affected modesty is often a special effort made to avoid any overtones of an attitude of superiority or even arrogance towards the great civilisations of the Orient and Islam. They think that to allow it to be believed that they possess "the truth" is to be ethnocentric, perhaps even "racist". The universal validity of scientific knowledge would be one of the victims of this generous effort.

Science contra *Western Science: Territorial and Social Limits on the Validity of Science*

In the past half-century, it has become fashionable in some circles to say that the scientific knowledge and the method of its discovery which have developed in Western civilisation, and which have spread far beyond the countries in which they first flourished in modern times, are "Western science". In some respects, this assertion about the "Westernness" of modern science is unexceptionable. Scientific knowledge and the ways of going about its discovery have indeed been fruitfully cultivated in Western countries in the past four centuries on a scale greater than elsewhere in the world. They have also so entered into the culture and latterly into practical activities of the societies of these Western countries that it is certainly reasonable to say that this body of knowledge and methods are, in that sense, characteristic of Western societies.

The intermittent assertion of the "Westernness" of modern scientific knowledge has not, however, been intended in either of these two "factual" senses. It is not an assertion about the prominence of scientific discovery or of the promulgation of scientific theories in Western civilisation; it is not a reference to the "scientistic" appreciation of scientific knowledge and its methods in the moral values which have been characteristic of Western societies. The assertion that modern science is "Western" has a different, and deeper, intention.

The most far-reaching charge against Western science is that it has no validity outside those societies which together constitute Western civilisation. The statement is made with derogatory intentions, in order to deny any allegation of superiority of Western civilisation and to enhance the dignity of other civilisations. It is an attempt to throw off the stigma of the peripherality of Asia in scientific knowledge over the past four centuries and deny that "the West" is the centre of the world-wide scientific community. If each civilisation has its own science, then there are no longer European or American centres to Asian and African peripheries. The periphery is thereby made into a centre of its own, not by its own scientific achievements but by denying the compelling claim to validity of what is called "Western science".

This polemic is intended to discomfit those who have accepted that the science practised in Western societies is science *tout court*. It is a criticism

of the belief that all knowledge, anywhere and of any objects, has to be assessed according to the criteria of science as understood in these Western societies. It is a rejection of the conviction that whatever knowledge does not conform with those criteria is simply not scientific knowledge.

Another more specific variant of the charge is that Western science is a "dominative science", which, by implication is contrasted with a science which does not aim at mastery. It is a science intended for mastery, "*Herrschaftswissen*" as German crypto-Marxists say. Since "Western science" aims at "domination", it seeks to dominate whole peoples and continents and not only nature. This is alleged to be inherent in the structure of scientific propositions but that is not more than alleged. "Western science" is said to be "imperialistic". Its domination, it is claimed, is distinctive of and integral to Western civilisation in which the highest value, the main goal, of human life is "mastery" or "domination". It is suggested that there are other ways to knowledge of nature no less valid but different in substantive content, fundamental patterns and objectives. When the universal validity of science as practised mainly in Western societies is denied, the ascendancy of scientific centres over scientific peripheries is automatically dissolved.

In Black Africa and in South Asia, one sometimes hears similar, no less general and vague declarations of the territorially or "culturally" limited validity of scientific propositions. The sensitivity of persons living in formerly colonial countries to the dignity of their national culture is understandable. But the arguments they make to support their desires for an enhanced estimation of scientific achievements of their own countries, and particularly for their indigenous science done in a traditional mode, are not strong. The most frequently used argument is to point to some important achievement of traditional medical practice, of the traditional *materia medica*, or to some important astronomical observations connected with calendrical technique, etc.

Ambivalent Marxism: an early forerunner of the denial of the universality of science: In a very crude way, this limitation on the validity of modern Western science was adumbrated decades earlier. It was contained in the Marxian assertion that there is a "bourgeois science" and a "proletarian science". It was first suggested in the Marxian distinction between the "*Unterbau*" or economic "substructure" of society, and the "*Überbau*" or "superstructure" which was the realm of culture, the realm of symbolic configurations, e.g., art, literature, science, religion and law. The *Unterbau*, by which was meant "the relations of production", or ultimately, the distribution of ownership of "private property in the means of production", was said to determine the *Überbau*. Since scientific knowledge, according to the Marxian view, was located in the superstructure, it too was determined by the relations of "production". According to this view, intellectual works which are part of the superstructure are only "reflec-

tions" of the substructure; they are intended to support or realise the interests of the class which has produced them.

Although much weight was attributed by Marxists to these erroneous contentions, the truthfulness of science was not wholly denied. There was however an implication of the limited cognitive value of scientific knowledge in the distinction between "bourgeois science" and "proletarian science". Where the service of practical interests is the primary objective of scientific activity, its cognitive validity is rendered a secondary consideration. The universal validity of scientific knowledge is excluded from attention.

The functions of the capitalistic "relations of production" would, of course, be different from the functions of communistic (or socialistic or proletarian) "relations of production". The superstructure of a society formed around a communistic, etc., substructure must be different from the superstructure of a capitalistic society. Science being regarded a part of the superstructure, a proletarian society would inevitably have a different kind of scientific knowledge from the scientific knowledge developed by the bourgeoisie. All of this was asserted dogmatically; there was nothing to support it. No progress was ever made in setting out the character of the science which would be distinctively communistic or proletarian. The implication was that "bourgeois science", even though it was science, could not deal effectively with certain problems while "proletarian science" could do so. This too was never taken up for detailed treatment.[2]

The contentions of Friedrich Engels and Otto Bauer that Newtonian physics was a "reflection" of the competitive market of the seventeenth century was never pursued very far because this raised questions regarding the validity of scientific knowledge which were otherwise left unquestioned in Marxian "scientific socialism". The essay by N. Hessen on "The Social and Economic Roots of Newton's 'Principia' "[3] was not an attempt to delimit the validity of modern "bourgeois science". It purported only to demonstrate that it was not scientific curiosity working within a tradition of scientific knowledge which advanced scientific knowledge; it was rather the product of the scientist's intelligence working as an instrument for the end of solving practical problems, such as arose in navigation and the construction of mines.

The Marxian view did not delimit the validity of scientific knowledge to the civilisation in which it was created; it delimited its validity to the epoch and the class structure of the type of society in which it was created. It did so very inconsistently and half-heartedly because Marx and

[2] See, Bukharin, N., (ed.), *Marxism and Modern Thought* (New York: Harcourt Brace, 1935), and Bukharin, N., *Science at the Cross Roads* (London: Kniga, 1932); Bauer, Otto, "Das Weltbild des Kapitalismus", in Jenssen, Otto (ed.), *Der lebendige Marxismus* (Vienna: Braunmüller, 1924), pp. 423, 425, 429, 431.

[3] In Bukharin, N. (ed.) *Science at the Cross Roads, op. cit.*, pp. 19-20, 21. (Offprint.)

those who followed him were at the same time great admirers of scientific knowledge and they intermittently believed in its truthfulness.

Sociologists' disregard for any questions regarding the validity of scientific knowledge: More recently there has been a recrudescence of views not entirely different from Marxism but without the overt political intention of Marxism and without the Marxist admiration for science. This is the "strong programme" of the "sociology of knowledge". The "strong programme" derives ultimately from Karl Mannheim who insisted that "knowledge" was socially or "existentially" determined (*seinsverbunden*). Karl Mannheim had said that the categories and even the criteria of truth were determined by the "social position" of the scholar or scientist, but he stopped short of subsuming mathematics and the natural sciences under this sociological proposition. The proponents of the "strong programme" are not satisfied with Mannheim's self-restraining ordinance. They would not stop where Mannheim stopped. The "strong programme" attempts to follow the more radical potentiality of Mannheim's ideas which Mannheim himself was hesitant to espouse. It goes beyond Mannheim's reluctance to declare that the natural sciences might not be subject to the "social determination" to which other types of knowledge are susceptible. The "strong programme" extends sociological analysis to the natural sciences. Like Mannheim's sociological studies of political thought which never inquired into the truth or falsity of the knowledge which he analysed sociologically, the "strong programme" likewise disregards the significance of truth as the objective of scientific activity.

Neither Mannheim nor the "strong programme" attribute any weight to intellectual curiosity, the concern for truth, the inherence of the criteria of validity in cognitive activity and the rational power of the human mind. The "strong programme" treats questions of validity as of no interest. According to this view, scientific knowledge is the product of "interests". The "strong programme" even reaches the point of suggesting that success in the disclosure of the reality of the external world is not to be considered when one speaks of science; the question of validity is at best an epiphenomenon of the practical interests which are pursued in the real world. The "strong programme" is not far removed from Pyrrhonism which assumes that no truths are attainable, except that the "strong programme" does not directly deny that any valid knowledge is possible. It simply disregards the possibility of its existence. It suggests that the very idea of the pursuit of truth by scientists is an illusion. (The proponents of the strong programme do seem to exempt themselves from this Pyrrhonic scepticism about the possibility of the acquisition of valid knowledge by other persons. They think that what they say is true, but that everyone else who works in any branch of science is ultimately under the domination of "interests"; the categories of truth or falsity do not, according to them, apply to the propositions of science.)

There are other variants of the sociological approach to science which would do away with intellectual curiosity and the compellingness of the

criteria of truthfulness as constituents of the pursuit and growth of knowledge. A not uncommon view is that scientists seek knowledge just so that they can exchange it for rewards such as deference, income and professional appointment.[4] Fundamental to these sociological approaches is the elimination from scientific activity of rationality and the application of criteria of validity of scientific knowledge. The argument that the search for scientific knowledge is a service of practical interests—even when the individual scientist himself does not have the practical goal as his objective—does not consider the possibility that there are individuals who love intellectual exertion, who regard the truthfulness of the knowledge they acquire as decisive for its acceptability. This argument is blind to the integral place of the application of the criteria of validity in the growth of scientific knowledge. The same argument eliminates rationality in any form other than that of instrumental rationality exercised in the pursuit of material, pecuniary or social interests. The idea that the exercise of rationality can be regarded as a moral obligation in scientific research, that rational arguments can be made and can be persuasive has no place in these sociological analyses. The scientist for them is an automaton propelled by "interests", which are very seldom, if ever, intellectual interests.

The sociological approach to the study of scientific activity fails to acknowledge that, in the course of scientific activity, criteria of validity are continuously being applied by the scientists. If this is disregarded, then the sociological study of scientific activity is misleading. Somehow, sociologists of science cannot understand that observational powers and the exercise of rationality are inherent in the intellectual confrontation with nature. Observation and the criteria of the validity of observations and the exercise of rationality are not solely the products of culture mediated through social institutions. They are of course influenced by the institutional and cultural setting in which they are aroused and exercised. They are, however, inherent in cognitive or intellectual activity as such. A scientist learns the criteria of validity when he studies science and his facility in applying them is enhanced as he does research. But criteria of validity are not just afterthoughts. The disposition to possess and apply them is inherent in the act of seeking knowledge. Of course, the young scientist learns all sorts of procedures for testing and checking his observations and inferences but the need to do so is given by the very act of engagement in cognitive activities. Even rats learn to discriminate between true and false knowledge, although they do so on a very elementary level.

I conclude from this that criteria of validity are not "culture-bound", even if we mean by "culture", in this context, the "culture" of science in general or the "culture" of any particular science. Scientists in all

[4] Hagstrom, Warren, *The Scientific Community* (New York: Basic Books, 1965).

civilisations seek evidence of validity of observations and consistency of reasoning. The criteria of validity increase in discriminatory power, *pari passu*, with the growth in the reliability and differentiation of the body of knowledge—e.g., the discipline—in which they are applied.

Meta-history as a denial of the possibility of truthful knowledge: These sociological arguments are not very far from the assertions of the "meta-historians" who assert that historical works are to be considered only as narratives and are not to be considered for their validity. They do not speak simply as literary critics who have a legitimate interest in the literary form and style of historical works, while allowing for the possibility of their truthfulness as historical works. They insist that historical works are only narratives and not at all works to be judged with regard to their cognitive plausibility or to their fidelity to the data elicited from literary documents, archaeological artefacts, etc. For the meta-historians, the question of cognitive validity or truth does not arise. The question of the universal validity of scientific knowledge does not occur to them.[5]

The "meta-historians" do not however think that their own writing of "meta-history" is only narrative; they think that their statements about meta-history, and the exclusively narrative character of historical works, should persuade their audience because what they say is true to their empirical observation and analysis of historical works in general or in particular. The meta-historians, like the proponents of the "strong programme" in the sociology of knowledge and even for that matter the proponents of the Mannheimian or "weak programme" in the sociology of knowledge, do not think that their refusal of any questions as to the truthfulness of the knowledge and the search for knowledge which they study is applicable to their own statements. When one argues with them, they cite evidence and they insist that what they say is true. But in their analysis of the works of historians, they simply disregard the question of the truthfulness of scientific propositions, of the theoretical and empirical evidence bearing on their truthfulness or untruthfulness, and the rationality or logical rigour of analysis. They proceed as if the human mind has no such capacities.

The Movement for the Derogation of Science

In Great Britain in the second half of the nineteenth century, there was a movement called Endowment of Research.[6] It was intended to arouse public opinion and government to the fact that valuable opportunities for

[5] White, Hayden, *Metahistory: The Historical Imagination in Nineteenth Century Europe* (Baltimore: Johns Hopkins University Press, 1973).

[6] See MacLeod, Roy M., "The Support of Victorian Science: The Endowment of Research Movement in Great Britain, 1868-1900", *Minerva*, IX (April 1971), pp. 197-230.

scientific discovery were being lost in Great Britain because of the prejudice against the financial support of scientific institutions, particularly on the part of government. It attracted the support of very eminent Victorians.

In the years since the Second World War, at a time of great scientific achievements, science has been faced with the reverse image of the movement for the Endowment of Research. It is a "movement for the derogation of science".

The Marxist challenge to the universal validity of science, although aggressively and arrogantly trumpeted for a time, had little direct influence on the actual evaluation of scientific knowledge; some of its proponents were themselves eminent scientists whose research was not affected by their doctrinaire views about "bourgeois science". Despite all of its portentous lightheartedness regarding "*Unterbau*" and "*Überbau*", Marxism accepted the achievement of science as given. Except among the "theoreticians" and propagandists of Marxism-Leninism and the intervention of the power of the police in genetics in the Soviet Union, Marxism was very scientistic. It even claimed that Marxian socialism or communism was itself a science.

The work of the "strong programme" of the sociology of knowledge has very little circulation; it is read mainly by sociologists and sociologically inclined historians of science. The main significance of the programme lies in its being part of a wide intellectual movement to derogate civil or bourgeois society, democratic liberalism, Western civilisation as a whole and science as one of the greatest achievements of that civilisation. The enemies of Western civilisation wish to strike down that achievement of which it is most proud and which is regarded outside the West as its greatest claim to respect all over the world. If science is dethroned, then Western civilisation will be dethroned also. So they think!

The campaign is carried over a wide front. There are those who concentrate on the protection of animals from vivisection in medical research; there are those who blame science as a whole for the pollution of the air and waters through industrial waste. Some fear that genetic engineering might have devastating effects on plant life; others fear the arrogation to themselves by scientists of powers over life and death in consequence of the progress in medical technology. Others are repelled by the pride and self-confidence of scientists. Perhaps at bottom there is a very diffuse fear of the destructive capacity of nuclear weapons and of the danger of accidents at plants for the production of electricity through nuclear energy. Then too there is the lingering resentment against modern scientists for having, so some critics think, discredited the traditional religious view of the world which they consider was necessary to social order and to the assurance that there is an ultimate—and comforting—meaning in cosmic existence.

Among this diverse multitude of critics of science, there is no unanimity. There are those who accept the benefits of scientific research to which they owe their wealth, prosperity and comfort, but who denounce it nevertheless. They are apprehensive of its consequences. They do not doubt its cognitive validity. Then there are those who think that science is part and parcel of a capitalistic, imperialistic, "racist" civilisation. They say that science is no more than an "ideology", an elaborate effort to transfigure and promote exploitation on a world-wide scale. This last group probably does not care much or fundamentally, one way or the other, about the question of the universal validity of science. Questioning it however is a means of bringing the present social order of Western, liberal-democratic societies into disrepute.

Does Every Civilisation Have its Own Criteria of Scientific Validity?

The question of the universal validity of scientific knowledge is raised very seriously by the great work of Joseph Needham and his collaborators on *Science and Civilization in China*. Dr Needham has been a boundless admirer of Chinese civilisation for more than 50 years and a stern socialist critic of Western capitalistic bourgeois society for an even longer period. He has assembled, by an exercise of unprecedented erudition, an extraordinarily large and well-ordered body of information about the existence of scientific activities in China, the intellectual or scientific merit of which is undeniable.

But can it be called scientific knowledge? Is it something different from what is called scientific knowledge in the West? Can it legitimately be assessed by applying to it the same criteria as are applied to the assessment of scientific works in the West? Dr Needham sometimes, in an apparently regretful tone, says that the same criteria must be applied. He will not have any relativism in the assessment of scientific knowledge.

Other serious authors who have studied the same material seem to demur or to be not perfectly comfortable with either the affirmation or the denial of the universal validity of scientific knowledge. They do not express their views passionately because they are uncertain of their ground. Nevertheless, they are hesitant to reach firm conclusions, as Dr Needham seems to do on the basis of his earlier work in biochemistry and embryology and his profound and encyclopaedic study and reflection since the middle of the 1930s on Chinese science and technology. There is however a steady overtone in the uncertain discourse: what is done in China is not the same thing that is done under the rubric of science in the West. It is wrong to try to understand Chinese science as an activity comparable to scientific work done in the West and to judge it according to the same criteria of scientific validity as that science is judged.

I will put the question baldly: are scientific propositions formulated in the West valid universally, assuming they meet the other requirements of rules of observation and analysis? Are what we call scientific propositions

true for China, Japan, India, Black Africa, etc., i.e., are they true when enunciated in China regardless of whether they are enunciated by Chinese or Germans or Englishmen? Are they true in China, regardless of who asserts them? Do the Chinese have their own scientific way of understanding the world which is different from and incompatible with the scientific way of understanding the world of persons raised and educated in Europe or North America?

Traditionality and truth: to say that a particular belief is traditional in a particular society usually carries with it overtones of "relativism". William Graham Sumner, who was a famous American sociologist about a century ago and who is now almost entirely forgotten, said: "The mores can make anything right."[7] Right is what the mores sanctify; it is what tradition transmits. The fact of transmission is its validation. That was Sumner's view of the matter. He thought that one could not assess traditions with respect to their truthfulness. Whatever traditions transmitted was true by virtue of the authority inherent in traditionality. This might be true of some traditions but not all. Indeed, all traditions or beliefs transmitted by tradition have a cognitive content. In so far as they have that cognitive content, they can be affected by experience and observation.

Hence, the fact that a proposition has reached the present generation as a tradition from the past does not mean that it is untruthful. The fact that it is a part of tradition or, to put it differently, is itself a tradition, says nothing at all about whether it is true or false. It is entirely possible that the proposition has become part of tradition because it has been discovered to be true. Propositions might persist in tradition because they have been recurrently received under the severe scrutiny of a sequence of generations of critical recipients.

It is certainly true that many traditions are received on the ground that they are already there and that they are recommended explicitly or implicitly by authority. But it must also be said that some, perhaps many, human beings—especially educated and experienced ones—have a demand for truthfulness, for evidence and reasons which are integral to their cognitive apparatus. In other words, when they hear or read a proposition, when they are presented with a proposition by tradition, they invariably ask whether it is true, they wish to know the evidence for it or the reasons. In the same way, they raise similar questions about their own observations, recollections and assertions. Individual human beings are not indifferent to the truthfulness of what they accept or assert. What they have come to believe on the basis of their critical observations and their rational analysis becomes a tradition for their next generation. The

[7] Sumner, William Graham, *Folkways: A Study of the Sociological Importance of Usages, Manners, Customs, Mores and Morals* (Boston: Ginn, 1906).

fact that a proposition has been received through a sequence of transmissions does not constitute an argument against its cognitive validity.

Thus, the traditionality of the criteria of validity of scientific propositions could be interpreted as evidence that the criteria of validity are accepted in a given civilisation because they have been recommended by persons in positions of authority. Alternatively, the traditionality of the criteria of validity might be interpreted as evidence that their validity, having passed through a sequence of generations of intelligent, informed and critical persons who have applied the criteria of validity in their own work and have found them appropriate to discriminating the true from the erroneous, have passed them on to the ensuing generation. The latter too, applying them with critical discrimination, have found them no less truthful. So they have been passed down from generations. In each generation they are tested through disciplined observations and experience and rational judgement.

Thus to declare that the criteria of validity are traditional, meaning that each new generation when it comes to do scientific research, finds these criteria being applied by their elders, does not mean that the criteria of validity are applicable only in the particular society in which the criteria have been traditionally transmitted. The validity of the criteria transcends the tradition which carries them. Similar criteria may be carried by traditions indigenous to their countries and other civilisations. It is important to be cognisant of the fact that indigenous traditions can transmit truths of universal validity.

Evidence and reason apply to the external world, the physical world; and they belong to the world of symbolic objectivations or patterns (what Karl Popper has called World 3[8]). They have a certain measure of compellingness regardless of the civilisation in which they are found.

Beauty and truth: The question regarding the universal validity of scientific propositions and of the criteria for their assessment is different from the question of whether the aesthetic standards accepted in the West are valid for China. Is the Chinese criterion for the assessment of the beauty of a painting the same as the criterion used in the assessment of the beauty of a Western painting? This is a question which is difficult to formulate precisely but it is also moderately easy to answer. Works of art are not to be ranged on an exact scale of degree of proximity to the universal ideal of beauty. Works of art are also not cumulative and progressive in the movement towards the ideal. The approximation to the ideal of beauty does not increase over the course of the history of art within any given civilisation. The historical course of artistic works is not cumulative in the same degree as the course of scientific works, which

[8] Popper, Karl R., *Objective Knowledge: An Evolutionary Approach* (Oxford: Clarendon, 1975), pp. 106-190.

must build on the latest state of valid knowledge. A new work of art does not supersede its immediately or its remotely preceding works of art, as is the case with works in the natural sciences. There is nothing in art like the phenomenon of an earlier proposition in a particular science having been superseded by a better set of subsequent observations, or a more rigorous analysis of observations, or a more comprehensive theory with greater explanatory power or a better proof. Of course, works of art are assessed as being of differing merit, but a later work of art does not render a previous painting "out of date" and artistically uninteresting. There is, in principle, no need for a scientist to re-read scientific papers of the seventeenth century except to satisfy his curiosity about the history of his subject and to appreciate the greatness of great scientists of the past. (In fact, however, reading and re-reading older scientific works sometimes discloses ideas which are important for contemporary work but which have not been followed up.) The earlier papers have been rendered scientifically superfluous by subsequent work on the subject. Earlier paintings are not rendered aesthetically superfluous by later paintings.

The same things are true about the comparison of the artistic and the scientific works of different civilisations. The great artistic works of the past, the "existing monuments", in T.S. Eliot's words, "form an ideal order among themselves".[9] Eliot adds that when a new work becomes established as of high value, it takes its place in the "ideal order" of works created in the past, and its inclusion alters that whole ideal order. But it does not supersede them as is the case with scientific works. Chinese paintings and sculptures stand alongside modern European and ancient Greek paintings and sculptures. There is no explicitly formulated common denominator which permits each of the works to be placed on a scale of beauty, regardless of its country or civilisation of origin. The criterion of beauty is vague and it is incapable of precise application. The criterion of truth is also vague but in the comparison of one scientific work on a particular problem with another scientific work on the same problem, it is susceptible of rigorous application, although a complete unanimity of the assessors is not always achieved. Yet, the assessors have fairly precise notions of the tests to which they put a work in order to determine whether what it asserts is true. Scientific works produced in different civilisations may be compared with each other and it can be reasonably decided which is truer according to the criteria of validity. It is possible to compare the scientific propositions produced in one civilisation with a proposition about some similar part of nature produced in another civilisation.

The Comparability of Propositions about the Order of Natural Phenomena

Each civilisation has its characteristic or distinctive traditions. They are what constitute in its uniqueness. The civilisation is unique but it is one

[9] Eliot, T.S., "Tradition and Individual Talent", in *The Sacred Wood* (London: Methuen, 1920), p. 50.

civilisation among others; the fundamental categories in which its uniqueness is described are the same as those used for describing other civilisations. Unique civilisations are not unique in every respect; they are made up, in the last analysis, of human beings who have determinate properties around which every civilisation is formed. This places limits on their uniqueness, although it might be stated by the proponents of the argument for incomparability that "the Chinese do not have or use the same criterion of truthfulness as do the Europeans". This is sometimes said but it is wrong. The Chinese do have an idea of truthfulness of propositions about the natural world. There are definitely differences between the relatively coherent bodies of knowledge in the physical and biological sciences which have been formed in the West and the concrete, logically and empirically incoherent bodies of knowledge on comparable objects formed in China. The differences in the criteria of validity lie more at the level of the synthetic or theoretical treatment than at the level of concrete observations.

The ultimate general categories under which they subsume their particular explanatory propositions have been different in China from those of the Europeans, but this does not gainsay that the Chinese have produced vast bodies of relatively concrete descriptive propositions which they thought to be true. These properties can be justly assessed by persons brought up in the scientific tradition prevailing in Western societies, and using the criteria which have been developed in the course of the growth of that tradition.

This last sentence might be interpreted as "begging the whole question". It admits that the assessment by Western scientists of scientific works done in China applies the criteria which are used by Western scientists. The admission is correct but the criteria are, I would emphasise, not Western criteria of truthfulness; they are the criteria used in the West for assessing the degree of truthfulness of propositions put forth as truthful. They are criteria of truthfulness which may be justifiably applied to propositions about natural phenomena, enunciated anywhere on the surface of the earth and in any civilisation. The validity of the criteria does not depend on their place of origin or exercise. The only question about the criteria is whether they are adequate to discriminate true from false propositions or, in other words, to assess them, approximately, with regard to their approach to or distance from truthfulness.

The assertion of the universal validity of scientific propositions does not mean that the application of the criteria of truthfulness will invariably and inevitably find that propositions asserted by scientists in China or India are incorrect, and that those asserted by scientists in the West are correct. The opposite might be the case. In this event, the proposition on that particular topic enunciated by scientists in the West might be corrected in accordance with the better substantiated proposition discovered in China. It is exactly this that is meant by the universality of science. The

comparability of scientific propositions of whatever provenience is its precondition.

The criteria which are central to the universal validity of science do not depend for their validity on their consistency with the religious, political or literary culture of any society, nor do they depend on declarations by authorities in kinship or lineage groups or in the political order or in ecclesiastical bodies. They are the criteria of validity which are necessarily applied in the critical assessment of phenomena of the natural order. Empirical evidence, consistency with theories which are themselves consistent with critically assessed empirical evidence, are necessary to learning the truth about nature. To learn the truth about nature requires constant critical scrutiny of theories in relation to evidence and the constant critical scrutiny of evidence in accordance with theories; it requires constant critical scrutiny of theories in relation to theories, and the constant critical scrutiny of evidence in relation to other evidence about the same and related phenomena.

The crucial term here is "critical". This means detachment and scepticism in the scrutiny of observations and reasons together with belief that the external world exists and is knowable by careful study. "Critical" refers to reluctance to believe and readiness to believe, but only on the basis of the scrutiny of evidence and reasons. These are not "constructions" or the products of traditions; they are products of the powers of the human mind. They are what enters into the tradition of scientific knowledge.

The Autonomy of the Criteria of Scientific Validity vis-à-vis *Society and Culture*

The fact that criteria of truthfulness (or error) of scientific propositions developed originally in ancient Greece, and have with modification been applied in the West, says nothing at all about whether they are of territorially limited validity. The fact that a proposition was promulgated in one country, e.g., Greece, does not mean that it is incapable of application in the United States more than two millennia later. The fact that the Red Indians knew nothing about Greek scientific reasoning does not invalidate what the Greeks discovered and invented. A scientific proposition is about reality; it is not about the persons or society which produced it. The evidence of its truth is in no way to be assessed by examining the individuals who produced it. During the National Socialist regime in Germany, two eminent physicists, both Nobel prize-winners, Johannes Stark and Philip Lennard, contended that there was a Jewish physics and an Aryan or German physics. Bieberbach, a German mathematician, made similar assertions about mathematics. Their views, quite properly, were treated with revulsion and contempt. No decent scientist or scholar would tolerate for a moment the assertion of a connection between scientific propositions and the race or nationality of the scientist who asserts it.

Nevertheless, when a similar connection between a civilisation and the scientific propositions which are enunciated in it is asserted, it is treated with respect. No one will assert that the genealogy of a scientist supplies a basis for deciding whether his assertion of a particular proposition is true or false.

One sometimes hears the question being asked about a particular scientific proposition, "But is it true for China or India?" What does that question mean? Does it mean that a mathematical proposition promulgated in Germany in the early twentieth century loses its validity in consequence of having been learned and taught by a Chinese or an Indian in China or India three quarters of a century later? Is there a fundamental disjunction between the truth of what the Chinese scholar believed when he was in China or an Indian scholar believed when he was in India, and the truth of what he now believes in Germany or Great Britain? No, it certainly cannot mean that. At least no sensible person would agree that the validity of the proposition is affected by the transfer of its propounder or expositor from Germany to China or India or vice versa.

Being Chinese or Indian has not prevented a Chinese or an Indian scholar from learning mathematics in Germany or Great Britain at a high level and teaching mathematics and doing original research in it in China or India. He might have studied more or less the same mathematics in France, Great Britain or the United States; his return to a career in teaching and research in China or India would not affect the validity of the mathematics which he teaches in China or India.

The career of Srinivasa Ramanujan illuminates these topics.[10] From thinking about Ramanujan, I have concluded that there are no territorial or social or religious or ethnic limitations on the validity of what a scientist discovers. The discoveries of a scientist of one civilisation or nationality can be received, assessed and assimilated by scientists of any other civilisation or nationality, assuming, of course, that the recipient scientist is sufficiently informed regarding the state of the subject and has the intelligence and scientific training to comprehend what is offered to him.

The mathematics which Ramanujan did in India could be assessed by British mathematicians of the highest order to the extent that they could re-trace the steps which his intuitive powers had enabled him to leap

[10] I first encountered Ramanujan and learned of his career, his genius and his misfortunes about 45 years ago. I have read whatever I could find on him. He was repeatedly before my mind whenever I taught or wrote about tradition and about centre and periphery. Sometimes in the middle of the 1970s, I had the privilege of a long evening after dinner with Professor Littlewood from whom I learned much that is of value. On several occasions, Professor Chandrasekhar has spoken to me about Ramanujan and allowed me to read some of his unpublished writings on Ramanujan. I have been informed and stimulated to think more about Ramanujan by Bruce Berndt's article on him, "Srinivasa Ramanujan", in *The American Scholar*, LVIII (1989), pp. 234-244, and by Mr Robert Kanigel's *The Man Who Knew Infinity: A Life of the Genius Ramanujan* (New York: Scribner's, 1991).

over. It was not the "Indianness" of Ramanujan's mathematics which baffled the first British mathematicians, Hobson and Baker, whom he approached; it was their exceptionally advanced originality. It required two mathematicians of the very high quality of Hardy and Littlewood to appreciate, to learn from and to contribute to Ramanujan's work. As the years passed, and his notebooks have been studied, his mathematics have been interpreted, proved and assimilated by Western mathematicians.

I have not read any references to the specifically Indian character of Ramanujan's mathematics. His mathematics are mathematics, indifferently of the place and circumstances of their creation. Apart from the qualities conferred on them by his genius, some of the qualities are functions of his limited formal education in mathematics; they arise from his autodidactic learning of mathematics and the absence of opportunities to engage in strenuous mathematical discussions with mathematicians who were approximately of the same standard as himself. His mathematics are not to be seen as Indian or British or Western mathematics; they are mathematics as such. Mathematicians do not, when they study Ramanujan's work, think that they are dealing with the mathematics of a different civilisation. They know that they are confronted with mathematics of exceptional depth and originality. The country or civilisation in which they were created is irrelevant to the assessment of their mathematical merit. Ramanujan himself who was an observant Hindu and thought that he was impelled by the inspiration of Namagiri never suggested that his mathematics had their intellectual point of departure in the traditions of Hindu mathematics. He regarded himself as doing mathematics which were not limited in their validity to Indian or European or British civilisation.

It may be asked whether Ramanujan would have been able to attain the same level of mathematical achievement if he had never studied mathematics at all but had to create it entirely by himself. Obviously not. He had to assimilate some of the tradition of mathematics before he could go on to do original research in it. The tradition of the mathematics that he learned was not indigenous in India but that seems never to have troubled him. He was utterly untroubled by any thought that his mathematics were any less or more valid in Germany, France, Great Britain, the United States, etc., than they were in India.

Ramanujan's achievements do much to answer the question of whether it is permissible to speak of the universal validity of science. Indeed, his life seems to show that there is fundamentally nothing to the question. Still, does not the non-empirical character of mathematics as a science put mathematics into a special position? Its reality is entirely free of any preoccupation with physical substances which are located at particular places. Does this make any difference to the question about the universal validity of science? I do not think so.

As far as the empirical or material substances are concerned, the chemistry developed in Germany, France, etc., is as valid in China or

India as it is in the countries in which it was originally developed. The chemistry developed in China in the periods dealt with by Joseph Needham might, in many topics, be identical with the chemistry developed in Germany, etc. Where the two traditions of chemistry are not substantively identical, it might well be that the Chinese variant is less valid than the German variant, but it might be the other way around. Whether it is the other way around can be determined only by applying the criteria by which the truthfulness of propositions in chemistry has been established in the course of the growth of knowledge in chemistry. Chemistry as a science has developed mainly in Western civilisation—although all other civilisations have achieved some chemical knowledge—but the territorial provenience of any proposition in chemistry does not affect its status as a scientific proposition.

The same may be said about Chinese traditional medicine. Where the Chinese medical and pharmaceutical knowledge presents propositions which are not to be found in modern Western medicine, those propositions, if they are tested by observation and experiment and turn out to be truthful, must be added to or correct the body of medical knowledge taught and applied to the West. In any case, even if they deal with pharmaceutical substances which are to be found only in China, their validity is not confined to China. There might be a disease which is found only in China but the medical analysis of that disease, if it meets the criteria of scientific validity, is valid all over the earth's surface, wherever that disease appears.

The Universal Validity of Science and the Geographical Distribution of Scientific Activity

It is no territorial restriction of the validity of scientific knowledge to observe that it has developed at a higher level and with greater intensity in some parts of the world than in others. The universal validity of scientific knowledge—the universality of science—is not the same as the universality of scientific activity. The differences in the intensity or density of the cultivation of science in the different regions of the earth's surface depend in part on the indigenous traditions of the respective regions or societies. It is probably a function of the extent to which traditions, in themselves not scientific, in the indigenous cultures are congenial or conducive to the scientific mode of conceiving of natural phenomena and to the scientific ethos. It is also a function of the extent to which those specifically scientific traditions are sustained or frustrated by institutions which direct and concentrate the transmission of the proto-scientific and scientific traditions.

The sociological study of the growth of scientific knowledge can contribute to our understanding of how institutions affect the advancement of knowledge through the selection of students, teachers and research workers, through policies of financial support by private and

governmental and ecclesiastical patrons, through the provision of means of communication among scientists, etc. The sociological study of the role of institutions—of "society"—on the growth of scientific knowledge, can, however, say nothing at all about whether the propositions so discovered are true or false. The sociological study of the growth of scientific knowledge is powerless to deal with the validity or invalidity of the proposition arrived at in the institutions it studies.

Scientific traditions and non-scientific traditions: Traditions are numerous. There are, in the first instance, the traditions of scientific knowledge and scientific methods. These are the traditions which are decisive in the growth of science. Without these traditions, there can be little growth of scientific knowledge, only perpetual rediscovery, with a rare surpassing of the previously discovered.

Ramanujan's mathematical discoveries are a case in point. With all his genius, a substantial fraction of his discoveries were re-discoveries of propositions already promulgated by mathematicians in Europe. That is what he and the world lost by his exclusion from Government College, Kumbakonam, and Pachaiyappa's College and by his physical confinement for the first 27 years of his life to a periphery of the mathematical world. It may be said that growth of the scientific tradition has been a function of the extent to which that tradition could become separated from the other traditions of the society, most noticeably the traditions of institutions like those of churches, lineages, monarchies and empires.

These numerous non-scientific traditions have influenced the unfolding of the scientific tradition. I say "unfolding" deliberately. By "unfolding" I mean the release of an "epigenetic" potentiality contained in previous scientific knowledge in confrontation with the constant stratum of the phenomenon of external and symbolic realities, and the exercise of powerful human minds working on these realities and within those scientific traditions.

A powerful mind is not bound entirely by its scientific tradition; if it were, there could be no growth of scientific knowledge. Nor is it entirely bound by the non-scientific traditions of the society in which it has grown up. The experience of Srinivasa Ramanujan is a very good testing ground for this assertion. Ramanujan remained all his life an observant and probably believing Hindu, devoted to his deities and especially to Namagiri, the goddess of his family; he adhererd strictly to Hindu rituals and rules of life, except for breaking the prohibition on "sea-voyage". In this sense, Ramanujan remained within his indigenous non-scientific tradition. In mathematics, however, there is no indication that he worked forward directly from the tradition of Hindu mathematics. When he was a student in the Town High School at Kumbakonam and the Government College there, and then at Pachaiyappa's College, he made his first connection with the tradition of the mathematics which had developed in Europe in modern times.

He did not remain long at either of the latter two institutions and, even while he was there, he was mostly on his own in mathematics. He ran far beyond the mathematical knowledge and skill of his teachers. In other words, he ran far beyond the traditions of mathematics which were available to him. He went far beyond the point at which he received the tradition of mathematics of European origin which he received from his textbooks. He also went beyond the point reached by the traditions of mathematics being done in Europe, after the point reached by the traditions which he had encountered. He also, as the continuing study by later mathematicians of his notebooks shows, went well beyond the tradition of mathematics in Europe of which he became aware in Cambridge.

He owed much to the first volume of A.G. Carr's *Synopsis of Elementary Results in Pure and Applied Mathematics*, published in 1880. Carr was a private coach in mathematics; he taught his pupils the mathematics needed for the Cambridge tripos in mathematics of the 1860s and 1870s. His *Synopsis* was completed as he himself was completing his own belated studies for the BA at Cambridge in 1880, when he was already in his early forties. It is likely that his own studies for the tripos in mathematics affected what went into the *Synopsis*. This was a body of mathematical knowledge which was already being seen by continental and leading British mathematicians, like Hardy himself, as retrograde. The Cambridge tripos of that period was said to have been rather narrow and somewhat isolated from important developments in mathematics on the European continent in that period. Hence Ramanujan had his point of departure in the particular part of the tradition of Europe which was represented in the archaic Cambridge tripos of the two decades between 1860 and 1880. Ramanujan, through formal education and his own studies, acquired knowledge of some of the traditions of mathematics at the point which it had reached in England, mainly in Cambridge, in the third quarter of the nineteenth century.

Ramanujan had in his own mathematical work in India in the first decade of the twentieth century gone far beyond some of the points attained in mathematics in Great Britain up to that time. In about 1905, he entered a tradition of mathematics which had been reached in Europe at a number of points and which had already been transcended by far in continental Europe and Great Britain in the first decade of the present century. He himself had, at that time, no contact with those later achievements, since he became familiar with the tradition at the point which British mathematics had reached in 1880.

Such however was Ramanujan's genius as a mathematician that, by his own exertions, he not only advanced far beyond the tradition at the point at which he had entered it, but he had also advanced in certain respects well beyond the points reached by some of the best mathematicians in Europe, or at least in Great Britain, in 1911. He did so to such an extent that on the

day when Hardy received his letter and had spent a good part of that day preoccupied and puzzled by his first inspection of the letter, and then spent the late afternoon and evening of that same day discussing them with Littlewood, he and Littlewood decided that Ramanujan was a genius.

These advances and those he made during his years in Cambridge were pronounced extensions and elaborations of the mathematical tradition which had been attained in Europe. Some of his advances have still not been fathomed. His notebooks are in the process of being edited and those mathematicians who have studied them are confident that, even now, they contain ideas which have not yet been attained, although they were recorded more than 70 years ago.

Centre and Periphery

The conception of centre and periphery in its most elementary form describes a situation in which high talent is relatively concentrated at the centre or centres and the peripheries are populated by persons of less talent, small accomplishments and far smaller reputations. The case of Ramanujan requires the recognition of a variant on the elementary patterns of centre and periphery just indicated. It shows the potentiality for ideas of the centre to arouse the potentialities of a genius at the periphery to the point at which a sector of the periphery can become a centre.

It is possible that India could have become a centre of mathematics had Ramanujan remained in India. By the time Ramanujan began to attract the attention of a small number of Indian mathematicians, a small and dispersed mathematics community had already come into a rudimentary existence in India. Without the Indian mathematicians who took an interest in him, Ramanujan would have been even more neglected than he was in India. The Indian mathematicians played some part in the complex arrangements which led to his going to Cambridge—e.g., the granting of the research fellowship for two years at the University of Madras—and before that in finding an appointment for him in the Madras Port Trust.

Could Ramanujan have succeeded in creating a mathematical centre in India, or more particularly in Madras, had he survived for a decade or two after his return to India in 1919? It is certainly not out of the question that he might have done so. Had he been successful, it would have demonstrated not only the variability of the relationship between centre and periphery, but it would also have borne witness to the universal validity of science which underlies the co-existence of centre and periphery and which forms their bearers into an intellectual and moral community.

Genius and Traditions: Primordial, Transcendental and Scientific

Ramanujan's career testifies to the capacity of a great mind to transcend the primordial and the transcendental traditions in the midst of which it entered the world and in which it made early contact with them.

Most minds formed in those primordial and transcendental traditions remain within them. More powerful minds are capable of reworking those traditions and of assimilating and advancing them beyond their point of entry. Ramanujan did not rework and advance those primordial and transcendental traditions beyond the point at which he entered them; he did not rework South Indian Brahminical Hinduism; he was content to receive that as it was presented to him. He adhered to it as strictly as he could while he lived in England. What was remarkable was the co-existence of this form of traditionality alongside his remarkable exercise in an almost totally exogenous tradition—the tradition of modern mathematics. This other tradition—the scientific or, more specifically, the mathematical tradition—which he received aroused his exceptional intellectual powers. He quickly mastered what was offered to him and then he went far beyond that.

Contrary to the Romantic idea that tradition and genius, or tradition and originality, are invariably antithetical to each other, the opposite is the case. A tradition receives into intself the product of the exertions of individuals of powerful intelligence, imagination, courage and sensibility. Genius takes its point of departure in tradition; it extends and elaborates what is given by tradition. It begins in tradition and departs from it and reaches destinations hitherto unreached. It begins in tradition and its subsequent advances from tradition bear within themselves traits of its point of departure in tradition. It never cuts loose from them completely, however far it advances from its starting point. This accounts for some of the difficulties Littlewood encountered when he tried to bring Ramanujan's use of more recently developed mathematical methods up to the level attained in Europe after 1880. Nevertheless, the achievements of the genius take their place in the tradition which becomes significantly modified by those advances.

The intimate reciprocal relationships between genius and tradition are evident in the case of Ramanujan. He made a connection with some older and incomplete condensations of the tradition of the mathematics developed in Europe; he retraced, in his own way and in ignorance of them, paths of the tradition which had already been traversed in Europe. In other respects he shot well ahead of the points reached by the movement of the tradition in Europe. It is unlikely that the tradition of Hindu belief in which Ramanujan participated steadfastly obstructed his advances from the tradition of mathematics which had developed in Europe. Ramanujan thought that the Hindu goddess in whom he believed had in fact inspired his mathematical advances. That is a matter which a sceptic cannot adjudicate definitively; at the same time, it seems unlikely. It does however demonstrate that primordial and transcendental traditions can co-exist with a universal tradition—in this case the scientific tradition—which is alien to it. (Perhaps it should rather be said that the universal scientific tradition is also a transcendental tradition. That problem is not of the first importance here.)

One interpretation of the combination of Ramanujan's reception of the Hindu tradition without originating any new paths for it, and his extraordinary originality in moving forward from the mathematical tradition which he received, is that the primordial and transcendental traditions permit a very pronounced measure of independence in other categories of intellectual activity. Mathematics was not in conflict with his orthodox Brahminical Hinduism; his mathematical genius could therefore move as freely as that genius permitted within the mathematical tradition that he acquired from books written in the West and from his studies and discussions in Cambridge.

In mathematics at least, the universal validity of knowledge had a counterpart in the capacity of a very strong intelligence to enter very creatively into the scientific tradition—whatever its indigenous primordial and transcendental ancestry and current attachments.

Genius, Centre and Periphery

Genius in science must have its point of departure in tradition. Ramanujan had that point of departure in Carr's *Synopsis* and other textbooks, and in the teaching he received in secondary school, and his brief periods of study at Government College, Kumbakonam, and at Pachaiyappa's College in Madras.

Was it however really necessary for the full play of his genius as a mathematician for him to go to Cambridge? Would he have become the influential mathematician who exerted an influence on the subsequent development of mathematics had he remained in India?

Why did Ramanujan go to England? It was not Anglophilia and a feeling of gratification at being brought into close contact with the country of the rulers of India, although a trace of these might have been present since they were so widespread in the educated classes in India at the time. It was surely not just vanity, although he was undoubtedly flattered by an invitation from a famous British mathematician in a famous British university which was held in great awe in India.

Ordinarily when one discusses the relations between centre and periphery, one thinks of the preoccupation of the periphery with the centre, admiration of the cultural standards and achievements of the centre, etc. I doubt whether these attitudes were very prominent in the mind of Ramanujan. Ramanujan was on the whole indifferent to British literary achievements, to the political importance of Great Britain all over the world, etc., but there might have been a slight tincture of these too in his acceptance of the second invitation to come to Cambridge. His attention and interest, aside from his attachment to his family and to Hinduism, were given only to mathematics. He knew enough mathematics in 1910 to have heard of the distinction of Cambridge in the mathematical part of the English-speaking world. It is, of course, possible that having suffered the slights of failure in India, he regarded an invitation to Great Britain,

and especially to Cambridge, as a vindication in the Indian world. It compensated him for the disregard and for the mortification which he had borne in consequence of his failures in college.

Nevertheless, these considerations were probably secondary to his appreciation of the prospect that in Cambridge he could do mathematics up to the highest levels, where he would be in the presence of mathematicians of his own class from whom he could learn and who could learn from him, who could understand his mathematics and respond to them with critical understanding. It was an opportunity to participate in an ideal world of mathematics.

These were the grounds on which he decided that he should go to Cambridge. Cambridge was the centre of mathematics in the English-speaking world of mathematics. He undoubtedly knew practically nothing of Göttingen and Paris as great mathematical centres. They would have in any case been out of the question, since he did not know German or French. Furthermore, they did not invite him. Cambridge was the place where mathematics was done with the utmost acuity and skill; it was the centre of the greatest mathematical learning. It was the place in the world which to his knowledge had the largest and densest concentration of mathematicians of the highest level of intellectual quality. It was the place where he could do mathematics without distraction and where he would meet with mathematical understanding.

It was probably in that perspective that he viewed the second invitation to Cambridge. He might have thought that he could gain confirmation in Cambridge for his achievement in mathematics. He probably cared only minimally for the prestige which accrued to him in Madras for having been invited to Cambridge. He wanted confirmation for his mathematics; his need for self-esteem required that confirmation. He lived for truth in mathematics and to be recognised as being right. A certain amount of egotism is probably mixed with any desire to discover the truth.

Could Ramanujan have accomplished what he did accomplish had he remained in India? To what extent did his discovery by the centre raise him to heights of achievement which he could not have attained had he not been discovered by Hardy and brought to Cambridge?

Of course, this is a question which cannot be answered with certainty. There is no possibility of comparing a Ramanujan who remained in India with the Ramanujan who went to Cambridge. Nevertheless, it seems unlikely that he would have become what he became, had he remained in India.

Had he remained in India, he would not have remained the indigent amateur mathematician that he had been before his employment in the Madras Port Trust. He had already received Hardy's appreciation and the first invitation to come to Cambridge. Neville had already been very active on his behalf, Sir Francis Spring had already shown interest in him, the University of Madras had already granted him a research fellowship

with tenure of two years. His situation, personal and professional, was already improving. With all that recognition, remaining close to his wife and mother and to the scene he knew and to which he was very attached, having the food which his beliefs and taste demanded, living close to his temples and to the community of his fellow-believers, he would probably have been happier. He might have been happier than he had been, and even more productive mathematically; he might have remained in good health and not lost the half-year in sanatoria and hospitals in England when he could do no mathematics. He would probably have lived longer and therefore might have had an even more voluminous output and a probably undiminished mathematical fertility. He might have formed about him a circle of productive mathematicians.

Yet, on the other side, he would not have had the continuous intellectual pressure created by the friendship and proximity of Hardy and Littlewood and his nearly daily discussions with them. Without these, he would probably have been less knowledgeable about the achievements of continental European mathematics of the past century and more. All these pressed upon him a discipline which his torrential originality resisted and which made his mathematical discoveries take into account the substance and methods of mathematics in Europe. (Professor Littlewood once told me that he had been assigned by Hardy to the task of bringing Ramanujan up to date in the more rigorous methods of European mathematics which had emerged subsequently to the state reached by Ramanujan's studies in India; he said that it was extremely difficult because every time some matter, which it was thought Ramanujan needed to know, was mentioned, Ramanujan's response was an avalanche of original ideas which made it almost impossible for Littlewood to persist in his original intention.) Hardy and Littlewood saved him from wasting his talents on the rediscovery of what was already known. They stimulated him to even more strenuous exertions.

If he had stayed in India, he would have lost these benefits and he certainly would not have become a fellow of Trinity College, Cambridge. Nor would he have become a fellow of the Royal Society; if he had, it would have been much later in his life. These two great distinctions received in acknowledgement of his accomplishments as a mathematician never had any effect on his mathematical work because they came to him when he was ill and, temporarily, relatively unproductive mathematically.

Passionately concentrated on his work in mathematics, Ramanujan seems to have had little vanity. Nevertheless, he was not indifferent to the esteem which was accorded him by Hardy and Littlewood and by the electors of the Royal Society and Trinity College. Had he remained in India, he would not have had any of these and with them the expectations which they embodied of continuing work at the highest level of imagination and rigour.

The fact is that his five years in England, with all his miseries given their full due, was a period which conferred immense benefits on him. He

probably ancitipated some of these benefits in a very general way before he went.

Centre, Periphery and the Universal Validity of Science

The pattern of centre and periphery, which comprises inequality of achievement and status, attests to the universal validity of science. The centre would not be the centre that it is in a particular field of intellectual activities if the periphery did not regard itself as inferior to it in the knowledge possessed by its incumbents and their works.

The universal validity of science postulates consensus in the scientific community. Although the scientific community can be concentrated in a single continent or part of a single continent, as it was in Europe from the sixteenth to the eighteenth centuries, the universal validity of science has no corresponding territorial delimitation. The universal validity of scientific knowledge is not in itself a social phenomenon. The universal validity of scientific knowledge is something assumed by scientists in their scientific activities. It is a phenomenon of the "objective mind", or as Sir Karl Popper renamed it, of "World 3". Scientific knowledge would in principle be valid even if there were no human beings to receive it. Nevertheless, scientific knowledge is addressed by its creators to an audience made up of those capable of receiving it with critical understanding.

The proposition of the universal validity of science is not a proposition about the structure of the scientific community. It is about truth. The truth of a scientific proposition is not at all a statement about the territorial distribution of the proposition or of those who know it. The relationship is rather the opposite of that. The existence of a scientific community, national, regional or world-wide, depends on consensus regarding the universal validity of scientific knowledge. The consensus is not solely about the substance of scientific knowledge; it is also about the criteria of validity or truthfulness. Within that consensus which constitutes it as an intellectual community, there is also consensus about the substance of many propositions. There is also disagreement about the substance of many of the propositions. Within that community, there is also inequality of status of centre and periphery about which there is agreement. That agreement about the inequality of status of members of that community presupposes agreement about the universal validity of scientific knowledge and about the criteria of validity.

When Ramanujan was a youth, when his preoccupation with mathematics was taking form, he did not have a local intellectual community of peers to support him. He had, it is true, his teachers of mathematics who were his superiors only for a very short period. He acquired, a little later, the respect of a number of educated gentlemen who were interested and somewhat instructed in mathematics, and they provided him with interlocutors and a small appreciative audience but they were not at his level. He left them behind whenever they entered into an exchange of ideas.

The persisting value of the notebooks which Ramanujan produced during his years of worry and privation through much of the second and third decades of his life is evidence that those hard years did not extinguish the creative powers of his mind. It is very probable that his mathematical genius would never have been exhausted as long as he lived. So powerful was his genius that he was able, in an environment where there was no one who was remotely on his level, to generate within himself and with very little sustenance or stimulation from others, a vast volume of important mathematical knowledge. This is, in one respect, evidence that a genius once launched on a path, through partial contact with the tradition of his field of intellectual activity, is able to continue with a minimum of the intellectual interchange which is indispensable for the intellectual fertility of persons who lack such extraordinary creative powers.

Yet, even an individual with the unique intellectual character and gifts of Ramanujan benefited from being at the centre. I disregard the stimulating gratification which comes from the deference of his peers, such as is necessary for nearly all intellectuals, regardless of their field of activity. What Ramanujan seems to have obtained in Cambridge was an intense intellectual conviviality which raised him to a higher level of creative power. I do not mean the *bonhomie* and friendship which is an important element of intellectual conviviality. Ramanujan never lunched or dined in Hall; he never went to Feasts. As far as I know, he did not go to the Combination Room; he did not go to wine after dinner. That kind of conviviality he never had with Hardy, Littlewood or Neville. He did have a great deal of intellectual conviviality, a great deal of very intense interchange about mathematical problems with equals. That is what Ramanujan received from his years at Cambridge. These years were also years of deprivation—separation from his wife and mother, unfamiliar street scenes and landscapes, physical discomfort in a chilly climate, garments which were uncomfortable, a poor diet which was uncongenial both to his taste and his religious prohibitions, remoteness from his temple and the absence of Hindu fellow-devotees, painful and ultimately fatal illness—all of these contributed to his unhappiness in Cambridge. Despite these handicaps, the genuine but reserved friendship, and especially the appreciation for his intellectual achievements which he received from Hardy and Littlewood, undoubtedly compensated him to some extent for what he lost by being out of India. The presence of a small number of persons who were his mathematical peers and the intellectual conviviality in which that presence took form were what was important to him. It was indeed so important that for a time he seemed to be ready to consider spending much of the rest of his life out of India.

The result was a heightening and refinement of his mathematical powers. It was during that short period that Ramanujan reached the peak of his creative accomplishments. The centre was a centre even for one

with such irrepressible powers of mind, a mind the intellectual ardour of which could never be dampened even by the worst of circumstances. For Ramanujan, the centre was a reality. It was a place where he was intellectually at home.