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History of Science, Philosophy and Culture in Indian Civilization

General Editor D. P. Chattopadhyaya

Volume X Part 4

Cultural Foundations of Mathematics

The Nature of Mathematical Proof and the Transmission of the Calculus from India to Europe in the 16th c. CE

C. K. RAJU

PHISPC

Centre for Studies in Civilizations

Cultural Foundations of Mathematics

Publications in PHISPC–CONSSAVY Series

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Cultural Foundations of Mathematics: The Nature of Mathematical Proof and the Transmission of the Calculus from India to Europe in the 16th c. CE

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II The Calculus in India

were then lacking in Europe. This provided huge motivation for transmission to Europe of precise Indian trigonometric values, and through them the infinite series and the calculus. Coincidentally, the first Roman Catholic mission in India was founded in Cochin, in 1500, and later turned into a college for the indigenous Syrian Christians, in the neighbourhood, who spoke Malayalam. The Raja of Cochin simultaneously patronized both Portuguese and the authors of key texts documenting expositions of the Indian infinite series used to derive accurate trigonometric values. This provided a splendid opportunity for the Jesuits, who systematically gathered knowledge by applying the Toledo model of mass translation to Cochin, soon after they took over the Cochin college in 1550 CE. Apart from the local languages, the Jesuits were soon trained also in practical mathematics and astronomy. Also, sailors and travellers returning from India routinely brought back books, as souvenirs or to be sold to collectors in Europe. From the mid-16th c. CE onwards, circumstantial evidence of the knowledge of Indian mathematical and astronomical works begins to appear in the works of Mercator, Clavius, Julius Scaliger, Tycho Brahe, de Nobili, Kepler, Cavalieri, Fermat, Pascal, etc. Indian sources were rarely directly acknowledged by these Europeans due to the terror of acknowledging "pagan" sources during the Inquisition, and the church doctrine of Christian Discovery, which preceded racism. (This is in striking contrast to the Arabs in the 9th c. CE who had enough religious freedom to acknowledge Indian sources.) The prolonged difficulties that Europeans had in understanding the epistemological basis of the calculus further characterizes the calculus as knowledge imported into Europe like the algorismus.

Berkeley's objections re ect the doubts about the nature of uxions, infinitesimals etc., which neither Newton, nor Leibniz, nor their supporters could coherently explain to sceptical contemporaries. These doubts led eventually to the formalisation of "real" numbers using Dedekind cuts and set theory (itself formalised only in the 1930's), which finally gave a formulation of the calculus acceptable in the West. These prolonged European difficulties with the calculus arose because the Indian derivation of the infinite series used a philosophy of non-representables similar to *śūnyavāda*, and incompatible with Platonic idealism or formalism—thoughtlessly taken as the "universal" basis of mathematics in Europe. The central problem of representation was left unresolved by the formalisation of real numbers, which achieved nothing of any practical value. A similar problem had arisen earlier in Europe, in the dispute between abacus and algorismus, which involved zeroing of non-representables in a calculation. The *śūnyavāda* philosophy regards idealistic conceptualizations (as in Platonism or formalism) as empty and erroneous (e.g., in direct opposition to Platonism it regards an ideal geometrical point as an erroneous representation of a real dot). It is also better suited than Platonic idealism or formalism to numbers on a computer which make the representation problem explicit, for both integers and real numbers.

IV The Contemporary Relevance of the Revised History

European historical difficulties with Indian mathematics and present-day learning difficulties in mathematics

Using the principle that phylogeny is ontogeny, the historical European difficulties in understanding the algorismus and the calculus are here related to difficulties that students today have in understanding elementary mathematics. Historically, both algorismus and calculus greatly enhanced the ability to calculate, but only in a way regarded as epistemologically insecure in Europe for periods extending to several centuries. Since, in fact, the formalist epistemology of mathematics is too complex to be taught at the elementary level, the same situation persists in "fast forward" mode today in the classroom. This epistemic divide has been exacerbated by computers which have again greatly enhanced the ability to calculate, albeit in a way regarded as epistemologically insecure. In view of the preceding considerations, it is proposed to accept mathematics-as-calculation as epistemically secure, and to teach mathematics for its practical value, along with the related notion of number, despite Plato and assorted footnotes to him.

Difficulties with the continuum approach to the calculus and an example of how advanced formal mathematics needs empirical inputs

The belief that the calculus found a final and satisfactory solution with the formalisation of real numbers is not valid. The formalisation of real numbers only side-stepped the central problem of representation, which persists even in to the present-day formal mathematical extensions of the calculus in the Schwartz theory of distributions. The differences between the two philosophies of mathematics—(a) formalism vs (b) *śūnyavāda* [empiricism + acceptance of non-representability]—though subtle, are here demonstrated to have practical applications also to areas other than computing and math education, particularly to physics and engineering. Thus, the alternative philosophy of mathematics is here related to suggested improvements in (a) the current renormalization procedure used to tackle the problem of infinities in quantum field theory, to allow use of any polynomial Lagrangian, and (b) the theory of shock waves, to make it more accurate in real uids like air, water etc. The suggested improvements, however, require empirical inputs to finalize the mathematical derivation. Thus, the other key idea, like that of Śrīharṣa, is to bring out the limitations of formal mathematics also from within formal mathematics—namely, to demonstrate that formal mathematics, without empirical inputs, quickly reaches a sterile end.

General Editor and Author

D. P. CHATTOPADHYAYA, M.A., LL.B., Ph.D. (Calcutta and London School of Economics), D. Litt. (*Honoris Causa*), researched, studied Law, Philosophy and History and taught at various universities in India, Asia, Europe and USA from 1954 to 1994. Former Chairman of the Indian Council of Philosophical Research (1981–1990) and Former Chairman of the Indian Institute of Advanced Study, Shimla (1984–1991), Chattopadhyaya is currently the Project Director of the multidisciplinary 96–volume Project of History of Science, Philosophy and Culture in Indian Civilizations (PHISPC) and Chairman of the Centre for Studies in Civilizations (CSC). Among his 35 publications, authored 18 and edited 17, are *Individuals and Societies* (1967), *Individuals and Worlds* (1976), *Sri Aurobindo and Karl Marx* (1988), *Anthropology and Historiography of Science* (1990), *Induction, Probability and Skepticism* (1991), *Sociology, Ideology and Utopia* (1997), *Societies, Cultures and Ideologies* (2000), *Interdisciplinary Studies in Science, Society, Value and Civilizational Dialogue* (2002) and *Philosophy of Science, Phenomenology and Other Essays* (2003). Besides, he has also held high public offices, namely, of Union Cabinet Minister and State Governor.

C. K. RAJU, Ph.D. (Indian Statistical Institute) taught in both mathematics and statistics departments of the University of Poona for several years (1981–88), before joining C-DAC (Centre for Development of Advanced Computing) to play a key role in building the first Indian national supercomputer Param (1988–95). He has proposed a new mathematical basis for physics (using functional differential equations or a "tilt" in the arrow of time), leading to a new understanding of quantum mechanics, in *Time: Towards a Consistent Theory* (Kluwer Academic, 1994; Fundamental Theories of Physics, vol. 65). In *The Eleven Pictures of Time* (Sage, 2003) he examined time as the critical link between science and various religions, in the context of the "clash of civilizations", and formulated a new ethical principle. He has been a Fellow of the Indian Institute of Advanced Study (1991–93), the National Institute for Science Technology and Development Studies (1995–98), and an Affiliated Fellow of the Nehru Memorial Museum and Library, where he coordinated a project of the Indian National Science Academy (1998–2001) on the historical origin of the calculus. He has been on the editorial board of the *Journal of Indian Council of Philosophical Research*. For the past several years (since 2000) he worked as a Professor of Computer Science, has also taught computer courses on television, and has authored texts, and software for educational and industrial use. He has been associated with the PHISPC since its early days, and has contributed many articles to PHISPC volumes since 1991.

General Introduction

I

It is understandable that man, shaped by Nature, would like to know Nature. The human ways of knowing Nature are evidently diverse, theoretical and practical, scientific and technological, artistic and spiritual. This diversity has, on scrutiny, been found to be neither exhaustive nor exclusive. The complexity of physical nature, life-world and, particularly, human mind is so enormous that it is futile to follow a single method for comprehending all the aspects of the world in which we are situated.

One need not feel bewildered by the variety and complexity of the worldly phenomena. After all, both from traditional wisdom and our daily experience, we know that our own nature is not quite alien to the structure of the world. Positively speaking, the elements and forces that are out there in the world are also present in our body-mind complex, enabling us to adjust ourselves to our environment. Not only the natural conditions but also the social conditions of life have instructive similarities between them. This is not to underrate in any way the difference between the human ways of life all over the world. It is partly due to the variation in climatic conditions and partly due to the distinctness of production-related tradition, history and culture.

Three broad approaches are discernible in the works on historiography of civilization, comprising science and technology, art and architecture, social sciences and institutions. Firstly, some writers are primarily interested in discovering the general laws which govern all civilizations spread over different continents. They tend to underplay what they call the noisy local events of the external world and peculiarities of different languages, literatures and histories. Their accent is on the unity of Nature, the unity of science and the unity of mankind. The second group of writers, unlike the generalist or transcendentalist ones, attach primary importance to the distinctiveness of every culture. To these writers human freedom and creativity are extremely important and basic in character. Social institutions and the cultural articulations of human consciousness, they argue, are bound to be expressive of the concerned people's consciousness. By implication they tend to reject concepts like archetypal consciousness, universal mind and providential history. There is a third group of writers who offer a composite picture of civilizations, drawing elements both from their local as well as common characteristics. Every culture has its local roots and peculiarities. At the same time, it is pointed out that due to demographic migration and immigration over the centuries an element of compositeness emerges almost in every culture. When, due to a natural calamity or political exigencies people move from one part of the world to another, they carry with them, among other things, their language, cultural inheritance and their ways of living.

In the light of the above facts, it is not at all surprising that comparative anthropologists and philologists are intrigued by the striking similarity between different language families and the rites, rituals and myths of different peoples. Speculative philosophers of history, heavily relying on the findings of epigraphy, ethnography, archaeology and theology, try to show in very general terms that the particulars and universals of culture are essentially' or secretly' interrelated. The spiritual aspects of culture like dance and music, beliefs pertaining to life, death and duties, on analysis, are found to be mediated by the material forms of life like weather forecasting, food production, urbanization and invention of script. The transition from the oral culture to the written one was made possible because of the mastery of symbols and rules of measurement. Speech precedes grammar, poetry prosody. All these show how the matters' and forms' of life are so subtly interwoven.

II

The PHISPC publications on History of Science, Philosophy and Culture in Indian Civilization, in spite of their unitary look, do recognize the differences between the areas of material civilization and those of ideational culture. It is not a work of a single author. Nor is it being executed by a group of thinkers and writers who are methodologically uniform or ideologically identical in their commitments. In conceiving the Project we have interacted with, and been in uenced by, the writings and views of many Indian and non-Indian thinkers.

The attempted unity of this Project lies in its aim and inspiration. We have in India many scholarly works written by Indians on different aspects of our civilization and culture. Right from the pre-Christian era to our own time, India has drawn the attention of various countries of Asia, Europe and Africa. Some of these writings are objective and informative and many others are based on insufficient information and hearsay, and therefore not quite reliable, but they have their own value. Quality and view-points keep on changing not only because of the adequacy and inadequacy of evidence but also, and perhaps more so, because of the bias and prejudice, religious and political conviction, of the writers.

Besides, it is to be remembered that history, like Nature, is not an open book to be read alike by all. The past is mainly enclosed and only partially disclosed. History is, therefore, partly objective or real' and largely a matter of construction. This is one of the reasons why some historians themselves think that it is a form of literature or art. However, it does not mean that historical construction is anarchic' and arbitrary. Certainly, imagination plays an important role in it.

But its character is basically dependent upon the questions which the historian raises and wants to understand or answer in terms of the ideas and actions of human beings in the past ages. In a way, history, somewhat like the natural sciences, is engaged in answering questions and in exploring relationships of cause and effect between events and developments across time. While in the natural sciences, the scientist poses questions about nature in the form of hypotheses, expecting to elicit authoritative answers to such questions, the historian studies the past, partly for the sake of understanding it for its own sake and partly also for the light which the past throws upon the present, and the possibilities which it opens up for moulding the future. But the difference between the two approaches must not be lost sight of. The scientist is primarily interested in discovering laws and framing theories, in terms of which, different events and processes can be connected and anticipated. His interest in the conditions or circumstances attending the concerned events is secondary. Therefore, scientific laws turn out to be basically abstract and easily expressible in terms of mathematical language. In contrast, the historian's main interest centres round the specific events, human ideas and actions, not general laws. So, the historian, unlike the scientist, is obliged to pay primary attention to the circumstances of the events he wants to study. Consequently, history, like most other humanistic disciplines, is concrete and particularist. This is not to deny the obvious truth that historical events and processes consisting of human ideas and actions show some trend or other and weave some pattern or other. If these trends and patterns were not there at all in history, the study of history as a branch of knowledge would not have been profitable or instructive. But one must recognize that historical trends and patterns, unlike scientific laws and theories, are not general or purported to be universal in their scope.

III

The aim of this Project is to discover the main aspects of Indian culture and present them in an interrelated way. Since our culture has in uenced, and has been in uenced by, the neighbouring cultures of West Asia, Central Asia, East Asia and South-East Asia, attempts have been made here to trace and study these in uences in their mutuality. It is well known that during the last three centuries, European presence in India, both political and cultural, has been very widespread. In many volumes of the Project considerable attention has been paid to Europe and through Europe to other parts of the world. For the purpose of a comprehensive cultural study of India, the existing political boundaries of the South Asia of today are more of a hindrance than help. Cultures, like languages, often transcend the bounds of changing political territories.

If the inconstant political geography is not a reliable help to the understanding of the layered structure and spread of culture, a somewhat comparable problem is encountered in the area of historical periodization. Periodization or segmenting time is a very tricky affair. When exactly one period ends and another begins is not precisely ascertainable. The periods of history designated as ancient, medieval and modern are purely conventional and merely heuristic in character. The varying scopes of history, local, national and continental or universal, somewhat like the periods of history, are unavoidably fuzzy and shifting. Amidst all these difficulties, the volume-wise details have been planned and worked out by the editors in consultation with the Project Director and the General Editor. I believe that the editors of different volumes have also profited from the reactions and suggestions of the contributors of individual chapters in planning the volumes.

Another aspect of Indian history which the volume-editors and contributors of the Project have carefully dealt with is the distinction and relation between civilization and culture. The material conditions which substantially shaped Indian civilization have been discussed in detail. From agriculture and industry to metallurgy and technology, from physics and chemical practices to the life sciences and different systems of medicines—all the branches of knowledge and skill which directly affect human life-form the heart of this Project. Since the periods covered by the PHISPC are extensive—prehistory, proto-history, early history, medieval history and modern history of India—we do not claim to have gone into all the relevant material conditions of human life. We had to be selective. Therefore, one should not be surprised if one finds that only some material aspects of Indian civilization have received our pointed attention, while the rest have been dealt with in principle or only alluded to.

One of the main aims of the Project has been to spell out the first principles of the philosophy of different schools, both pro-Vedic and anti-Vedic. The basic ideas of Buddhism, Jainism and Islam have been given their due importance. The special position accorded to philosophy is to be understood partly in terms of its proclaimed unifying character and partly to be explained in terms of the fact that different philosophical systems represent alternative world-views, cultural perspectives, their con ict and mutual assimilation.

Most of the volume-editors and at their instance the concerned contributors have followed a middle path between the extremes of narrativism and theoreticism. The underlying idea has been this: if in the process of working out a comprehensive Project like this every contributor attempts to narrate all those interesting things that he has in the back of his mind, the enterprise is likely to prove unmanageable. If, on the other hand, particular details are consciously forced into a fixed mould or pre-supposed theoretical structure, the details lose their particularity and interesting character. Therefore, depending on the nature of the problem of discourse, most of the writers have tried to reconcile in their presentation, the specificity of narrativism and the generality of theoretical orientation. This is a conscious editorial decision. Because, in the absence of a theory, however inarticulate it may be, the factual details tend to fall apart. Spiritual network or theoretical orientation makes historical details not only meaningful but also interesting and enjoyable.

Another editorial decision which deserves spelling out is the necessity or avoidability of duplication of the same theme in different volumes or even in the same volume. Certainly, this Project is not an assortment of several volumes. Nor is any volume intended to be a miscellany. This Project has been designed with a definite end in view and has a structure of its own. The character of the structure has admittedly been in uenced by the variety of the themes accommodated within it. Again it must be understood that the complexity of structure is rooted in the aimed integrality of the Project itself.

IV

Long and in-depth editorial discussion has led us to several unanimous conclusions. Firstly, our Project is going to be unique, unrivalled and discursive in its attempt to integrate different forms of science, technology, philosophy and culture. Its comprehensive scope, continuous character and accent on culture distinguish it from the works of such Indian authors as P. C. Ray, B. N. Seal, Binoy Kumar Sarkar and S. N. Sen and also from such Euro-American writers as Lynn Thorndike, George Sarton and Joseph Needham. Indeed, it would be no exaggeration to suggest that it is for the first time that an endeavour of so comprehensive a character, in its exploration of the social, philosophical and cultural characteristics of a distinctive world civilization-that of India-has been attempted in the domain of scholarship.

Secondly, we try to show the linkages between different branches of learning as different modes of experience in an organic manner and without resorting to a kind of reductionism, materialistic or spiritualistic. The internal dialectics of organicism without reductionism allows fuzziness, discontinuity and discreteness within limits.

Thirdly, positively speaking, different modes of human experience-scientific, artistic, etc., have their own individuality, not necessarily autonomy. Since all these modes are modification and articulation of human experience, these are bound to have between them some finely graded commonness. At the same time, it has been recognized that re ection on different areas of experience and investigation brings to light new insights and findings. Growth of knowledge requires humans, in general, and scholars, in particular, to identify the distinctness of different branches of learning.

Fourthly, to follow simultaneously the twin principles of: (a) individuality of human experience as a whole, and (b) individuality of diverse disciplines, are not at all an easy task. Overlap of themes and duplication of the terms of discourse become unavoidable at times. For example, in the context of *Dharmaśāstra*, the writer is bound to discuss the concept of value. The same concept also figures in economic discourse and also occurs in a discussion on fine arts. The conscious editorial decision has been that, while duplication should be kept to its minimum, for the sake of intended clarity of the themes under discussion, their reiteration must not be avoided at high intellectual cost.

Fifthly, the scholars working on the Project are drawn from widely different disciplines. They have brought to our notice an important fact that has clear relevance to our work. Many of our contemporary disciplines like economics and sociology did not exist, at least not in their present form, just two centuries ago or so. For example, before the middle of nineteenth century, sociology as a distinct branch of knowledge was unknown. The term is said to have been coined first by the French philosopher Auguste Comte in 1838. Obviously, this does not mean that the issues discussed in sociology were not there. Similarly, Adam Smith's (1723–90) famous work *The Wealth of Nations* is often referred to as the first authoritative statement of the principles of (what we now call) economics. Interestingly enough, the author was equally interested in ethics and jurisprudence. It is clear from history that the nature and scope of different disciplines undergo change, at times very radically, over time. For example, in India *arthaśāstra*' does not mean the science of economics as understood today. Besides the principles of economics, the *arthaśāstra* of ancient India discusses at length those of governance, diplomacy and military science.

Sixthly, this brings us to the next editorial policy followed in the Project. We have tried to remain very conscious of what may be called indeterminacy or inexactness of translation. When a word or expression of one language is translated into another, some loss of meaning or exactitude seems to be unavoidable. This is true not only in the bilingual relations like Sanskrit-English and Sanskrit-Arabic, but also in those of Hindi-Tamil and Hindi-Bengali. In recognition of the importance of language-bound and context-relative character of meaning we have solicited from many learned scholars, contributions, written in vernacular languages. In order to minimize the miseffect of semantic inexactitude we have solicited translational help of that type of bilingual scholars who know both English and the concerned vernacular language, Hindi, Tamil, Telugu, Bengali or Marathi.

Seventhly and finally, perhaps the place of technology as a branch of knowledge in the composite universe of science and art merits some elucidation. Technology has been conceived in very many ways, e.g., as autonomous, as standing reserve', as liberating or enlargemental, and alienative or estrangemental force. The studies undertaken by the Project show that, in spite of its much emphasized mechanical and alienative characteristics, technology embodies a very useful mode of knowledge that is peculiar to man. The Greek root words of technology are *techne* (art) and *logos* (science). This is the basic justification of recognizing technology as closely related to both epistemology, the discipline of valid knowledge, and axiology, the discipline of freedom and values. It is in this context that we are reminded of the definition of man as *homo technikos*. In Sanskrit, the word closest to *techne* is *kalā* which means any practical art, any mechanical or fine art. In the Indian tradition, in *Śaivatantra*, for example, among the arts (*kalā*) are counted dance, drama, music, architecture, metallurgy, knowledge of dictionary, encyclopaedia and prosody. The closeness of the relation between arts and sciences, technology and other forms of knowledge are evident from these examples and was known to the ancient people. The human quest for knowledge involves the use of both head and hand. Without mind, the body is a corpse and the disembodied mind is a bare abstraction. Even for our appreciation of what is beautiful and the creation of what is valuable, we are required to exercise both our intellectual competence and physical capacity. In a manner of speaking, one might rightly affirm that our psychosomatic structure is a functional connector between what we are and what we could be, between the physical and the beyond. To suppose that there is a clear-cut distinction between the physical world and the psychosomatic one amounts to denial of the possible emergence of higher logico-mathematical, musical and other capacities. The very availability of aesthetic experience and creation proves that the supposed distinction is somehow overcome by what may be called the bodily self or embodied mind.

V

The ways of classification of arts and sciences are neither universal nor permanent. In the Indian tradition, in the *Ṛgveda*, for example, *vidyā* (or sciences) are said to be four in number: (i) *Trayī*, the triple Veda; (ii) *Ānvikṣikī*, logic and metaphysics; (iii) *Daṇḍanīti*, science of governance; (iv) *Vārtta*, practical arts such as agriculture, commerce, medicine, etc. Manu speaks of a fifth *vidyā* viz., *Ātma-vidyā*, knowledge of self or of spiritual truth. According to many others, *vidyā* has fourteen divisions, viz., the four Vedas, the six Vedāṅgas, the Purāṇas, the Mīmāṁsā, Nyāya, and Dharma or law. At times, the four *Upavedas* are also recognized by some as *vidyā*. *Kalā*s are said to be 33 or even 64.

In the classical tradition of India, the word *śāstra* has at times been used as a synonym of *vidyā*. *Vidyā* denotes instrument of teaching, manual or compendium of rules, religious or scientific treatise. The word *śāstra* is usually found after the word referring to the subject of the book, e.g., *Dharma-śāstra*, *Artha-śāstra*, *Alaṁkāra-śāstra* and *Mokṣa-śāstra*. Two other words which have been frequently used to denote different branches of knowledge are *jñāna* and *vijñāna*. While *jñāna* means knowing, knowledge, especially the higher form of it, *vijñāna* stands for the act of distinguishing or discerning, understanding, comprehending and recognizing. It means worldly or profane knowledge as distinguished from *jñāna*, knowledge of the divine.

It must be said here that the division of knowledge is partly conventional and partly administrative or practical. It keeps on changing from culture to culture, from age to age. It is difficult to claim that the distinction between *jñāna* and *vijñāna* or that between science and art is universal. It is true that even before the advent of modern age, both in the East and the West, two basic aspects of science started gaining recognition. One is the specialized character of what we call scientific knowledge. The other is the concept of trained skill which was brought close to scientific knowledge. In the medieval Europe, the expression the seven liberal sciences' has very often been used simultaneously with the seven liberal arts', meaning thereby, the group of studies by the *Trivium* (Grammar, Logic and Rhetoric) and *Quadrivium* (Arithmetic, Music, Geometry and Astronomy).

It may be observed here, as has already been alluded to earlier, that the division between different branches of knowledge, between theory and practice, was not pushed to an extreme extent in the early ages. *Praxis*, for example, was recognized as the prime *techne*. The Greek word, *technologia* stood for systematic treatment, for example, of Grammar. *Praxis* is not the mere application of *theoria*, unified vision or integral outlook, but it also stands for the active impetus and base of knowledge. In India, one often uses the terms *Prayukti-vidyā* and *Prayodyogika-vidyā* to emphasize the practical or applicative character of knowledge. *Prayoga* or application is both the test and base of knowledge. Doing is the best way of knowing and learning.

That one and the same word may mean different things' or concepts in different cultures and thus create confusion has already been stated before. Two such words which in the context of this Project under discussion deserve special mention are *dharma* and *itihāsa*. Ordinarily, *dharma* in Sanskrit-rooted languages is taken to be conceptual equivalent of the English word *religion*. But, while the meaning of religion is primarily theological, that of *dharma* seems to be manifold. Literally, *dharma* stands for that which is established or that which holds people steadfastly together. Its other meanings are law, rule, usage, practice, custom, ordinance and statute. Spiritual or moral merit, virtue, righteousness and good works are also denoted by it. Further, *dharma* stands for natural qualities like burning (of fire), liquidity (of water) and fragility (of glass). Thus one finds that meanings of *dharma* are of many types—legal, social, moral, religious or spiritual, and even ontological or physical. All these meanings of *dharma* have received due attention of the writers in the relevant contexts of different volumes.

This Project, being primarily historical as it is, has naturally paid serious attention to the different concepts of history-epic-mythic, artistic-narrative, scientific-causal, theoretical and ideological. Perhaps the point that must be mentioned first about history is that it is not a correct translation of the Sanskrit word *itihāsa*. Etymologically, it means what really happened (*iti-ha-āsa*). But, as we know, in the Indian tradition *purāṇa* (legend, myth, tale, etc.), *gāthā* (ballad), *itivṛtta* (description of past occurrence, event, etc.), *ākhyāyikā* (short narrative) and *vaṁśa-carita* (genealogy) have been consciously accorded a very important place. Things started changing with the passage of time and particularly after the effective presence of Islamic culture in India. Islamic historians, because of their own cultural moorings and the in uence of the Semitic and Graeco-Roman cultures on them, were more particular about their facts, figures and dates than their Indian predecessors. Their aim to bring history close to statecraft, social conditions and the lives and teachings of the religious leaders imparted a mundane character to this branch of learning. The Europeans whose political appearance on the Indian scene became quite perceptible only towards the end of the eighteenth century brought in with them their own view of historiography in their cultural baggage. The impact of the Newtonian Revolution in the field of history was very faithfully worked out, among others, by David Hume (1711–76) in *History of Great Britain from the Invasion of Julius Caesar to the Revolution of 1688* (6 Vols., 1754–62) and Edward Gibbon (1737–94) in

The History of the Decline and Fall of the Roman Empire (6 Vols., 1776–88). Their emphasis on the principles of causality, datability and continuity/linearity of historical events introduced the spirit of scientific revolution in European historiography. The introduction of English education in India and the exposure of the elites of the country to it largely account for the decline of the traditional concept of *itihāsa* and the rise of the post-Newtonian scientific historiography. Gradually, Indian writers of our own history and cultural heritage started using more and more European concepts and categories. This is not to suggest that the impact of the European historiography on Indian historians was entirely negative. On the contrary, it imparted an analytical and critical temper which motivated many Indian historians of the nineteenth century to try to discover and represent our heritage in a new way.

VI

The principles which have been followed for organizing the subjects of different volumes under this Project may be stated in this way. We have kept in view the main structures which are discernible in the decomposible composition of the world. The first structure may be described as physical and chemical. The second structure consists, broadly speaking, of biology, psychology and epistemology. The highest and the most abstract structure nests many substructures within it, for example, logic, mathematics and musical notes. It is well known that the substructures within each structure are interactive, i.e., not isolable. The more important point to be noted in this connection is that the basic three structures of the world, viz., (a) physico-chemical, (b) bio-psychological, and (c) logico-mathematical are all simultaneously open to upward and downward causation. In other words, while the physico-chemical structure can causally in uence the bio-psychological one and the latter can causally in uence the most abstract logico-mathematical, the reverse process of causation is also operative in the world. In spite of its relative abstractness and durability, the logico-mathematical world has its downward causal impact on our bio-psychological and epistemological processes and products. And the latter can also bring about change in the structures of the physical world and its chemical composition. Applied physics and bio-technology make the last point abundantly clear.

Many philosophers, life-scientists, and social scientists highlight the point that nature loves hierarchies. Herbert Simon, the economist and the management scientist, speaks of four steps of partial ordering of our world, namely, (i) chemical substances, (ii) living organisms, tissues and organs, (iii) genes, chromosomes and DNA, and (iv) human beings, the social organizations, programmes and information process. All these views are in accord with the anti-reductionist character of our Project. Many biologists defend this approach by pointing out that certain characteristics of biological phenomena and process like unpredictability, randomness, uniqueness, magnitude of stochastic perturbations, complexity and emergence cannot be reduced without recourse to physical laws.

The main subjects dealt with in different volumes of the Project are connected not only conceptually and synchronically but also historically or diachronically. For pressing practical reasons, however, we did not aim at presenting the prehistorical, proto-historical and historical past of India in a continuous or chronological manner. Besides, it has been shown in the presentation of the PHISPC that the process of history is non-linear. And this process is to be understood in terms of human praxis and an absence of general laws in history. Another point which deserves special mention is that the editorial advisors have taken a conscious decision not to make this historical Project primarily political. We felt that this area of history has always been receiving extensive attention. Therefore, the customary discussion of dynastic rule and succession will not be found in a prominent way in this series. Instead, as said before, most of the available space has been given to social, scientific, philosophical and other cultural aspects of Indian civilization.

Having stated this, it must be admitted that our departure from conventional style of writing Indian history is not total. We have followed an inarticulate framework of time in organizing and presenting the results of our studies. The first volume, together with its parts, deals with the prehistorical period to A.D. 300. The next two volumes, together with their parts, deal with, among other things, the development of social and political institutions and philosophical and scientific ideas from A.D. 300 to the beginning of the eleventh century A.D.

The next period with which this Project is concerned spans from the twelfth century to the early part of the eighteenth century. The last three centuries constitute the fourth period covered by this Project. But, as said before, the definition of all these periods by their very nature are inexact and merely indicative.

Two other points must be mentioned before I conclude this General Introduction to the series. The history of some of the subjects like religion, language and literature, philosophy, science and technology cannot for obvious reason be squeezed within the cramped space of the periodic moulds. Attempts to do so result in thematic distortion. Therefore, the reader will often see the over ow of some ideas from one period to another. I have already drawn attention to this tricky and fuzzy and also the misleading aspects of the periodization of history, if pressed beyond a point.

Secondly, strictly speaking, history knows no end. Every age rewrites its history. Every generation, beset with new issues, problems and questions, looks back to its history and reinterprets and renews its past. This shows why history is not only contemporaneous but also futural. Human life actually knows no separative wall between its past, present and future. Its cognitive enterprises, moral endeavours and practical activities are informed of the past, oriented by the present and addressed to the future. This process persists, consciously or unconsciously, wittingly or unwittingly. In the narrative of this Project, we have tried to represent this complex and fascinating story of Indian civilization.

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D. P. Chattopadhyaya
General Editor

Preface

According to a widespread stereotype, history is of two kinds—"mainstream" Western history and assorted chauvinistic accounts. From an Indian perspective, the choice is wider: for it is easier to see that recent chauvinistic Indian history is profoundly imitative of chauvinistic Western history! This parallelism is readily explained since both attempts to manipulate history arise from the same cause: the use of religion as an instrument to attain and retain state power. However, historians from across the political spectrum have unfortunately failed to notice this parallelism earlier, and the current account of the history of science continues to be regarded as broadly representative of the truth.

The received account, of course, makes science entirely a domestic Western affair, starting from the "Greeks" and developing during the European renaissance. Therefore, it is hardly possible today to write a meaningful history of Indian science without contending with the received account and the stereotypes which reinforce it by suggesting derogatory labels for dissenting accounts.

A further obstacle is the way the philosophy of science reinforces the received history. As I have earlier remarked, science means never having to say you are sure: certitudes of any sort are the hallmark of religious belief. However, science is often demarcated using the criterion of falsifiability which supposes (as does most Western philosophy) that deduction is certain while induction is not. This belief in the certainty of deduction is the anchor also of the present-day formalist philosophy of mathematics which equates mathematics with deductive proof—hoping to make mathematics the currency of certainty. This certitude, one naturally suspects, is interlaced with theology.

To bring out the theological underpinnings of present-day formalist mathematics—or the theological origins of the art of theorem-proving—it is necessary, first, to trace the historical development of formalism from Platonism to Neoplatonism via Islamic rational theology to Christian rational theology to the present-day. Secondly, the theological moorings of formalist beliefs about logic and number come into sharper focus when we confront formalism with Buddhist and Jain logic on the one hand, and the *śūnyavāda* philosophy of non-representables and computer technology on the other. Finally, it is helpful to demonstrate the practical advantages of the revised philosophy of mathematics in various contemporary contexts ranging from mathematics education to computer technology and quantum field theory.

This book brings together these diverse but interconnected streams of thought that I have articulated in various papers and talks over the past decade.

1. Śūnya and non-representable numbers

   "The Mathematical Epistemology of Śūnya," Invited paper, summarizing interventions during the *Seminar on the Concept of Śūnya*, INSA and IGNCA, New Delhi, Feb 1997. In: *The Concept of Śūnya*, ed. A. K. Bag and S. R. Sarma, IGNCA, INSA and Aryan Books International, New Delhi, 2002, pp. 168–181.

   "Śūnya: from Zero to Java. Number Representations in Algorismus, Formal Mathematics, and Computers". Invited talk delivered at Haldwani, 8 October 2002.

2. Models of information transmission

   "India's Interactions with China, Central and West Asia, in Mathematics and Astronomy," in : A. Rahman, ed., *Interactions between India, Western and Central Asia, and China*, PHISPC, Oxford University Press, New Delhi, [1998] 2002, pp. 227–254.

3. Navigational instruments and precise angle measurements

   "Kamāl or Rāpalagāi" [A Medieval Navigational Instrument and its Relation to Mādhava's Sine Series] paper presented at the *Ninth Indo-Portuguese Seminar on History*, INSA, New Delhi, Dec 1998. In: *Indo-Portuguese Encounters: Journeys in Science, Technology and Culture*, ed. Lotika Varadarajan, Indian National Science Academy, New Delhi, and Universidade Nova de Lisboa, Lisbon, 2006, vol. 2, pp. 483–504.

4. Approximation, error, and proof in the *Yuktibhāṣā* derivation of infinite series

   "Approximation and Proof in the *Yuktibhāṣā* Derivation of Mādhava's Sine Series", paper presented at the *National Conference on Applied Sciences in Sanskrit*, Agra, Feb 1999. In: Proc., B. R. Ambedkar University, Agra.

5. The history and philosophy of the *Elements*

   "How Should Euclidean' Geometry be Taught", paper presented at the International Workshop on *History of Science, Implications for Science Education*, Homi

Bhabha Centre, TIFR, Bombay, Feb, 1999. In Nagarjuna G., ed., *History and Philosophy of Science: Implications for Science Education*, Homi Bhabha Centre, Bombay, 2001, pp. 241–260.

6. Mathematics as social construction

"Mathematics and Culture", in *History, Culture and Truth: Essays Presented to D. P. Chattopadhyaya*, ed. Daya Krishna and K. Satchidananda Murthy, Kalki Prakash, New Delhi, 1999, pp. 179–193. Reprinted in *Philosophy of Mathematics Education* **11** (1999). Available online at http://www.people.ex.ac.uk/PErnest/pome11/art18.htm.

(Book review) *Social Constructivism as a Philosophy of Mathematics* (Paul Ernest), State University of New York, in: *Journal of Indian Council of Philosophical Research*, **18** (1) 2001, pp. 267–270.

"The Religious Roots of Mathematics", *Theory, Culture & Society* **23**(1–2) Jan-March 2006, pp. 95–97. Spl. Issue on *Problematizing Global Knowledge*, ed. Mike Featherstone, Couze Venn, Ryan Bishop, and John Phillip. Also, "The Religious Roots of Western Mathematics", invited talk at JNU seminar on "Science and Spirituality", IIC, Feb 2006 (to appear) in Proc.

7. Time and logic in Buddhism, Jainism, and quantum mechanics

"Quantum Mechanical Time", *Physics Education* **10** (2), 1993, pp. 143–61.

More details on the structured-time interpretation of quantum mechanics in chp. 6b in *Time: Towards a Consistent Theory*, Kluwer Academic, Dordrecht, 1994.

"Some Remarks on Ontology and Logic in Buddhism, Jainism and Quantum Mechanics." Invited talk at the conference on *Science et engagement ontologique*, Barbizon, October, 1999.

"Culture, logic and rationality", postscript to chp. 10, in *The Eleven Pictures of Time*, Sage, 2003.

"Why Deduction is MORE Fallible than Induction", invited talk at International Conference on Methodology and Science, Vishwabharati, Shantiniketan, Dec 2004. Abstract at http://www.IndianCalculus.info/Santiniketan.pdf.

8. The alternative epistemology of the calculus in the *Yuktibhāṣā*, and its relevance to present-day computing, and mathematics education

"Computers, Mathematics Education, and the Alternative Epistemology of the Calculus in the YuktiBhāṣā", invited plenary talk at the *8th East-West Conference*, University of Hawai'i, Jan, 2000. In *Philosophy East and West*, **51**:3, July 2001, pp. 325–362.

9. The import of calculus into Europe, to overcome European ignorance of the 3 "ells" of navigation

   "How and Why the Calculus Was Imported into Europe." Talk delivered at the International Conference on *Knowledge and East-West Transitions*, National Institute of Advanced Studies, Indian Institute of Science Campus, Bangalore, Dec 2000. At http://www.IndianCalculus.info/Bangalore.pdf.

   "The Calculus: its Indian Origins and Transmission to Europe prior to Newton and Leibniz", invited talk, conference on "Indian Contributions to the Renaissance", Univ. of Louisiana, Lafayette, Oct 2004. Also invited talk, Dept of Maths, Univ. of Iowa at Ames, and public lecture with the same title, Oct 2004.

   "The Calculus: its Indian Origins and Transmission to Europe prior to Newton and Leibniz. Part I: Series Expansions, and the Computation of π in India from Āryabhaṭa to *Yuktidīpikā*", and Part II: "Lessons for Mathematics Education", Dept. of Maths, Univ. of Auckland, Oct 2005.

10. (Aryabhata group)

    (with Dennis Almeida) "Transmission of the Calculus from India to Europe, Part I: Motivation and Opportunity", Paper presented at the International *Aryabhata Conference*, Trivandrum, Jan 2000.

    (with Dennis Almeida) "Transmission of the Calculus from India to Europe, Part II: Circumstantial and Documentary Evidence", Paper presented at the International *Aryabhata Conference*, Trivandrum Jan 2000.

11. Relevance to present-day mathematics education

    "Math Wars and the Epistemic Divide in Mathematics", invited talk at the Centre for Research in Mathematics and Science Education, Univ. of San Diego, Oct 2004, and paper presented at Episteme-1, Goa, Dec 2004. At http://www.hbcse.tifr.res.in/episteme1/allabs/raju_abs.pdf and http://www.hbcse.tifr.res.in/episteme1/themes/ ckraju_finalpaper.

12. Products of distributions

    "Products and Compositions with the Dirac Delta Function." *J. Phys. A: Math. Gen.* **15** (1982) 381–96.

"Junction Conditions in General Relativity." *J. Phys. A: Math. Gen.* **15** (1982) 1785–97.

"On the Square of x^{-n}." *J. Phys. A: Math. Gen.* **16** (1983) 3739–53.

"Renormalisation, Extended Particles and Non-Locality." *Hadronic J. Suppl.* **1**, 1985, pp. 352–70.

"Distributional Matter Tensors in Relativity." In: *Proc. MG5*, D. Blair and M. J. Buckingham (eds), R. Ruffini (series ed.), World Scientific, Singapore, 1989, pp. 421–23.

These talks and papers on seemingly diverse topics actually pertain to a single stream of thought, which seamlessly relates the history and philosophy of science and mathematics to its contemporary practice, even though the linkages are not necessarily explicit. The implicit linkages may be all the harder to understand because the papers are very widely scattered, in publications that may not be so readily accessible. These difficulties of access are aggravated by what appears to be a general belief among some conference organizers in India—that the natural thing is for conference proceedings to appear after a delay of five or six years, or sometimes never at all! Consequently, even I do not know exactly how many of these papers, public for the last several years, are actually available in printed format.

However, I think the stream of thought that ows through these papers is of some value, and it should not be wasted through improper dispersal. Accordingly, the arguments in these papers are here collected together, appropriately rearranged, and amplified or curtailed where necessary, with the aim of making them readily available, and establishing the links between them. The hope is that presenting a unified exposition of this important and fundamental aspect of the history of mathematics in relation to its contemporary practice would serve a useful purpose, not only to understand the past, but also to make clear the future directions of mathematics at the present turning point.

Considering the wide interest aroused in the topic of this book, one of the things that I was hoping to do was to make this book accessible to an interested layperson. However, given the enormity of the change in mathematics and its history the book proposes, it was hardly possible to avoid technicalities. Accordingly, the book for the interested layperson will have to wait, and the present book assumes the reader to be fully familiar with all the intricacies of all the subjects touched upon in this book. However, as the topics covered in this book sweep across from the intricacies of Buddhist, Islamic, and Christian theology to those of quantum field theory, I thought it prudent to allow for the horrifying possibility that there may be no one who is an expert in all the topics covered in the book! As a partial remedy, to make some of the complex interconnections clearer to a wider audience, each chapter begins with an extended overview, which provides a narrative-type account of the key points, without the supporting details. (Given the great value of this section, I intended to number it as section

0, but according to TEX the Roman for 0 is a blank space!) The intended overall organization and the ow of ideas across chapters is indicated in the synoptic table of contents.

This book has been prepared under difficult circumstances. Originally visualized as a full time, commissioned editorial effort, to be carried out quickly with the active participation of a number of other scholars, it turned out to be an honorary and part time effort, over eight years, to bring out a single-author volume! My various other commitments inevitably interfered with the time I could have devoted to this book, though ideally a book that is so ambitious and complex should have been written single-mindedly, with no other commitment, and no pre-stipulated time limits. (My inability to do so might have something to do with the management of science and technology in post-independence India—the subject matter of a future volume in this series!)

I am acutely aware of the possibility that, because of this time-squeeze, some defects may persist in this book as it stands. For example, some arguments and references are repeated across chapters. With present-day technology it would have been easy to identify all such repetitions, and replace them with cross references. However, I must admit to being inhibited in this by the rationalization that redundancy improves the accuracy of communication! That is especially the case, given the complexity and novelty of the thesis argued in this book. Moreover, this fits the usual format of the PHISPC volumes, which requires individual chapters to be reasonably self-contained, like separate articles. It is assumed that a careful reader who reads the book from cover to cover will be expert enough a reader to skip over all such repetitions.

Again, certain important topics are not properly covered in the book: for example there should have been a fuller account of the current practical importance of alternative logics, through an exposition of how alternative logics and the structured-time interpretation of quantum mechanics (as in my earlier book *Time: Towards a Consistent Theory*, Kluwer Academic, Dordrecht, 1994) relate to quantum computing, which hopes to achieve what present-day computers cannot. In particular, I wished to explain how the structured-time interpretation is superior to the many-worlds interpretation for purposes of quantum computing. Other contemporary consequences relating to mathematics education, and to renormalization and shock waves remain summarily articulated in chapter 9, and the appendix. I have left things as they stand with the view that publication should be timely, especially given the widespread interest in this book, and given that the aim of the present book is only to *indicate* the contemporary consequences of the revised history and philosophy of mathematics rather than to comprehensively resolve all issues. Hopefully, others too will take up these matters in more detail in future publications.

After the book was first very nearly completely typeset, I had to write a program to change the typesetting to TEX—a diabolical invention obviously intended to distract authors from the task of producing good books to the task of producing good-looking books! (When the author has to do his own typesetting, TEX's philosophy of separating form and content noticeably fails!) One reason for this shift was the difficulty with the combination of mathematics, multilingual text and diacritical marks, needed also for the numerical notation.

The diacritical marks used in this book are given on p. 117, and p. 130. For *anusvāra* and *visarga*, I have respectively used *ṁ* and *ḥ*. Since the English language has no consonant ending (*halant*), the word *yoga* is typically mispronounced as *yogā*. Common practice recognizes this difficulty—all Jain-s I know spell their name as Jain, and not Jaina, as required by the current conventions, which ought to be changed. Similarly, exible word boundaries arise also in computer programming (self-documenting code) where they are indicated by mixed capitalization—occasionally used in this book. Since, however, diacritical marks are hardly the focus of the battle in this book, I have generally adhered to the stock conventions.

Given the long time taken by this effort, it is hardly possible to thank all those who have helped out in one way or the other. I am grateful to the Project Director, Professor D. P. Chattopadhyaya, for patiently waiting for this volume to come out. I am grateful to the Indian National Science Academy, for a partial project grant, and to the National Institute of Science, Technology, and Development Studies, and the Nehru Memorial Museum and Library, for providing a base in the early stages of the development of this line of thought. I would especially like to record my gratefulness to the late Professor Ravinder Kumar, whose ideas about the futuristic nature of history are re ected in the emphasis on contemporary consequences in this book.

I am grateful to the late Professor K. V. Sarma for kindly letting me have an advance copy of his draft translation of the *Yuktibhāṣā*, which provided great impetus to this work in its early stages by helping to penetrate the primary sources. I regret that my citation of his unpublished (and unfinished) work created unnecessary problems for him.

I am grateful to Shri Sharad Chandra Behar, former Director General of MCRP University, Bhopal, for the rare act of encouraging scholarship in an Indian university, and for his cooperation and advice during the disturbing event of the transmission of the transmission thesis.

To Jaya, Suvrat, and Archishman, I owe an apology for having lavished on this book so much of my "spare time" that I should properly have devoted to them.

It is always a very pleasurable task to thank various people for the preparation of the final camera ready copy, and thus indirectly pass on to them the blame for any errors remaining in the book, while putting on a halo of virtue by seeming to accept the blame. However, I must acknowledge the many occasions on which I overruled the suggestions of the publication team. Given my theory of chains of causes with mundane time, which has proved to be especially popular with my children, it is probably best for me to say nothing further!

C. K. Raju
New Delhi

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Introduction

170 YEARS

IN writing the history of science in India, mathematics plays a major role—irrespective of whether or not one uses an Indian perspective to define the notion of science. This is particularly the case with the history of science in India around the 16th c. of the Christian Era (CE). The calculus has played (and continues to play) a major role in the development of present-day science. But the Indian role in the development of the calculus has gone almost unnoticed.

This is strange, for the calculus started with the use of infinite series, and the use of infinite series in India was publicly acknowledged even by Europeans some 200 years ago. The most well-known (though not the earliest) case of such European documentation is that by Charles Whish in 1832 CE.[1] Despite the 170 years that have elapsed since then, the connection of these Indian series to the infinitesimal calculus, as known in Europe, has yet to be established. Even the philosophical and mathematical underpinnings with which the originators used these infinite series have never been fully explicated. This is an extraordinary state of affairs. Accordingly, in writing this book, I felt that an in-depth analysis and documentation of this one case of the calculus would do rather more for the history of science in India, than an extensive survey across various fields.

THE CALCULUS AND THE FOUNDATIONS OF MODERN SCIENCE

The calculus, after all, was *the* key input to Newtonian physics. All the mathematics needed for Newton's *Principia* (and for classical mechanics down to this day) is encapsulated in the so-called Taylor-series expansion, which is the pinnacle of the calculus. As V. I. Arnol'd puts it,

> Newton's basic discovery was that everything had to be expanded in infinite series.... Newton, although he did not strictly prove convergence, had no doubts about it....What did Newton do in analysis? What was his main mathematical discovery? Newton invented Taylor series, the main instrument of analysis.[2]

The name "Taylor series" derives from Newton's pupil, Brook Taylor (1685–1731), whose work on it dates from 1715.[3] These infinite series expansions are to calculus and analysis what decimal fractions are to arithmetic. In India these infinite series expansions were used by Mādhava to derive trigonometric values accurate to the third sexagesimal minute, and by Nīlakaṇṭha to develop an accurate "Tychonic" planetary model with elliptic orbits by 1501. And it is remarkable that these very trigonometric values, astronomical models, and infinite series first started appearing in Europe in the works of Clavius, Tycho Brahe, Kepler, Cavalieri, Fermat, Pascal,[4] and James Gregory,[5] while Europe was still struggling to became acquainted with decimal fractions, with the publication in 1585 of Simon Stevin's *De Thiende*, and its subsequent translation under the title *La Disme*.

It was exactly this mathematical ability to expand in infinite series that enabled Newton to back-calculate and establish for "Kepler's" elliptical planetary orbits the inverse square law of gravitation that was widely believed by Newton's contemporaries to be the case for circular planetary orbits.[6] Though Newtonian physics, widely regarded as the foundation of modern science, today stands discredited, infinite series expansions continue to remain important in quantum field theory, for example. Hence, a book devoted entirely to the historical origins of the calculus seems worthwhile, as part of the enterprise of writing the history of modern science from an Indian perspective. That is especially the case since the present account of how the calculus actually developed in India, and was then transmitted to Europe, differs so vastly from the usual accounts[7] which jump from "Archimedes" to Newton and Leibniz, neglecting India entirely.

WHY HISTORY AND PHILOSOPHY OF SCIENCE MUST GO TOGETHER

Such an in-depth and novel account of the history of the calculus requires, however, a substantial deviation from earlier ways of writing the history of Indian mathematics. Specifically, one cannot ignore the old adage that the history of science without its philosophy is blind (and the philosophy of science without its history is lame). Doing the history of science together with its philosophy is as common in the West as are university departments there of the history and philosophy of science. Unfortunately, history-writing in India has so far ignored this adage, and has proceeded on the naive assumption that the philosophy of science or mathematics can be safely ignored for the purposes of writing its history. In a way, this is understandable, since there is, at present, not even a single department for the history and philosophy of science in any Indian university, so that work on the history of science has been administratively conceptualized as either a part-time hobby or a post-retirement pursuit. Therefore, historians of Indian mathematics remain inadequately informed about philosophy—not to speak of the philosophy of Indian mathematics.

If we shift our viewpoint from history to university departments of philosophy, there is unfortunately again a severe paucity, if not a complete absence, of people in India who have ventured to contribute anything fundamental to the philosophy of science or mathematics, that too in a historical perspective. Thus, doing the history of science together with its philosophy remains a pursuit as uncommon in India as university departments of the history and philosophy of science. Perhaps this can be put down to a colonial mentality which leads to a deep seated fear of challenging the cultural assumptions common to former colonial rulers and the present-day superpower.

THE DEFAULT PHILOSOPHY OF MATHEMATICS AND THE NEED TO RE-EXAMINE IT

While one may sociologically hope to understand why Indian scholars, in recent times, have been unable to put together the history and philosophy of science, there are two painful and unacceptable consequences of this unnatural disjunction. The first unacceptable consequence is that this attitude entrenches what has euphemistically been called Eurocentrism. If one excludes the philosophy of science from the ambit of a study of its history, then one is obliged to do history with the *default* philosophy of science. In our case this means that one must then accept the present-day Western philosophy of mathematics, not only as a privileged philosophy, but as the *only possible* philosophy of mathematics.

The present-day philosophy of mathematics, on the one hand, traces its historical and philosophical roots to an allegedly "Greek" tradition. On the other hand, this philosophy pretends that mathematics is universal and one, and that this sole possible kind of mathematics is the kind of formal mathematics that is today prevalent in the echelons of higher formal education. This attitude which equates mathematics with formalistic "rigour", and rules out any alternative philosophy of mathematics, risks losing valuable insights into the origin of the Indian infinite series, and eliminates altogether the possibility of understanding what was then regarded in India as a good mathematical calculation or a convincing mathematical demonstration. Using the default philosophy thus works against the grain of history regarded as an attempt to understand the past.

The second consequence follows from the first: for if the Indian infinite series were established using a method of calculation and demonstration that does not constitute a formal mathematical proof, valid according to the present-day belief in the potency of formalism, then the Indian infinite series may forever have to be consigned to the status of "proto-calculus", or at best "pre-calculus", for that is how Western historians of science would surely like to classify them, if at all they are compelled to link these Indian infinite series to the infinitesimal calculus in Europe. (I may add that this presupposition has been amply borne out in the recent discussions that transpired in the Historia Mathematica discussion list.) After all, Indian infinite series were very similar to, if not identical with, the series used by Cavalieri, Fermat, Pascal, Barrow, Gregory, and Wallis, and these efforts are already classified as "pre-calculus" by Western historians of science. While such a strategy of classification and labelling may suit the political interests and the morbid narcissism of the West, it works against the grain of history regarded as an attempt to reconstruct the past.

Hence, this book proceeds on the premise that to arrive at a proper evaluation of the Indian contribution to science one may need to depart radically from the way in which the history of science, and particularly the history of mathematics, has in the past been done in India, by ignoring its philosophy. In particular, I believe that traditional Indian mathematics cannot be fundamentally understood nor its history properly reconstructed without thoroughly re-examining the alleged universality of the current notion of mathematical proof. Conversely, despite Professor Daya Krishna's suggestion to the contrary, I believe that the related philosophical question about the nature of mathematical proof cannot be answered in the abstract, in a historical vacuum, and that the fresh historical perspective on transmissions in this book, throws fresh light on the nature and historical evolution of the present-day idea of mathematical proof, whether seen from an Indian or a European perspective.

THE DEFINITION OF MATHEMATICS AS PROOF

Indeed, it seems to me patently obvious that how one writes the history of mathematics naturally depends on what mathematics is. If, for example, mathematics is *defined* as something invented in Greece, that would make a cardinal difference to the history of Indian mathematics.

Defining mathematics as something invented in Greece might seem preposterous and unnatural. But there are two lines of thought, one from history, and one from philosophy, both of which implicitly converge onto the above definition of mathematics as something invented in Greece. For the historical line, it is adequate to examine even cursorily the current grand narrative of the development of science that can be found in almost any "standard" Western text in the history of mathematics. The overarching impression is that mathematics commenced in Greece, and was largely lost during the medieval period, until it was rediscovered in the European Renaissance. In this "standard" picture, it is accepted that other cultures did make a few scattered contributions here and there—for example India contributed exactly zero!—but these cultures remained basically clueless as to the real nature of mathematics.

And what is the real nature of mathematics? As any university professor of mathematics today would inform us, mathematics concerns theorems and proofs. That is how mathematics is today taught in the classrooms, and that is how mathematical research is presented in journals. This is apparently also how mathematics was done in Greece (according to existing histories of mathematics at any rate). But that was *not* how mathematics was done in India (or China or Babylon for that matter). Accordingly, what was done in India (or China or Babylon for that matter) was not quite mathematics, which really began in Greece!

This point of view is articulated explicitly by Rouse Ball who begins his "classic" account of the history of mathematics by triumphantly proclaiming:[8]

> The history of mathematics cannot with certainty be traced back to any school or period before that of the Ionian Greeks. The subsequent history may be divided into three periods... the first... under Greeks... the second the mathematics of the middle ages and the renaissance... the third modern mathematics....
>
> On the subject of prehistoric mathematics, we may observe in the first place that, though all early races which have left records behind them knew something of numeration and mechanics, and though the majority were also acquainted with the elements of land-surveying, yet the rules which they possessed were in general founded only on the results of observation and experiment, and were neither deduced from nor did they form part of any science.

Given that Westerners (and many Indians) often mistake such racist and narcissistic accounts for deep historical ("classic") scholarship, the prevailing situation is not so very different from the *de facto* definition of mathematics as something that was invented in Greece. For, the prevailing situation incorporates a definition of mathematics (as proof), and a definition of mathematical proof that together make it inevitable that mathematics was invented in Greece, and could have been developed only in Europe! To go a step further, according to the prevailing formalist philosophy of mathematics, definitions *are* arbitrary, and they are not required to accord with intuition or culture: all that is required is that they should be acceptable to people in appropriate positions of social authority (among mathematicians or historians of mathematics)! QED.

THE ADVANTAGES OF DOING HISTORY WITH PHILOSOPHY OF SCIENCE

Accordingly, in my opinion, a project on the history of Indian science, which seeks to write a proper history of science (and mathematics), must attempt to rewrite, side by side, the philosophy of science (and mathematics), and the accompanying implicit definition of mathematics. In doing so, if we find that we are no longer able to retain the present-day separation of mathematics from empirical science, we may have to accept such a conclusion.

Admittedly, this conclusion is fatal to the present-day (Western) notion of mathematics, just as much as the realization that deduction is more fallible than induction (Chapter 2) is fatal to much of Western philosophy. However, there is no remedy for it, since it emerges that the Western belief in the universality and infallibility of deduction is at bottom based on mere religious and cultural beliefs that have no place in a secular history of mathematics, or in a secular mathematics. In particular, to eliminate such religious bias in history, it is essential for us to begin by fundamentally re-examining the current definition of mathematical proof, and the definition of mathematics that it entails.

There are some definite advantages to this way of doing history, with a philosophical and secular perspective. First, it enables us to understand better the mathematics of the Indian infinite series from the viewpoint of its inventors. Secondly, the revised understanding of the philosophy of mathematics leads to a strikingly different understanding of the historical development of mathematics, in a more global and multicultural context, with epistemology as the major driving force, especially over the last thousand years in Europe. This, in turn, leads to a totally different evaluation of India's historical contribution to the development of modern science.

EPISTEMOLOGICAL STRIFE AND THE MATH WARS

This idea of epistemology as a driving force in the historical development of science (and particularly mathematics) is so novel to the history of science that it deserves some amplification right here. We are all familiar with the story of how the algorismus and zero were transmitted from India to Europe via the Arabs, and how algorismus and zero were received with deep suspicion in Europe, precipitating a five century long battle in Europe between algorismus and abacus. My claim here is that this battle (first math war) originated in the contrasting epistemology of number in Indian and European tradition, and was eventually settled in favour of the algorismus because of the pressure arising from the greater practical utility of the algorismus.

Exactly like the import of algorismus, the import of the calculus into Europe aroused deep epistemological suspicions about the infinities and infinitesimals that the calculus allegedly involved. These suspicions lasted for centuries (second math war), and could be partly settled only through a further transformation of the European understanding of the notion of number—leading to real numbers—rendered necessary because the pressure arising from the great practical utility of the calculus forced a revision of epistemological dogmas about number. Thus, the present approach seeks to understand the last thousand years of the history of mathematics in terms of the epistemological strife arising from transmission across cultures.

CONTEMPORARY RELEVANCE

This way of understanding history has immediate contemporary significance, for the recent rise of computer technology has precipitated a new epistemological strife (third math war) between mathematics as calculation and mathematics as proof, which seems to demand a fresh transformation of the notion of number. A better understanding of history leaves us better situated to decide whether calculations done on a computer are epistemologically secure enough to be regarded as mathematics today.

Another area of contemporary applications is to mathematics education. The mathematics that came to Europe over the last millennium is the same mathematics that is taught in schools today, from the abacus in Kindergarten through arithmetic and algebra to the calculus in the 12th standard. On the principle that phylogeny is ontogeny, some thousand years of epistemological strife are played out in "fast forward" mode as con icts in the minds of the young K-12 student who today seeks to assimilate the very same mathematics in 12 years rather than 1200 years. This suggests that the difficulty in learning mathematics today is linked to its hybrid epistemology, deriving from its multicultural origins. Thus, the revised account of the historical development of mathematics, over the last millennium, leads to a revised account of how mathematics should be taught today, in a way that can be easily understood.

It is interesting that we are also able to demonstrate the relevance of this new way of understanding mathematics to the frontier areas of present-day mathematics where formal mathematics has of necessity reached a sterile impasse. The suspicions about infinities and infinitesimals that surrounded the initial appearance of the calculus in Europe continue to linger in the suspicions that surround the current use of infinite series in places where the 19th c. CE formalisation of the calculus is inadequate—e.g., the renormalization problem of quantum field theory. The new historical insight provides a new way of resolving these suspicions.

It is perhaps somewhat unexpected that discarding the filters of contemporary knowledge (especially the current-day definition of mathematics) to produce a better history has (in this case) the effect of *enhancing* the contemporary relevance of that historical knowledge. The contemporary practical relevance of the alternative philosophy of mathematics proposed here may perhaps be an accidental consequence of, for example, the recent rise of computer technology. But the fact of this contemporary relevance is particularly gratifying, especially in view of the late Professor Ravinder Kumar's oft-repeated assertion that history is futuristic.

TERMINOLOGY AND THE CATEGORIES OF HISTORICAL STUDY

A note about terminology. Many people have used and continue to use terms such as "Hindu" mathematics, and "Keralese" mathematics—problematic terms that betray the epistemological illiteracy of the user unless the aim is crude political mischief of the kind that the British systematically introduced in this subcontinent. Given that people like Nīlakaṇṭha (from Kerala) identified themselves as followers of Āryabhaṭa (from Bihar), how did the epistemology of this allegedly "Keralese" mathematics differ from the epistemology of the "Bihari" mathematics of Āryabhaṭa? The term "Keralese" is as jarring and misplaced as the term "Telugese" logic would be, if it were to be applied to Nāgārjuna's logic.

The term "Hindu mathematics" is probably worse. For Western historians of mathematics this term has served the purpose of suggesting that whatever was done in India was not quite mathematics. The other purpose served by this term has been to delete the role of the Buddhists, in the manner in which Western historians have deleted the role of the non-West (perhaps because Buddhists themselves were deleted from India, so there was no one to represent their interest). From Chinese records of the calendrical and mathematical abilities of Buddhist monks from India, it would be hardly surprising for Nalanda to have played a prominent role in the time of Āryabhaṭa, and there is no case for a specifically "Hindu" mathematics, distinct from Buddhist (or Jain) mathematics. Undoubtedly, Naiyāyika-s and Buddhists, for example, have different ideas of epistemology, and different notion of *pramāṇa* or proof. But I have been unable to find any specific examples of "Naiyāyika" mathematics which uses anywhere in a demonstration either *śabda pramāṇa* or *upamāna*—two key points of difference in the notion of *pramāṇa* between Naiyāyika-s and Buddhists. Likewise, it is pointless to speak of Jain mathematics, for what Mahāvīra does in *GaṇitaSāra-Saṁgraha* differs little from the *Pāṭīgaṇita* of Srīdhara, or the *Līlāvatī* of Bhāskara: such differences as exist are a matter of detail rather than any fundamental epistemological difference. Indeed, given the sort of arguments used by Śriharṣa (an Advait-Vedāntin whose aim in his *Khaṇḍanakhaṇḍakhādya* was to use the tools of Nyāya to destroy Nyāya, but who clearly uses various Buddhist arguments against Nyāya[9]) it seems to be deeply problematic to equate Naiyāyika epistemology with "Hindu" mathematics.

There seems to me a stronger case for categories like "Christian mathematics" and "Gentile mathematics" (of Proclus or "Euclid") for one can very clearly identify the epistemological differences between "Neoplatonism" and Christian rational theology, and the way these differences historically changed the understanding of the *Elements*, and the notion of mathematical proof. Likewise, there is a stronger case for categories like "Roman Christian mathematics" and "Protestant mathematics" for while many mathematicians have identified their religion as Roman Christian or Protestant, no traditional Indian mathematician, to my knowledge, ever referred to himself as a "Hindu"—though they may have identified themselves as worshippers of Shiva, or Brahmā, for example.

Finally, the category "Hindu" mathematics is epistemologically misleading in a serious way: *śūnya* and *śūnyavāda* are generally regarded as being of Buddhist origin and the present history identifies a leading role for the non-representable in Indian mathematics. Nāgārjuna refuted ideas like that of a creator-god and the soul just because his *śūnyavāda* philosophy is strongly realist and anti-idealist. The non-existence of the soul was inferred not by denying cosmic recurrence or anything like that, but by simply denying the validity of the common notion of a person—for a person keeps changing from instant to instant, and there is no proof (on the Buddhist principles of *pramāṇa*) that anything "essential" remains constant. Thus, Nāgārjuna's argument is that *it is the ideal that is erroneous*, because any representation of reality necessarily conceals certain aspects of reality that are left unrepresented, or voided or zeroed. This is a very novel position from the Platonic perspective which regards the ideal (e.g. ideal point) as mathematically real, and the real (e.g. real dot on paper) as erroneous. Strangely, the Platonic perspective has remained unquestioned in the history of Western mathematics although it seems to me quite elementary that reality cannot be erroneous, though philosophers, howsoever revered, can be! In fact, it was the realist and practical perspective that enabled the proper handling of the non-representable, and this enabled Indian mathematicians to elegantly overcome the problems with infinitesimals and infinities which left European mathematicians befuddled for centuries about the calculus (a state which still lingers despite formal real numbers and non-standard analysis).

Brie y, if the object is to *understand* the history of mathematics, then one must use the appropriate categories, and these are epistemological categories rather than religious or geographical ones.[10]

SOME MORE LABELS

There is another sort of categorization that needs to be mentioned here. This book, since it presents a new account of Indian history, inevitably involves a critique of Western history. However, some Western scholars, recognizing the intrinsic weakness of that history, tend to respond to any critique of Western history not by examining the evidence (which would expose it) but by launching personal attacks on the critic with labels—in this case, the label "Hindu nationalist" seems to commonly arise to the tongues of shallow scholars. Now I completely fail to see why the only choice one has is between different kinds of hate politics—why the rejection of Western racist history necessarily implies the acceptance of some other kind of hate politics.

My belief in the principle of universal harmony is clearly formulated and stated in my book *The Eleven Pictures of Time*. Contrary to what many religions teach, there is no room in my belief system for hatred of any set of persons, and I am proud of this tradition that has historically been in place since Ashoka's edicts of tolerance. The politics of religious hatred arises when religion is mixed with state power, and I also believe, and have also stated explicitly, in the above book, that those who seek to attain or retain state power in the name of religion are the worst enemies of that religion, *regardless* of what religion they claim to represent—whether Christianity, Islam, or Hinduism. (Thus, they continuously reinterpret the religion to suit the requirements of the state.) It is easy to find many people who oppose one kind of hate politics while being "soft" on another set: however, as stated above, I fail to see why one's choice should be restricted to different brands of hate politics. I am not in any such camp, my stated system of ethics does not admit hate politics of any kind, and I oppose *all* attempts to mix religion with politics. I do realize that the different hate camps have become so widespread today that the political space for people like me is limited, and I am grateful especially to the late Dr Arun Ghosh for having encouraged me in this direction, despite the obvious difficulties involved.

Secondly, the real concerns of "Hindu nationalists" with academics emerge from their actions—of starting some twenty university departments of astrology (but not a single department of the history of science), and whole universities in subjects like journalism. Obviously, financially self-sustaining superstitions, and slanted news are a better source of power and votes. The politics that emerges from my own actions is clear enough. As one of the few persons in the Indian university system to have publicly opposed such attempts,[11] I have twice been unceremoniously removed from academic positions when and where these forces were in power. I wonder whether critics who argue from thoughtless labels of this sort have an equally clear record of action against the superstitions and untruths systematically promoted by the ruling religious establishments on their own home turf.

What these defenders of Western history need to think about is this. Suppose "Hindu nationalists" were to seize power, strangle dissent by passing laws to kill dissenters, in painful ways, and then continuously expand their power through multiple genocide for the next 1700 years. What sort of history would emerge? We do not need to imagine very hard, for we have a concrete model before us, in the sort of Western history that has been written since Eusebius! Because of the long history of brutal suppression of dissent in the West, various fantasies, contrary to the barest common sense, have been allowed to pile up, and these continue today to masquerade as the scholarly truth. The time has come for things to change, and this project has aimed, from its earliest conception in the early 1990's, to bring about such a change by setting aside the one-sided Western accounts of history that have been prevalent to date, challenging Western biases where necessary, and presenting a fresh formulation of history, in a pluralistic way. An argument from labels is not going to halt that change: either hard evidence would have to be procured, at least at this late stage, for the myths propagated by Western history, or else these myths would have to be abandoned.

Finally, to restate a trivial point. Just as being against "Hindu nationalists" is not to be anti-Hindu, and being against "Islamic terrorists" is not to be anti-Islam, so also being against Christian chauvinism is not to be anti-Christian. Fortunately, there still are at least some enlightened people—like my esteemed friend the late Dr Paulos Mar Gregorios, Metropolitan of Delhi—who understand this perfectly well. Although this book is intended for all people interested in the history and philosophy of mathematics, it is to such people that this book is especially addressed in all earnestness.

NOTES AND REFERENCES

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2. V. I. Arnol'd, *Barrow and Huygens, Newton and Hooke*, trans. E. J. F. Primrose, Birkhauser Verlag, Basel, 1990, pp. 35–42.
3. Nowadays known as the Taylor theorem, this appeared as Proposition 7, Corollary 2, in Brook Taylor, *Methodus Incrementorum directa et inversa*, London, 1715. Translation in L. Feigenbaum, *Brook Taylor's "Methodus Incrementorum": A Translation with Mathematical and Historical Commentary*, Ph.D. Dissertation, Yale University, 1981. Apart from Newton, the series was known earlier to James Gregory. L. Feigenbaum, "Brook Taylor and the Method of Increments," *Arch. Hist. Exact. Sci.* **34** (1) (1984) 1–140.
4. See, e.g., C. H. Edwards, *The Historical Development of the Calculus*, Springer, Berlin, 1979.
5. In a letter of 15 Feb 1671 to John Collins, Gregory had supplied Collins with seven power series around 0, for arctan θ, tan θ, sec θ, log sec θ, etc., H. W. Turnbull, *James Gregory Tercentenary Memorial Volume*, London, 1939. Gregory's series, however, contained some minor errors in the calculation of the coefficient of the fifth-order term in the expansion.
6. Unlike the situation prevailing earlier in Europe, where uniform circular motion was taken as not requiring any explanation, while departures from this did require an explanation, by the time of Newton, uniform *rectilinear* motion was taken as not requiring any explanation, as in Newton's first law, while uniform circular motion was explained as being due to an inverse square law force. Thus, what Newton did was to use the calculus to show that the same explanation could be extended also to elliptic orbits.
7. E.g. Carl B. Boyer, *A History of Mathematics*, Wiley, New York, 1968. C. H. Edwards, *The Historical Development of the Calculus*, Springer, Berlin, 1979.
8. W. W. Rouse Ball, *A Short Account of the History of Mathematics*, Dover, New York, 1960, pp. 1–2.
9. Possibly under Buddhist in uence, especially of Nāgārjuna, the founder of *śūnyavāda*.
10. That is to say, roughly speaking, that while I would be somewhat sympathetic to Spengler's idea, that Cultures are appropriate for the study of history, and though there may be a loose sort of correlation of cultural and epistemological boundaries with religious and political boundaries, I would disagree with Toynbee that each "civilization" can be identified with a unique dominant religion or "universal church". The marked departure from Toynbee's ideas is particularly the case with India—prior to 1757 CE in any case!
11. E.g., C. K. Raju, "The emperor's new course", Invited talk at the India International Centre meeting on *Vedic Astrology in University Education: a Sound Decision?* Dec 2001. The two other participants, Dr Raja Ramanna, and Dr Pushpa Bhargava (who has taken a rather more active part than me in this debate), were both not from the university system. Also, "Astrology: Science or Superstition", public lectures and discussions at Almora, and Nainital, Lok Vigyan Centre, May 2003.

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Part I

The Nature of Mathematical Proof

CHAPTER 1

Euclid and Hilbert

History of geometry and the genesis of the current notion of mathematical proof

OVERVIEW

THE notion of mathematical proof is at the heart of the present-day socially dominant notion of mathematics, and the resulting notion of "mathematics" differs fundamentally from the notion of "mathematics" as it historically developed in India from the *śulba sūtra* (ca. −500 CE) to the *Yuktidīpikā* (16th c. CE). Therefore, to assess the worth of the Indian contribution to mathematics it is first necessary to re-examine the notion of mathematical proof, without a whole complex of cultural presuppositions. In line with the principle that the history and philosophy of mathematics must go together, we first re-examine the historical perceptions which shaped the present-day notion of mathematical proof.

The current notion of mathematical proof is said to have originated in Euclid's *Elements*. To understand the genesis of the philosophy underlying the *Elements*, it would help to know the socio-political context of Euclid. The historical information about "Euclid" is, however, too meagre to enable us to determine this context; indeed, from the available historical information it is very doubtful that "Euclid" existed. The key historical source of information about "Euclid" is a single remark in a manuscript ("Monacensis 427"). Since the manuscript is on paper, which became prevalent in Europe only in the 13th c. CE, it is a late manuscript, although some historians have optimistically dated it as early as the 10th c. CE. The manuscript relates to a commentary on the *Elements* attributed to Proclus, from *at least* 500 years earlier. Since Proclus would have written on fragile papyri, for Proclus' commentary to have survived it must have been rewritten several times, affording ample opportunity for interpolation.

The Monacensis remark speculates about Euclid, stating that this "Euclid" remained unknown to other historians of geometry for the 750 years that further separate Proclus from the supposed date of "Euclid"! (That "Euclid", if he existed, was little known for at least seven centuries after his alleged date is corroborated by the archaeological evidence of the only three scientific (geometry) papyri known from Alexandria—no *definitive* text of the *Elements* was prevalent until the 4th c. CE, when the "received" text of the *Elements* was probably first put together by Theon and Hypatia.) The Monacensis remark does not fit the rest of Proclus' Prologue: for it attributes to this "Euclid" a "formalist" mathematical philosophy (of "irrefragable demonstration") quite at variance with the Platonic philosophy (of eternal truths) advocated in the rest of Proclus' Prologue.

The Monacensis remark seeks to date "Euclid" based on the claim that Archimedes somewhere referred to "Euclid". However, the isolated reference to the *Elements* (not "Euclid") in the works of "Archimedes" has been regarded as spurious, since, in the absence of standardized texts, it was not the custom in Archimedes' time to make such references, especially in the style of Christian theology, and references could have been made at many other places. Furthermore, since the author of the remark in the Monacensis manuscript knew of the spurious Archimedes reference, the author of the Monacensis remark must date from later—probably the 16th c. CE, when Byzantine Greek texts arrived in Europe. The Monacensis remark, therefore, is an interpolation that was no part of Proclus' original text.

The mistaken belief in an actual person called "Euclides" may possibly have originated in a Toledan howler—a mistranslation of "Uclides" (meaning "key to geometry") as referring to the name of a person who authored the *Elements*. In any case, the "irrefragable demonstrations" of "Euclid" of Monacensis were vaguely stated to have been based on "causes and signs",[1] which hardly suits the purposes of formalist philosophy! The actual philosophy and history of the *Elements* accordingly needs to be re-examined.

The arrangement of the theorems in the *Elements* relates better to Proclus' explanation of mathematics as meaning, *by derivation*, the science of learning, for learning must ideally proceed on the basis of what the learner has previously learnt. Since Proclus, like Plato and Socrates, regarded all learning as reminiscence of knowledge that the soul had acquired in previous lives, this closely tied mathematics to his religious beliefs about the soul and reincarnation. Proclus thought the soul, being eternal, was sympathetically stirred by eternal mathematical truths—which entailed the eternity of the cosmos. These beliefs about the soul and cosmos, though compatible with the early Christianity of Origen, were in sharp con ict with the later-day Augustinian doctrines of resurrection, creation, and apocalypse.

Proclus' philosophy of mathematics, and its linkage to religious beliefs, assumes special significance in his socio-political context, which was a time of intense religious turmoil. Proclus sees mathematics as an instrument of religion, and uses geometry to advocate political equity. He explains that mathematics is valuable not so much for its practical applications but because it leads to knowledge of the soul and helps attain the blessed life. That is, Proclus' Prologue explains mathematics as a technique (like *haṭha yoga*) to make a person introspective or meditative, and links this process of meditation (induced by mathematics) to the ultimate religious goal—the realization of the soul as one and equal to the immanent Nous. Thus, Proclus writes on mathematics as someone might today write a book on yoga explaining that yoga is not a form of physical exercise, for the well-being of the body, but is, as its name shows, a technique to achieve the union (*yoga*) of *ātman* with Brhman—the ultimate goal of life. For Proclus, geometry is an Egyptian form of *rāj yoga*.

This religious doctrine of immanence or oneness (and the consequent equality of all souls) was explicitly and widely related to political equity also, for example, in Islamic rational theology (*aql-ī-kalām*) advocated by the Mu'tazilah, who called themselves people of unity (*tauhīd*) and justice (*adl*). (The linkage of immanence to equity is also found among the *falāsifā*, the sufī-s, and as in the story of Śaṅkara and the *caṇḍāla*, or in Śrī Nārāyaṇa Guru's interpretation of Advaita Vedānta.) In fact, immanence was linked to equity and justice, even in the theology of early Christian teachers like Origen (who explicitly argued that God had demonstrated equity by creating all souls equal, and that he demonstrated his justice by rewarding or punishing souls according to the merits or demerits earned in the previous life, and that all souls would again be equal at the end of time, when God would be all *in* all).

By Proclus' time, however, the church had already aligned with the Roman state, and was dead-opposed to equity, since equating Christian souls with non-Christian souls would have driven it out of business. Inequity (between Christians and non-Christians) was being touted as the basis of a new moral doctrine, and the new source of justice that the transcendent Christian God would dispense on the day of judgement (ensuring, as we are reassured by Dante, that no non-Christians went to paradise). Naturally, the church had little hope of persuading people of such doctrines by straightforward argument. Accordingly, by Proclus' time, in 5th c. CE Alexandria, the Christian church had long been brutally attacking all "pagans" and using state and mob repression to target their intellectual leadership (like Hypatia) especially in the school to the headship of which Proclus succeeded.

Thus, Proclus was obliged to defend his "pagan" religion, and especially the belief in political equity, against the communal mob violence, systematic book burning, and state repression targeted especially against his school by the new ruler-priests. It is in this context that Proclus turns to mathematics, for he regarded mathematics, and reason generally, as a key instrument in persuading those who violently advocated faith and inequity. It is not incidental that most theorems in the book are about equality (later reinterpreted as "congruence"). Read in the manner explained by Proclus, the *Elements* is a text which refutes point by subtle point all the key elements in the changed Christian doctrine of the 4th c. CE (reason vs faith, immanence vs transcendence, equity vs inequity, learning as reminiscence, hence past lives vs creation in the recent past, reincarnation vs resurrection, eternal truths hence an eternal cosmos vs apocalypse, images as aids to learning vs charges of idolatry). Naturally, Proclus was declared a heretic and Justinian (in 529 CE) declared a legal death penalty on heretics, and shut down all schools of philosophy in the Roman empire. A few years later, Justinian and the new church also cursed Origen. The Dark Age had begun.

Only when it started emerging from the Dark Age did Europe first come to know of the *Elements*—through 12th c. translations from Arabic into Latin by Adelard of Bath and Gerard of Cremona—after the capture of the Toledo library, and the setting up there of a translation factory. However, at this time of the Crusades, there was a strong sense of shame in learning from the Islamic enemy. Also at the time of the Inquisition, the fears that Toledo was a Trojan horse that would spread heresy could not be lightly discounted. The *shame* was contained by the strategy of "Hellenization"—all the world knowledge, up to the 11th c. CE found in the Arabic books (including, for example, Indian knowledge) was indiscriminately assigned an early Greek origin, with the Arabs assigned the role of mere transmitters (and the Indians nowhere in the picture). The *fear* of heresy was contained by the strategy of Christianization of this incoming knowledge, by reinterpreting it to bring it in line with the requirements of Christian theology.

This background helps us to understand the popularity of the Euclid myth. The mere name "Euclid" suggested a "theologically correct" early Greek (as opposed to an earlier black Egyptian or later Theonine or Hypatian) origin of the *Elements*, and de ected charges of heresy, which invited a legal death penalty from Justinian to the Inquisition.

The existence of a "Euclid" about whom we know nothing is in any case of little use from the present viewpoint which seeks to *understand* the history of mathematics. However, from the perspective of the church and later racist history, used as a vehicle for cultural glorification, the mere *name* "Euclid" was critical to claim a "culturally pure" Greek origin of geometry, and to appropriate geometry as a Western invention—Western historians have built a huge structure on a single name of doubtful parentage, while using it to erase the solid evidence that it was preceded by some two thousand years of black Egyptian geometry, which had both practical and religious significance, which continued until the time of Theon, Hypatia and Proclus, when a definitive version of the *Elements* came into existence.

Further, considering that all European versions of the *Elements* up to the 16th c. were translations from the Arabic, it is equally remarkable how the subsequent Arabic-Islamic contribution to the *Elements* was eliminated, by relegating the Arabs (like the later Alexandrian philosophers) to the status of mere transmitters. Hence, for use in later chapters of this book, we note here the extraordinarily imsy "evidence" on which these claims of the origin and transmission of geometry are based. Thus, the long-standing claim of Euclid's existence also provides an example of the trick of *de facto* double standards of evidence in Western historiography, concerning transmission—an excessively lax standard of evidence for claims of origin and transmission from "Greeks", and an excessively stringent standard of evidence for claims of origin and transmission from non-West to West. The persistent reliance on such shabby standards of evidence, and the corresponding over-reliance on authority, has led to the long-term Western manipulation of history as merely an instrument of religious and racist propaganda, unconnected with the real past.

We are now better situated to understand the real philosophy of mathematics underlying the *Elements*, and its subsequent development. Procluvian philosophy of mathematics often refers to Plato who thought mathematics, like music, should be taught for its beneficent effects on the soul. Plato rejected the empirical (as perishable), and regarded mathematics itself as an inferior discipline. Proclus concurs that all knowledge is reminiscence, that geometry helps evoke this reminiscence, and that geometric diagrams are the easiest way to remind the soul of its past knowledge. Proclus, however, only regards the *applications* of mathematics (and not mathematics itself) as inferior. Further, he permits the empirical at the beginning of mathematics on the ground that proof must vary with the "kinds of being", thus *permitting the empirical in proofs* of key results in the beginning of mathematics, as in every text of the *Elements* up to the 20th c. CE, specifically in Propositions 1.1, 1.4, etc.

The connection between mathematics and religion articulated by Proclus persisted in the subsequent Islamic tradition of rational theology (*aql-ī-kalām*). However, the Platonic or Procluvian understanding of mathematics was subsequently transformed within Islamic theology as follows. First, the early Islamic theological tradition used the *Elements* in the manner of Proclus' *Elements of Theology* (attributed to Aristotle), to illustrate how everything could be rationally deduced from the two basic principles of divine justice and divine unity (equity). This tradition received a boost when, under Caliph al Ma'mūn, the intellectual diaspora of Alexandrian philosophers arrived in strength at the Baghdad Bayt al Hikma (House of Wisdom) (9th c.), via Jundishapur (6th–8th c. CE). So strong was the in uence of the *falāsifā* (philosophers=lovers of wisdom), and so high was their praise for reason, that even their theological opponents within Islam, like al Ghazālī, conceded that God was bound by the laws of logic—a concession naturally accepted by al Ghazālī's key opponent Ibn Rushd, who had a decisive in uence on Western thought.

In fact, al Ghazālī conceded the point about logical inference, since his real concern was to attack *causal* inference, in a way later echoed by Hume. Al Ghazālī argued that Allah may be bound by the laws of logic but was not bound by any laws of cause and effect. Thus, Allah was free to (continuously) create the empirical facts of his choice in the world, every instant, in any sequence (e.g., smoke without fire), howsoever surprising. The believed necessity of mathematical truths now acquired a new meaning compatible with the belief in the continuous creation of the cosmos. Mathematical truths were now regarded as necessary truths not in the sense of being eternal (and requiring an eternal cosmos), but in the sense of being true in every possible world that Allah could create. Hence, also, logic which bound God came to be perceived as more powerful than empirical facts which did not.

The nature of mathematics was further transformed by Christian rational theology, when the *Elements* first arrived in Europe, through Latin translations of Arabic texts in the 12th c. CE. Christian theologians were interested in persuasion and had little knowledge of calculation. They came to regard "reason" as a more powerful means of persuasion than the scripture since Muslims accepted reason but rejected the Christian scripture. Accordingly, mathematics was projected as a religious tool to teach the method of argument to theologians. They came to regard mathematical proof as providing the standard of a "universally" convincing argument, for it was accepted as convincing also by the Islamic theologians—the only other culture that Christian theologians in Europe then knew about.

It is well known that Christian rational theology, in its initial (Thomist) stages, was deeply in uenced by Islamic rational theology, and Ibn Rushd (Averröes) in particular. Although, Christian rational theology discredited al Ghazālī's idea of providential intervention (as echoed by Dunsmen), because it did not fit well with the belief in a transcendent God, and one-time creation, it accepted al Ghazālī's argument that the empirical world *had* to be regarded as contingent to allow freedom to God to create the world. Thus, on the view that mathematics concerns only necessary truths, which would have bound even God, *the empirical had to be rejected in mathematics* to suit the understanding of mathematics among the schoolmen, who advocated Christian rational theology.

That is, the rejection of the empirical in mathematics, and the belief that the empirical is contingent, both, ultimately depend upon religious beliefs about the soul, God, and creation. This belief in the contingency of the empirical world is also used today in the philosophy of science, in the criterion of refutability, for example.

As a further part of this Christianization process, the ideas of immanence and equity stressed by Proclus were dropped in Christian theology, for reasons already explained. Thus, *only the goal of rational deduction was retained in mathematics.*

It is these narrow religious concerns and theological ideas about proof that are ultimately re ected in the current notion of formal mathematical proof, based on the attempt by Hilbert, Russell, etc. to "clarify" the foundations of geometry (i.e. *Elements*). This was hardly the first such attempt—because of the weight attached to the *Elements* by Proclus, and the subsequent state patronage extended to it by two caliphs, people have continuously sought to clarify the obscurities in the *Elements*, and bring it in line with their philosophy, since the 9th c. CE. The crux of the formalist "clarification" of the *Elements* is to eliminate the empirical from mathematical proof. Thus, Proposition 4 of the *Elements*, which appeals to an empirical procedure, has been replaced by a *postulate* (the SAS postulate), entirely eliminating the empirical from the *Elements*. Secondly, the now-embarrassing notion of equality was replaced by the notion of "congruence", eliminating the political component of equity, in line with the belief in inequity in Augustinian theology (which Proclus had sought to confront). Finally, "reason" itself was reinterpreted mechanistically to suit the new theological vision of the cosmos as God's clockwork, mechanically obedient to the laws of God. (Since Augustinian theology supposed a transcendent God, repeated providential intervention went against morality by making God too powerful. Hence, it was thought that God "remotely" controlled the world through rigid laws of cause and effect that came into operation after the creation of the cosmos.) Accordingly, rational deduction was redefined to mean *not* the application of the creative faculty of intelligence (*aql*), as in Islam, but a process that can be *mechanically* checked by a moron, or a machine, eliminating also the relation of mathematics to the human mind, as in Proclus or in Islamic rational theology. Thus, Hilbert's notion of proof is derived from the Procluvian notion of proof by eliminating all empirical, political, and human significance in the latter, and bringing it in line with later-day Christian theological beliefs.

Hilbert's reinterpretation of the *Elements* does not, however, appear to be sound, since Hilbert's synthetic axiom set for the *Elements*, and the reinterpretation of equality as "congruence" clearly fails beyond Proposition 1.35 where equality in the *Elements* refers to incongruent but equal *areas*, as in the "Pythagorean" theorem. In contrast, Birkhoff's metric axiom set for the *Elements* reduces to triviality the theorems in the *Elements* and their particular arrangement. Thus, Hilbert's interpretation does not fit the entire *Elements*, while Birkhoff's interpretation trivializes the *Elements*. Hence, neither interpretation can be regarded as valid. Thus, the claim that the *Elements* related solely to deduction is both historically and philosophically unsound.

The net result is the present-day definition of mathematics-as-proof that is dubiously "linked" to the "Greek" way of doing mathematics, although all human, religious, political, and empirical significance is stripped from Proclus' approach to mathematics in the present-day formalistic approach which equates mathematics with a ritualistic way of manipulating a grammar of unreal and meaningless symbols to make rational deductions that can be mechanically checked by morons or machines, and are sought to be imposed as universally valid. All this has not only destroyed the aesthetics underlying the Neoplatonic vision of mathematics, but has resulted in making it near impossible to teach geometry to children, and these difficulties are re ected in current school texts.

Indian school texts have further confounded traditional Indian geometry with formal. The humble rope of traditional Indian geometry however scores over both the straight edge and collapsible compasses of synthetic geometry and the ruler and compasses of metric geometry since it enables the direct measurement of the length of curved lines—a feature critical to the development of the calculus.

I
INTRODUCTION

As argued in the general introduction to the book, it is imperative that the history and philosophy of science be considered together. To write the history of science or mathematics we first need to know what constitutes "science" or "mathematics". The typical approach today takes the meanings of these terms as (a) unproblematic and (b) universal, at least so far as the historian is concerned. However, the resulting history of mathematics is as unsatisfactory as the history of mathematics resulting from the *definition* of mathematics as something invented in Greece.

Preposterous though it seems, we are, today, not far from such a definition. For, a natural way to decide the nature of mathematics is to refer to a professional mathematician (although few professional mathematicians spend their time re ecting upon the nature of mathematics engrossed as they are in *doing* what they consider mathematics). A professional mathematician today sees his job as that of proving theorems, so that proof is central to mathematics today. The current notion of mathematical proof, as proposed by Hilbert, arises from a reinterpretation of "proof" as understood in certain "Greek" works—particularly those attributed to Euclid. A notable feature of this notion of proof is that it excludes the empirical from mathematics—in striking contrast to other cultures, as we shall see in the next chapter. Accordingly, if we accept the present-day socially dominant definition of mathematics as unproblematic and universal, there is nothing much to write about the history of mathematics in other cultures, for whatever it was that transpired in other cultures, it could not have been mathematics as currently understood.

Hence, if at all there is anything to write about the history of mathematics in India, it is essential to begin it by re-examining the current notion of mathematical proof. As a first step to this end, the actual historical genesis of the current notion of mathematical proof helps to understand it better.

The Historical Origin of Mathematical Proof

The current notion of mathematical proof is regarded as having originated in

> Euclid, who brought together the *Elements*, collecting many of Eudoxus' theorems, perfecting many of Theaetetus', and also bringing to irrefragable demonstration the things which were only somewhat loosely proved by his predecessors.[2]

The key point here is the "irrefragable demonstration"; for that is what a mathematical proof is today believed to represent—something incorporating *necessary* truth, universally valid and beyond reproach for all time to come.

This irrefragable demonstration ("as certain as 2 plus 2 is 4") was supposed to have been achieved by virtue of the *arrangement* of the theorems in the *Elements*, so that the proof of each theorem relied only on the statements already proved in the preceding theorems. (The modern-day notion of mathematical proof is somewhat similar; and though mathematical *theorems* are no longer believed to incorporate necessary truth, since axioms may be arbitrary, mathematical *proof*, which connects the theorems to axioms, is still believed to incorporate necessary truth.) It is clear from the above quote that Proclus does not regard this Euclid as having originated the mathematical theorems with which the name is today associated, but rather Proclus (or whoever it was who penned the remark) regards Euclid as chie y responsible for their particular arrangement.

The better to understand this remarkable philosophy, which seeks to locate truth in the harmonious arrangement of things, one may want to know about the socio-political context in which it originated. What was Euclid's historical context?

Euclid the Geometer: A Name or a Person?

What is known at present . . . about . . . "Euclid"? Nothing.

David Fowler[3]

Unfortunately, we seem to know nothing at all about Euclid's historical context. Indeed, it is not so clear that there was any actual person called Euclid who wrote the *Elements*. The only Euclid known to classical Greek tradition was Euclid of Megara, a contemporary of Plato. When medieval Europe first came to know about the *Elements* and Aristotle from the Arabs, Europeans thought the term "Uclides" (which some Arab sources had explained as "key to geometry" from *Ucli*=key + *des*=space, measure) was a reference to the person Euclid of Megara. This baseless belief about this standard text was taught in universities such as Paris, Oxford, and Cambridge for some five *centuries:* the first English translation of 1570, for instance, attributed the *Elements* to Euclid of Megara.[4] The scholarship of the late nineteenth century has, however, veered around to the view that it was impossible that Euclid of Megara could have been the author. The reasons for this shift need to be made quite explicit.

If one discounts Arab sources and later Byzantine Greek sources, as Heath does, our belief in the historicity of Euclid has a very fragile basis. Whether one believes that "nothing" is known about Euclid or that "nothing much" is known about him depends upon how seriously we take the following remark about Euclid, attributed to Proclus. The remark is not particularly definite about Euclid, for the language admittedly shows that the author of the remark is the first to speak of Euclid, and is proceeding on speculative inferences about events long before his time—and some 750 years before Proclus:

> All those who have written histories [of geometry] bring to this point their account of the development of this science. Not long after these men [pupils of Plato] came Euclid. . . He *must have been* born in the time of the first Ptolemy, for Archimedes [who comes after the first Ptolemy] mentions Euclid; and further, they say that Ptolemy once asked him if there was in geometry any shorter way than that of the *Elements*, and he answered that there was no royal road to geometry. He is then younger than the pupils of Plato but older than Eratosthenes and Archimedes; for the latter were contemporary with one another, as Eratosthenes somewhere says.[5]

Although attributed to Proclus, the actual source of this key remark about "Euclid" is a manuscript called "Monacensis 427".[6] Since the manuscript is on paper, and since the first paper mills started in Europe only towards the 13th and 14th c. CE,[7] the manuscript is quite likely from a later period, as we shall see, but has been dated, with the usual optimism to the earliest horizon of 10th c. CE. Thus our key source of information about "Euclid" is the above vague remark from an undated manuscript which comes realistically from 1600–1900 years and optimistically from 1300 years after this "Euclid" allegedly lived. Apart from just this one reference to "Euclid", the rest of the text tells us nothing serious about his philosophy. How should this evidence be interpreted?

There are two questions here:

1. Is this remark actually due to Proclus?
2. If so, why doesn't the text contain any further elaboration of "Euclid's" philosophy?

Heath does not raise Q. 2. He does not explicitly raise Q. 1 either, but uncritically presupposes its answer is in the affirmative. His concerns now are the following.

Proclus himself lived some seven hundred and fifty years after this "Euclid". If Proclus is right and Euclid was much younger than the pupils of Plato, then he could not possibly have been Euclid of Megara, a contemporary of Plato. If, however, Proclus is wrong about the date of Euclid, we could well conclude that he was also confused about the person, in this vague paragraph, so we would be left with no basis to believe in any person called Euclid. (As Heath points out, the story about there being no royal road to geometry has been told also about Alexander and Menaechmus; the relation of this story about political equality to the geometric equality in the *Elements* is considered later.)

From the historiographic angle, the confounding of Euclid of Megara with Euclid the supposed author of the *Elements* is interesting. While the occurrence of such a mistake is understandable, its persistence for five centuries is not. The persistence of this error for centuries shows that that stories about "Euclid" were propagated, by historians in Europe, exactly in the uncritical manner of myth.

Prior to Proclus, this Euclid, if at all there was such a person, did not have the stature that he acquired in later times through the combined in uence of Islamic and Christian rational theology, and colonial history.

For example, Theon of Alexandria (4th century CE) does not mention Euclid. but does refer to *his* book on the *Elements*:

> that sectors in equal circles are to one another as the angles on which they stand *has been proved by me in* my *edition of the Elements*. . . .[8]

It is believed that Theon's work on the *Elements* was completed by Hypatia, and the fact is that almost all known texts of the *Elements* are "Theonine" in origin. That is, as Heath[9] points out, all Greek manuscripts of the *Elements*, up to the 19th c., state in their titles that they are either "from the edition of Theon" or "from the lectures of Theon". The solitary exception is a single manuscript in the Vatican, for which we have Heiberg's word and fertile imagination to establish that it derives from an earlier version (even though it comes to us from a manuscript physically much later than the earliest Arethas text of 888 CE).[10]

So, is the alleged quote from Proclus adequate to establish the historicity of Euclid or the antiquity of the *Elements*? Imagine for a minute that we are dealing with Arab tradition. Although the earliest Arabic sources do not mention "Euclid" at all, al Qifti informs us that Euclid was domiciled at Damascus, and born at Tyre. Because this suggests that Euclid might have been Arabic (or horrors, a black Egyptian!) this is dismissed by Heath with some racist comments. More seriously, we could ask: what, after all was the source that al Qifti had? And in the absence of sources that can be cross checked, why should we believe al Qifti?

Considering that Proclus comes some 750 years after "Euclid", and could not have had any direct knowledge of "Euclid", the same logic can and should be applied to Proclus. There is no reason why we should believe this remark, without a knowledge of Proclus' sources. However, Proclus has no clear cut source of information about Euclid, but is proceeding indirectly by inference.

> All those who have written histories [of geometry] bring to this point their account of the development of this science. Not long after these men [pupils of Plato] came Euclid...

The logic is this: since this Euclid is NOT mentioned by earlier historians of geometry he must come after them. The only other source Proclus (or whoever authored the above remark) has for these events from at least 750 years before his time is the claim that Archimedes mentions Euclid (as the author of the remark believes), Euclid must come after those earlier sources, and before Archimedes.

As if this were not bad enough, it is surprising that Proclus, who dwells at great length on his own philosophy and that of Plato, should have nothing further to say about the philosophy of this Euclid on whose book he is supposed to be writing a commentary, especially since the Platonic philosophy of mathematics is so very different from the philosophy of "irrefragable demonstration" attributed to "Euclid". The above remark is, therefore, an isolated remark.

We do not have to rely entirely on this "scriptural method" of analysing stray remarks which allude to further stray remarks—all of which are of doubtful authenticity. There is the archaeological evidence of papyri. Despite the vastly in ated claims of the "Hellenic" scientific achievements, there is a great paucity of anything that could be called "scientific" in the thousands of papyri recovered from Alexandria in Africa, and apparently only some three papyri from these thousands (and 0 out of the 2 recovered from Greece proper) relate to geometry.[11] These three fragments are believed to date from the 2nd to the 4th c. CE, and correspond to about 60 full lines of text of the *Elements*, together with some fragmentary information on another 60 lines. However, *none of these available fragments follows the "received" text,*[12] or the current text of the *Elements*. Had there really been a "Euclid", who compiled a definitive book on geometry called the *Elements* as early as −300 CE, then a standardized text of the *Elements* ought to have been subsequently prevalent.

The evidence however suggests to the contrary that no definitive text of the *Elements* was prevalent for the next seven hundred years, until the 4th c. CE. This, of course, creates further problems for the "Euclid" hypothesis. If this "Euclid" was truly so little known for so long, how did his version of the *Elements* survive even until the time of Proclus? It would be difficult enough, even today to source a text by an obscure author from 750 years ago, and it was obviously more difficult in the time of Proclus who lived in the times of papyri technology, and repeated book burning ordered by the state.[13] On the other hand, if the text did survive, given the fragility of papyri, the text would have had to be repeatedly copied out by hand, by different scribes over the centuries. Accordingly, a number of different people must have been willing to invest money and time in it, to have it copied out, so that this Euclid ought to have been famous long before Proclus.

Therefore, the archaeological evidence refutes the "Euclid" hypothesis. Of course, it is well known from the philosophy of science that any evidence whatsoever can be made consistent with any theory whatsoever by introducing enough auxiliary hypotheses—e.g., it has been argued that the recovered papyri relate to someone writing out the *Elements* for practice, making many mistakes (!) etc. Similarly, the discrepancy between the Monacensis remark about "irrefragable demonstration" and the rest of Proclus' text is "explained" by saying that Proclus was a bad mathematician, or that he sought to impose his philosophy on Euclid's. That is, we weigh the remark and the rest of the text side by side, and find the remark about "Euclid" heavier! There is not the slightest doubt that every piece of empirical evidence can be explained away by one who wants to hang on to the myth of Euclid, just as every piece of evidence against astrology can be explained away by those who make a living from it. The point is that this makes the claim about "Euclid" as irrefutable as any other myth, and each piece of contrary evidence, to be explained away, needs an auxiliary hypothesis, so that there is an accumulation of hypotheses.

Then there is the question of the date of this "Euclid". First of all, this is intrinsically improbable. The time of Ptolemy I was a time of constant strife, and hardly conducive to scholarship. Ptolemy I, who became satrap of Egypt on Alexander's death, had an army of only 4000 people. With this small force he was busy fighting numerous wars, and also placating the Egyptians whose unhappiness with the earlier Persian rulers had toppled them, and helped Alexander win Egypt without a blow. Since Ptolemy I remained preoccupied with pressing affairs of the state, the bulk of Alexander's loot of books lay neglected in Alexandria,[14] and was first catalogued only by Callimachus at the time of Ptolemy II. Therefore, whether the word "γ γo " in the Monacensis remark is translated as "was born", or as " ourished", Euclid (if he existed) would probably have been drafted into Ptolemy's army—for conditions were hardly appropriate in the time of Ptolemy I for scholarship to ourish. Interestingly, some historians have indirectly acknowledged this difficulty which so constrains possibilities that the only possibility that it leaves open is that Euclid was born exactly in −325 CE!

Secondly, the Monacensis remark fixes the date of Euclid by claiming that Archimedes refers to this Euclid. Of course, we know even less about the works of Archimedes, than we do about "Euclid", but in the late (16th c.) text of the *Sphere and the Cylinder*, somehow attributed to Archimedes, from some 1800 years earlier, there is a reference to the *Elements*, though not to Euclid.[15] This isolated reference has been regarded as spurious[16] for the reason that it was not the custom in the time of Archimedes to make such references to texts (naturally, since "standard editions" did not exist prior to the use of print technology for mass producing books). This was also not the custom among Arab scholars (standard editions did not exist even in 9th c. Baghdad, as clear from the book-bazaar attempts towards standardization in the *Fihrist*), but such citations were the custom especially in later-day Christian theology. Moreover, there are many other occasions on which a reference could have been made. Therefore, whosoever may have been the author(s) of the "Archimedes" text, that reference to the *Elements* in it certainly was not due to Archimedes.

However, if the reference to the *Elements* in the "Archimedes" text was spurious, and the author of the Monacensis remark was familiar with that spurious reference, he must post-date that spurious reference. That would place the Monacensis remark some time in the 16th c. when Byzantine Greek texts arrived in bulk in Europe.

Therefore, from the present non-Western perspective, the least one can do is to explore alternatives to the traditional belief in the historicity of Euclid and thereby arrive also at the proper philosophy of the *Elements*.

The first and most likely possibility is that, since the Monacensis remark fits so uncomfortably into the rest of Proclus' text, since the earliest date we can assign to it (the manuscript in which the remark is found, or even the author to whom its text is attributed) is long after "Euclid", and since the author of remark tries to infer the date of "Euclid" from the failure of earlier authors to mention "Euclid", and since the papyri evidence gives no indication of a definitive text prior to the 4th c. CE, and the remark shows awareness of a late spurious interpolation, the Monacensis remark must itself be an interpolation by some later-day scribe.

Such forgeries were common enough: for example, the Vatican owes its origin and special status to one such document, called the "Award of Constantine" today acknowledged by all concerned as a forgery. So unenviable was the reputation that priests had acquired in this matter that Isaac Newton spent 50 years of his life trying to undo the forgeries that he thought various priests had incorporated into the Bible, to serve their temporal ends. And the only answer to his scholarly and voluminous accusations was to hide them for some 250 years—in fact they still remain secret.[17]

Having been a naive victim of the trick by historians of referring to the Monacensis remark as "Proclus' remark", I had earlier thought that Proclus himself might have invented "Euclid", to escape religious persecution by Christians, but in view of the above argument, it now seems unlikely that Proclus was the author of that remark. It now seems that the author of the Monacensis remark was merely repeating, with an added dash of "expertise", an earlier story about "Euclid". However, the practical reasons for propagating this story remain the same—to escape religious persecution by the Christian church. Proclus had been declared a heretic. A death penalty was legally prescribed for heretics, since the time of Justinian, and the Inquisition made sure that the death would be painful. Mere possession of a heretical work was ample and complete confirmation of guilt in the days of the Inquisition, when a person was presumed guilty until proven innocent, and when even children were sentenced to death if it was discovered that they had not eaten pork on Friday. Therefore, even copying out a manuscript by a recognized heretical author like Proclus, during the thousand years from Justinian to the Inquisition, would have presented a potentially grave risk to a scribe, against which such an interpolation would have insured the scribe. The name "Euclid" made clear that Proclus' commentary was not on the *Elements* collected by another heretical author like Hypatia, something that a scribe might have had a hard time convincing an Inquisitor about.

Mathematics and Religion

Why should a work on *mathematics* have bothered the church from Justinian to the Inquisition? This point needs to be made clear since the presumption today is that mathematics is secular, and universal, and unconnected with religion.

That however was not the case in the time of Proclus who clearly and explicitly relates mathematics to religion in his prologue. Further, we need to set off Proclus' prologue to the *Elements* against the politics of the Roman empire in his time—with violent priest-led Roman-Christian mobs attacking Neoplatonists, murdering the most brilliant among them like Hypatia, and invoking state-support to smash or takeover Neoplatonic places of worship,[18] and burn down the Great Library of Alexandria.[19]

In this heated religious context, mathematics was viewed not as a "universal" or "secular" science, but as a key vehicle to propagate the religious and political philosophy of what is today called Neoplatonism. The chief aim of Proclus' prologue to the *Elements* is to bring out this dimension of mathematics which he felt was neglected by some of his contemporaries.

> Pythagoreans recognized that everything we call learning is remembering, ... although evidence of such learning can come from many areas, it is especially from mathematics that they come, as Plato also remarks. "If you take a person to a diagram," he says [Phaedo 73b], "then you can show most clearly that learning is recollection." That is why Socrates in the Meno uses this kind of argument.

> This part of the soul has its essence in mathematical ideas, and it has a prior knowledge of them....[20]

The famous Socratic argument, which sought to establish reincarnation, using mathematics, was as follows.

> The soul, then, as being immortal, and having been born again many times and having seen all the things that exist, whether in this world or in the world below, has knowledge of them all; and it is no wonder that she should be able to call to remembrance all that she ever knew about virtue and about everything; for as all nature is akin, and the soul has all things, there is no difficulty in her in eliciting or as men say learning out a single recollection all the rest, if a man is strenuous and does not faint; for all enquiry and all learning is but recollection.[21]

Socrates then gave a practical demonstration of this by questioning a slave boy and eliciting first the wrong responses, and then the right responses regarding geometry. The wrong responses demonstrated that the slave boy was untutored, while the right responses demonstrated that he nevertheless had an intrinsic knowledge of mathematics. The untutored slave boy's innate knowledge of mathematics, according to Socrates, thus established the existence of the soul and its past lives.

What Proclus is explaining here ("That is why Socrates in the Meno used this kind of argument.... ") is why Socrates specifically used mathematics (and not some other form of knowledge) to demonstrate that learning is reminiscence—because he thought mathematics incorporates eternal truths, and, as in sympathetic magic, the soul being eternal is specifically attracted to these eternal truths.

This belief that knowledge of mathematics was innate, and that this demonstrated the past (and future) lives of the soul, or reincarnation, was embedded in the view of a recurrent cosmos. This cosmology directly went against the key ideas of resurrection, creation, and apocalypse that were the cornerstones of the new Augustinian doctrine of the state-church. This is why Justinian and the fifth ecumenical council pronounced it as anathema.[22] And that is why Nietzsche (although misled by Augustine into confounding quasi-recurrence with eternal recurrence) made cosmic recurrence the basis of his anti-Christian stance.[23]

The issue of images was already so much a burning point of confrontation with Christians, given their attacks on idols and "idolatry", that Porphyry had written an entire book *On Images*,[24] where he sought to explain that the idols in temples are like books written in stone:

> ...images...sketch invisible things in visible forms.... To those who have learned to read from statues as from books I will show the things there written concerning the gods. Nor is it any wonder that the utterly unlearned regard the statues as wood and stone, just as also those who do not understand the written letters look upon...books as woven papyrus.

By Proclus' time, after the destruction of Serapis, this issue of idols must have been a very sore point indeed. Look at the amount of upheaval today caused by the destruction of the semi-abandoned Babri Masjid—and Serapis was the most magnificent place of worship in the Roman empire. It is, therefore, not incidental that every known text of the *Elements* makes liberal use of images or geometrical diagrams. These images are, from the viewpoint of current mathematics, inessential. From the strict formalist perspective they are even misleading, for the images of points and lines could be replaced by those of coffee mugs and coffee tables. However, it would be conceded that the existence of images makes the proofs so much easier to follow: images help learning. For Proclus, mathematics was the science of learning, and the figures helped learning, just because they served to move the soul (in a way that the sight of coffee mugs would not). That is why, explains Proclus, Socrates in *Meno* drew a diagram.

For Proclus, then, mathematics was not a "secular" activity, but was, like *haṭha yoga*, a key discipline which prepared a person for the ultimate religious experience: encounter with the immanent Nous within oneself. This is the concluding thought of part I of his prologue:

> This, then, is what learning (α θ [mathesiz]) is, recollection of the eternal ideas in the soul; and this is why the study that especially brings us the recollection of these ideas is called the science concerned with learning (αθ α [mathematike]). Its name thus makes clear what sort of function this science performs. It arouses our innate knowledge... takes away the forgetfulness and ignorance [of our former existence] that we have from birth,... fills everything with divine reason, moves our souls towards Nous,... and through the discovery of pure Nous leads us to the blessed life.[25]

This belief in immanence was linked to equity as also is the case in the Mutāzilā or Islamic rational theology (*aql-ī-kalām*), the sufi's, and Advaita Vedantists for that matter (as in the story of Śaṅkara and the *cāndāla*, or as in Śrī Nārāyaṇa Guru's interpretation). Of course, the Mutāzilā described themselves as people of *tauhīd* (unity) and *adl* (justice). And it is hardly a matter of surprise that so many of the theorems in the *Elements* relate to the equality of things that are superficially different. The story of there being no royal road to geometry is thus a mystery story about how the key to geometry is the teaching of equity.

Accordingly, mathematics was for Proclus a key means of propagating his fundamental religious beliefs. The belief was that everyone had an eternal soul, and had gone through various earlier lives; that, in the course of mundane existence, people forgot this divine element within themselves; and mathematics served to remind people of their souls and to draw their minds inwards. Mathematics was, for Proclus, an instrument to arouse one's innate spirituality—it was like an advanced Egyptian Mystery.

Proclus and Origen

By Proclus' time, well after Constantine, the environment for these specific religious beliefs, within the Roman empire, had turned excessively hostile. Thus, the idea of all learning as recollection, as e.g. propounded by Socrates, involved the idea of a soul that had experienced a variety of past lives. In the terminology of Christian theologians, this has nowadays come to be known as the doctrine of pre-existence, and its original form is not very different from what is also known as the doctrine of *karma-saṁskāra*.

These very same religious beliefs ("pre-existence", *karma*) were earlier championed *within* the Christian church by Origen of Alexandria. Origen believed:

> Every soul...comes into this world strengthened by the victories or weakened by the defeats of its previous life. Its place in the world...is determined by its previous merits or demerits. Its work in this world determines its place in the world which is to follow this.... The hope of freedom is entertained by the whole of creation....[26]

He cited the scriptures in his support:

> this world, which is itself called an age, is said to be the conclusion of many ages. ...that after this age, which is said to be formed for the consummation of other ages, there will be other ages again to follow, we have clearly learned from Paul himself....[27]

Origen clearly discriminated between quasi recurrence, and its stock misrepresentation in the West as eternal recurrence, since Augustine:

> So therefore it seems to me impossible for a world to be restored for the second time, with the same order and with the same amount of births, and deaths, and actions; but that a diversity of worlds may exist with changes of no unimportant kind, so that the state of another world may be for some unmistakeable reasons better (than this), and for others worse, and for others again intermediate. But what may be the number or measure of this I confess myself ignorant, although, if any one can tell it, I would gladly learn.[28]

Furthermore, though this might seem a little strange today, Origen quite explicitly related this belief in "cyclic" time to equity and justice:

> In which certainly every principle of equity is shown, while the inequality of circumstances preserves the justice of a retribution according to merit.[29]

That is, in Origen's view, God demonstrated the principle of equity by creating all people equal, and also demonstrated his justice by rewarding and punishing them suitably in future lives, according to merit—and that accounted for the observed inequality of circumstances. Thus Proclus and Origen had similar beliefs, which is not surprising, since they belonged to the same school.

However, by Proclus' time, these religious beliefs ("doctrine of pre-existence", equity) were exactly what were being abusively targeted and cursed by the church and its key ideologues (Augustine, Jerome, Justinian). Fundamental aspects of present-day Christian religious dogma, such as resurrection (as opposed to reincarnation) creation (as opposed to "pre-existence"), apocalypse (as opposed to an eternal cosmos), *eternal* (as opposed to temporary) heaven and hell, inequity (as opposed to essential equity), transcendence (as opposed to immanence), faith (as opposed to reason) etc., came about from the rejection of Origen and the acceptance of Augustine during this period, starting from Constantine (4th c. CE) and ending with Justinian (6th c. CE).[30]

The reason for this theological transformation was very simple: the church had turned imperial, and equity (which made Christian souls equal to non-Christian souls) went against its imperial objectives to which its theology had to be adapted. Moreover, by the time of the emperor Julian, the priests, through temporary loss of power, recognized the insecurity of ruling without weapons. Accordingly, they converted the doctrine itself into a weapon intended to strike superstitious terror in the hearts of simple folk. Reincarnation (repeated lives after death), which guaranteed eventual "deliverance" for all, made the priest irrelevant, and was hence rejected in favour of resurrection (life after death just once). Immanence which made the priest an intruder in the communion with oneself, was rejected in favour of transcendence, where the priest could legitimately claim a role in brokering salvation, etc.

The educated Romans, however, simply refused to buy any of this, and refused to turn Christian. Instead they used reason to question the aggressive advocacy of blind faith. For example,

> referring to Mark 16:18, Porphyry writes: In another passage Jesus says: "These signs shall witness to those who believe: they shall lay hands on the sick and they shall recover. And if they drink any deadly drug, it will hurt them in no way." Well then: the proper thing to do would be to use this process as a test for those aspiring to be priests, bishops or church officers. A deadly drug should be put in front of them and [only] those who survive drinking it should be elevated in the ranks [of the church].
>
> If there are those who refuse to submit to such a test, they may as well admit that they do not believe in the things that Jesus said. For if it is a doctrine of [Christian] faith that men can survive being poisoned or heal the sick at will,

> then the believer who does not do such things either does not believe them, or else believes them so feebly that he may as well not believe them.[31]

Also, people like Porphyry were not intimidated by the priestcrafty trick of an assumed moral superiority, as in the charge of idolatry laid by Christians against them. On the contrary, we have seen how Porphyry responded by arguing that Christians were intent upon destroying idols just because they were uneducated.

The church reacted by adopting a systematic policy of inciting mob and state violence against non-Christians. What happened in Alexandria was no isolated or incidental outburst of emotion—for the same things happened across the space of the Roman empire. Moreover, even some twelve centuries after what happened in Alexandria, exactly the same policy of petitioning the state to destroy all temples, or to exile or violently torture dissenting individuals etc. was repeated in Goa, in the 16th c. CE.[32] The policy of systematic violence was, in turn, morally justified by denigrating all non-Christian cultures, save only the (early) Greeks.

How did the Alexandrian philosophers respond to the violence incited by the priests? Naturally, it would have been incompatible with their philosophy to respond violently to violence. Since they regarded knowledge as the source of virtue, their natural response would have been to try and educate the Christians. What better way to spread knowledge than to use the science of learning—mathematics? Therefore, Proclus, like his predecessors Hypatia and Theon, in choosing to focus on the *Elements*, was responding to an urgent need of his times. Unlike Porphyry, Proclus' approach is more indirect, in that he does not once mention Christianity. However, Proclus' commentary emphasizes how the *Elements* brings out, point by subtle point, all the key elements that refute the revised Christian doctrine: reason vs faith, past lives vs creation, reincarnation vs resurrection, immanence vs transcendence, equity vs inequity, images vs charges of idolatry.

We also know that Proclus argued explicitly against both creation and apocalypse. Proclus wrote a book *On the Eternity of the World* giving some eighteen arguments to this effect.[33] Proclus' notion of the soul, like Origen's was related to the notion of "cyclic" time. What is the relation between cyclic time and the eternity of the soul? Proclus explains this in *his* book *Elements*, nowadays also called *Elements of Theology*.[34]

Thus, Proclus' understanding of mathematics, as incorporating eternal truths, entailed also an eternal cosmos in direct con ict with the "end-of-the-world-is-near" fear of apocalypse the ruler-priests wanted to peddle to promote their power.

Justinian responded by shutting down all schools of philosophy in the Roman empire, and instituting a legal death penalty on heretics, i.e., all those who dissented with the church. We also know that the Christian theologian John Philoponus responded with a book called *On the Eternity of the World: Against Proclus*, defending the Christian view of apocalypse against Proclus, who had been declared a heretic.

It is, thus, clear that the mathematics of the *Elements*, according to Proclus' understanding, was right at the eye of the vicious religious and political storm that attended the transformation of Christianity in the two centuries between Constantine and Justinian.

The Doctrine of Cultural Purity

These circumstances also help to understand how and why the myth of "Euclid" might have been fabricated. The imperial church, sought universal domination, and the physical elimination of all opponents. Unlike the Nazis, who did not succeed in either objective, the imperial church did succeed in physically liquidating a large number of people. Starting with the elimination of "pagans" in the Roman empire, and then in Europe, this was followed by the elimination of Muslims in Europe, and the purges of Jews. Encouraged by these "successes", the 15th c. papal bulls (still in force, see Chapter 6), explicitly called upon Christians, as their religious duty, to kill, enslave, and rob non-Christians, as actually happened in the subsequent multiple genocides proper in the two Americas and then Australia.

The attempts to physically liquidate all non-Christians and dissenters, were accompanied by the attempt to physically eliminate their thoughts. Hence, burning "heretical" books remained continuously on the church agenda for over a thousand years. Theodosius and Valens ordered the burning of "pagan" books in the Roman empire, while Louis IX in 1248 ordered the burning of all Hebrew books in Paris, and the "Synod of Diamper" burned the Indian Aramaic Bibles in 1599.

It is not surprising, therefore, that church historians sought to physically eliminate from history any significant role for non-Christians. (This is not a medieval matter: in most Western universities today, the history of science means, *de facto*, the history of science in the West. This is true not only of teachers, but also of researchers—most conference organizers quietly assume that the history and philosophy of science is synonymous with the history and philosophy of Western science.)

These triumphalist Christian attitudes were put to severe test at Toledo, when the works of Muslims in Arabic books started being translated for Christians into Latin. This was during the Crusades, when the church had whipped up intense religious hatred. The church having proclaimed the superiority of Christians for centuries, many Christians felt ashamed about openly acknowledging the achievements of others, and felt embarrassed about having to learn from books written by Islamic Arabs. This was especially the case in learning from the Islamic enemy during the Crusades. This sense of shame and shock is illustrated by the following remark of Daniel of Morley, a Toledo translator.

> Let no one be shocked if, with reference to the creation of the world, I should invoke the testimony of pagan philosophers rather than the church fathers.... Let us then borrow from them and, with God's help and command, rob the pagan

> philosophers of their wisdom and eloquence. Let us take from the unfaithful so as to enrich ourselves faithfully with the spoils.[35]

An obvious strategy to remove this sense of shame was to modify history to make the origins of this knowledge more palatable. It was in this context that a systematic attempt was first made to fabricate ancient Greece. The early Greeks (as distinct from later Greeks like Theon, Proclus etc.) were regarded as the theologically correct predecessors of Christianity, since Eusebius. Accordingly, the story was told that Greeks were the real originators of all knowledge at Toledo (and elsewhere), and the Arabs were depicted as mere passive transmitters. The Christians were not learning from the Muslims, they were only getting back their own stuff from the Greeks—knowledge which they had lost during the Dark Ages! For a church accustomed to propagating all sorts of fabulous and implausible propositions, it was no difficult task to propagate such a historical doctrine of "Hellenization". To demonstrate the "Hellenic" origin of all knowledge up to the 11th c. CE, one or two talking points were regarded as adequate, and for such talking points, a few stray remarks here and there, forged where necessary, were thought to be sufficient. The church had long experience in fabricating history, since Eusebius—who openly advocated it. While adequate for the believer, the sort of evidence on which this history is built is obviously unacceptable to the sceptical. (For more details, see Appendix 1.A.)

Apart from the shame, there was also the fear that Toledo was a Trojan horse, which would spread heresy, and we will see later in this chapter how this fear was handled by the additional process of Christianization of selected texts through reinterpretation. This reinterpretation drastically changed the understanding of the texts, transforming also mathematical philosophy in the process.

A definite answer to the question of exactly when and why "Euclid" was invented, however, requires further historical investigation.

Under the circumstances that prevailed from roughly Justinian to the Inquisition, almost anybody from Proclus onwards could have invented the name "Euclid" to provide an acceptable "Greek" ancestry to this thought, and thus de ect religious persecution.

However, to suppose that this actually happened in the Byzantine empire would be to put the cart before the horse. How do we know that the text of the *Elements* at all survived in the Byzantine empire between Proclus and the Arethas text of 888 CE? (The question seems not to have been raised earlier.) Clearly there is no evidence in this direction, and this is not very probable, given the repeated edicts by Christian emperors, and ecumenical councils to burn books and attack heretics. It is, therefore, unlikely that the *Elements* survived in the Byzantine empire, after Proclus.

It is rather more likely that the *Elements* followed the trail of the intellectual diaspora of the Alexandrian philosophers as they shifted from Alexandria to Athens, and then, after 529 CE, relocated to Jundishapur, in Iran, under Khusrau I Anushirvan ("Immortal soul"). Here they stayed for over a couple of centuries, before trickling into Baghdad from the time of Haroun al Rashid, and then at the invitation of Khalifa al Ma'mūn (9th c. CE), which is when we next hear of the *Elements*.

At Jundishapur, the Alexandrian philosophers re-established the Alexandrian model first established in the time of Ptolemy II of Alexandria, and again started importing and translating knowledge from all over the world. In particular, Khusrau sent the physician Burzoe (Peroze) to India to bring back Sanskrit texts for translation. Jundishapur was where the *Pañcatantra* stories were first translated into Pahlavi as *Keliléh va Demneh*. Indian astronomy texts, too were imported and translated as the *zij-i-Shahryar*. Peroze also brought back the game of chess.[36] Both the *Pañcatantra* and chess were regarded as useful for the education of kings, the one to teach them justice, and the other to teach them strategy. Khusrau's successor, Khusrau II, was also famed as a patron of culture, and continued to support the activity of the philosophers at Jundishapur.

Therefore, it is to be supposed that the *Elements* was translated from Greek to Pahlavi (in Jundishapur), to Arabic (in Baghdad), and then back to Byzantine Greek. It is possible that the Arethas text of 888 CE too is a result of such multiple translations, for the text dates from over half a century after the formation of the Baghdad House of Wisdom. In fact, it is strange that the to-be-Archbishop of Caesarea openly commissioned a scribe to copy out a heretical work, and even recorded this in his copy for all to see, thus endangering his future ambitions in the church hierarchy. Even if the Alexandrian Greek text of the *Elements* somehow survived in Greek, it is clear that the texts derived from Arabic would have been the ones that were more easily accessible.

The point here is, however, a bit different. Unlike the 12th c. CE translations from Arabic to Latin at Toledo, attributed to people like Gerard of Cremona who knew neither Arabic nor mathematics, but nevertheless translated the *Elements* from Arabic into Latin, the translations at Alexandria-Jundishapur-Baghdad-Antioch were done by more knowledgeable people. Nevertheless, these (multiple) translations did result in Toledan howlers like the term "sine" or the term "surd" or "deaf numbers" for numbers like $\sqrt{2}$, as we shall see. Therefore, it is quite possible that, in the course of these translations, the epithet "Uclides" attached to the *Elements* was misinterpreted as the name of a Greek author.

Is the Existence of Euclid Important?

What difference does it make whether Euclid was real or invented? Whether Euclid was an invention or a real person makes a great deal of difference from the "cultural purity" angle, to those who seek to establish the Greek origin of geometry. This is the reason why present-day Western scholars want to hang on to this "Euclid", and make him the star figure of the *Elements*, even though nothing definite is known about this Euclid. From the name we cannot, of course, decide the colour of the skin, either for Euclid or for Archimedes, but

Arabic claims that contain anything contrary can always be hotly contested. With the same facility as Euclid was asserted to be from Megara, he is now asserted to be from Alexandria.

From our point of view, however, the name "Euclid", followed by a hypothetical date and no further information, is of little use in understanding the historical evolution of the philosophy of mathematics in the *Elements*. Therefore, for our purpose of *understanding* the historical evolution of mathematics, rather than glorifying "culturally pure" European tradition, it makes little difference whether Euclid was real or invented; if we have virtually no information about him, then Euclid is as good as non-existent, and should be treated as such.

In particular, irrespective of whether Euclid was real or invented, the Monacensis remark about his alleged philosophy of "irrefragable demonstration" is obviously a later-day interpolation. On the other hand, Proclus' philosophy of the *Elements*, which fits the *Elements*, and also very well fits his socio-political context, is better regarded as the "original" philosophy of the mathematics in the *Elements*.

However, for use in later chapters of the book, we note here that the long-standing claims of Euclid's existence, and the surprisingly imsy evidence on which they are based, also provide an example of the *de facto* standards of evidence in historiography—standards to decide origin and transmission that should either be uniformly applied elsewhere or rejected here as well.

In particular, there is the erasure of Egypt. Herodotus informs us not only that the Greeks learnt geometry from Egyptians, but that they also borrowed most of their religious practices from the Egyptians. From this perspective, Proclus' philosophy of the *Elements* makes it just a continuation of Egyptian mystery-geometry texts, and there is no clear evidence of what, if anything, the Greeks from Pythagoras onwards added to this tradition.

Can Authorship be Attributed to a Single Individual?

There is another way of looking at the question of authorship. It is clear that, from at least the time of Theon and Proclus, through the Arabic and European rational theologians, right down to the time of Hilbert, Birkhoff, and the US School Mathematics Study Group, there has been a continuous attempt to remove the obscurities in the *Elements*, and to update it, and bring it in line with the philosophy of the updaters. To look for a unique author of the *Elements* is like trying to trace the origin of all the water in a mighty river back to its visually apparent source in a small pond: this transparently neglects the vast underground drainage system that contributes most of the water to the river on its way to the sea.

As for the apparent source itself, Christian Europe got its knowledge of the *Elements* from the decaying Arab empire in Europe, the Arabs got their knowledge of the *Elements* from the decaying Persian empire, where the philosophers of the Roman empire had got sanctuary; the Romans got their knowledge from the decaying Greek empire, and the Greeks, as

Herodotus records, got their knowledge of geometry from the Egyptians. As I have argued, elsewhere[37] and in Chapter 6 the typical pattern is that the direction in which information ows has been *from* the vanquished *to* the military victor, though this fact has often enraged the descendants of the military victors. It has been argued that 18th–20th century CE European historians of science reinvented history in a racist[38] way to make it appear that this entire chain of information transmission had a unique beginning in Greece. In this book I argue that this process of manipulating history had already commenced at Toledo. At any rate, these historians did not represent the (unknown) Alexandrian Greek texts as merely one in a chain of translations and adaptations into English, from Latin and Byzantine Greek, from Arabic, from Pahlavi, from Greek, and from Egyptian and other texts from across the world, but represented the Greek texts as the absolute beginning of this chain—as the original creative fount of practically all human thought! Since the geographical origin of the *Elements* (and all its earliest commentaries) in Alexandria, in the African continent, could hardly be denied, the *name* Euclid, suggesting a Greek legacy, was critical to the process of appropriation via Hellenization.[39]

Why was this appropriation first attempted? Why were the *Elements* so important to the rational theologians of Christianity? This is a complex issue to which we will return when we address the importance of the *Elements* for Islamic rational theology, and for education. However, one point is clear enough. The *Elements* have long formed an important part of the curriculum in Islamic rational theology, then Christian rational theology, and, until quite recently, in modern industrial capitalism. Accordingly, multiple authorship, or the "clarification of the obscurities", in the *Elements*, has proceeded from multiple objectives, and these multiple objectives were often conditioned by the prevailing objectives of education.

II
THE MOST RECENT CLARIFICATION OF OBSCURITIES IN THE *ELEMENTS*

Let us first examine the *most recent* example of clarifying obscurities in the *Elements*, for it was this process that led to the current-day notion of mathematical proof. In recent times, a major step to modify the text and teaching of "Euclidean" geometry was taken in 1957 when the US School Mathematics Study Group issued its recommendations on the teaching of geometry.[40] Those recommendations followed the studies into the foundations of geometry by Hilbert,[41] Russell,[42] Birkhoff,[43] etc. These authors addressed a variety of obscurities in the *Elements*. The most obvious of these obscurities may be put into the following classes.

1. **Unsound definitions:** e.g., those of point, line, plane, etc.
2. **Missing definitions.**
3. **Hidden assumptions:** e.g., the correspondence of lines with real numbers.

In addition to these, there are subtler problems, relative to the current formalistic notion of mathematics, such as

4. **Axioms taken as self-evident truths** (about empirical reality): this is also true of the constructions used in proofs.
5. **Redundant assumptions:** e.g., the parallel postulate becomes redundant if one admits reals and rigid motions, or the notion of distance.

In judging these obscurities in the light of current formalistic mathematics, one must, of course, keep in mind that the present-day formalistic epistemology of mathematics (axiom-definition-theorem-proof) itself historically originated from the analysis and clarification of these obscurities in the *Elements*. Furthermore, one must also bear in mind that there is nothing universal or "natural" about the formalistic approach, and that it is steeped in a particular theological and cultural tradition.[44]

The Unreal and Meaningless as the Sole Concern of Mathematics

The obscurities of type 1 are clear enough. One can define something ostensively (e.g., one can define the word "dog" by pointing to an instance of a dog) or one can define it in other words. In the case of a geometric point, an ostensive definition seems somewhat unsuitable: Platonic philosophy requires that geometry should deal with idealizations that have no real existence. Hence one cannot point to a point. One *can* point to a dot on a piece of paper; but no real entity like a dot can ever correspond to the ideal notion of a geometric point which is required not to have any real existence. As Proclus explicitly points out, even the image of the geometric dot or line that one has in one's mind is tainted by reality.

The alternative is a verbal definition. Consider the definition in the *Elements:* "A point is that which has no part, or which has no magnitude." (The "Heiberg" version has only the first part of this definition.) A person familiar with atoms and magnitudes may not question this definition: but it communicates nothing to anyone else. (Besides, is one talking of *real* atoms here—elementary particles of some sort? The particle which is closest to a point is the electron. But the electron cannot be a Euclidean point, for a circuit around a Euclidean point brings us back to where we started, whereas *two* circuits around the electron are needed to return to the starting point, because the electron has the paradoxical property of half-integral spin.) Clearly, a verbal definition of a non-real notion cannot avoid an infinite regress, for at no point can it terminate in an ostensive definition.

Thus, Platonic philosophy, by its insistence on the non-reality of the ideal, eliminates both possibilities of an ostensive or a verbal definition, and the only option left is that of current formalistic mathematics, which regards the notions of point, line, etc. as meaningless, undefined notions. In other words, the current way of removing the obscurities in the *Elements* is to adopt Russell's definition of mathematics: "Mathematics may be defined as a subject in which we never know what we are talking about..."![45]

Real Numbers and Euclidean Proportions

Obscurities of type 2 are examined later. Obscurities of type 3 are manifest in the very first proposition of the *Elements*. The first proposition constructs an equilateral triangle on a given segment AB. This process involves drawing two circles, the first with centre at A and radius AB, the second with centre at B and radius BA. One obscurity is that the two circles may fail to intersect, in the sense that the point of intersection need not exist in a formal mathematical sense. If points on the circles correspond to (pairs of) rational numbers, there may be "gaps" between them, such as the gaps between the numbers 1, 2, 3. Indeed one is led to expect such gaps since the "Euclidean" approach to proportions suggests a reluctance to use irrational numbers like $\sqrt{2}$. It was the attempt to clarify this obscurity in the first proposition of the *Elements* that led Dedekind to the idea of the real line as something that could be "cut" without leaving any gaps. Needless to say, the formal real numbers, as conceptualized by Dedekind, are something necessarily unreal, for there is no real process by which one can specify or fully name a real number such as π.

The SAS Theorem/Postulate

The other obscurity in the proof of Proposition I.1 is this: why is the radius measured out *twice*? Can't the first measurement of AB be re-used for BA? This is related to the key obscurity concerning Proposition I.4. This difficulty must have been noticed by every schoolchild who did geometry using the older "Theonine" texts, like those of Todhunter, current in India up to the end of the 1960's. In the "Heiberg" version, Proposition 4 of the *Elements* states that

> If two triangles have the two sides equal to two sides respectively, and have the angles contained by the equal straight lines equal, they will also have the base equal to the base, the triangle will be equal to the triangle, and the remaining angles will be equal to the remaining angles respectively, namely those which the equal sides subtend.[46]

In brief: if two sides and the included angle of one triangle are equal to those of another triangle, then the two triangles are equal. We will refer to this as the side-angle-side proposition, or SAS for short.

The key obscurity is this. In the *Elements* the *proof of this proposition involves superposition:* it involves picking up one triangle, moving it through space, rotating it as necessary, and applying it to the other triangle. The later theorems on the equality of triangles (with the exception of I.8) do not, however, use this procedure: they rely instead on SAS.

There is no doubt at all that physical motion in space is implied, and there is a specific Common Notion or Axiom to enable this proof to go through. Common Notion 4 of the

"Heiberg" version asserts: "Things which coincide with one another are equal to one another".[47] For those accustomed to reinterpreting this in terms of congruence, it should be pointed out that this clearly applies to distinct geometrical objects that are brought into contact, and superposed, through motion. Likewise, Axiom 8 of the "Theonine" version asserts: "Magnitudes which coincide with one another, that is, which fill the same space, are equal to one another." If this is not a tautology, it must refer to distinct objects which are made to coincide with each other, by moving them about.

Physical Movement and Motion Without Deformation

The doubt that must have entered the mind of every schoolchild is the following. This method of picking and carrying greatly simplifies the proofs of all other theorems and riders: if it can be used in one place, why can't it be systematically used in other places as well? My teacher had no satisfactory answer why it was all right to do this in one place, but wrong to do it elsewhere. He simply said it is better not to do it, but could not explain why. But one may attempt an answer as follows.

Picking and carrying line-segments is a common enough thing: one must do this every time one ordinarily makes a measurement. By the late 19th century European mathematicians were sceptical about the very possibility of making a measurement: moving an object might deform it. What sense did it make to say that a figure remained identical to itself as it was moved about in space? A shadow moving on uneven ground is continuously deformed; perhaps space itself is similarly "uneven", so that any motion may involve deformation, and measurement may require more complicated notions like a metric tensor. The avoidance of picking and carrying in the proofs of the subsequent theorems was interpreted, by the 20th century, as an implicit expression of this doubt about the very possibility of measurement. It was argued against Helmholtz that measurement required (a) the notion of motion; furthermore this motion must be without deformation, so that it required (b) the notion of a rigid body, and neither of these was the proper concern of the geometer, who ought to be concerned only with motionless space. (The notion of rigid body depends on physical theory; e.g., the Newtonian notion of rigid body has no place in relativity theory, for a Newtonian rigid body would allow signals to travel at infinite speed.)

Geometry and Motion

Historically, this doubt about measurement was expressed as a doubt about (a) the role of motion in the foundations of mathematics, and (b) the possibility and meaning of motion without deformation. In favour of (a) the authority of Aristotle was invoked to argue that motion concerned physics, and that mathematics was "in thought separable from motion". Thus, "Aristotle" asserts:[48]

> The next point to consider is how the mathematician differs from the physicist. Obviously physical bodies contain surfaces and volumes, lines and points, and these are the subject matter of mathematics. ... Now the mathematician, though he too treats of these things, nevertheless... separates them; for in thought they are separable from motion.

The authority of Kant was implicitly invoked to argue that motion was not *a priori*, but involved the empirical, and *hence* could not be part of mathematics:

> an empirical proposition cannot possess the qualities of necessity and absolute universality, which, nevertheless, are the characteristics of all geometrical propositions. ... Take, for example, the proposition: "Two straight lines cannot enclose a space, and with these alone no figure is possible," and try to deduce it from the conception of a straight line and the number two.... All your endeavours are in vain, and you find yourself forced to have recourse to intuition, as, in fact, geometry always does. You therefore give yourself an object in intuition. But of what kind is this intuition? Is it a pure a priori, or is it an empirical intuition? If the latter, then neither an universally valid much less an apodeictic proposition can arise from it, for experience can never give us any such proposition.[49]

All these worries are captured in Schopenhauer's criticism of the "Theonine" Axiom 8 (corresponding to the "Heiberg" Common Notion 4) which supports SAS:

> ... *coincidence* is either mere tautology, or something entirely empirical, which belongs not to pure intuition, but to external sensuous experience. It presupposes in fact the mobility of figures; but that which is movable in space is matter and nothing else. Thus, this appeal to coincidence means leaving pure space, the sole element of geometry, in order to pass over to the material and empirical.[50]

In short, motion, with or without deformation, brought in empirical questions of physics, and Plato, Aristotle, and Kant, all concurred that mathematics *ought* not to be based on physics, but *ought* to be *a priori*, and that geometry *ought* to be concerned only with "immovable" or *a priori* space.

The Synthetic and the Metric Axiom Sets

The Hilbertian reading of the *Elements* hence denies the possibility of measurement, so that the proof of Proposition 4 (SAS) fails. To preserve the structure of the *Elements* it is then necessary to assume Proposition 4 as a postulate (the SAS postulate) that cannot be proved from any more basic principles. This approach is called the **synthetic approach**.[51] One way to describe this approach is by distinguishing synthetic instruments from those found in the common instrument box of school geometry. The synthetic instruments are the straight edge (*unmarked* ruler) and "*collapsible* compass". The last term is De Morgan's graphic description of the impossibility of measurement with the synthetic approach: distances cannot be reliably picked and carried because the synthetic compasses are loose and "collapse" as soon as they are lifted from the paper. ("Collapsible compasses" may well be an accurate description of the then-prevailing state of technology!) Hence, also, the ruler is left unmarked. In this synthetic approach, the term *equal* used in the "original" *Elements* is changed to the term *congruence*: motion is replaced by a mapping, so that it is not necessary to transfer figures from one place to another; one only needs to shift one's attention from one figure to the other.

The other way of clarifying the obscurity in the original *Elements* is to accept the possibility of measurement, and to accept that the proof of Proposition 4 (SAS) is valid. This is called the **metric approach**, and has been championed by Birkhoff. The main problem with a full metric approach is that it completely devalues the *Elements*. Even Proclus (i.e., the Monacensis remark) does not claim any originality for his Euclid; the value of the *Elements* derived from the nice arrangement of the theorems, so that the proof of any theorem used only the preceding theorems. With a full metric approach, even the arrangement of theorems in the *Elements* loses its significance: it is quite possible to prove the "Pythagorean theorem" (I.47), by cutting, picking and carrying, without recourse to the preceding theorems.

The synthetic and metric approaches being so different, the problem is to choose one of them.

It is in deference to the synthetic formulation of the *Elements* that proposition 4 of the "original" *Elements* is now taught as the SAS *postulate*. This permits one to continue teaching the *Elements* as a valid example of the deductive method of proof used in modern mathematics.

This is unacceptable for several reasons.

(1) A metric approach makes "Euclidean" geometry very simple: a straightforward metric approach could prove the "Pythagorean" "theorem" (Proposition I.47) in one step, as in the *Yuktibhāṣā* proof.[52] The synthetic approach was originally motivated by the desire to *justify* the apparently needless complexity of the proofs in the "original Euclid". The justification was needed because of the importance attached to this text by Christian rational theology. The justification was sought by denying the possibility of picking and carrying segments without deformation; hence, also, the possibility of measurement was denied. Thus, the synthetic approach makes proofs more difficult, and is counter-intuitive—for it denies the everyday ability to pick and carry, and compare and measure. (The ultimate justification for denying the manifest ows from the Platonic–Kantian idea that mathematics is *a priori*, and so *ought* not to be contaminated by the empirical. The other way of looking at this idea is that it demands that mathematics *ought* not to correspond to anything real, and hence *ought* to remain perfectly meaningless.)

(2) The synthetic interpretation of the *Elements* substitutes the key term *"equal"* in the "original" by the new term *"congruent"*. This key substitution clearly does *not* work beyond Proposition I.34. Thus, Proposition I.35 states: "Parallelograms on the same base and in the same parallels are equal to one another." This proposition asserts the equality of areas that are quite clearly non-congruent (when not identical). It follows that *one must either abandon all propositions after Proposition I.35 (including the "Pythagorean theorem" I.47), or else one must abandon the synthetic interpretation* of the *Elements*. It does not help to try to define a general area through triangulation, as Proclus' contemporary, Āryabhaṭa did[53] since the notion of area is not defined anywhere in the *Elements*, and the usual formula for the area of a triangle is itself derived from I.35. Some attempts have been made to supplement the synthetic approach by axiomatically defining area in a way analogous to the Lebesgue measure (overlooking the connection of the Lebesgue measure to the notion of distance). Area, however, is an intrinsically metric notion; indeed, it would be a rather silly enterprise to define area without first defining length (and, in fact, maintaining that length ought not to be defined at all).

The schizophrenic method of denying metricity until Proposition I.35, and admitting it thereafter, is only confusing to young minds. The whole project is born of the compulsions of theology and racist history.[54]

III
THE CURRENT INDIAN SCHOOL TEXT IN GEOMETRY

It is interesting to take a short detour and brie y consider the effects of this racist history as they are re ected in contemporary mathematics education in India. After independence, we have not, of course, accepted this racist history as it stands, but we have substituted this with our own schizophrenic project. The schizophrenia derives from multiple inheritance.[55] The formal structure of our educational system—schools, colleges, universities—continues to be patterned on the system prevalent in Europe, rather than the indigenous tradition of *pāṭhśālā*-s or Nalanda and Takṣaśilā. The educational system in Europe was for several centuries quite explicitly oriented towards theological concerns. With the rise of industrial capitalism, in the last hundred years or so, there was a partial shift in the West towards more practical and utilitarian concerns. "Euclidean" geometry, for example, is no longer taught in British schools.

Independent India accepted industrial capitalism, and the elite in this country still continue to regard education as a means of forging links to the metropolitan centre. Accordingly, maintaining inequality has remained an important objective of education, so that even 50 years after independence most of the country remains illiterate, and education remains the preserve of the elite for one excuse (shortage of government funds) or another (need to commercialize). Accordingly, while education has been "de-moralized", and some of the theological concerns of the West have been removed, these have been substituted by elitist chauvinism.

In line with the British legacy of bureaucracy, and the clerk's *dharma* of evading responsibility, our school texts are produced in clerkdom (which still controls education), by a duly constituted committee. The committee has sought to balance the requirements of industrial capitalism (which needs the products of education), with those of chauvinistic history (which seeks to correct racist history without understanding tradition).

These contradictory requirements are re ected in the earlier NCERT text[56] for Class 9. On the one hand, this is how that NCERT text justifies the teaching of geometry: "For instance, those of you who will become engineers, technicians and scientists will not only find all this information useful but will also realize that you cannot do without it." (Needless to say, there is no other concrete instance in the "explanation" which occupies one paragraph in this vein of redundancy improving communication!) But if practical usefulness were the sole justification for teaching geometry, then metric geometry ought to be taught. Engineers, technicians, and scientists, all, have no use for geometry without measurement. (Not even relativists care much for spacetime geometry based on the connection rather than the metric.)

On the other hand, a similar conclusion follows from the historical assertions with which the NCERT exposition of geometry begins (pp. 123–124).

> The *Baudhayana Sulbasutras*. . . contains [sic] a clear statement of the so-called Pythagoras theorem. The proof of this theorem is also implicit in the constructional methods of the *Sulbasutras*.

The subtle way in which Western historians have exploited the notion of "proof" seems to have quite escaped the authors of the text. Western historians have readily conceded that Babylonians, Egyptians, Chinese, and Indians all knew earlier *that* the Pythagorean theorem was true. They have maintained, however, that none of them had a proof; hence, none of them knew *why* it was true: they knew of the theorem only as an empirical fact which they did not quite comprehend, much as an ass might know the theorem without comprehending it. Comprehension, therefore, still dawned with the Greeks. To refer to constructional methods as implicit proofs is to miss the central issue clarified above: the motivation for synthetic geometry is that empirical knowledge is not only distinct from mathematics but that it cannot logically precede mathematics. Hence, if the second sentence in the above quote is true, then the very notion of mathematical proof would need to be changed to accept empirical inputs. Needless to say, the committee did not intend any such revolutionary challenge to mathematical authority which was entirely beyond its terms of reference!

Therefore, on the third hand (surely committees have at least three hands!), the text lapses back into the synthetic geometry recommended by the US School Mathematics Study Group. Like a proper committee report, the resulting text has included a little something to suit every taste. So the text introduces the SAS postulate (p. 162) as the "SAS (Side-Angle-Side) Congruence Axiom", where "axiom" is to be understood as follows (p. 125): "basic facts which are taken for granted (without proofs) are called *axioms*. Axioms are sometimes intuitively evident." That is, an axiom, like a *fact*, belongs to the domain of empirical and physical, rather than the intuitively *a priori*—exactly the thing that was denied to motivate the SAS postulate and the notion of congruence in the first place! One wonders why, unlike most other committee reports, this report was not left to gather dust!

The natural casualty is the student who has to digest the *whole* thing, and so may be put off geometry for the rest of his life, especially if he is clear-headed. If congruence is explained through superposition ("Heiberg" Common Notion 4, or "Theonine" Axiom 8), as the text does (pp. 159–161), one has clearly a metric approach. Within a metric approach, it is trivial to prove the synthetic congruence results proved in the text—in fact there is then no need for a SAS congruence *axiom*, one has a SAS *theorem*, the way it was proved in the "original" *Elements*. To now prove these results, in the manner of synthetic geometry, on the ground that one is teaching the axiomatic method, is to teach the axiomatic method as a completely mindless and elaborate ritual that one must complete on the strength of the state authority that NCERT enjoys. What children are being taught is not the sceptical attitude which underlies the need for a proof, but its antithesis—mindless obedience to rituals that cannot be justified.

The *khichdi* geometry in the NCERT text for Class 9 is indigestible because it has mixed up the *Elements* by mixing up elements that ought not to be taken together—like diazepam and alcohol—unless the object is to induce a comatose state. To make the text digestible, one needs to sort out *which* geometry one wants to teach: metric, synthetic, or traditional. Even if one wants to teach all three one should recognize their separate identities, and keep them in separate compartments: it is *not* a good idea to make the synthetic notion of congruence more intuitive by defining it metrically as the NCERT text does! The authors need to appreciate the incompatibility of the metric and synthetic approaches, and the way these differ from the traditional approach, which incorporates an altogether different notion of mathematical proof.[57] (Needless to say, the authors, some of whom are well-known mathematicians, have proceeded with the desire to "clarify the obscurities" in the *Elements*.)

Traditional Geometry Distinguished from the Metric and the Synthetic

Enough has been said above about the incompatibility of the metric and synthetic approaches, and I will brie y summarize the way in which both these approaches are incompatible with the traditional approach. (The differences are considered in more detail in the next chapter.)

First, the authoritative traditional literature is the *sūtra* literature; the *sūtra* style is well known for its extreme brevity—like a telegraphic message, further distilled by digital compression. The *sūtra*-s are *not* intended to serve primarily a pedagogical function, and they are not intended to be accessible to all. (Indeed, for the knowledgeable, the *sūtra*-s could well serve a mnemonic function.) Consequently, the *sūtra*-s have no place for proofs. Texts dealing with rationale, on the other hand, being less authoritative, have not been translated. The key text on rationale, available in English translation,[58] is the *Yuktibhāṣā*, which, as stated earlier, proves the "Pythagorean theorem" in one step, by drawing the figure on a palm leaf, cutting it, and rearranging the cut parts. An examination of rationale in traditional geometry shows the following.

What distinguishes traditional geometry from both metric and synthetic geometry is the traditional notion of proof (*pramāṇa*), and issue examined in greater detail in Chapter 2. Brie y, though there have been many debates in Indian tradition on what constitutes *pramāṇa*, the one ingredient that went unchallenged was the physically manifest (*pratyakṣa*) as a means of proof. The traditional notion is not embarrassed by the empirical, and does not regard it as intrinsically inferior to metaphysics. Both the Baudhāyana and the Kātyāyana *śulbasūtra*-s begin by explaining the use of the rope for measuring lengths and areas. On the other hand, Descartes who is credited with present-day metric geometry asserted that "geometry should not include lines that are like strings...."[59]

Asserting the *śulbasūtra* tradition would thus clash with the entire tradition of education in medieval and renaissance Europe, which was geared to theological purposes, and hence reinforced the philosophy of authorities like Plato, and later Kant—which justified the deprecatory attitude towards the physical world, and glorified a mathematics divorced from the empirical. For Proclus, the key object of teaching mathematics was not its military or political utility, which he regarded as subsidiary, but its ability to make the student forget the practical concerns of everyday life and thereby discover his real self.

> [T]he soul has its essence in mathematical ideas, and it has a prior knowledge of them...and brings of them to light when it is set free of the hindrances that arise from sensation. For our sense-perceptions engage the mind with divisible things...and...every divisible thing is an obstacle to our returning upon ourselves. ...Consequently when we remove these hindrances...we become knowers in actuality....[60]

Rejecting this attitude is not a trivial matter, for all of current-day mathematics depends upon the belief that mathematics is *a priori* and divorced from the empirical.

Nevertheless, the fact is that *all* traditional Indian notions of proof proceed from a realistic philosophical standpoint directly opposed to Platonic idealism. Classical Indian tradition saw no need to regard mathematics as something necessarily metaphysical, and consequently, there was no need for two separate procedures of validation: (1) a notion of mathematical proof, and (2) criteria (such as logical and empirical falsifiability) to decide the validity of a physical theory. Therefore, though metric, traditional Indian geometry does not need to proceed from Birkhoff's axioms. Against this background, the differences between synthetic, metric, and traditional geometry are summarized in Table 1.1.

Table 1.1: A comparison of metric, synthetic,

Type of geometry	I Metric	II Synthetic
Fundamental setup	(S, L, P, d, m)	$(S, L, P, B, \quad)$
Distance	d	Not mentioned
Measure for angles	m	Not mentioned
Congruence for segments	From d	Given (for segments)
Congruence for angles	From m	Given (for angles)
SAS	Theorem	Postulate

"Euclidean", and traditional geometry.

III	IV
"Euclidean"	Traditional
Semi-idealized (not real, not ideal)	Real space
Lengths	Measured with a rope
Only equality and inequality with right angles	Measured physically by measuring the arc with a rope
Not mentioned (only equality, presumed pre-defined)	Equality through measurement
Not mentioned (only equality, presumed pre-defined)	Equality through mea-surement
Theorem (empirically proved)	Similarity and rule of three (equality a special case)

Table 1.1: continued

Type of geometry	I Metric	II Synthetic	III "Euclidean"	IV Traditional
Area	Additional definition needed	Not defined (else length would be defined)	Not defined (only equality, presumed pre-defined)	Explicitly defined through triangulation/ rectangulation
Addition/ inequality	Real numbers	Congruence classes	Geometric construction	Floating point arithmetic
Proportion	Real numbers	Congruence classes + complex assertions (using "betweenness", inequality, and integer addition)	Complex assertions using inequality and integer addition. Not in Book 1	Rule of 3
Instruments	Scale, protractor, and compass ("geometry box")	Unmarked straight-edge and "collapsible" compasses	Not explicitly stated	Rope

Indian Rope Trick

Another key distinguishing feature of traditional geometry, which will be important to us in what follows, is the use of the exible rope instead of the straight-edge or the ruler. The use of the rope enables direct measurement of the length of curved lines, hence also of angles, in the natural radian measure. However, this process of assigning a length to curved lines is very hard to understand if one is accustomed only to the ruler which can measure the length of only straight lines. With a ruled straight-edge, assigning a length to curved lines requires essentially the calculus. (This is one reason for the difficulty with the calculus that present-day students have.) In fact, we find the difficulty explicitly articulated by a major Western thinker, Descartes, who says in his *Geometry* that it is beyond the capacity of the human mind to understand the ratios between straight and curved lines!

> [T]he ratios between straight and curved lines are not known, and I believe cannot be discovered by human minds, and therefore no conclusion based upon such ratios can be accepted as rigorous and exact.[61]

By holding the rope taut, it can easily be used to draw straight lines. It can, of course, be calibrated by a system of knots, which can be amazingly accurate as we will see in the case of the *kamāl* (Chapter 5). By fixing one end of the rope (on the ground), the rope can also be used as a compass. Hence, obviously, it can also be used to construct a right angle and the other angles commonly found in set squares. Thus, the lowly (and low-cost) rope (or string) is a complete and superior substitute for the elaborate and ritualized geometry box. It does not even require inputs like paper and pencil (which so many school students in India can ill afford). Amazingly, however, this fact has not struck any of our educators so far, including those who never tire of referring to the *śulba sūtra*-s, or those who keep talking of taking education to the masses!

IV
THE OBJECTIVES OF EDUCATION, AND THE PHILOSOPHICAL SUBSTANCE OF THE *ELEMENTS*

We now have before us three distinct models of "Euclidean" geometry: synthetic, metric, and traditional. Which model one ought to teach depends upon the objectives of education. This is a question which is postponed to a later chapter.

However, with some examples of the historical transformation of the *Elements*, through a process of reinterpretation and "clarification of its obscurities", whether due to varying objectives of education or otherwise, we can now proceed to answer two questions that were postponed earlier. Why were the *Elements* so important to Islamic and to Christian rational theology? Why were they such a necessary part of the theological curriculum? (This is the sort of thing that modern-day mathematicians do not usually understand, since their education, geared to the needs of industrial capitalism, encourages a narrow view of the world, together with an unquestioning acceptance of the postulates and rules of inference laid down by mathematical authority.) However, an understanding of this is necessary to understand the development of the context in which the efforts of Hilbert, Russell, and Birkhoff were situated.

Very brie y, to understand this, one must situate Christian rational theology in the context of the two traditions which it inherited. The first is that of Arabic–Islamic rational theology, which reached medieval Europe through Toledo, and the works of Averröes, and his response to al Ghazālī in the debate that preceded him in Islam,[62] and deeply in uenced the beginnings of Christian rational theology.

For the Arab rationalists (Mutāzilā and *aql-ī-kalām*) and philosophers (*falāsifā*) Uclides, or the key to geometry, became important as a demonstration of the old-Egyptian/Neoplatonic/Sufi principles, related to key aspects of their theology. In fact, this theology, accepted also by Plotinus, Porphyry, and Proclus, was presumably carried by the intellectual diaspora of the Alexandrian philosophers, expelled from the Roman empire in 529, which reached the Bayt al Hikma via Jundishapur. Caliph al Ma'mūn's aim was quite simply to encourage an intelligent reading of the Koran.

Thus, al Ma'mūn accepted justice as the cornerstone of a strong society; since the corresponding principles of Islamic jurisprudence (*fiqh*) derived from the Koran, al Mamun agreed with the Mutāzilā view that *aql* (intelligence, creative reason) should be applied to the reading of the Koran, against the literal interpretation, or *naql* (mimesis), advocated by the traditionalists. The Arab rationalists aimed to deduce everything from the two key principles of unity (*tauhīd*) and justice (*adl*). The similarity with Proclus' thinking is striking: and Proclus' *Elements of Theology* was one of the first books to be translated into Arabic, at the Bayt al Hikma as the *Kalam fi l mahd al-khair* ("The Theology of the Pure Good"). Although it came to be known as the "theology of Aristotle", the Arabic or Toledan "Aristotle" should not be con ated with Aristotle of Stagira.[63] It is, therefore, understandable that the Mutāzilā found the *Elements* useful for the same religious reasons as Proclus. In particular, the *Elements*, for them, provided a striking model of how even physically manifest differences could be reduced to equality. The book also acquired a practical use as a model for teaching and learning—by arranging things in a manner so as to make the teaching accessible to all persons, including those who might be completely ignorant. Thus, the significance of the arrangement of the theorems in the *Elements* was that it facilitated learning.

Naturally, there was a traditionalist response to the Mutāzilā, most persuasively by al Ghazālī. Wisdom and medicine which went together in Alexandria, from the time of Ashoka's delegation (of wise men and medicinal plants), also went together at Jundishapur, where the philosophers had set up not a temple but a hospital—whose great practical value fetched them immediate acceptance—and this was also their point of entry in Baghdad.

Likewise, medicine and wisdom went together in Baghdad as in the word *hakim* (doctor, wise man), an epithet still applied to a Ph.D.!

A key aspect of the successful practice of medicine was the notion of cause. The idea was that a disease could be successfully treated only if its cause was correctly understood, just as a mechanic can set a machine right only if he correctly understands the cause of the failure. Al Ghazālī's attack against the philosophers, widely and persistently misrepresented in the Western literature, was directed against this notion of cause. His key worry was that this encouraged a mechanistic view of the cosmos: if the present state of the cosmos was entirely the result of past causes then there was no role left for Allah to create anything new. It should be pointed out here that al Ghazālī, being a sūfī, and accepting immanence, naturally regarded the creation of the cosmos as a continuous process, rather than the one time affair described in the Bible.[64]

Therefore, al Ghazālī argued that Allah was not bound by any laws of cause and effect, and that observation could lead us to conclude merely that Allah created things in a habitual sequence. However, this habit was not binding. He could possibly create things in a different or surprising order. In modern language, all that al Ghazālī was saying was that the observed occurrence of a sequence n times does not imply its occurrence $n + 1$ times. In the process of denying the validity of causal (or inductive) inference, al Ghazālī incidentally conceded that Allah was bound by the laws of logic, and could not create an illogical world.

This incidental concession provided a new meaning to the idea that mathematical truths are necessary truths, not in the sense that they are eternal, but in the sense that they are true in all possible worlds—that Allah could create. It also placed logic on a pedestal: logical truths which bound Allah were more powerful than empirical truths which did not. This was a situation with which al Ghazālī's opponent Ibn Rushd concurred (although he beats about the bush and is unable to cogently answer al Ghazālī's primary argument against cause).

In the initial stage, after Arabic knowledge reached Europe, Ibn Rushd had many enthusiastic followers in the university of Paris, for example. However, these Averröists soon found themselves on the hit list of the Inquisition. In the resolution of the 1210 Council of Paris, all works of "Aristotle" were banned. In 1270 and again in 1277, some 232 propositions derived from various Arabic works were banned. Enthusiastic Averröist scholars at the University of Paris, like Siger of Brebant, were targeted by the Inquisition, and Siger ed but died mysteriously.

The fears that these teachings involved heresies—i.e., ideas that would weaken the power of the ruler-priests—were well founded. The notion of equality in the *Elements* has political and philosophical overtones of equity, which are quite lost upon those now accustomed to thinking in terms of congruence. But the proximity to Arab thinking, then, made it easier to understand the absence of a royal road to geometry as an assertion about the political content of the *Elements*. Equity is contrary to Platonic ideas of the republic, and Proclus' stated aim in writing his commentary on the *Elements* was to inform people about its deep philosophical content—the doctrine of the oneness of humankind. While Arab rational theology retained this old-Egyptian/Neoplatonic aim, we have seen that equity ran counter to the revised Christian doctrine of the 4th c., and was consequently rejected by Christian rational theology.

Thus, the second thing that Christian rational theology inherited was the legacy of the early Roman church and its confrontation with old-Egyptianism/ Neoplatonism over the issue of equity. Though the very early church doctrines clearly favoured equity, and Origen's theology is barely distinguishable from Neoplatonism, the state-church after Constantine found this doctrine of equity a gross political inconvenience. We have already noted the church's confrontation with old-Egyptianism/Neoplatonism, beginning about the time of Augustine and ending with closure of the Alexandrian school and the formal condemnation of Origen by the Fifth Ecumenical Council.[65]

Impelled by these contradictory inheritances, Thomist philosophy

1. retained rational deduction, but
2. rejected equity, immanence, etc. as irrelevant.

The philosophical importance of the Christianized *Elements* was now confined to the process of rational deduction which could be used to persuade the non-believer, since both Islamic rationalists and al Ghazālī accepted that God was bound by "Aristotelian" logic. The method of reasoning in the *Elements* was, therefore, projected by Christian rational theology as providing the universal model of necessary truth.

Exactly how universal is this model of rational deduction, which underlies present-day mathematics?

V
CONCLUSIONS

1. The key evidence for "Euclid" and his philosophy of "irrefragable demonstration" is a remark in the Monacensis manuscript of Proclus' *Commentary* on the *Elements*. This isolated remark does not fit the rest of the text, and is not based on any reliable earlier sources of information about events 750 years before Proclus. The remark is not genuine, but postdates the spurious "Archimedes" reference to which it alludes, and is probably from the 16th c. CE. The archaeological evidence of papyri supports the absence of a definitive text of the *Elements* up to the 4th c. CE. As such nothing is reliably known about "Euclid" or his philosophy, so that "Euclid" must be regarded as pure myth.

2. Proclus' philosophy must be accepted as the appropriate philosophy underlying the *Elements*. Proclus' key concern is to present mathematics as a religious instrument for spiritual progress through learning. On Proclus' exposition, the *Elements* refutes point

by point all the key changes in Christian doctrine carried out in the 4th and 5th c. CE (without ever directly referring to Christianity). Proclus was declared a heretic, and the like-minded Origen, the key expositor of early Christianity, was anathemized by the Christian church.

3. The mere name "Euclid" helped to de ect religious persecution by suggesting a "theologically correct" Greek ancestry to a Neoplatonist work probably put in its present form by Hypatia, a key opponent of the church, and an early victim of church brutality. This "Hellenizing" process of inventing a theologically correct Greek ancestry to all world knowledge commenced at Toledo, to overcome the sense of shame felt in learning from Arabic books translated into Latin during the Crusades. In turn, Hellenization helped justify further religious persecutions, by denigrating all non-Christian cultures save only the Greeks. This racist-religious doctrine is nakedly re ected in the claim of Western historians that mathematics (and, indeed, all knowledge) originated with the Greeks. This monumental and implausible claim is built, like claims about "Euclid", on the excessively tenuous evidence of stray remarks of doubtful authenticity in very late texts, typically from ca. 12th c. CE to the 16th c. CE.

4. Islamic rational theology retained the original emphasis in the *Elements* on equity and justice, in both the religious and political sense. Its opponent, al Ghazālī, incidentally gave a new interpretation to logical truths as necessary truths in the sense of being true in all possible worlds. Thus, mathematical truths could be necessary without being eternally true, and without con icting with continuous creation by Allah. This placed logical truths, which bound Allah, on a higher footing than empirical truths, which did not.

5. Christian rational theology accepted the above valuation of logical truths as necessary and binding on God, compared to contingent empirical facts, which were not, thus permitting God to create a world of his choice. Further, the *Elements* was reinterpreted and aligned with the prevailing Christian theology, by disregarding its linkages to immanence and equity, as explained by Proclus. Equality was further reinterpreted as congruence by Hilbert. Further, the Procluvian exposition of mathematics as a means of inducing meditation to elicit "the prior knowledge of the soul" and achieve union with Nous, was also eliminated, since anathema in the prevailing Christian theology. Mathematics thus came to be regarded as being of theological value solely because mathematical proof provided a means of persuasion, accepted "universally"—since accepted also by Islamic theologians who did not accept Christian scriptures.

6. Hilbert's synthetic interpretation of the *Elements* exactly fits the concerns of Christian rational theology, but does not fit the entire *Elements*, while Birkhoff's metric interpretation trivializes the *Elements*. Just as Hilbert regarded the original proof of SAS as

erroneous, Hilbert's ideas of mathematical proof must be regarded as erroneous, from the Procluvian point of view.

7. Though traditional Indian geometry is metric it is incommensurable with both the above synthetic and metric approaches, since it accepts the empirical as a perfectly valid means of proof within mathematics. Also it uses the rope as the primary geometric instrument, distinct from the unmarked straight edge and collapsible compasses of Hilbert's synthetic interpretation, or the ruler and compasses of Birkhoff's metric interpretation. The length of curved lines (hence angles) could hence be readily measured in traditional Indian geometry. The various distinct types of geometry need to be treated as pedagogically distinct in school texts.

APPENDIX 1.A
THE SOURCES OF "GREEK" TRADITION

The extraordinary historical theory of the Greek origin of all science, which seeks to appropriate all intellectual achievement to the West, though it has become quite widespread, rests on very shaky foundations.[66] We brie y re-examine this historical theory here, for this is the sort of history that has provided the basis for the philosophy of mathematical proof.

There are two key textual sources of "Greek" tradition. One consisted of the Arabic texts that came into Europe after the fall of Toledo, and were translated into Latin in the 12th c. The translations were carried out under the control of Dominico Gundisalvi, the organization of Raymond, Archbishop of Toledo, and were funded by the gold supplied by Peter the Venerable, Abbot of Cluny—obtained as part of the church's $\frac{1}{4}$th share of the loot from the Crusades. A second round of translation was funded in the 13th c. CE by king Alfonso X. Although some translations from the Greek to Latin did take place even in the 13th c., this was a mere trickle compared to the ood of the Byzantine Greek texts that came into Europe in the 15th and 16th c. CE, after the fall of Istanbul (in 1452) to Mohammed the Conqueror. For example, the first Latin versions of the *Elements* were translations from the Arabic by Adelard of Bath and Gerard of Cremona in the 12th c. CE. The first Latin translation of the *Elements* from Byzantine Greek was published nearly four centuries later, in the early 16th c.

It should be pointed out that even in the matter of allocating credits for translations, Western historians could not resist persistent dishonesty for centuries: we are asked to believe that Gerard translated some 87 books from the Arabic, without knowing either Arabic or mathematics or astronomy! The translations were actually carried out with the help of Mozarab and Jewish intermediaries, who remained largely nameless and disappeared from history since they were not theologically correct, and were regarded as non-persons. This already gives us a foretaste of the *de facto* balance between historical accuracy and theological correctness.

Now, how were these texts in another language, from another place, correlated with their alleged Greek authors from 1500 to 2000 years earlier? We have seen how this was done in the case of "Euclid"—on the basis of a Greek-sounding name and a stray passage here and a remark there, which passage or remark could date from any time in the intervening period, and which name might or might not correspond to any real person. If this is the situation with one of the best known names, then one can imagine that the situation with other authors like Archimedes, Aristotle, Ptolemy etc. is not likely to be very different—though Aristotle's existence at least is not in doubt!

Even the terminology of "interpolations", as in the case of the Monacensis remark, involves an unacceptable underlying hypothesis of an "original text". This hypothesis needs to be put on the table and made perfectly visible: the hypothesis is that as a rule the "original texts" were transmitted verbatim over this entire period. This is an extraordinary hypothesis in itself, and one can hardly think of any situation in which this verbatim transmission ever actually took place. In India, a large group of people was freed from economic necessity, and given the most extreme and rigorous training to try to ensure that the Veda-s were transmitted verbatim. Nevertheless, differences cropped up. The Bhagvad Gita, commonly memorized, contains, as Kosambi pointed out, 12th c. CE interpolations. Where no such extreme measures were taken to preserve the text verbatim, the differences could be expected to be significantly larger. The Aramaic Bible, for instance, was so very different from the Bible prevalent in 16th c. Europe, that the Portuguese tricked the Indian Bishops in the Council of Udayamperoor ("Synod of Diamper") to burn all the older Bibles.

If this is the extent of variation with regard to scriptures, where there is some reason to expect some sort of verbatim transmission, one can imagine the variation in the case of other books. For books pertaining to practical knowledge, there would obviously have been little interest in verbatim transmission, and the most natural thing would be to update them with the latest available knowledge. It would be rather pointless and confusing to retain in these books information that was incorrect or defective or inaccurate. That is to say, books on science and mathematics would naturally be propagated accretively, with the addition of numerous anonymous updates, though no one maintained a revision history. Certainly Arab authors in Baghdad, for example, were actively disinterested in verbatim translations, but were interested rather more in useful paraphrases and creative reworking.

Furthermore, Arabs were not much interested in questions of priority, so that authorship was loosely attributed to any famous early source. The authorship imputed in these texts was largely nominal and not intended to be understood literally, as in the case of the authors of the Pythagorean school who imputed all their writings to Pythagoras. Similarly, for the Arabs, "Aristotle" was merely another name for "the Greek sage", while for Thomas Aquinas, he was merely "the Philosopher", the archetypal Neoplatonist.

As a concrete example of such nominal attribution and accretive propagation, let us consider a navigator's manual, published by INSA in 1998 to throw light on "traditional" navigational methods in the Lakshadweep islands. Like so many Arabic navigation manuals, this manual too is attributed to Ibn Majid, the most famous of Arab navigators, who lived in the 15th c. However, it contains updated information found in British sailing manuals of the 19th c. CE. The natural interpretation is that the attribution to Ibn Majid is nominal and symbolic, that the manual has been propagated accretively, motivated by the navigator's life-and-death concern to have the best possible knowledge, and hence the manual has borrowed also from British sailing manuals of the 19th c. It would be laughable to assert that the manual is due to Ibn Majid who had anticipated all this knowledge which was somehow transmitted to the British sailing manuals of the late 19th c. CE.

Nevertheless, this laughable hypothesis is exactly what has been adopted with the 12th and 16th c. sources of "Greek" or "Hellenic" tradition.[67] *Hence*, virtually all the knowledge prevalent in the 11th c. world, as known to Indians and Arabs, is attributed to Greeks like Aristotle, Archimedes, and Ptolemy. The fact is that the knowledge in these 11th c. texts accurately re ects the knowledge that then prevailed—as is naturally to be expected. However, Western historians explain this fact not by the simple and natural hypothesis of accretive updating of the texts, but by the extraordinary claim that all (or most of) the contemporary knowledge of the 11th c. world was derived by transmission from the Greeks, who had anticipated these developments. There is no other, or direct, evidence that these Greek authors wrote anything at all. Thus, by way of evidence, this extraordinary theory of transmission simply begs the question! To complete the story, it is thought enough to supplement it with a speculative chronology, attached to Greek names, based on stray remarks of doubtful authenticity in late texts. This sort of story-telling may be perfectly consonant with the standards of theology (and most early Western historians were priests), but is completely unconvincing from a somewhat more sceptical and down-to-earth point of view.

Now, the the natural thing to expect is that the (scientific) books of the 11th c. CE re ect the knowledge that prevailed in the 11th c. CE. So the issue boils down to this: should we interpret literally the imputed authorship in these texts? Western historians ask us to believe that all or most of a 12th c. or later text, imputed to an author, such as Archimedes, was actually written by the named author. However, there *are* well known cases where the attributions to Greek authors are regarded by Western historians as not only nominal but false, and where it is believed that the author had nothing whatsoever to do with the text of which he is alleged to have been the author. For example, *Uthulijiyya Aristutelis*, otherwise known as the *Theology of Aristotle*, translated by the philosopher al Kindi, with the aid of a Syrian Christan intermediary Abd'ul Masih ibn Na'imah al-Himsi, was a key theology text, long attributed to Aristotle by the Arabs. This is today believed to be incorrectly attributed to him, and to be actually the *Enneads* of Plotinus with the commentary of Porphyry. Similarly, the *Kalam fi l mahd al-khair* ("The Theology of the Pure Good"), was also ascribed to Aristotle.[68] The *Kalam fi l mahd al-khair* is today believed to be a paraphrase of 32 propositions of Proclus' *Elements of Theology* (*Stoikheiosis Theologike*).

Then there is the "dishonesty effect" of the market. Attributing a book to a famous early source added not only to the authority of the book, but also to its market price in what was evidently a ourishing book bazaar in Baghdad. That many books were fakes and falsely attributed to famous early sources is evident from the *Fihrist* of al Nadim, a Baghdad shopkeeper of the 10th c., who *hence* prepared this *fihrist* or list of books he regarded as genuine. Of course, al Nadim was a shopkeeper, not a scholar, and his concerns about genuineness were limited to saleability—so, common hearsay was good enough for him—and he is unlikely to have been bothered by a well-established fake.

But if *some* attributions are accepted as invalid, there may be many more such doubtful attributions. How does one decide which attributions are valid and which are not? Where is the line between "Aristotle" and "pseudo-Aristotle"? How does one separate Aristotle of

Stagira from Aristotle of Toledo? Clearly it would be hard to find an objective basis for such decisions which have been based on the authority of historians, and it is remarkable how conveniently the "accepted" attributions line up with theological correctness! In this story of Greek origins, Aristotle has, by now, acquired a definite character!

Clearly, the least one can do in a critical (as opposed to a credulous and theological) approach to history is to try and discriminate between the two hypotheses:

1. attributed authorship taken literally + verbatim propagation of texts + transmission of this knowledge to others

vs

2. nominal and symbolic attribution of authorship + accretively updated propagation of texts.

Once these implicit assumptions are clearly visible, and laid out on the table, it is easy to see their consequences. For example, the first hypothesis would suggest that (a) there was no growth of knowledge outside of Greece (since 11th c. world knowledge largely coincides with what was allegedly mostly anticipated to the Greeks), and (b) that hence we should find the 11th c. ideas prevalent, no matter how far back we go: geometry should have been roughly constant since "Euclid", astronomy since "Ptolemy", logic since Aristotle, etc. Clearly enough, these consequences y in the face of the most elementary common sense—they are credible only to racists. Nevertheless, let us give a long rope and ask: has this really been the case?

Let us take, as a random example, "Aristotle's" theory of syllogisms, which is remarkably similar to the Indian Naiyāyika theory of the syllogism. The Naiyāyika theory of the syllogism could easily have been transmitted to Arabic texts via Jundishapur and/or Baghdad. On the other hand, the Aristotelian syllogism is certainly not prevalent in the Byzantine empire in its "Dark Age" between the 4th c. and 10th c. CE. So we find that, contrary to the expectation, the knowledge was not in fact prevalent earlier. Does that falsify the theory? No! Absolutely not! Immediately, a new hypothesis is invented, and we are asked to believe that people in the "Dark Age" had "forgotten" all about Aristotle. Of course, the Indian syllogism could also quite conceivably have been transmitted to Alexandria, prior to the commencement of the "Dark Age", but prior to the 4th c. CE, we find that in Alexandria the theory of syllogisms is attributed to Stoics like Chrysippus, and not Aristotle. So, where was the alleged Aristotelian syllogism hiding in the intervening fifteen centuries between Aristotle and the 12th c. texts? As in the case of a definitive text of the *Elements*, this vanishing act is not credible. If the real Aristotle wrote anything at all, he would have done so on papyrus, and for a text on papyrus to survive, it would have had to be repeatedly copied out, a process that required an investment of time and money. If the time and money was invested, and the text was copied out, it would not have simply disappeared, but would have remained in circulation. So a text which did a vanishing act for so long a period, probably did not exist. Is it not more plausible to suppose that the authorship of the logic texts was incorrectly assigned to Aristotle in much the same way as the authorship of the theology texts was incorrectly assigned to him? Is this not self-evident from the large number of works assigned to this "Aristotle" which have made him into a theologically correct encyclopaedia—an academic superman who wrote books on poetics, rhetoric, ethics, logic, and physics—while other Greeks did nothing even remotely comparable!

As another example, consider "Ptolemaic" astronomy. If it was really such a well developed system, why did the Alexandrian diaspora look towards Indian astronomy in 6th c. Jundishapur, and again in 9th c. Baghdad? How does one reconcile the grandiose claims about Ptolemy's *Syntaxis* with the persistent inaccuracy of the Roman calendar (until 1582) despite the attempts to reform it in the 5th and 6th c.—the attempts which led to the formulation of the Christian Era? So, like Aristotle's theory of the syllogism, and a definitive version of the *Elements*, Ptolemaic astronomy too did a vanishing trick, both during and before the Dark Age. Also no non-definitive texts, or texts by dissenting authors, have survived from that period. How did "Ptolemy" arrive at a sophisticated planetary model with neither any "Hellenic" predecessors nor successors who wrote books on astronomy? Why was the Greek calendar so hopelessly bad? And if the Greeks were not motivated to do astronomy, for whom did Ptolemy write a book on astronomy? (There are many other points here, and these are discussed in more detail later on in Chapters 3 and 6.) Therefore, it is hard to believe that there really was a 2nd c. Roman citizen called Claudius Ptolemy who could be regarded as the author of the 11th c. Arabic *Almagest*.

One can go on in this fashion. However, any number of facts and objections can be overcome by the stock trick of theology which is this: by inventing enough auxiliary hypotheses, *any* facts can be made compatible with *any* theory. Therefore, theology proceeds by first telling a convenient story, and then defending that story by piling on the auxiliary hypotheses, like dung in a pigeon's nest. However, it is evident that, *prima facie*, the verbatim propagation + transmission hypothesis cannot be defended without violating the elementary principles of clear thought. If, on the other hand, a given text was not propagated verbatim, but was repeatedly updated by anonymous contributions by later authors, how can we infer its original contents? The whole theory of the "Greek" or "Hellenic" origins of 11th c. world knowledge is an implausible hoax—or a fabrication as Bernal has called it.

We can approach the matter from another angle. This implausible theory of Greek origins has resulted in an image of the Greeks as an extraordinary culture. This may be true of their artistic or literary achievements, which are not my concern. However, so far as scientific achievements are concerned, this is an image that is intrinsically shaky in many respects. First of all, even on the stock accounts, a remarkably large number of "Greek" mathematicians (and scientists) hail from Alexandria, which is physically located in the African continent, and culturally located in Egypt. As for religion and philosophy, Egypt being the older of the two civilizations, it was natural for the Greeks to have borrowed extensively from Egypt. Herodotus attests to this, pointing out that the basic elements of Greek religious belief and many Greek cultural practices were borrowed from the Egyptians.

> Almost all the names of the gods came into Greece from Egypt... Besides these which have been here mentioned, there are many other practices...which the Greeks have borrowed from Egypt.... it seems to me a sufficient proof of this that in Egypt these practices have been established from remote antiquity, while in Greece they are only recently known.[69]

Since Herodotus also added that the Egyptians were "black-skinned and have woolly hair" (*History*, II.104), his idea that the Greeks were like children before the Egyptians was intolerable to racist European historians from 17th c. CE onwards—whether or not they personally owned black slaves.

While on the one hand the Greeks blindly aped black African (Egyptian) tradition, on the other hand the Ionian Greeks were a full- edged Persian colony, with their little fiefs, and their resentment about forced service in the Persian army. Meanwhile, the Athenians and Peloponnesians on the margins of the Persian empire were constantly engaged in petty warfare, as recounted by Thucydides in his *History of the Peloponnesian War*. Consequently, the Greeks in Athens had little time or leisure for scientific or philosophical speculations, which naturally tend to ourish in more settled times of peace.

This situation was aggravated by the anti-scientific culture of the Greeks. Thus, in Athens, at the time of Plato, scientific speculations were regarded as an act of impiety—an offence punishable with death. Plato recounts in his *Apology* that at his trial, Socrates was accused (p. 279) of teaching that the moon was but a clod of earth, and he vigorously denied it saying that he did not engage in physical speculations, that he believed in the divinity of the moon, and that his accusers had confounded him with Anaxagoras (who had earlier been imprisoned on a similar charge, but had escaped and ed). Similarly, Aristotle was forced to ee Athens after the death of Alexander. Clearly, in Greece proper, science was regarded as profane.

By what magic did things change so strikingly between Athens and Alexandria? Despite the enormous body of literature on Greek history, I am not aware of anyone who has raised this elementary question or sought to answer it. We might attempt an answer as follows. The Macedonians (Bulgars, Slavs) under Alexander being regarded as "barbarians" even by the Greeks in Athens, must also be regarded, from the viewpoint of Egypt and Persia, as the "barbarian" invaders, in Toynbee's terminology.[70] Accordingly, along the lines of the general theory articulated in Chapter 6, Alexander's military conquests naturally led to a huge in ow of knowledge *into* Greece, especially from Egypt, Babylon and Persia, compared to which the earlier in ow was but a trickle. Specifically, Alexander acquired a large number of books as military trophies; he got some of these books translated, and burnt the originals (as recounted in the Zoroastrian *Book of Nativities*—see Chapter 6, p. 278).

What happened to these books which Alexander fetched as war booty? Some of these books Alexander would naturally have referred to Aristotle. The existence of a large number of books in Aristotle's custody is confirmed by Strabo (*Geography*, 13.1.54) who says that Aristotle was the "first man [Greek]" known to have a library of books. (By "man" Strabo presumably meant "Greek", for the Egyptians certainly collected books in their temples.) Possibly, Aristotle translated (or got translated) some of these books, though we have no knowledge of what he actually did. Thus, Aristotle's reputation for scholarship already owed much to Alexander's military conquests, although later-day historians have failed to acknowledge it.

The bulk of Alexander's booty of books, however, seems to have been dumped in Alexandria. Ptolemy II, who ruled Egypt, subsequently got this partly catalogued thus initiating the Great Library of Alexandria, estimated to have had a collection of over half a million scrolls. Obviously the Greek city states were far too small to support the production of books in such vast numbers. Moreover, the army of 4000 Greeks with which Ptolemy ruled Egypt, could hardly have written so many books in so short a time—during which they were busy with military adventures. Hence, most if not all of these books were non-Greek in origin. While the Greeks were *not* known to have collected books earlier, every Egyptian temple *did* have a store of books—both religious books and records—going back thousands of years.

The library of Alexandria also included books subsequently brought in by travellers and traders coming to Alexandria—which were forcibly confiscated, and acquired for the library, only a copy being returned to the original owner, according to a law made by the Ptolemy II. Ptolemy III wrote to kings all around the world to send him their books, and to support the activities of the library, the export of papyrus was banned.

In this context, the location of Alexandria is significant: Alexandria, earlier called Pharos[71] (and nearby Rhakotis) was naturally selected by Alexander as the hub of a strategically important trade route (with its Red Sea Canal being equal in strategic importance to the present-day Suez Canal). Thus, the books available in Alexandria already re ected an accumulation of knowledge from much of the civilized world—certainly including India, which is known to have had a huge trade with the early Roman empire, via Alexandria. In fact, recorded contacts between India and Alexandria go back to the time of Ashoka the Great who recorded in his rock edicts, found across India, that he had sent delegations of wise men to various kings, including the king "Tulimaya" (Ptolemy II), and spoke (in the 13th edict) of the resulting victory of Dhamma—translated into Greek as *epidoeia* or piety in the Greek version of the rock edicts found in Kandahar, Afghanistan. (Ashoka also sent medicinal plants, "for both animals and men", and we recognize this combination of medicine and wisdom (*sophia*) in the later-day reincarnations of Alexandria in Jundhishapur and Baghdad, where hakim meant both a wise man and a medical doctor.) The continuing exchanges with India are recorded by Strabo, Porphyry, etc.

Apart from translating some of these looted or seized texts into Greek, exactly what further contributions did the Greeks make to this vast accumulation of knowledge in Alexandria? Merely, the language of subsequent texts being Greek would not make those texts (or the knowledge in them) Greek in origin, any more than Buddhist texts in Chinese can be said to be of Chinese origin, or the present text can be called British in origin or even orientation, just because its language is English. Alexandria had a particular practical need of such a common language, because, as the hub of trade route, it was a melting-pot of several languages and cultures, as is clear from Dio Chrysostom's description of his Alexandrian audience, which included Indians and Syrians.

So what is the evidence that Claudius Ptolemy, say, contributed anything original? Unfortunately none. It is only the Greek *names*, and a speculative chronology attached to them, that come from Alexandria. *As for the books, not even a single historical source of Greek books is available from Alexandria.* Thus, we do not have any direct access to any of those original Persian, Babylonian, and Egyptian sources, or to their early Greek translations in Alexandria. The actual information about these "Greek" books comes to us from a different place, at a different time many centuries later, in a different language. The huge gap in the evidence is filled up by speculations and story telling.

The internal evidence of these texts is not very reassuring. For example, "Ptolemy" has been accused of plagiarism on the grounds that he made his observations of stars not by gazing at the night sky, but in the Great Library, by copying manuscripts from there! (The "observations" have been back-calculated, and this certainly includes the "observations" in the passages used to date Ptolemy.) To my mind, the accusations are unsubstantiated, since even the existence of Ptolemy has not been established! The alternative is to suppose that the text is accretive, and this was presumably already the case in the 2nd c. CE. So, did "Ptolemaic" astronomy, like Hipparchus' star charts, add anything to the world knowledge in that vast collection of books?

The Alexandrian library was eventually burnt down by rampaging Christian mobs. The same sort of politics of cultural purity ("Doctrine of Christian Discovery"[72]) has motivated Western historians to expend centuries of effort to erase those books also from history: all those books apparently disappeared without leaving behind the smallest intellectual trace in later-day work! We are asked to believe on faith that Greek ideas were "immaculately conceived", and that "pure Greek" thought certainly did not have a black African ancestry.

Further, just as we are asked to believe that the translations from Egyptian and Persian etc. to Greek contributed nothing to Greek knowledge (from the time of Aristotle to the fall of the Alexandrian library), so also we are asked to believe that the translations from Greek to Arabic contributed nothing to Greek knowledge! *The label "Greek" or "Hellenic" thus appropriates both earlier Egyptian and later Arabic–Islamic sources.* Indeed, since information owed into Alexandria, Jundishapur and then Baghdad also from India, the "Hellenic" label also appropriates possible Indian developments known to the Greeks and also the Arabs who penned the *Almagest!* The "Greek" or "Hellenic" label thus appropriates to the West practically all the knowledge in the world up to about the 10th c. CE. This alleged knowledge of the Greeks is not re ected in non-textual sources. In this manner Western historians have built monumental theories of early "Greek" science largely on the strength of stray textual remarks in texts from 12th c. CE onwards, to extend into the intellectual domain the physical conquests of Alexander!

Apart from these Arabic sources, there are also texts in Byzantine Greek, from later-day Istanbul. These texts are typically much later than the Arabic sources, though Western scholars have optimistically dated a few to epochs as early as the late 10th c. CE. Even with such optimistic dating it is hard to see how these Byzantine Greek sources could be free of Arabic in uence. An unquestionably Indian source, the *Pañcatantra*, came to be translated into Persian (in Jundishapur, 6th c. CE) and then re-translated into Arabic (in Baghdad, 9th c. CE), then Greek by Simon Seth (in Antioch) ca. 1080 and finally into Latin for Alfonso X as *Calila e Dimna* in 1251 or 1261.[73] This would been *a fortiori* the case with scientific and mathematical texts written with a view to their immediate practical value, rather than to serve as a historical record for future historians; hence, they presumably sought to incorporate the latest available information, like Gerbert's 10th c. CE text "Rules for Computations with Numbers", which sought to incorporate into the abacus, as *apices*, the latest knowledge of the Indian numerals, obtained through the Arabic algorismus.[74] (Gerbert 940–1003 became Pope Sylvester II in 999 CE.)

Many Byzantine Greek texts are known to have involved translations from Arabic into Greek. Perhaps the most famous example of such translation from Arabic to Greek to Latin is the case of Copernicus who was not quite the revolutionary scientist he is made out to be but was rather a priest who translated from Greek to Latin the heliocentric theories of Ibn as Shātir of Damascus.[75]

It is generally acknowledged that many of the late Byzantine Greek texts (especially the scientific texts relating to mathematics, astronomy etc.) contain much material that is translated from the Arabic into Greek. (In fact, there is no reason why even the earliest of these texts, such as the Arethas text of the *Elements* from 888 CE, should have remained free of Arabic in uence, two centuries after the rise of Arabs, and half a century after the formation of the Baghdad House of Wisdom.) Since so many of these texts *do* contain later-day knowledge, how does one separate the "original" Greek knowledge (obtained from Egyptians and others in Alexandria) from later-day "interpolations" that might involve the knowledge of the Arabs or Indians and so forth? This, as already noted, is an extremely fertile field for speculation, where a decision is next to impossible by any critical standards: therefore the tendency is to rely on authority, i.e., on Western historical scholarship.

Speculations tend to be coloured by prejudice, and it is beyond the shadow of a doubt that very many of these Western authorities were racists, or had racist prejudices. This systematic process of racist cultural appropriation has been examined in many books,[76] perhaps the most well known of which is Martin Bernal's *Black Athena: The Fabrication of Ancient Greece.* Though Martin Bernal, unlike his father J. D. Bernal, does not say much about science and mathematics, the same situation prevails here. Consider, for example, the classic work of Heath, which speaks of the "apparently circumstantial accounts of Euclid given by Arabian authors" but clarifies that "the origin of their stories can be explained as the result of (1) the Arabian tendency to romance, and (2) ... misunderstanding." He goes on to assert (p. 4) that these accounts were intended "to gratify a desire which the Arabians always showed to connect famous Greeks in some way or the other with the East" and cites (p. 4, note 6) the *Haji Khalfa* to conclude that "The same predilection made the Arabs describe Pythagoras as a pupil of the wise Salomo, Hipparchus as an exponent of Chaldean philosophy or as the Chaldean, Archimedes as an Egyptian etc."[77] What, after all, makes it so improbable for Archimedes, who studied in Alexandria, to have been a short black man, as Arabic sources describe him? And, if Arabic sources are unreliable in this matter, how can they be relied upon for matters favourable to the opinion of Western historians? In fact, one could say with greater reason: the fabulous accounts of Greeks by Western historians can be explained as the result of racist fabrications. That is, to trust the authority of Western historical scholarship is to rest on the dangerous ground of speculations deeply coloured by racist prejudices.

NOTES AND REFERENCES

1. Specifically, "both proofs founded on causes and proofs based on signs". Proclus, *A Commentary on the First Book of Euclid s Elements*, trans. Glenn R. Morrow, Princeton University Press, Princeton, 1992, p. 57.
2. T. L. Heath, *The Thirteen Books of Euclid s Elements*, vol. I, Dover, New York, [1908] 1956, p. 75. It should be pointed out that Heath has a curiously ambivalent attitude towards Arab sources. accepting as true whatever suits him, and rejecting everything else with some racist remarks. See, further, Appendix 1.A.
3. In *Historia Mathematica* discussion list 10 Nov 2002 (in response to a query about this author's statement that Euclid perhaps did not exist). http://mathforum.org/kb/thread.jspa?threadID=381990&messageID=1175734
4. Heath, p. 109.
5. Translation adapted from, Proclus, *Commentary*, cited above, p. 56, and Heath p. 1 and footnotes 2 and 3. Heath omits the first sentence. His translation further safeguards Euclid's existence: his footnote 2 asserts that the word γέγονε "must... mean ourished'... and not was born', as Hankel took it", on the grounds that "otherwise part of Proclus' argument would lose its cogency". That is, Heath's argument seems to be that since there is no other cogent evidence for the existence of Euclid, whatever evidence there is ought to be interpreted in a way so as to make it cogent!
6. G. Friedlein, *Procli Diadochi in primum Euclidis Elementorum commentarii*, B.G. Teubner, Leipzig, 1873.
7. D. Hunter, *Papermaking: the History and Technique of an Ancient Craft*, Pleiades Books, London, 1947.
8. Heath, p. 46; emphasis Heath's, de-emphasis mine.
9. Sir Thomas Heath, *A History of Greek Mathematics*, Dover, New York, 1981, p. 360. As Heath further points out (p. 357), these manuscripts "commonly spoke of him [Euclid] as ... the writer of the *Elements*' instead of using his name".
10. Contrary to the elementary norms of critical scholarship, this single unusual manuscript is today taken as the "primary source" of "Euclid".
11. The material on these papyri is edited and reproduced in E. S. Stamatis, *Euclidis Elementa*, vol. I, Leipzig, 1969, pp. 187–190.
12. David Fowler, *The Mathematics of Plato s Academy: A New Reconstruction*, Clarendon Press, Oxford, 2nd ed. 1999, p. 216.
13. For instance, the book-burnings ordered by the Christian emperors Jovian, Valens, and Theodosius, Clarence A. Forbes "Books for the burning" *Transactions of the American Philological Society* **67** (1936) pp. 114–25.
14. Not only could the Great Library of Alexandria not have materialized overnight, not only did the small Greek city states on the margins of Persia lack the economies to sustain such book writing, the conditions were not conducive to *any* scholarly activity in Alexandria, at the time of Ptolemy I.
15. *On the Sphere and the Cylinder* I, Proposition 6, in *The Works of Archimedes*, trans. T. L. Heath, *Great Books of the Western World*, vol. 10, Encyclopaedia Britannica, Chicago, 1996, p. 407.
16. J. Hjelmslev, "Über Archimedes' Grössenlehre", *Kgl. Danske Vid. Selsk. Mat.-fys. Medd.* (Royal Danish Academy of Sciences) **25** (15) 1950.
17. C. K. Raju, "Newton's secret", *The Eleven Pictures of Time*, Sage, 2003, chp. 2. Drafts of Newton's *History of the Church* first became publicly available in the late 1960's leading to a revised biography of Newton. Richard S. Westfall, *Never at Rest: A Biography of Isaac Newton*, Cambridge University Press, 1980.
18. E.g., in 390 the temple of Serapis and the adjacent library of Alexandria were burnt down by a violent Christian mob. The magnificent temple of Dea Caelestis at Carthage remained open until ca. 400; but many laws were passed against pagan temples, and, in 401, the synod of Carthage twice asked the State to implement these laws. Eventually, in 407 the Catholics forcibly took possession of Dea Caelestis and Bishop Aurelius, Augustine's lifelong friend, triumphantly planted his cathedra at the exact spot occupied by the statue of the pagan goddess. H. Jedin and J. Dolan (eds) *History of the Church*, vol. II: *The Imperial Church from Constantine to the Early Middle Ages*, trans. Anselm Biggs, Burns and Oates, London, 1980, p. 205.
19. Standard Christian apologetics has tried to counter this in three ways. First there is the obvious canard about Caliph Omar having used the books for heating bath water. Apart from the fact that Arabs respected knowledge, this canard forgets to explain how this huge library survived the book burning edicts (cited above) of various Christian emperors, Jovian, Valens, Theodosius, especially after Serapis had been knocked down. Second there is the claim that the Library might have burnt down during an attack by Julius Caesar. Because there is a report of *some* random fire during some fight, *ergo*, this must have been

the fire that burnt down the Great Library. This theory fails to explain why Roman historians did not record such a significant event. It also neglects to explain why the members of the Alexandrian school did not attempt to rebuild the library, when Julian in a couple of years managed to rebuild various libraries that had been dismantled by Constantius II. (Gibbon points to Mark Antony's gift to Cleopatra.) Finally, it neglects to explain how the Alexandrian school continued to function in the absence of books. Lastly, there is the desperate claim that the library never existed! This relies on the stock trick of suddenly demanding an unreasonably high standard of evidence. I have nothing against stringent standards of evidence, but I would love to see exactly the same standard of evidence applied to a variety of other cherished beliefs, which would then have to be abandoned by the apologists. E. Gibbon, *Decline and Fall of the Roman Empire*, Encyclopaedia Britannica, Chicago, 1996, vol. 1, p. 462.

20. Proclus, cited earlier, 45, p. 37.
21. Plato, *Meno*, 81–83. *The Dialogues of Plato*, trans. B. Jowett, *Great Books of the Western World*, vol. 7, R. M. Hutchins, ed. in Chief, Encyclopaedia Britannica, Chicago, p. 180.
22. For a full analysis of this con ict, see C. K. Raju, "The curse on cyclic' time", *The Eleven Pictures of Time*, Sage, New Delhi, 2003, chp. 2. The whole book is devoted to an analysis of this con ict here summarized in three sentences.
23. So strong is the in uence of this doctrine, that a variety of thinkers including present-day scientists (in the context of the grandfather paradox of time travel, or like Hawking in the context of closed timelike curves) and philosophers of science like Popper (in the context of his pond paradox) have repeated this argument. See C. K. Raju, *The Eleven Pictures of Time*, Sage, 2003, and C. K. Raju, "Time travel and the reality of spontaneity", *Found. Phys.*, July 2006. http://doi.dx.org/10.1007/s10701-006-9056-x. Draft at http://philsci-archive.pitt.edu/archive/00002416/01/Time_Travel_and_the_Reality_of_Spontaneity.pdf.
24. Porphyry, *On Images*, trans. E. H. Gifford, Internet Classics Archive, http://classics.mit.edu/Porphyry/images.html.
25. Proclus, cited earlier, 47, p. 38.
26. Origen, *De Principiis*, as quoted in J. Head and S. L. Cranston, *Reincarnation: An East-West Anthology*, The Theosophical Publishing House, Wheaton, 1968, p. 36.
27. Origen, *De Principiis*, chapter on "On the beginning of the world and its causes". This chapter is numbered somewhat differently in the different online versions. It is IV-5 in the New Advent version at http://www.newadvent.org/fathers/04122.htm, while it is II.3.5 according to the Catholic Encyclopaedia.
28. Origen, *De Principiis*, IV-4 or II-3; see note above.
29. Origen, *De Principiis*, Book II, chp. 9. Frederick Crombie, trans., *The Writings of Origen*, vol. X in *Ante Nicene Christian Library*, ed. Alexander Roberts and James Donaldson, T&T Clark, Edinburgh, 1895, p. 136.
30. C. K. Raju, "The curse on cyclic' time", *The Eleven Pictures of Time*, Sage, New Delhi, 2003, chp. 2.
31. R. J. Hoffman, *Porphyry s Against the Christians: The Literary Remains*, Oxford University Press, 1994, p. 50
32. C. K. Raju, *The Eleven Pictures of Time*, pp. 72–73. P. S. S. Pissurlencar, "Govyāche Khristīkaraṇa", *Shri Shantadurga Quatercentenary Volume*, published by D. K. Borkar, Bombay, 1966, pp. 91–122. English summary in B. S. Shastry and V. R. Navelkar, eds, *Bibliography of Dr Pissurlencar Collection*, part I, Goa University Publication Series, No. 3, pp. 67–69. Moreover, the same method of capturing state power by converting the king "the Grand Moghul" (Akbar) was attempted in 1580.
33. Helen S. Lang and A. D. Macro, *On the Eternity of the World (de Aeternitate Mundi) by Proclus*, University of California Press, 2001.
34. This book is arranged like "Euclid's" *Elements* in a series of 211 propositions. Propositions 15 and 16 explain that "anything capable of reversion on itself must be incorporeal and must have an existence separate from the body. Proposition 17 states: "Everything self-moving is capable of reversion on itself," for it "links its end to its beginning". Against this background of "cyclic time" he then argues that anything capable of reversion on itself is self-constituted, hence imperishable and perpetual.
35. Anthony Pym, *Negotiating the Frontier: Translators and Intercultures in Hispanic History*, Jerome Publishing, Manchester, 2000. Sourced from the draft at the website: http://www.fut.es/~apym/on-line/studies/toledo.html.
36. E.g., *Encyclopaedia Britannica*, article on Khosrow I.
37. C. K. Raju, "Interaction between India, China, and Central and West Asia in mathematics and astronomy" in *Interactions between India, Western and Central Asia, and China*, ed. A. Rahman, PHISPC, New Delhi, 2002, pp. 227–254.
38. Martin Bernal, *Black Athena: The Afroasiatic Roots of Classical Civilization*, vol. 1: *The Fabrication of Ancient Greece 1785-1985*, Vintage, 1991. The use of the term "racist", as distinct from Spengler's term "Eurocen-

tric", refers *also* to the technology gap and the industrial revolution. See M. Adas, *Machines as the Measure of Men: Science, Technology and Ideologies of Western Dominance*, Oxford, New Delhi, 1990. While Bernal does not say much about the history of science *per se* (and neither do his detractors in the more recent debate in *Isis*), it is clear that the resurrection of Euclid, after the belated discovery that he could not have been Euclid of Megara, is very much in line with the belief in a 19th c. pattern of fabricating a Greek origin for everything under the sun. A closer look at the material basis (palimpsests etc.) of the conclusions of classical scholars will make clear the enormous amount of tinted speculation that underlies this belief.

39. The Hellenisation itself proceeded by reference to the military conquests of Alexander and Julius Caesar, and the in-between period of Ptolemaic rule. Consequently, the importance of these conquests got amplified out of all proportion to their global or even local significance.
40. School Mathematics Study Group, *Geometry*, Yale University Press, 1961.
41. D. Hilbert, *The Foundations of Geometry*, Open Court, La Salle, 1902.
42. B. Russell, *The Foundations of Geometry*, London, 1908.
43. G. D. Birkhoff, "A set of postulates for plane geometry (based on scale and protractor)", *Ann. Math.* **33** (1932).
44. C. K. Raju, "Mathematics and culture", in: *History, Time and Truth: Essays in Honour of D. P. Chattopadhyaya*, ed. Daya Krishna and K. Satchidananda Murty, Kalki Prakash, New Delhi 1998. Reprinted in *Philosophy of Mathematics Education* **11** (1999). Available at http://www.people.ex.ac.uk/PErnest/pome11/art18.htm.
45. The best that one can do is to *interpret* these meaningless notions using other meaningless notions like sets: e.g., a point is an element of a set, a line is a subset etc.
46. Heath, p. 247.
47. Heath, p. 224 et seq.
48. Aristotle, *Physics*, trans. R. P. Hardie, and R. K. Gaye, Book II, Encyclopaedia Britannica, Chicago, 1990, chp. 2, p. 270.
49. Immanuel Kant, *The Critique of Pure Reason*, trans. J. M. D. Meiklejohn, Encyclopaedia Britannica, Chicago, 1990, p. 31.
50. Schopenhauer, *Die Welt als Wille*, 2nd edn, 1844, p. 130, cited in Heath, p. 227.
51. For a detailed and easily accessible account, see E. Moise, *Elementary Geometry from an Advanced Standpoint*, Addison-Wesley, Reading, Mass., 1963; B. I. Publications, Bombay, 1966.
52. K. V. Sarma (ed. and trans.) *The GaṇitaYuktiBhāsā of Jyeṣthadeva* (to be published). For a description of the proof, see C. K. Raju, "Mathematics and culture", cited earlier.
53. *Gaṇita* 6–9. *Āryabhatīya of Āryabhata*, ed. and trans. K. S. Shukla and K. V. Sarma, INSA, New Delhi, 1976, pp. 38–45.
54. Martin Bernal, *Black Athena: The Afroasiatic Roots of Classical Civilization*, vol. 1: *The Fabrication of Ancient Greece 1785-1985*, Vintage, 1991.
55. As, e.g., in the DDD (Dreaded Diamond of Derivation) in object-oriented languages, or in corresponding human relationships.
56. A. M. Vaidya et al., *Mathematics: A Textbook for Secondary Schools, Class IX*, NCERT, 1989, Ninth Reprint Edition (sic) 1998, p. 124. As the date of the text shows, this text was produced well before the current controversy on the saffronization of education.
57. For the traditional notion of *pramāṇa* in relation to mathematics, see C. K. Raju, "Mathematics and culture", cited earlier.
58. Another text dealing with rationale, the *Karaṇapaddhati*, is now available in a Japanese translation, and being retranslated into English.
59. René Descartes, *The Geometry*, Book 2, trans. David Eugene and Marcia L. Latham, Encyclopaedia Britannica, Chicago, 1990, p. 544.
60. Proclus, cited earlier, 45.
61. René Descartes, *The Geometry*, cited above. We note, incidentally, that Descartes had a metric rather than a synthetic understanding of geometry.
62. For a more detailed exposition of the debate between Ibn Rushd and al Ghazālī, see C. K. Raju, *The Eleven Pictures of Time: The Physics, Philosophy, and Politics of Time Beliefs*, Sage, New Delhi, 2003.
63. The text is today believed to be a paraphrase of Proclus' *Elements of Theology*. See Richard C. Taylor, "A Critical Analysis of the Kalam fi'l mahd al-khair", *Neoplatonism and Islamic Thought*, ed. Parvez Morewedge, New York, 1992, pp. 11–40.
64. It is worth noting al Ghazālī's perceptive comment about al Hallāj of "an'al Haq" fame (that he should not have stated such things publicly).

65. Hair splitting over the exact anathemas of the 5th Ecumenical council is irrelevant here, since, for some 1400 years, Origen was clearly thought to have been condemned. See C. K. Raju, "The curse on cyclic' time", *The Eleven Pictures of Time: The Physics, Philosophy, and Politics of Time Beliefs*, Sage, New Delhi, 2003, chp. 2.
66. We consider in greater detail in Chapter 6, the political and religious foundations of this theory in the Doctrine of Christian Discovery. The latter doctrine was used to provide moral justification for the appropriation by the West of all the physical resources of the world. Brie y, this doctrine, based on the papal bulls Romanus Pontifex (1454), and the Bull Inter Caetera (1493), asserted that according to the Christian religion, and the Bible, a piece of land "belonged" to the first Christian to set foot on it, and these Christians were enjoined by the above papal directives (still in force) to kill, enslave, and ill treat the original inhabitants, as non-persons, as a form of religious duty (as actually happened). The Christian religious sanction for racism, genocide and slavery was implicit in the assertion that Columbus "discovered" America, an assertion which is part of present-day US law. The natural extension of this doctrine to the history of science has sought to appropriate also all intellectual resources in the world by asserting Western (Christian) priority in all important scientific discoveries. Since the Christians, rather obviously, had nothing whatsoever to do with science for the first 15 centuries of the Christian era, the extension of this doctrine to the intellectual domain required some additional support to prevent it from being openly regarded as ridiculous, even by those taught from childhood to subordinate the manifest truth to religious "faith". The requisite additional support is provided by the belief that what was not discovered by Christians was earlier discovered by their "Greek" predecessors, these being the one's who were regarded since Eusebius as the most theologically acceptable. Thus, the belief in a "pure Greek" origin of knowledge must be regarded primarily as a supportive religious belief—like the belief in immaculate conception—rather than a credible historical account.
67. All optimistically dated down to the 10th c. CE.
68. Richard C. Taylor, "A Critical Analysis of the Kalam fi'l mahd al-khair" in: *Neoplatonism and Islamic Thought*, ed. Parvez Morewedge, New York, 1992, pp. 11–40.
69. Herodotus, *History*, *Euterpe*,' 50–58, trans. G. Rawlinson, Encylopaedia Britannica, Chicago, 1990, pp. 60–61.
70. A. Toynbee, *A Study of History*, abridgement in 2 vols, by D. C. Somervell, Oxford University Press, 1957.
71. Homer, *Odyssey*, trans. Richmond Lattomore, Bk 4, line 355, p. 340, and by Thucydides, *History of the Peloponnesian War*, trans. Richard Crawley & R. Feetham, Bk 1, line 104, p. 375, Encyclopaedia Britannica, Chicago, 1990.
72. See note 66 above.
73. Anthony Pym, "Attempt at a Chronology of Hispanic Translation History", http://www.tinet.org/~apym/on-line/chronology/8-11.html and http://www.tinet.org/~apym/on-line/chronology/12.html.
74. Karl Menninger, *Number Words and Number Symbols: A Cultural History of Numbers*, trans. Paul Broneer, MIT Press, Cambridge, Mass., 1969, p. 325.
75. George Saliba, "Arabic Astronomy and Copernicus", *A History of Arabic Astronomy*, New York, 1994, chp. 15.
76. Martin Bernal, *Black Athena: The Afroasiatic Roots of Classical Civilization*, Vol. 1: *The Fabrication of Ancient Greece 1785-1985*, Vintage, 1991. George G. M. James, *Stolen Legacy: Greek Philosophy is Stolen Egyptian Philosophy*, Africa World Press, reprint, 1992. Cheikh Anta Diop, *The African Origin of Civilization: Myth or Reality?*, Lawrence Hill & Co. 1987. John M. Hobson, *The Eastern Origins of Western Civilisation*, Cambridge University Press, Cambridge, 2004.
77. T. L. Heath, *The Thirteen Books of Euclid s Elements*, Dover Publications, New York, [1908] 1956, vol. I, p. 75.

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CHAPTER 2

Proof vs *Pramāṇa*

Critique of the current notion of mathematical proof, and comparison with the traditional Indian notion of pramāṇa

OVERVIEW

IN contrast to the present-day notion of mathematical proof, all traditional Indian notions of *pramāṇa* accepted the empirically manifest (*pratyakṣa*), and this belief is carried over also into Indian mathematics from the days of the *śulba sūtra*, for mathematics was seen as a practical rather than a religious concern. Mathematics was *not* conceptualized as something separate from physics, and there was no fundamentally separate notion of *pramāṇa* for mathematics. Current-day mathematics, however, is divorced from the empirical (believed to be contingent), and rests entirely on a notion of proof based on rational deduction, believed to incorporate necessary truth.

Proof and deduction, however, depend upon logic, in the direct sense that the theorems derivable from a given set of axioms will vary with the logic used. But the particular choice of logic used today in mathematics is arbitrary, for logic varies with culture, as in the logic used by pre-Buddhist sceptics like Sañjaya, or the logic used in Buddhism, and Jain *syādavāda*. Hence mathematical proof is completely arbitrary: for the axioms are already admittedly arbitrary, and deduction rests on logic, so that the theorems will vary also with logic, while the choice of logic is arbitrary. Within the present-day philosophy of mathematics which regards mathematics as *a priori* and divorced from the empirical, there is simply no way that the choice of logic can be further justified, e.g. by appealing to the empirical—for if logic itself is to be founded on the empirical, then it is surely legitimate to use the empirical in mathematical proof.

Thus, social authority is the ultimate foundation of the present-day notion of mathematical proof, and it is manifest that social authority is rather more fallible than reliance on the empirical. On the other hand, if we do appeal to the empirical to decide the nature of logic, then we can hardly bypass our most sophisticated physical theories, regarding time and quantum mechanics, so that the eventual decision may well be in favour of quasi truth-functional Buddhist or quantum logic rather than two-valued truth-functional logic.

In any case, an empirical decision regarding logic can only be an inductive process. Thus, whether one uses social authority or appeals to the empirical to decide the nature of logic, in all cases deduction will forever remain *less* certain than induction, contrary to what has long been incorrectly advocated in Western philosophy, due to theological predilections. Hence, also, it seems desirable to shift back from mathematics-as-proof to mathematics as a practical and empirical matter of calculation.

I
INTRODUCTION

> In the nineteenth century the idea of "the white man's burden" helped justify the extension of Western political and economic domination over non-Western societies. At the end of the twentieth century the concept of a universal civilization helps justify Western cultural dominance of other societies and the need for those societies to ape Western practices and institutions. Universalism is the ideology of the West for confrontation with non-Western cultures.
>
> Samuel P. Huntington[1]

The East–West Civilizational Clash in Mathematics: Pramāṇa vs Proof

Exactly how universal is the method of rational deduction which underlies present-day mathematics, and which is alleged to be universal? In Huntington's terminology of a clash of civilizations, one might analyse the basis of the East–West civilizational clash as follows: the Platonic tradition is central to the West, even if we do not go to the extreme of Whitehead's remark, characterizing all Western philosophy as no more than a series of footnotes to Plato. But the same Platonic tradition is completely irrelevant to the East.

In the present context of mathematics, the key issue concerns Plato's dislike of the empirical, so the civilizational clash is captured by the following central question: *can a mathematical proof have an empirical component?*

The Platonic and Neoplatonic Rejection of the Empirical

According to university mathematics, as currently taught, the answer to the above question is no. Current-day university mathematics has been enormously in uenced by (Hilbert's analysis of) "Euclid's" *Elements*, and Proclus,[2] a Neoplatonist and the first actual source of the *Elements*, argued that

> Mathematics... occupies the middle ground between the partless realities... and divisible things. The unchangeable, stable and incontrovertible character of [mathematical] propositions shows that it [mathematics] is superior to the kinds of things that move about in matter.... Plato assigned different types of knowing to... the... grades of reality. To indivisible realities he assigned intellect, which discerns what is intelligible with simplicity and immediacy, and... is superior to all other forms of knowledge. To divisible things, in the lowest level of nature, that is, to all objects of sense-perception, he assigned opinion, which lays hold of truth obscurely, whereas to intermediates, such as the forms studied by mathematics, which fall short of indivisible but are superior to divisible nature, he assigned understanding.

In Plato's simile of the cave, the Neoplatonists placed the mathematical world midway between the empirical world of shadows, and the real world of the objects that cast the shadows. Mathematical forms, then, were like the images of these objects in water—superior to the empirical world of shadows, but inferior to the ideal world of the intellect, which could perceive the objects themselves.

Proclus explains that the term "mathematics" means, by derivation, the science of learning, and that learning ($\alpha\ \theta$) is but recollection of the knowledge that the soul has from its previous births which it has forgotten—as Socrates had demonstrated with the slave-boy. Hence, for Proclus, the object of mathematics is "to bring to light concepts that belong essentially to us" by taking away "the forgetfulness and ignorance that we have from birth", and re-awakening the knowledge inherent in the soul. Hence, Proclus valued mathematics (especially geometry) as a spiritual exercise, like *haṭha yoga*, which turns one's attention inwards, and away from sense perceptions and empirical concerns, and "moves our souls towards Nous" (the source of the light which illuminates the objects, of which one normally sees only shadows, and which one could better understand through their re ections in water).

In regarding mathematics as a spiritual exercise, which helped the student to turn away from uncertain empirical concerns to eternal truths, Proclus was only following Plato. The young men of Plato's *Republic* (526 et seq.) were required to study geometry because Plato thought that the study of geometry uplifts the soul. Plato thought that geometry being knowledge of what eternally exists, the study of geometry compels the soul to contemplate real existence; it tends to draw the soul towards truth. Plato emphatically added, "if it [geometry] only forces the changeful and perishing upon our notice, it does not concern us,"[3] leaving no ambiguity about the purpose of mathematics education in the Republic.

Rejection of the Empirical in Contemporary Mathematics: Proof as Necessary Truth vs the Empirical World as Contingent

A more contemporary reason to reject any role for the empirical in mathematics is that the empirical world has been regarded as contingent in Western thought. Any proposition concerning the empirical has therefore been regarded as a proposition that can at best be *contingently* true. Hence, such propositions have been excluded from mathematics which, it has been believed, deals only with propositions that are *necessarily* true: either eternally true, or at least true for all future time, or true in all possible worlds.[4]

In the 20th century CE, it has, of course, again been (partly) accepted that mathematical theorems are not absolute truths,[5] but are true relative to the axioms of the underlying mathematical theory. Nevertheless, the relation between the axioms and theorems is still regarded as one of necessity: the theorems are believed to be *necessary* consequences of the axioms—it is believed that every possible (logical) world in which the axioms are true is a world in which the theorems are also true. A mathematical theorem such as $2 + 2 = 4$ is no longer regarded as eternally true, but, since this theorem can be *proved*, since it can be logically deduced from Peano's axioms, it is believed that $2+2 = 4$ is a necessary and certain *consequence* of Peano's axioms. It is today believed that though neither any axiom nor the theorem can be called a "necessary truth", the relation between axioms and the theorem can be so called. A theorem being the last sentence of a proof, theorems relate to axioms through the notion of mathematical proof, which is believed to embody and formalise the notion of logical necessity. Contemporary Western mathematics has not abandoned the notion of "necessary truth", it has merely shifted the locus of this "necessary truth" from theorems and axioms to proof. From this perspective, admitting the empirical into mathematical proof would weaken and make contingent the relation of theorems to axioms, so that the empirical is still not allowed any place in the formal mathematical demonstration called "proof".

The current definition of a formal mathematical proof, as enunciated by Hilbert, may be found in any elementary text on mathematical logic.[6] This definition may be stated informally as follows. A mathematical proof consists of a finite sequence of statements, each of which is either an axiom or is derived from two preceding axioms by the use of modus ponens or some similar rules of reasoning. Modus ponens refers to the usual rule: A, $A \Rightarrow B$, hence B. The other "similar rules of reasoning" must be prespecified, and may include simple rules such as instantiation (for all x, $f(x)$, hence $f(a)$), and universalization ($f(x)$, hence, for all x, $f(x)$) etc. A mathematical proof being such a sequence of statements, a reference to the empirical cannot be introduced in the course of a proof.

Neither can there be any reference to the empirical in the axioms at the beginning of a proof. Here, the word "axiom" is used in the sense of "postulate". Axioms are not regarded as self-evident truths; axioms are merely an in-principle arbitrary set of propositions whose *necessary* consequences are explored in the mathematical theory. Since there is no reference here to the empirical, mathematical postulates and the primitive undefined symbols they involve are regarded as being, in principle, completely devoid of meaning.

Postulates relating to the empirical world lead to a physical theory, and not to mathematics. This difference between mathematical and physical theories is embodied also in Popper's criterion of refutability as follows. The theorems of the sentence calculus are exactly the tautologies. Though these tautologies may not be obvious, being tautologies, they are not refutable. Unlike a mathematical theory, a physical theory must be (logically) refutable, and hence must contain some hypotheses and conclusions that are not tautologies. Mathematics concerns the tautologous relation between hypothesis and conclusions, while physics involves the empirical validity of the hypothesis/conclusions. Thus, no mathematical theory is a physical theory according to this widely-used current philosophical classification, since no mathematical theory involves the empirical.

Acceptance of the Empirical in Indian Thought

However deep rooted may be this rejection of the empirical, in Western ways of thinking about mathematics, it seems to have gone unnoticed that not all cultures subscribe to this elevation of metaphysics above physics. Not all cultures and philosophies subscribe to this belief that the empirical world is contingent, and that only the non-empirical can be necessary. For example, the Lokāyata (popular/materialist) stream of thought in India adopts exactly the opposite viewpoint. It explicitly rejects any world except that of sense perception. It admits the *pratyakṣa* or the empirically manifest as the *only* sure means of *pramāṇa*, or validation, while rejecting *anumāna* or inference as error-prone, and fallible. That is, in terms of the Platonic gradation of reality, Lokāyata places intellectual ways of knowing on a *lower* footing than knowledge relating directly to sense perception. Howsoever odd this may seem from a Western perspective, and notwithstanding the orientalist characterization of Indian thought as "spiritual", all major Indian schools of thought concur in accepting the *pratyakṣa* as a valid *pramāṇa*, or means of validation. Moreover, *pratyakṣa* is the sole *pramāṇa* that is so accepted by all schools, since Lokāyata rejects *anumāna*, while Buddhists accept *anumāna* but reject *śabda* or authoritative testimony, though Naiyāyika-s accept all three, and add the fourth category of analogy (*upamāna*).

That is, the means of proof acceptable to all in Indian tradition consist of only

(1) ***pratyakṣa*** (the empirically manifest),

while the Buddhists and Jains accept also

(2) ***anumāna*** (inference),

and the Naiyāyika-s accept also proof based on

(3) ***śabda*** (authority/authoritative testimony), and

(4) ***upamāna*** (analogy).

As explained in box 2.1, *pratyakṣa* should not be confounded with induction.

Box 2.1. *Pratyakṣa* vs induction

Pratyakṣa should not be confused with induction. The con ict between deduction and induction is peculiar to Western thought, with deduction being divorced from the empirical, and induction being associated with the empirical. (The principle of mathematical induction, as articulated in Peano's axioms, should be classified with deduction, even though it is a postulate rather than a rule of reasoning.) The *pratyakṣa*, though it is associated with the empirical, differs from induction in that it contains no claim or overtone of any inference about the future, and no attempt to generalize the observation to all categories. *Pratyakṣa* should be regarded as mere observation, not an inference from it.

Of course, the *pratyakṣa* is fallible in the same sense that observations may have errors. This fallibility is recognized in the classical example of the situation where a rope is mistaken for a snake or vice versa. Tradition does not explicitly state any remedy for this situation, but it would have no difficulty in agreeing to the idea that in case of doubt the matter must be settled by subjecting it to test (*parikṣā*)—tap the rope/snake with a stick.

After a sufficient number of proddings (i.e., a repeated series of experimental observations), the doubt should be settled from a practical perspective, although it is possible to hang on to the philosophical doubt long after the rope/snake is dead with prodding.

From the Western perspective, contrary to what Popper has maintained, this series of observations is indeed an inductive process. Popper's argument is that probabilities are not ampliative; therefore, repeated observation does not change probabilities. Popper has in mind a formal Kolmogorov model of probabilities. Granting this, the problem that Popper overlooked is that one never knows what the probabilities actually are. All one has is an *estimate* of the probabilities, or likelihood. It is an elementary thing that likelihood will and does change with repeated observations, and that one may adopt, for example, a maximum likelihood estimate: when two experiments were for and one was against the violation of Bell's inequalities, the likelihood of Bell's inequalities being violated was different from what it became with five experiments for and two experiments against it, which eliminated all practical doubt regarding the violation of the inequalities. Thus, likelihoods may be ampliative, unlike probabilities, so that the process of repeated observations with, say, maximum likelihood estimation is an inductive process.

However, given the Western obsession with prophecy and foretelling the future as the test of truth, there is another sense in which the term induction is used: viz. in the sense of using a series of observation to foretell the future. We observe the sun rising from the east 10,000 times and conclude that it will rise from the east for all future time. This sense of the term induction, related to inductive inference, is completely missing in *pratyakṣa*, which relates to observation here and now.

The idea of prophecy was rejected early in Indian tradition. Specifically, at the time of the Buddha, earning a living by predicting the future was regarded as unethical, by common people, as the Buddha states in the *Dīgha Nikāya*. Thus, *pratyakṣa*, as observation, must be separated from induction as a means of generalizing that observation.

While *anumāna* is similar to deduction, there is a little twist related to the nature of logic. This is summarily explained in box 2.2, and is considered in more detail later on.

Box 2.2. *Anumāna* vs deduction

Anumāna or inference is closer to deduction than *pratyakṣa* is to induction, but *anumāna* nevertheless needs to be separated from deduction. A subtle but fundamental difference is in the nature of the underlying logic. Though Buddhists, Jains, and Naiyāyikas all accept the use of *anumāna* for *pramāṇa*, they disagree on the logic underlying inference. Summarily, these are quasi truth-functional logic (Buddhist), three-valued logic (Jain), many-valued logic (Sañjaya), and two-valued logic (Nyāya). These differences in logic pertain to differences in the perception of time, and these differing time perceptions are at the core of the respective philosophies.[7]

The concept of *śabda* is similar to authority, except that it, too, is accepted as fallible. (An example is provided in box 2.3.)

Box 2.3. *Śabda* vs scriptural testimony

Śabda is the (spoken) testimony of a credible person. This is accepted as a means of proof in present-day law, as in the testimony of a credible witness. This is also accepted as a means of proof in present-day science, as in the report of an experiment, perhaps costing several billion dollars, performed by a credible laboratory, though it is expected to be documented or written down, in the manner in which Western scriptures are written, rather than spoken.

Although formal proofs in present-day mathematics are deductive in theory, testimony (as in a proof published in a reputed journal) is also the only real means of proof that many an expert has for believing in many complex mathematical results, for which one has perforce to rely upon the authority of the person and the journal wherein the result is published, since it would be impracticable and too time consuming to check out the proof on one's own, and the human life span is limited. This tendency (to believe formal mathematical results on authority) will surely increase as computers are used to produce more and more complex formal mathematical proofs that stretch further and further beyond the understanding of most human beings.

However, like *pratyakṣa* and *anumāna*, *śabda* too is not regarded as infallible either. At any rate, it is not regarded as being necessarily true, or true for all time. An example is provided by Varāhamihīra, who asserts, in his *Pañcasiddhāntikā*,[8] about the authority of the *Vedāṅga Jyotiṣa*, that our ancestors were no doubt right, but things have manifestly changed since then. This also shows that *śabda* must yield to *pratyakṣa*.

Mathematics was valued in Indian tradition, but it was not accorded the glorified place it has in Western philosophy. In particular, there was no distinction between mathematics and physics of the sort prevalent in the West from the time of Aristotle. This was particularly true with regard to the *pratyakṣa*, or the empirical.

Accordingly, the *pratyakṣa* enters explicitly also into mathematical rationale, in the Indian way of doing mathematics from the time of the *śulba sūtra*-s (ca. –600 CE),[9] through Āryabhaṭa (ca. 500 CE)[10] and up to the time of the *Yuktibhāṣā* (ca. 1530 CE). For example, the geometry of the *śulba sūtra*-s, as the name suggests, involves a rope (*śulba*) for measurement. Aryabhata defines water level as a test of horizontality, and the plumb line as the test of perpendicularity (*Gaṇita* 13):

> The level of ground should be tested by means of water, and verticality by means of a plumb.

The *Yuktibhāṣā* proves the "Pythagorean" "theorem"[11] in one step, by drawing a diagram on a palm leaf, cutting along a line, picking and carrying. The rationale is explained in the accompanying figure (Fig. 2.1): the figure is to be drawn on a palm leaf, and, as indicated, it is to be measured, cut, and rotated.

> Now, draw a square [with its side] equal to the *koṭi* [longer side of the triangle], and another equal to the *bhuja* [shorter side of the triangle]. Let the *bhuja* square be on the northern side and the *koṭi* square on the southern side, in such a way that the eastern side of both the sides [squares] falls on the same line, and in such a manner that the southern side of the *bhuja*-square lies alongside the *koṭi*. [Since the *koṭi*] is longer than the *bhuja* on the [*koṭi*] side, there will be an extension [of the *koṭi*] towards the western side further than the *bhuja*. From the north-east corner of the *bhuja*-square, measure southwards up to the *koṭi*, and mark [the spot] with a point. From this [point] the line towards the south will be of the length of the *bhuja*. Then cut on lines from the point to the south-west corner of the *koṭi*-square and the north-west corner of the *bhuja*-square, dividing the squares [into equal triangles]. Allow a little clinging at the two corners so that the cut portions do not fall away. Now break off the two parts [i.e., the triangles]

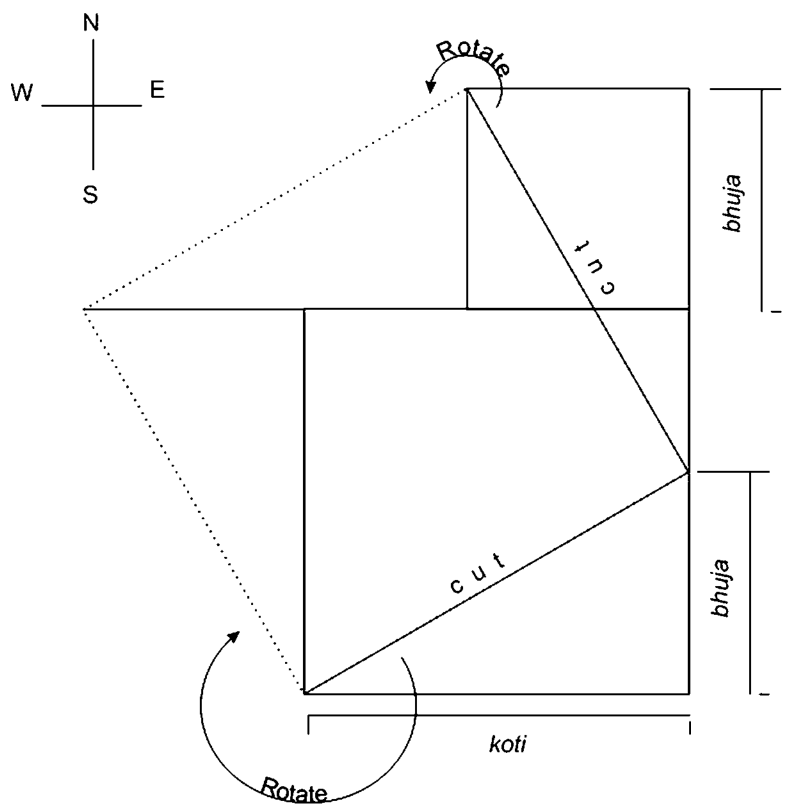

Figure 2.1: ***Yuktibhāṣā* proof of the "Pythagorean" theorem.** The square corresponding to the smaller side (*bhuja*) is drawn on a palm leaf and placed on the square corresponding to the bigger side (*koṭi*), as shown. The *bhuja* is measured off from the SE corner of the larger square, and joined to the SW corner of the larger square and the NW corner of the smaller square. Cutting along the joining lines and rotating gives the square on the hypotenuse. This simple proof of the "Pythagorean" "theorem" involves (a) measurement, and (b) movement of the figure in space.

> at the point, turn them round alongside the two sides of the bigger (i.e., *koṭi*) square, so that they meet at the north-east, and join them, so that the inner cut of one joins with the outer cut of the other. The figure formed thereby will be a square. And the side of this square will be equal to the hypotenuse of the original *bhuja-koṭi* [rectangle]. Hence it is established that the sum of the squares of the *bhuja* and *koṭi* is equal to the square of the *karṇa* [hypotenuse]....[12]

The details of this rationale are not our immediate concern beyond observing that drawing a figure, carrying out measurements, cutting, and rotation are all empirical procedures. Hence, such a demonstration would today be rejected as invalid solely on the ground that it involves empirical procedures that *ought* not to be any part of mathematical proof.

Genesis of the Current Notion of Mathematical Proof: SAS and the Empirical

We recall from Chapter 1 the historical process by which the empirical was eventually eliminated from Western mathematics, and how the persuasiveness of the *Elements* became the sole element for its acceptance by Christian rational theology, discarding equity. Paradoxically, though the currently dominant notion of mathematical proof, as formulated by Hilbert at the turn of the century, is essentially modelled on "Euclid's" *Elements*, the empirical is *not* entirely rejected in the *Elements*. "Mathematically proved" is, even today, virtually synonymous with "incontrovertible". In Christian rational theology, this was in contrast to empirical procedures which were *not* "incontrovertible", since the empirical world had to be regarded as contingent.[13]

As we have seen, in contemporary Western philosophy of both mathematics and science, this belief in the contingency (hence unreliability) of the empirical world is very deep rooted, in Popper's criterion of falsifiability, for example. In a historical perspective, the need to regard the empirical world as contingent can be readily located in the requirements of theology, and specifically Christian rational theology. If "necessary" is interpreted to mean true for all time, then a necessary world could hardly have been created by God. On the other hand, if "necessary" is interpreted to mean true for all future time, then God would be unable to destroy the world, as in the doctrine of apocalypse. Finally, if "necessary" is interpreted to mean true in all possible worlds, God would not have a choice in the kind of world to create. Asserting the necessity of the empirical world in any sense con icts with fundamental theological ideas about God's role in creation and apocalypse.

The roots of these difficulties can be traced to the Augustinian modification of Christian theology, which made God transcendent and all powerful. Islamic rational theology, in contrast, viewed creativity as immanent, and hence was willing to admit limits to what God could do. This was similar to the belief among old-Egyptians/Neoplatonists like Proclus who were far closer to the theology of Origen which regarded God as immanent, and hence regarded creation as an ongoing process, rather than a one time affair lasting for a week. Even al Ghazālī championed the notion of ontically broken time, which makes creation a continuous process.

Proclus, further, quite explicitly accepted the eternity of the cosmos. He regarded it as related to necessity of mathematical truths regarded as eternal truths. Accordingly, Proclus did not need to reject any role for the empirical in mathematics.

Thus, while Proclus regarded mathematics as a means of moving away from the empirical, he did *not* regard mathematics as *disjoint* from the empirical; he did *not* think the empirical had no role at all in a mathematical proof—he thought a proof must suit the thing to be proved.[14]

> Proofs must vary with the problems handled and be differentiated according to the kinds of being concerned, since mathematics is a texture of all these strands and adapts its discourse to the whole range of things.

Since Proclus accorded to mathematics an intermediate status, between the gross empirical world and the higher Platonic world of ideals, *Proclus was ready to accept the empirical at the beginning of mathematics*, just as much as he was ready to accept that diagrams had an essential role in mathematical proof, to stir the soul from its forgetful slumber. While there was a change between Proclus and Hilbert, this change did not constitute "progress": had Hilbert preceded Proclus, then Hilbert's view of mathematics would have been rejected as unsound by Proclus.

As we have seen, in actual fact, this reference to the empirical in *Elements* I.4 was subsequently eliminated following Hilbert,[15] Russell,[16] etc. who suggested that "Euclid" had made a mistake in proving the theorem. Hence, that *theorem* was incorporated as the SAS *postulate*, today taught in school geometry.[17] The theorem asserts that if two sides and the included angle (side-angle-side) of one triangle are equal to those of another triangle, then the two triangles are equal ("congruent" in Hilbert's terminology, which bypassed also the political significance of equity in the *Elements*, which was a key aspect of the *Elements* for Neoplatonists and Islamic rational theologians). The proof of this theorem, as actually found in all known manuscripts of the *Elements*, involves picking one triangle, moving it and placing it on top of the other triangle to demonstrate the equality—an empirical procedure similar to that used in the *Yuktibhāṣā* proof of the "Pythagorean" "theorem". The proofs of subsequent theorems of the *Elements*, however, avoid this empirical process, with the possible exception of I.8.

The question before us is this: is it legitimate to accept the empirical at one point in mathematical discourse, and to reject it elsewhere?

From the point of view of Proclus, the appeal to the empirical in the proof of I.4 was acceptable, since proofs must be differentiated according to the kinds of being, and the empirical was the starting point of mathematics, though not its goal. Empirical procedures were therefore acceptable in proofs at the beginning of mathematics, though the proofs of subsequent propositions must move away from the empirical, to suit the objectives of mathematics. For Hilbert, who sought the standardization and consistency suited to an industrial civilization, a notion of mathematical proof that varied according to theorems, or "kinds of beings", was not acceptable. Indeed, in Hilbert's time, in the West, industrialization was practically synonymous with civilization, as in the statement: "Civilization disappears ten feet on either side of the railway track in India". So it is no surprise that Hilbert's view of mathematics was entirely mechanical[18]—where Proclus sought to persuade human beings, Hilbert sought to persuade machines! Hilbert's notion of proof, therefore, had to be acceptable to a machine; a proof had to be so rigidly rule-bound that it could be mechanically checked—an acceptable proof had to be acceptable in *all* cases. Hence, exceptions do not prove the rule; a single exception disproves the rule—a belief that is the basis also of Popper's criterion of falsifiability. Hence, Hilbert et al. chose to reject as unsound the proof of *Elements* I.4. As we have seen in Chapter 1, in rejecting the traditional demonstration of *Elements* I.4, Hilbert also thought that he re ected the Western view since Aristotle which sought to separate physics from mathematics.

The Epicurean Ass

The requirement of a consistent notion of proof limited Hilbert's options. If an appeal to the empirical is permissible in the proof of *one* theorem (*Elements*, I.4), then why not permit an appeal to the empirical in the proof of *all* theorems? Why not permit triangles to be moved around in space to prove the "Pythagorean" theorem (*Elements*, I.47), as in the *Yuktibhāṣā* proof? Why not permit length measurements? Accepting the empirical as a means of proof (or even introducing a measure of length axiomatically, as done by Birkhoff[19]) simplifies the proofs of the theorems in the *Elements*. In fact, so greatly does it simplify the proofs that it makes most of the theorems of the *Elements* obvious and trivial! Since the indigenous Indian tradition of geometry relied on measurement, one strand of Indian tradition hence rejected the *Elements* as valueless from a practical viewpoint, until the 18th century when they were first got translated from Persian into Sanskrit by Jai Singh. (This simple answer to a question raised by Needham shows, incidentally, that even a relatively unbiased historian like Needham could not entirely transcend the prejudices that prevailed in his time.)

That the *Elements* are trivialised by the consistent acceptance of the empirical, definitely was the basis of the objections raised by the Epicureans, who may be regarded as the counterpart of the Lokāyata, in Greek tradition. The Epicureans argued, against the followers of "Euclid", that the theorems of "Euclid's" *Elements* were obvious even to an ass. They particularly referred to *Elements* I.20, which asserts: in any triangle the two sides taken together in any manner are greater than the third. The Epicureans argued that any ass knew the theorem since the ass went straight to the hay and did not follow a circuitous route, along two sides of a triangle. Proclus replied that the ass only knew *that* the theorem was true; he did not know *why* it was true.

The Epicurean response to Proclus has, unfortunately, not been well documented. The Epicureans presumably objected that mathematics could *not* hope to explain *why* the theorem was true, since mathematics was ignorant of its own principles. They presumably quoted Plato (*Republic*, 533)[20]

> geometry and its accompanying sciences...—we find that though they may dream about real existence, they cannot behold it in a waking state, so long as they use hypotheses which they leave unexamined, and of which they can give no account. For when a person assumes a first principle which he does not know, on which first principle depends the web of intermediate propositions and the final conclusion—by what possibility can such mere admission ever constitute science?

It is to this objection that Proclus presumably responds when he asserts that Plato does not declare that

> mathematics [is] ignorant of its own principles, but says rather that it takes its principles from the highest sciences and, holding them without demonstration, demonstrates their consequences.[21]

This appeal to Plato's authority, and to the Platonic gradation of the sciences, is obviously inadequate to settle the issue—for the Lokāyata would reject as non-science what Plato regards as the "highest science" (though they would have agreed with Proclus about equity). Contrary to Plato, the Lokāyata would insist that mathematics must take its principles from the empirical world of sense-perceptions, a move that would also destroy the difference between mathematics and physics in current Western philosophical classification.

Though Proclus has gone largely unanswered down the centuries, presumably because no Epicureans were left to respond to him, the present chapter will provide an answer from the perspective of traditional Indian mathematics.

Mathematics as Calculation vs Mathematics as Proof

The trivialization of the *Elements* by the acceptance of the empirical can be viewed from another angle: what is mathematics good for? why do mathematics? As already stated, Proclus explains at great length in his introduction to the *Elements* that though (a) mathematics has numerous practical applications, (b) mathematics must be regarded primarily as a spiritual exercise. Thus, Proclus states:

> Geodesy and calculation are analogous to these sciences [geometry, arithmetic], ...[but] they discourse not about intelligible but about sensible numbers and figures. For it is not the function of geodesy to measure cylinders or cones, but heaps of earth considered as cones and wells considered as cylinders; and it does not use intelligible straight lines, but sensible one, sometimes more precise ones, such as rays of sunlight, sometimes coarser ones, such as a rope or a carpenter's rule.[22]

Clearly, for Proclus, the practical applications of mathematics were its lowest applications involving "sensible" objects rather than "intelligible" objects:

> instead of crying down mathematics for the reason that it contributes nothing to human needs—for in its lowest applications, where it works in company with material things, it does aim at serving such needs—we should, on the contrary, esteem it highly because it is above material needs and has its good in itself alone.[23]

This echoes the Platonic deprecation of the applications of mathematics (*Republic*, 527):

> They talk, I believe in a very ridiculous and poverty-stricken style, for they speak invariably of squaring and producing and adding, and so on, as if they were

> engaged in some business, and as if all their propositions had a practical end in view: whereas in reality I conceive that the science is pursued wholly for the sake of knowledge.[24]

Plato clearly thought of mathematics-as-calculation as distinctly below mathematics-as-proof, and this Platonic valuation led to the implicit valuation of pure mathematics as superior to applied mathematics, and to the resulting academic vanity of pure mathematicians, who regarded (and still regard) themselves as superior to applied mathematicians—a vanity so amusingly satirized in Swift's *Gulliver s Travels*.

> His Majesty discovered not the least curiosity to enquire into the laws, government, history, religion, or manners of the countries where I had been; but confined his questions to the state of mathematicks, and received the account I gave him, with great contempt and indifference....[25]

In traditional Indian mathematics, however, there never was such a con ict between "pure" and "applied" mathematics, since the study of mathematics never was an end in itself, but always was directed to some other practical end. Geometry, in the *śulba sūtra,* was not directed to any spiritual end, but to the practical end of constructing a brick structure. Contrary to Plato, calculation was valued and taught for its use in commercial transactions, as much as for its use in astronomy and timekeeping. Proof was not absent, but it took the form of rationale for methods of calculation. The methods of calculation were regarded as valuable, not the proofs by themselves—there was no pretence that rationale provided any kind of absolute certainty or necessary truth. Rationale was not valued for its own sake. Hence, rationale was not considered worth recording in many of the terse (*sūtra*-style) authoritative texts on mathematics, astronomy, and timekeeping. On the other hand, rationale was not absent, but was taught, as is clear, for example, from the very title *Yuktibhāṣā*, or in full form, the *GaṇitaYuktiBhāṣā*, which means "discourse on rationale in mathematics".

The Epistemological Discontinuity

We now have before us several different ways in which mathematics has been historically perceived. For example:

(a) The Procluvian view of mathematics as the science of learning, hence an instrument of spiritual progress.

(b) The view of Christian rational theology that mathematics is an instrument of persuasion, since it (supposedly) incorporates universal and certain knowledge. Deriving from this is the formalist view of mathematics as proof—which proof (supposedly) incorporates necessary truth.

(c) The Indian view of mathematics as primarily an instrument of practical calculation which is not disjoint from the empirical.

Thus, the belief in the universality and certainty of mathematics has certainly not been universal across cultures! Nor has it been universal across time. We have already seen in Chapter 1, how the condemnation and banning of the Procluvian view followed by its reinterpretation led to the view of Christian rational theology, and how this evolved into the present-day view of formal mathematics.

Historically speaking, this quaint mediaeval theological belief in the universality and certainty of mathematics proved to be a serious impediment in accepting the practical benefits of mathematics. We can see this in the two key cases of the algorismus and the calculus.

Thus, it is natural that those Europeans who valued the practical applications of mathematics—the Florentine merchants—played a major role in first importing the Indian techniques of calculation into Europe, as algorismus texts. (Algorismus, as is well known, is a Latinization of al Khwarizmi, and refers to the Latin translations of al Khwarizmi's Arabic translation of Sanskrit manuscripts like those of Brahmagupta.) The Florentine merchants clearly saw that the ability to make rapid calculations conferred a competitive advantage in commercial transactions. Hence they adopted the algorismus. However, the algorismus notion of number differed from the abacus notion of number, and this led to difficulties. The simplest of these difficulties was that the algorismus enabled efficient calculation by using the place value system, and especially zero, but this did not fit into the additive system of Roman numerals tied to the abacus. There were other subtler difficulties related to representability: for common commercial problems, the algorismus used techniques like the algorithm for square-root extraction. This made manifest the difficulty in representing numbers like $\sqrt{5}$, for which one could find a good practical approximation, but no exactitude. These difficulties of representation were of absolutely no consequence for purposes of practical or commercial computation, since a number such as $\sqrt{5}$ could be represented to any desired degree of accuracy, e.g. $\sqrt{5} = 2.2360679774$ and the remaining $0.00000000000997896964091736687313\ldots$ (non-representable) could always be treated as if it were zero. These difficulties were also not of any philosophical consequence, from the perspective of a philosophy such as *śūnyavāda*, which accepts non-representability, and denies the existence of any underlying ideal entity—as we shall see in more detail in a later chapter.

However, these difficulties arising from the different epistemology underlying the algorismus were almost insurmountable for Europeans who regarded mathematics as universal, and *their* understanding of mathematics as the only possible one, and hence tried to hang on to the idealist understanding of number. A common way to express these difficulties was that mathematics being perfect, even the smallest quantity could not be discarded. Thus, these contrasting epistemologies of mathematics led to major difficulties in Europe in accepting the algorismus. Though the practical applications of mathematics were valued *de facto* in the West, so enormous were the difficulties that the West had in understanding the Indian tradition of mathematics, that the acceptance of algorismus texts in Europe took around *five centuries*,[26] from the first recorded attempts to relate to the algorismus from the time of the 10th c. CE Gerbert (Pope Sylvester II) to the eventual triumph of algorismus techniques as depicted on the cover of Gregor Reisch's *Margarita Philosophica*.[27] Indeed, it took a little longer than that, for the British Treasury continued to use the competing abacus techniques as late as the 18th c. CE, since the algorismus techniques were not regarded as reliable enough for use by the state exchequer. Thus, formalist epistemology severely inhibited even the acceptance of elementary arithmetic in Europe.

A closely analogous epistemological discontinuity arose in connection with the import of the calculus in Europe. As we will see in the next few chapters, the "Pythagorean" theorem is merely the starting point of the *Yuktibhāṣā* which goes on to develop infinite series expansions for the sine, cosine, and arctan functions, nowadays known as the "Taylor" series expansions, to calculate very precise numerical values for the sine and cosine functions. These expansions arose naturally in the course of determining the length of the arc, since Indian geometry was unabashedly metric and used a rope to measure the length of curved lines, so that the notion of the "length of the arc" did not present the slightest conceptual problem.

In the 16th c. CE, Indian mathematical and astronomical manuscripts, because of their practical application to navigation through astronomy and timekeeping, engaged the attention of Jesuit priests in Cochin. Christoph Clavius, who reformed the Jesuit mathematical syllabus at the Collegio Romano, emphasized the practical applications of mathematics. A student and later correspondent of the famous navigational theorist Pedro Nunes, Clavius understood the relation of the date of Easter to latitude determination through measurement of solar altitude at noon, as described in the 7th c. CE texts of Bhāskara I—the *Mahā Bhāskarīya* and the very widely distributed *Laghu Bhāskarīya*.[28] In his role as head of the committee for the Gregorian calendar reform, Clavius received inputs from correspondents and former students like Matteo Ricci whom he had trained in mathematics, astronomy, and navigation, and who visited Cochin to learn about Indian methods of timekeeping. (The Jesuits, of course, knew Malayalam, the language of the *Yuktibhāṣā*, and had even started printing presses in Malayalam by then, and were teaching Malayalam to the locals in the Cochin college, latest by 1590.)

Precise sine values were needed in Europe for various practical purposes related to navigation—to calculate loxodromes, for example—hence precise sine values were a key concern of European navigational theorists, and astronomers like Nunes, Mercator, Simon Stevin,[29] and Christoph Clavius,[30] who provided their own sine tables.

Despite the practical value of the calculus, the contrasting epistemologies of Indian and Western mathematics, however, led to another protracted epistemological struggle. This

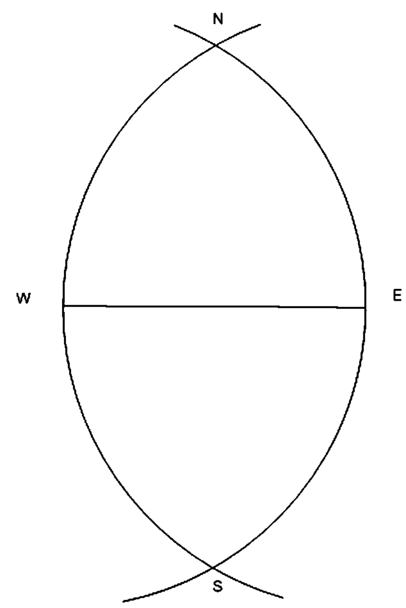

Figure 2.2: **The fish figure.** With W as centre and WE as radius two arcs are drawn, and they intersects the arcs drawn with E as centre and EW as radius at N and S. The above construction, called the "fish figure", was used in India to construct a perpendicular bisector to the EW line and thus determine NS. In *Elements*, I.1, a similar construction is used to construct the equilateral triangle WNE on the given segment WE. Though it is empirically manifest (*pratyakṣa*) that the two arcs must intersect at a point, to *prove* their intersection, without appeal to the empirical, formal real numbers are required, for, with rational numbers, the two arcs may "pass through" each other, without there being any (exact) point at which they intersect, since there are "gaps" in the arcs, corresponding to the "gaps" in rational numbers.

involved various issues, such as the meaning to be assigned to the length of a curved line. The computation of precise sine values is closely related to the numerical determination of the length of the arc of a circle, and we have seen (p. 38) how Descartes declared in his *La Geometrie* that "the ratios between straight and curved lines...cannot be discovered by human minds" and that conclusions based on such ratios could never hope to be "rigorous and exact",[31] so that they did not constitute mathematics. Descartes' pompous assertion about "human minds" did apply to minds steeped in Western culture: the "infinitesimals" and "infinities" of the calculus also puzzled other leading European minds like Newton and Leibniz, who could not give a clear account of them. This initiated the protracted epistemological struggle in Europe concerning the meaning and nature of infinitesimals (according to idealistic mathematics). It was only towards the end of the 19th century that Dedekind's formalisation of the real numbers partly resolved the issues regarding infinitesimals, while also providing a metaphysical basis to the implicit and less-noticed reference to the empirical in the proof (Fig. 2.2) of the very first proposition in the *Elements*. Needless to say, this formalisation of real numbers did not add an iota of practical value to the real numbers as used since the days of the *śulba sūtra*-s. However, the felt need for a theologically correct proof once again inhibited the acceptance of a practically useful technique for which, as we shall see in the next chapter, there was adequate *pramāṇa*.

Towards an Alternative Epistemology of Mathematics

The present-day schism between mathematics-as-calculation and mathematics-as-proof is one of the consequences of the above historical discontinuities and continuities: on the one hand, the practical and empirical is rejected, on the other hand there is the persistent attempt to assimilate practical/empirical mathematics-as-calculation into spiritual/formal mathematics-as-proof. Practical mathematics, as in the Indian tradition, regarded mathematics as calculation, whereas the idea of mathematics as a spiritual exercise has developed into the current Hilbert–Bourbaki approach to mathematics as formal proof, which has dominated mathematical activity for most of the 20th century CE. Side by side, the attempt to assimilate practical and empirical mathematics into the tradition of theological and formal mathematics has gone on now for over a thousand years. However, despite the apparent epistemological satisfaction provided by mathematical analysis, for example, it is still the calculus which remains the key tool for practical mathematical calculations, and few physicists or engineers, even today, study Dedekind's formalisation of real numbers, or the more modern notion of integral and derivative—either the Lebesgue integral or the Schwartz derivative. The practical seems to get along perfectly well without the need for any metaphysical seals of approval!

This schism within mathematics is today again being rapidly widened by the key technology of the 20th c. CE, the computer, which is a superb tool for calculation. The availability of this superb tool for calculation has accentuated the imbalance between mathematics-as-calculation and mathematics-as-proof. With a computer, numerical solutions of various mathematical problems can be readily calculated even though one may be quite unable to *prove* that a solution of the given mathematical problem exists or is unique. For example, one can today calculate on a computer the solution of a stochastic differential equation driven by Lévy motion, though one cannot today prove the existence or uniqueness of the solution. The advocates of mathematics-as-calculation suggest that the practical usefulness of the numerical solution—the ability to become rich through improved predictions of price variations in the stock market—overrides the loss of certainty in the absence of proof. The advocates of mathematics-as-proof argue that what lacks certainty cannot be mathematics, irrespective of its usefulness.

Is this schism in mathematics a "natural law"? Must useful mathematics remain epistemologically insecure for long periods of time? Or is this state of affairs the outcome of the narrow, theologically-motivated view of mathematics in the West? From an understanding of the civilizational tensions that have determined the actual historical trajectory of mathematics, can we modify mathematics to resolve these tensions? Can an alternative epistemology of mathematics be found, which is better suited to mathematics-as-calculation? I believe the first step in evolving an alternative epistemology is to probe the alleged epistemological security of mathematics-as-proof by re-examining the very notion of mathematical proof—is mathematical "proof" synonymous with certainty?

Interim Summary

To recapitulate, in mathematics, the East–West civilizational clash may be represented by the question of *pramāṇa* vs proof: is *pramāṇa* (validation), which involves *pratyakṣa* (the empirically manifest), not valid proof? The *pratyakṣa* or the empirically manifest is the one *pramāṇa* that is accepted by all major Indian schools of thought, and this is incorporated into the Indian way of doing mathematics, while the same *pratyakṣa*, since it concerns the empirical, is regarded as contingent, and is entirely rejected in Western mathematics. Does mathematics relate to calculation, or is it primarily concerned with proving theorems? Does the Western idea of mathematical proof capture the notions of "certainty" or "necessity" in some sense? Should mathematics-as-calculation be taught primarily for its practical value? or should mathematics-as-proof be taught for its theological correctness?

II
THE CULTURAL DEPENDENCE OF LOGIC

Plato and Proclus rejected the practical and empirical as valueless or inferior relative to the ideal; subsequent developments stripped away the spiritual and political content of Neoplatonic mathematics; formal mathematics has discarded also meaning and truth. If mathematics exclusively concerns the impractical, the unreal, the meaningless, and the arbitrary, then of what value is mathematics? Why should one continue to accept Plato's injunction to teach this sort of mathematics to one's children? The only potentially valuable element left in Western mathematics, today, is the notion of "proof". The notion of "proof" is the fulcrum of Western mathematics—the whole edifice of 20th century mathematics has been made to rest on the notion of mathematical proof.

One can enquire more closely into the nature of this "proof" or criterion of validity. One can enquire into the cherished belief that mathematical proof, since it involves only reason or logical deduction, is universal and certain—for it is this belief in its universality and necessity which makes the notion of mathematical proof potentially valuable. Can one maintain universality for the criterion of validity? Can one assert that there is a *necessary* relation between the meaningless and unreal assertion $2 + 2 = 4$, and the arbitrary set of axioms known as Peano's axioms? The short answer is no. The validation of $2 + 2 = 4$ requires proof—one is able to prove $2 + 2 = 4$ from Peano's axioms. But this proof relies on modus ponens, and modus ponens implicitly involves a notion of implication that requires 2-valued logic. Thus, the entire value of formal Western mathematics rests on the belief in the universality of a 2-valued logic.

Lukasiewicz 3-Valued Logic and Quasi Truth-Functional Logic

But in what sense is 2-valued logic universal? Surely this is not the only type of logic that there is. The West has known from the 1930's that there are different kinds of logics available. One kind of logic is 3-valued logic of the sort formulated by Lukasiewicz (though he was surely not the first to have formulated such a logic). In this logic, the logical connectives are given by the following truth tables (Table 2.1). One can similarly have many other many-valued logics.

	$\neg p$	$p \wedge q$	$p \vee q$	$p \Rightarrow q$	$p \quad q$
$p \quad q$	-	T I F	T I F	T I F	T I F
T	F	T I F	T T T	T I F	T I F
I	I	I I F	T I I	T T I	I T I
F	T	F F F	T I F	T T T	F I T

Table 2.1: **Truth table for 3-valued logic**. This table is read exactly like an ordinary truth table, except that the sentences p and q now have three values each, with I denoting "indeterminate" (and T and F denoting "true" and "false" as usual). With this system, $p \vee \neg p$ does *not* remain a tautology. A somewhat similar system was used by Reichenbach in his interpretation of quantum mechanics.

Of course, even in the Western understanding of logic, truth tables are not at all essential to logic. One can have, for example, a quasi truth-functional logic which does not have any clear-cut truth tables (Table 2.2). Connectives in such a logic might be defined as follows (Table 2.3).

The "truth table" is no longer adequate, but the meaning is made clearer by means of the semantic interpretation using possible logical worlds, as illustrated in the accompanying figure (Fig. 2.3). A proposition is "true" if it is true in all possible worlds, false if it is false in all possible worlds, and indeterminate otherwise. Although the figure shows only two possible worlds, there may be any number of them. Whether or not such a logic applies to the physical world, i.e., whether or not these "possible" worlds have a real physical existence, is something that depends upon the nature of time.[32] For instance, if the nature of time is such that at the microphysical level there are closed time loops, as even Stephen Hawking now concedes,[33] then more than one logical "world" may really exist at a single instant of time. These are not disjoint physical worlds which never interact with each other as in the Many-Worlds interpretation of quantum mechanics. Rather, we are describing a state of affairs in a single physical world by treating it as if it were a collection of logical worlds in each of which two-valued logic holds.

The existence of a multiplicity of logics creates a fundamental problem for formal mathematics. In present-day formal mathematics, what is or is not a theorem depends not only upon the underlying axioms or postulates (accepted as arbitrary), but it also depends upon

	$\neg p$	$p \wedge q$	$p \vee q$
$p \setminus q$	-	T ? F	T ? F
T	F	T ? F	T T T
?	?	? (? or F) F	T (? or T) ?
F	T	F F F	T ? F

Table 2.2: **Quasi truth-functional logic**. The quasi truth-functional system cannot be defined using a truth table, since a definite truth value cannot always be assigned. Hence, the "?" should *not* be construed as a third truth-value. This table should be seen only as an analogy. With this system, $p \vee \neg p$ remains a tautology, but $p \wedge \neg p$ need not be a contradiction.

	$p \Rightarrow q$	$p \quad q$
$p \setminus q$	T ? F	T ? F
T	T ? F	T ? F
?	T (? or T) ?	? (? or T) ?
F	T T T	F ? T

Table 2.3: **Possible definition of conditional**. This table shows a possible "definition" of the conditional, using $p \Rightarrow q$ for $\neg p \vee q$, and $p \quad q$ for $(p \Rightarrow q) \vee (q \Rightarrow p)$. The precise definition of "if" is very important for axiomatic quantum mechanics.

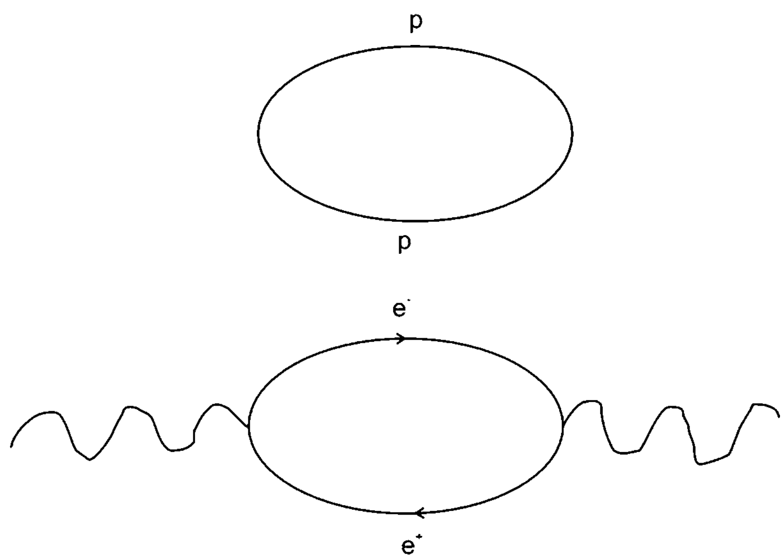

Figure 2.3: **Quasi truth-functional world.** The upper figure shows a quasi truth-functional (QTF) world which has two branches or possibilities corresponding to two 2–valued logical worlds (at a single instant of time). The relevant statements which are true in each branch are displayed. This explains how $p \vee \neg p$ remains a tautology, but $p \wedge \neg p$ need not be a contradiction. Physically, a QTF world might represent the various possibilities at a single instant of time (corresponding to a closed loop in time). QTF logic has the features of a quantum logic, and has been proposed as an appropriate way to describe the microphysical world according to the structured time interpretation of quantum mechanics proposed by this author. The lower figure show a Feynman diagram for the photon self-energy, in which a photon simultaneously creates an electron–positron pair, which recombines to give back the photon. This corresponds to the possible empirical realization of such QTF worlds since the positron may be regarded as the electron going back in time. For the mathematics of the photon self-energy, see Chapter 10.

the logic used to derive the theorem from the axioms. For example, $(A \wedge \neg A) \Rightarrow B$ is a theorem of 2-valued logic, and it is a theorem which is used to derive many theorems of present-day mathematics. But $(A \wedge \neg A) \Rightarrow B$ is NOT a theorem with quasi truth-functional logic. What is counted as a theorem therefore varies with both the axioms and logic. From the intuitionist controversy, it is well known that mathematics would change substantially if just the above rule of inference (*reductio ad absurdum*) is denied, without even changing logic. Even contemplating a change of logic, of course, goes far beyond intuitionism, since many other rules would, then, need to be re-examined.

Western thought has long regarded deduction as universal and infallible. However, deduction rests on logic, and logic, unfortunately, is not unique, as the West seems to have incorrectly assumed for millennia. The result of deduction will vary with the logic used, so deduction can be universal only if logic is universal. But in what sense can a particular choice of logic be declared to be universal? Having entirely eliminated the empirical from mathematics, present-day mathematics can no longer appeal to the empirical world to establish the nature of logic. (As we shall see later on, even if one does appeal to the empirical, there is nothing obvious about 2-valued logic. Moreover, an appeal to the empirical would involve induction, so, in that case, induction, based on the empirically manifest, would have to regarded as more certain and more universal, so that the Western valuation of deduction over induction would also need to be revalued.) The alternative is to appeal to intuition in the manner of Kant.

The Kantian belief in the universality of logic is not based on any profound study but on the opposite: mere parochialism and lack of information about other cultures, coupled with facile historical claims. Thus, in a profoundly parochial way, Kant asserted:

> Whether the treatment of that portion of our knowledge which lies within the province of pure reason advances with that undeviating certainty which characterizes the progress of *science*, we shall be at no loss to determine.... That logic has advanced in this sure course, even from the earliest times, is apparent from the fact that, since Aristotle, it has been unable to advance a step and, thus, to all appearances has reached its completion.[34]

Unlike Kant, our story of alternative logics begins from long before Aristotle of Toledo, from before even Aristotle of Stagira, and with things that Aristotle probably ought to have known (if at all the texts on logic attributed to him can be validly traced to him), and people Kant categorically ought to have heard of. (As Paulos Mar Gregorios remarked, in the West, a person who has not read something of Plato would be regarded as improperly educated; shouldn't one similarly regard a person who has not even heard of Akśapād Gautam or Nāgārjuna?)

The Kantian error in trying to base the universality of logic on *a priori* intuition is clear enough. Intuition is conditioned by culture, so if different cultures used different logics, as we now proceed to show, then deduction would refer to a cultural truth rather than a certain or universal truth. Logic is *not* culturally universal: so the tacit assumption of a two-valued logic in present-day mathematics involves a cultural bias. This is the antithesis of the Platonic view that mathematical "truths" are somehow out there, independent of culture:[35] for the theorems of mathematics can hardly be certain if logic is not. The importance of a difference of logic cannot be overstated: it throws into doubt the Western notion of "proof" and the entire edifice of formal mathematics built on it. It also throws into doubt inferences about physical "facts" drawn from this mathematics. (In particular, there is a close link between the nature of physics, the nature of time, and the nature of logic, as I have elaborated elsewhere. The relation of time beliefs to logic on the one hand, and to culture on the other, enables us to understand better the link of culture to logic.)

Syādavāda and the Logic of Structured Time

As our first example, let us examine alternative logic in the context of the Jain system of *syādavāda*, which has been much discussed in recent times. The distinguished commentators who have sought to make this logic a new basis for statistics,[36] referred to its significance for experimental physiology,[37] and to Bohr's complementarity principle,[38] have incorrectly assumed[39] that non–2-valued logic is exclusively a Jain phenomenon.

The Jain logic[40] of *syādavāda* involves seven categories. The system is attributed to the commentator Bhadrabāhu. Jain records and literature mention two Bhadrabāhu-s who lived about a thousand years apart. Between the two sects of the Jains there is no agreement as to the date of the later Bhadrabāhu, who may have lived as early as the −4th or as late as the 5th–6th century CE,[41] as his elaborate ten-limbed syllogism suggests.

The word *syat* means "may be", and the quickest way to see this is that the word *shāyad* in current Hindustani means "perhaps". Hence, *syādavāda* means "perhaps-ism" or "may-be-ism" or "discourse on the may be". In this view certainty is not possible, and uncertainty requires the making of judgements (*naya*). The seven-fold judgements (*saptabhaṅgīnaya*) are: (1) *syādasti* (may be it is), (2) *syātnāsti* (may be it is not), (3) *syādasti nāsti ca* (may be it is and is not), (4) *syādavaktavyah* (may be it is inexpressible [=indeterminate]), (5) *syādasti ca avaktavyasca* (may be it is and is indeterminate), (6) *syātnāsti ca avaktavyasca* (may be it is not and is indeterminate), (7) *syādasti nāsti ca avaktavyasca* (may be it is, is not, and is indeterminate). (According to some there is an eighth category (8) *vaktavasya avaktavasyaca*.)

Haldane relates this to human perception.

> In the study of the physiology of the sense organs it is important to determine a threshold. For example a light cannot be seen below a certain intensity, or a solution of a substance which is tasted as bitter when concentrated cannot be distinguished from water when it is diluted. Some experimenters order their subjects to answer "yes" or "no" to the question "Is this illuminated?", or "Is this bitter?". If the experimenter is interested in the psychology of perception he will permit the subject also to answer "It is uncertain".

Suppose now that a subject is given a randomized series of stimuli, and we record his responses. The experiment is repeated a few times. Especially for stimuli very close to the threshold, it is now possible that the subject may say "no" to a stimulus to which he had earlier said "yes"; or "uncertain" (="may be") to a stimulus to which he had earlier said "no". After at least three repetitions of the experiment, the responses to a given stimulus may be naturally classified in a seven-fold way: (1) Y, (2) N, (3) Y and N, (4) U, (5) Y and U, (6) N and U, (7) Y and U and N, though the last possibility seems a bit unlikely. These predications correspond exactly to the *saptabhaṅgīnaya*. On this interpretation, what we have here is something like a 3-valued logic, so the proposed relation to Bohr complementarity is exactly like the (unsuccessful) one of Reichenbach.[42]

Haldane's interpretation of Bhadrabāhu resolves the apparent contradiction in asserting that something both is and is not by making these statements true at different moments of time. While Haldane's interpretation is very clear in itself, it is *not* clear that this captures the original *syādavāda* meaning which is also associated with *anekāntavāda* or no-single-point-of-view-ism. Thus, when it is asserted that "The pot is both red and black" this is a statement intended to be true at a single instant of time from different perspectives.

We may therefore need to consider a situation where Haldane's different moments of time are not perceptually different, but are packed within the same atomic instant of time.[43] As the name atom suggests, one might want to treat this atomic instant as really indivisible, as a single atom of time. In that case, one way to make sense out of this logic (for those accustomed to 2-valued logic) is to attach multiple (2-valued) logical worlds to the same instant of time. This corresponds to the idea of a quasi truth-functional logic.

III
CATUṢKOṬI: THE BUDDHIST LOGIC OF FOUR ALTERNATIVES

That contradictory attributes are expected to hold *simultaneously* (i.e., at a single instant of time) is unambiguously clear in the Buddhist context, which also quite definitely predates Aristotle and Plato. Contrary to the belief of even some learned scholars that this logic originated with Nāgārjuna, we find this directly in the exposition given by the Buddha of various wrong views concerning the world, in the *Brahmajāla Sutta* of the *Dīgha Nikāya* itself.

One such view described there is the one which we have earlier attributed to Plato and Socrates: that man's life is ephemeral, but a part of him (the soul) is eternal. Therefore, a man (while alive) has both an eternal and non-eternal part. The contradictory properties of eternality–ephemerality or eternality–non-eternality are required to hold simultaneously. Unlike a pot where one might point out the part which is red and the part which is black, no such ostensive indication can be given for the part of man which is supposed to be eternal.

The point is made clearer with the next example, which concerns four (wrong) views about the world.[44]

> "...I know that the world is finite and bounded by a circle." This is the first case.... "...I know that this world is infinite and unbounded". This is the second case. And what is the third way?... "...I ...perceive the world as finite up-and-down, and infinite across. Therefore I know that the world is both finite and infinite." This is the third case. And what is the fourth case? Here a certain Śramaṇa or Brāhmaṇa is a logician. From his reasoning (*tarka*) he understands: "This world is neither finite nor infinite. Those who say it is finite are wrong, and so are those who say it is infinite. Those who say it is both finite and infinite are also wrong. This world is neither finite nor infinite." This is the fourth case. These are the four ways in which these ascetics and Brahmins are Finitists and Infinitists....There is no other way.

Thus, the four wrong views about the world, described by the Buddha, are:

1. The world is finite.
2. The world is not finite.
3. The world is both finite and infinite.
4. The world is neither finite nor infinite.

Maurice Walshe refers to this as "the four alternatives' of Indian logic: a thing (a) is, (b) is not, (c) both is and is not, and (d) neither is nor is not."[45] This Four Cornered logic (as it is called in Chinese), certainly did not apply to all Indian logic, but was frequently used by Nāgārjuna.

The semantic interpretation of (3) is that the world is finite up-and-down and infinite across. The semantic interpretation[46] of (4) is obtained by considering a person (such as Sañjaya Belaṭṭhaputta) who denies that any of the three preceding views are right.

Sañjaya Belaṭṭhaputta was one of the five wanderers, a contemporary of the Buddha, to whom King Ajātasattu addressed his sceptical question about the mundane (*sāmānya* = *pratyakṣa*) benefits of leading the life of a homeless wanderer. His reply, as summarized by Ajātasattu, ran as follows.

> If you ask me whether there is another world—well, if I thought there were, I would say so. But I don't say so. And I don't think it is thus.... And I don't think it is otherwise. And I don't deny it. And I don't say there neither is nor is not, another world. And if you ask me about the beings produced by chance; or whether there is any fruit, any result, of good or bad actions; or whether a man who has won the truth continues, or not, after death—to each or any of these questions do I give the same reply.[47]

Prior to the Buddha, there must have been prevalent various logics different from that subsequently adopted by Naiyāyika-s and Aristotle, as noted by Barua.[48] Sañjaya's formula for a five-fold negation is summarized in the Pali *śloka*: *evam pi me no, tathā ti pi me no, annathā ti pi me no, itī ti pi me no, no ti ti pi me no*.

Ajātasattu himself thought that Sañjaya Belaṭṭhaputta had simply evaded his question.

> Thus, Lord, Sañjaya Belaṭṭhaputta, on being asked about the [manifest (*pratyakṣa*)] fruits of the homeless life, did not say anything definite. Ask about a mango, and get a reply about a breadfruit (*kaṭahala*), ask about a breadfruit and get a reply about a mango. How can someone like me [a king] remove a Śramaṇa or a Brāhmaṇa from the country? So I neither applauded nor condemned his words, nor showed any displeasure, but got up and left.[49]

In two-valued logic accepting a statement and its negation implies every other statement. But this acceptance of 4-alternative logic did not mean that anything at all was both true and false. A little later in the same *Brahmajāla Sutta* of the *Dīgha Nikāya*, we find the discourse of the Buddha rejecting another of the wrong views labelled as the Wriggling of the Eel.[50]

> Because of his dullness and stupidity, when he is questioned he resorts to evasive statements and wriggles like an eel. "If you ask me whether there is another world—if I thought so, I would say there is another world. But I don't say so. And I don't say otherwise. And I don't say it is not, and I don't not say it is not." "Is there no other world?..." "Is there both another world and no other world?..." "Is there neither another world nor no other world?..."

Unlike Ajātasattu's account of Sañjaya Belaṭṭhaputta, we have here clearly a list of seven negations: (1) I don't say so, (2) I don't say *otherwise*, (3) I don't say it is not, (4) I don't not say it is not, (5) I don't affirm that there is no other world, (6) I don't say there both is and is not another world, (7) I don't say there is neither another world nor no other world. If we add to this the affirmative proposition of which these are negations, then we obtain the eight possibilities. (It is clearly rather hard to describe so many negations using natural language.[51]) The Buddha rejected this proliferation of negations.

Not too much should be read into the particular semantic interpretation for the case (3) above. Thus, Nāgārjuna, in his famous tetralemma (*catuṣkoṭi*) puts forward the proposition:[52]

> Everything is
> such
> not such
> both such and not such
> neither such nor not such.

As we shall see, later on, although the word "law" suggests that those who break it are criminals, Nāgārjuna's "middle way" is founded on a denial of the "law of the excluded middle", with four examples of which his *Mūlamādhyamakakārikā* begins.

Matilal[53] accordingly accepted that the "standard" negation does not fit Buddhist logic. Despite the Buddha's own rejection of numerous truth values as leading to confusion, a distinguished biologist, G. N. Ramachandran has suggested[54] another interpretation which applies the many-valued logic point of view to Buddhist logic as expounded by Nāgārjuna: namely that this could be seen as an 8-valued logic[55] with a cyclic negation. (Given the evolution of opinion and the various divisions of opinion within Buddhism, after the Buddha, it is not necessary that there is a uniform notion of logic across various Buddhist schools today.)

My own reading is that Buddhist logic is quasi truth-functional, and that this quasi truth-functionality of the underlying logic is closely related to the structure of time or the structure of the instant implicit in the Buddhist thesis of *paticca samuppāda*, which, as the Buddha stated, is the key to the *dhamma*. Since I have amplified on this elsewhere, I will not go into the details here, but only brie y recapitulate.

Logic relates to time beliefs: and Buddhist logic relates to the belief in time as instant. While the yogi regards even an entire cycle of the cosmos, lasting for billions of years, as an ephemeral instant, the Buddha proceeds in the other direction, dilating each microcosmic instant of time into an analogue of the macrocosm. An obvious consequence is the non-persistence of identity—and its relation to difficulties of representation is considered in a later chapter. Another important consequence of the Buddhist idea of time as instant, a consequence only dimly noticed by earlier commentators, is this: *the dilation of the instant* into an analogue of a cycle of the cosmos *also gives* a structure to the instant, i.e., *a structure to time*, in the sense of temporal logic, *if* we were to replace the atomic instant by a *point* of time. Within the microcosm of an atomic instant there could be both growth and cessation, in complete analogy with both birth and death within a cycle of the cosmos. But if we insist upon thinking of the atomic instant as a *point* of time (Naiyāyika-s like Udyotkara did just that) then *one must alter the logic* of discourse: for Udyotkara's act can then be simultaneously both begun and complete, like Schrödinger's cat which can be simultaneously alive and dead. This altered notion of simultaneity alters the very logic of debate, making it very difficult for opponents to refute the Buddha's view. Udyotkara who came some 15 centuries after the Buddha still gives completely tangential arguments in an attempted refutation of the Buddhist logic of the instant, following the above plan of deducing a contradiction.

(The quasi truth-functional logic, as we have seen,[56] corresponds to a quantum logic, and gives genuine complementarity.) Alternatively, one may use a many-valued logic, though the two are NOT equivalent (since the structured-time interpretation of quantum mechanics is not the same as Reichenbach's interpretation).

However, the suggestion to use many-valued logic is not necessarily orthogonal to the suggestion to use quasi truth-functional logic: one can well conceive of a quasi truth-functional logic, in which the multiple logical worlds attached to a single instant of time are themselves not 2-valued. In Haldane's model used to interpret Jain logic, this would happen if the different moments of time that he uses were treated as perceptually indistinguishable.

That the base logic of sentences is itself not two-valued is also clear from the work of Diṅnāga, a celebrated Buddhist logician, who developed something like a predicate calculus. We do not know his exact date, but he taught with distinction at the University of Nālandā, from where some of his works were obtained by the Chinese traveller Huen Tsang, and first translated into Chinese in 557–569 CE. Diṅnāga must have been alive in 480 when his teacher Vāsubandhu lived. He wrote in Sanskrit, rather than Pali, and his treatise on logic was composed in the *anuṣṭhub* metre, as we can infer from the fragments of it quoted by his opponents. Tibetan prose translations are, however, extant.

An enigmatic and very terse (2 printed pages) treatise on the "logic of nine reason" by Diṅnāga is the *Hetu-cakra-hamarū* (*hetu*=reason, *cakra*=wheel; in Tibetan this is called the

Wheel of Reason put in order). Because of its classical terseness (46 lines of verse = about 20 lines of prose + 1 diagram), this treatise admits diverse interpretations. Those who know Tibetan or Chinese are invited to clarify matters. The adoption of such a classically terse style suggests that the author was recognized as an all-time great authority, as indeed he was. The first three and last three stanzas read as follows.[57]

> ......
> I am expounding the determination of
> The *probans* with three-fold characteristics.
>
> Among the three possible cases of "presence, "absence" and "both"
> Of the *probans* in the *probandum*,
> Only the case of its "presence" is valid,
> While its "absence" is not.
>
> The case of "both presence and absence" is inconclusive.
> It is therefore not valid either.
> The "presence, "absence" and "both"
> Of the *probans* in similar instances,
> Combined with those in dissimilar instances,
> There are three combinations in each of three.
>
> ....
>
> Since there are nine classes of *probans*
> Accordingly we have nine sets of examples:
>
> Space-pot, pot-space,
> Pot-lightning-space,
> Space-pot, (space-pot), space-pot-lightning,
> Lightning-space-pot,
> Pot-lightning-space,
> Space-atom-action-pot.
>
> The above concerns the determined *probans* only;
> As regards the "doubtful" ones,
> There are also nine combinations of
> "Presence", "absence" and "both".
>
> The Treatise on the Wheel of Reasons by Ācārya Diṅnāga.

S. C. Vidyabhushan, an adherent of Nyāya, has suggested one interpretation.[58] This has been strongly disputed by R. S. Y. Chi,[59] who asserts that Vidyabhushan "had confused the notions of like' and unlike' altogether.... As a result his translation is almost incomprehensible."

There is a definite difficulty in understanding the three possible cases of "presence", "absence", and "both" mentioned in the *Hetucakra*, the last term being particularly obscure in Tibetan. In the *Nyāyavarttikā* of Udyotkara, the Sanskrit formulae used are "for all" (*vyāpaka*), "for none" (*avṛtti*), and "for some" (*ekādesavṛtti*), corresponding to the quantifiers of modern predicate logic. While I agree that Diṅnāga was the first logician to have introduced logical quantification, as generally believed, (1) I do not see why it should be assumed that Diṅnāga's predicate calculus was based on a two-valued logic.[60] (2) Also, I do not see why Diṅnāga, a Buddhist who taught at Nālandā, should have automatically ignored the question of identity across time,[61] in the manner of undergraduate courses[62] in logic taught at Oxford and Cambridge today.[63] (The absence of any meaning of identity across time is the focus of the Buddhist philosophy of *paticca samuppāda*.)

The Non-Universality of Logic

To summarize, logic varies with culture: the 2-valued logic, assumed *a priori* in the West, is *not* universal.

If the logic underlying present-day formalistic mathematics were to be changed, that would, of course, change also the valid theorems derivable from a given set of axioms, as we have seen earlier in this chapter (p. 78). Hence, not only are the axioms of a formal mathematical theory arbitrary, but the allegedly universal part of mathematics—the relation of axioms to theorems through "proof"—is arbitrary since this notion of "proof" involves an arbitrary choice of logic. Logic is the key principle used to decide validity in formal mathematics, but it is not clear how this principle is to be fixed without bringing in either empirical or social and cultural considerations.

We see that the "universal" reason of the schoolmen was underpinned by the alleged authority of God to which the schoolmen indirectly laid claim. If this authority is denied, as Buddhists inevitably would, there is nothing except practical and social authority that can be used to fix the logic used either within a formal theory or in a metamathematics that rejects appeal to the empirical

Accordingly, all of present-day formal mathematics, in practice, or in principle, depends upon social and cultural authority; for whether or not a proposition is a mathematical theorem depends upon Hilbert's notion of mathematical proof, and that notion of mathematical proof tacitly presupposes a 2-valued logic which is not universal, but depends upon social and cultural authority. Thus formal mathematics of the Hilbert–Bourbaki kind is entirely a social and cultural artefact. Proof or deduction provides only a social and cultural warrant for making cultural truth-assertions; it does not provide certain or secure knowledge.[64]

Reassessing the Role of the Empirical

It is possible, of course, to argue that 2-valued logic has social approval just because it is a matter of mundane empirical observation. But such arguments would hardly suit the 20th century Western vision of mathematics-as-proof, because once the empirical has been admitted at the base of mathematics, to decide logic itself, by what logic can it be excluded from mathematics proper? If the empirical world provides the basis of logic, why should the empirical be excluded from the process of logical inference? If the validity of *anumāna* is based on *pratyakṣa*, why should the *pratyakṣa* be excluded from valid *anumāna*.

If one does eventually decide to appeal to the empirical, in support of logic, a 2-valued logic need not be the automatic choice. Consider a meaningful but apparently contradictory proposition of the form: "This pot is both red and black". One may try to resolve the contradiction by breaking the identity of the pot and decomposing the proposition into the propositions: "This part of the pot is red", and "That part of the pot is black". But precisely what does "this" and "that" refer to? If the statements refer to the empirical, as we have now supposed, such a decomposition of the proposition may end up referring to ever smaller physical parts of the object. Thus, moving to atomic propositions may also drive one to the atomic domain in the physical world, where quantum mechanics certainly does apply. But are things two-valued in the physically atomic domain? The best physical theory we have as of now is quantum mechanics, and it is well known that quantum logic cannot be 2-valued, unless we fundamentally change the theory. On the contrary, according to the structured-time interpretation of quantum mechanics[65] the key postulates of quantum mechanics can be obtained by supposing logic to be quasi truth-functional. (Of course, the physical theory itself will have to be reviewed if we change the underlying mathematics.) Thus, one might perhaps need to start with a quantum logic as the empirical basis of logic, so that *no* conclusion could be drawn from the statement that Schrödinger's cat is both dead and alive. (In 2-valued logic, *any* conclusion could be drawn from this statement.) Specifically, the logic of the empirical world should not be regarded as a settled issue, solely on the basis of mundane experience. There is no guarantee at all that an appeal to the empirical will establish 2-valued logic.

Further, accepting the empirical may well make mathematics explicitly fallible, like physics. No one denies the fallibility of the empirical: as when one mistakes a rope for a snake or a snake for a rope. However, it seems to me manifest that social authority (e.g. that of Hilbert and Bourbaki) is *more* fallible than empirical observation. I regard the *pratyakṣa* as *more* reliable than *śabda* or authoritative testimony. Accordingly, I regard mathematics-as-calculation, based on the empirical, as *more* secure, and more certain than mathematics-as-proof, which bypasses the empirical altogether.

To return to $2+2=4$, the particular case of $2+2=4$ still remains persuasive because, for example, 2 sheep when added to 2 sheep usually make 4 sheep (though they may produce any number of sheep over a period of time). However, this involves an appeal to mundane human experience; it involves an appeal to the empirical, not the *a priori*.

Mundane experience may not be universal, but it is *more* universal than the *a priori*—there is less disagreement about mundane physical things than there is about metaphysics. Thus, the way to make mathematics more universal, and the way to evolve an East–West synthesis is to accept the empirical in mathematics. The best route to universalization through an East–West synthesis is through everyday experience, through physics rather than metaphysics, through shared experience rather than shared acceptance of the same arbitrary social authority. Stable globalization needs *pramāṇa* rather than proof!

IV
FORMAL MATHEMATICS AS A SOCIAL CONSTRUCTION

In attempting to resolve the East–West civilizational clash in mathematics, we examined the key question: are mathematical theorems "necessary"? are they universal truths? We found that neither mathematical theorems nor mathematical proof can be regarded as incorporating universal truths. I will now argue that the theorems of formal mathematics are social constructs, and that belief in their validity or necessity rests on nothing more solid than social authority. Various arguments have been given in this direction, but I regard the arguments above about the cultural dependence of logic as conclusive.

Nevertheless, making an allowance for the irrational basis of the belief in present-day mathematics, a belief deriving from social authority, this argument needs to be developed in two further ways. First, the existing social consensus regarding mathematics involves a certain uniformity of opinion, but this uniformity, anchored in present-day social processes, should not be confused with universality. The uniformity arises from present-day social processes which encourage reliance upon mathematical authority. These social processes are examined in greater detail in Appendix 2.A.

Secondly, for the purposes of our historical study, apart from the question of proof there is the question of number. The present-day idealistic construction of number is also a social construct: the notion of number has been different in the past and may change further in the future in response to various social pressures, such as the technology of computation. Hence, the current (non-universal) notion of number must be carefully distinguished from the notion of number in Indian mathematics. This chapter takes up this question in a preliminary way, and further details are postponed to a later chapter.

Integers (ints) and Real Numbers (Floats) on a Computer

Understanding present-day formal mathematics as a pure social construction helps to clarify the distinction between the different notions of number in present-day formal mathematics, and in traditional Indian mathematics. One of the authoritative dogmas of the present-day mathematical understanding of the calculus is that an understanding of the calculus requires formal real numbers. Thus, the calculus requires limiting processes, and, unlike the integers or rational numbers, the formal real numbers are complete, in the sense that every sequence that is intrinsically trying to converge (Cauchy sequence) can find a value to converge to. Hence, the limiting processes of the calculus make sense in formal real numbers.

Now, traditional Indian mathematics, from the earliest known times of the *śulba sūtra*-s, was not averse to using "irrational" (non-ratio) numbers like $\sqrt{2}$. However, the present-day formal understanding of real numbers is impractical—for there is no way to represent real numbers in practice. In fact, even the present-day formal understanding of natural numbers is impractical. Hence, this understanding is unacceptable to traditional Indian mathematics (and for present-day computers). This suggests that, before proceeding to the specifics of traditional Indian mathematics, we should re-examine the notion of number in present-day formal mathematics, based on the understanding of formal mathematics as a pure social construction.

Consider a formal mathematical theorem, an apparently certain universal mathematical truth, such as $2 + 2 = 4$. Is $2 + 2 = 4$ a universal truth or is it a social construction, hence a cultural truth? Perhaps one should first take up the easier case of $1 + 1$! The usual belief is that $1 + 1 = 2$. One could also amplify this belief negatively, as what $1 + 1$ is not: if $1 + 1 = 2$ is a universal truth, then $1 + 1 = 0$ or $1 + 1 = 1$ or $1 + 1 = 3$ must all be universally false. However, if 0 and 1 denote truth values, we know, for instance, that $1 + 1 = 1$ holds in classical 2-valued logic, with + denoting "inclusive or", 0 denoting "false", and 1 denoting "true". We know that $1 + 1 = 0$ holds in classical 2-valued logic with + denoting "exclusive or". $1 + 1 = 0$ is also the case if 0 and 1 denote binary digits (bits) and + denotes addition with carry. And this case is one that is commonly implemented thousands of times in the chips of a computer.

We see that if at all $1 + 1 = 2$ is a universal truth, it is at best a qualified universal truth. It is necessary to specify what 1, +, and = are; these are merely symbols which, lacking any empirical reference, could be performing multiple duties. Today we would tend to qualify that in $1 + 1 = 2$, 1, +, =, and 2 relate to "natural numbers" or to integers or to rational numbers or real numbers. However, in current formal mathematics, since the axioms, lacking any empirical reference, are practically arbitrary, there can be no real restriction on how one specifies the syntactic rules for using 1, +, =. To return to the harder case of $2 + 2$, it is, for example, perfectly possible, in current formal mathematics, to specify 2, +, and = so that $2 + 2 = 5$. Thus, let $a + b = a \quad b \quad 1$, where is an unusual notation for usual addition (socially conventional addition in "natural numbers"). One cannot say that such a formal theory is useless, for like all pure mathematics it may find a use some day. (Indeed it has a use already in philosophy for purposes of illustration!) At best one can say that this or that mathematician, who enjoys a certain degree of social recognition, finds it uninteresting. So the theory of numbers with $2 + 2 = 5$ is not false; it is, at worst, a way to handle numbers that some existing social authorities may find socially uninteresting.

What is socially interesting or uninteresting can naturally vary with the cultural circumstances: for instance, $2 + 2 = 5$ may be a socially interesting case for native South Americans,[66] and similar differences about exactly what is regarded as socially interesting do exist in the mathematics in African arts, architecture and crafts.[67]

What is socially interesting or uninteresting can also vary across time with varying technology. Computers are widely used today, but one cannot make a computer "understand" or work with natural numbers or real numbers. For the purposes of programming a computer, the standard convention is that an integer (int data type) is something that can be represented using 2 bytes, which is usually 16 bits. Setting aside one bit to represent the sign (positive or negative) the largest (signed) integer that can then be represented is 111111111111111 (15 1's), in binary notation, or $2^{14} + 2^{13} + \cdots + 2^2 + 2^1 + 2^0 = 2^{15} - 1 = 32767$. This convention suits the 8-bit architecture; but nothing will change, except the value of the upper limit, if we move from an 8-bit to a 128-bit machine, or use static storage, with any finite number of bits ("arbitrary precision arithmetic"). The number 32767 may change with changing technology and changing conventions, but the point is that for any computer whatsoever there will always be such an upper limit, so long as we are dealing with actual computers rather than abstract Turing machines with infinite memory, which are as imaginary and non-existent as "a barren woman's son" or "a rabbit with horns".

The existence of an upper limit creates a serious problem in computer arithmetic, relating to the Western mathematical conceptualization of "natural numbers" asserted by Dedekind to have been given by God. One can have $2 + 2 = 4$ on a computer, but only at the expense of admitting that

$$20000 + 20000 = -25536.$$

Anyone who disbelieves this is welcome to use the accompanying computer program in the C language (box 2.4) to check this out. (Note: This program was first written when 16 bit systems were in vogue, and has been retained for clarity of exposition; if one actually wants to do the same thing on a 32 or 64 bit system, one must increase the number of zeroes appropriately.)

One can represent the natural numbers needed for all or for most *practical* purposes, but one cannot represent the idea of a "natural number" on a computer, and one cannot represent addition according to Peano's axioms on a computer. *It is impossible to program the syntax of natural numbers on any actual computer.*

Box 2.4. Adding integers on a computer

```
    /* Program name:  addint.c
Function:  To demonstrate how a computer adds integers */

#include <stdio.h>
#include <conio.h>

main ()
{
   int a, b, c;
   printf (" n Enter a = ");
   scanf ("%d", &a);
   printf (" n Enter b = ");
   scanf ("%d", &b);
   c = a+b;
   printf (" n %d + %d = %d", a, b, c);
   getch();
   return;
}

Program Input and Output:

Enter a = 20000
Enter b = 20000
20000 + 20000 = -25536
```

A desktop calculator usually manages to get the above sum right—how is this achieved? One can get the expected answer by using oating point numbers, which roughly correspond to real numbers. The upper limit becomes much higher, but we can now validly have

$$2 + 2 = 4.00000000000000001 \quad (16\ 0\text{'s}).$$

which is typically the case in a computer (which observes the IEEE standard[68] for oating point arithmetic). From a practical point of view, this arithmetic is quite satisfactory. From the point of view of the current formal mathematics of real numbers, this type of arithmetic only *seems* more satisfactory: serious problems arise, because the above equation means that oating point numbers do *not* obey the same algebraic rules as real numbers. The associative law, for example, fails for arithmetic operations with oating point numbers. Thus,

$$(0.00000001 + 1) - 1 = 0$$

but

$$0.00000001 + (1 - 1) = 0.00000001$$

Once again, one can achieve a higher precision, one can arrange things so that in the above equation the number of zeros dazzles the eye. One can arrange for a number of decimal places adequate for all practical, physical, and engineering purposes. But one cannot bypass, in principle, the failure of the associative law. There will always remain not one or two but an uncountable infinity of "exceptions" to the associative law for addition. Similarly, the associative law and cancellation law for multiplication fail, and so does the distributive law linking addition and multiplication. Hence, the numbers on a computer can never correspond to the numbers in the formal systems of natural numbers or real numbers. Since computers are socially interesting, so are numbers not corresponding to formal natural or real numbers.

The other point I am trying to drive at is the following: formal real numbers may help to bypass the appeal to the real world in *Elements* I.1, but in the real (empirical) world, as distinct from some imagined or ideal Platonic world, there is no satisfactory way to *represent* the natural or real numbers, since there is no way to represent any real number with only a finite number of symbols. Hence also there is no satisfactory way to *represent* the alleged universal truth that 2+2=4, since there is no satisfactory way to *state* the required qualification that the above equation concerns natural or real numbers. The representation of natural numbers according to Peano's axioms involves a supertask, an infinite series of tasks, usually hidden by the ellipsis, but made evident by computer arithmetic, which can hence never be the arithmetic of Peano's natural numbers or Dedekind's real numbers.

For practical purposes, no supertask is necessary: the representation of numbers on a computer is satisfactory for mathematics-as-calculation, but it is unsatisfactory or "approximate" or "erroneous" from the point of view of mathematics as proof. Indian mathematics, which dealt with "real numbers" from the very beginning ($\sqrt{2}$ finds a place in the *śulba sūtra*-s), does not represent numbers by assuming that such supertasks can be performed, any more than it represents a line as lacking any breadth, for the goals of mathematics in the Indian tradition were practical not spiritual. The Indian tradition of mathematics worked with a finite set of numbers, similar to the numbers available on a computer, and similarly adequate for practical purposes. Excessively large numbers, like an excessively large number of decimal places after the decimal point, were of little practical interest. Exactly what constitutes "excessively large" is naturally to be decided by the practical problem at hand, so that no universal or uniform rule is appropriate for it.

On the other hand, theoretically speaking, formal Western mathematics is not formulated with a view to solving practical problems: it treats both natural and real numbers from an idealist standpoint, hence it runs into the difficulty with supertasks, made evident by computer arithmetic.

Social Change and Changing Social Construction: The Case of Śūnya

The above argument being abstract, a concrete example (*dṛṣṭānta*) is in order. If mathematics is a social construction, then one can expect mathematics to change with changing technology and changing social circumstances. Can one point to instances of such change? Clearly that part of mathematics is most susceptible to change which is furthest away from the empirically manifest or *pratyakṣa*.

To bring this out, let us consider something for which there is no obvious empirical reference, such as division by zero. From the East–West point of view, *śūnya* is a particularly interesting case. We know that *śūnya* travelled from India to Europe via the algorismus texts, starting 10th c. CE, and that the epistemological assimilation of *śūnya* required some five to six hundred years. As late as the late 16th century CE we find mathematicians in Europe worrying about the status of unity as a number, and the following question was still being used as a challenge problem: "Is unity a number?" The expected answer was that unity was not a number, but was the basis of number. With the changed social circumstance, those metaphysical concerns about the status of unity now merely serve to amuse us, and zero is now firmly regarded as a number, an integer. However, the nature of zero has changed.

Thus, Brahmagupta maintained that $0 \quad 0 = 0$. This is something that a modern-day mathematician will immediately regard as an error, for division by zero is not permitted. In current-day formal mathematics, 0 is the additive identity; hence, for any number x, from the distributive law, $0 \cdot x = (0 + 0) \cdot x = 0 \cdot x + 0 \cdot x$, so that $0 \cdot x = 0$. Thus 0 cannot have a multiplicative inverse. Hence one cannot divide by zero, for division is nothing but the inverse of multiplication. Hence, Datta and Singh[69] assert that Brahmagupta was mistaken. At a conference on *śūnya*,[70] almost all the participants agreed with this perception of Datta and Singh (I was the exception). This goes to show the extent of acculturation, but not, of course, the universal validity of the belief. The above proof of the illegitimacy of division by zero tacitly assumes that the numbers in question must form a field, but as we have already seen, this is not the case for numbers on a computer, where the distributive law, used in the above proof, fails.

As a matter of fact, there are, even in current mathematics, common situations where $0 \quad 0 = 0$ may be implicitly used as part of the arithmetic of extended real numbers. Thus, consider the Lebesgue integral

$$ {}_{0}^{1} \; \frac{1}{\sqrt{x}}\, dx = 1. \tag{2.1} $$

The integrand is ill behaved only when $x = 0$, when the denominator becomes zero. Since the integral is a Lebesgue integral rather than a Riemann integral, we do not omit 0 from the region of integration, but appeal to the rules of the extended real number system,[71] which admits the additional symbols ∞, $-\infty$.

Now, either the limit

$$\lim_{x \quad 0} \frac{1}{\sqrt{x}} = \infty, \tag{2.2}$$

or the corresponding *unwritten* convention

$$1 \quad 0 = \infty \tag{2.3}$$

allows us to regard the integrand as

$$f(x) = \begin{matrix} \frac{1}{\sqrt{x}}, & x = 0 \\ \infty, & x = 0. \end{matrix} \tag{2.4}$$

However, the integrand is infinite only at a single point, i.e., it is infinite only on a set of Lebesgue measure zero. Hence, we appeal to the standard convention, used in the theory of the Lebesgue integral, that[72]

$$0 \cdot \infty = 0. \tag{2.5}$$

We see that (2.1), (2.3), and (2.5) together amount to saying that $0 \quad 0 = 0$. I would emphasize that the convention (2.5) $0 \cdot \infty = 0$ is a very important convention, for one cannot do modern-day probability theory or statistics without it; a statement that is true with probability 1, i.e., true except on a set of probability zero, is said to be true almost everywhere, and "almost everywhere" occurs almost everywhere in current probability theory. Thus, $0 \quad 0 = 0$ is certainly not a convention every use of which is *necessarily* incorrect. This was presumably believed to be so in 1937, by Datta and Gupta, but we now have good reasons for admitting the convention, at least in some situations—reasons relating both to mathematical practice and to computer arithmetic. But can one make $0 \quad 0 = 0$ a universal rule? That depends, in the first place, on what one means by 0.

Under different social and cultural circumstances, zero was regarded differently. As I have argued elsewhere,[73] in Brahmagupta's case, *śūnya* or 0 is not the additive identity in a field, but refers to the non-representable, in line with the meaning given to it in the *śūnyavāda* of Nāgārjuna. With calculations involving a representable, hence a finite set of numbers, such non-representable numbers are bound to arise, and some rule is needed to handle these cases. Brahmagupta's rule should be read as

$$nr \quad nr = nr,$$

where nr = non-representable.

We see that changed social circumstances have transformed the notion of zero, but further changes could change it further. As observed above, computers can represent only a finite set of numbers. Hence, exactly this problem of dealing with non-representable numbers arises in computing. Here, too, we have a situation very similar to $nr \quad nr = nr$, as can be seen by writing and executing the accompanying short C program (box 2.5).

Box 2.5. Adding reals on a computer

```
/* Program name:  sunya.c */
/* Function:  To show how a computer handles non-representable numbers
according to the IEEE standard */

#include <stdio.h>
#include <conio.h>
#include <values.h>

main()
{
   float a, b, c;
   a = MAXFLOAT;
   b = MINFLOAT;
   printf ("a = %e n, b= %e n", a, b);
   getch();
/* Now try putting in values of a, and b, larger than
MAXFLOAT or values of b smaller than MINFLOAT */
   printf (" n n Enter a = ");
   scanf("%f", &a);
   printf (" n Enter b = ");
   scanf ("%f", &b);
   c = a/b;
   printf ("%e/%e = %e", a, b, c);

   /* printf ("%f/%f = %f", a, b, c);*/ /* uncomment */
   getch();
   return 0;
}
```

Program Input and Output:

```
a = 3.37000e+38
b = 8.43000e-37
Enter a = 1e40
a = +INF
Enter b = -1e40
b = -INF
Floating point error:  Domain
```

In accord with Western mathematical sensibilities, the IEEE standard, however, permits a few different *types* of non-representables. Anything smaller in absolute value than 1.40130×10^{-45} is non-representable, and is represented by zero. Anything larger than 3.37×10^{38} is non-representable, but is represented by +INF, while anything smaller than -3.37×10^{38} is represented by −INF. Even though the associative and distributive laws fail for numbers on a computer, in accordance with prevalent Western mathematical conventions, the IEEE standard specifies that arithmetic operations involving non-representables, such as 0 0, *always* lead to an undefined result, which is treated as an error. This is not the full story, and there are other kinds of non-representables, such as subnormal numbers, an account of which would take us too far afield.[74] (Indeed, by uncommenting the line marked "uncomment", i.e., removing the first pair of /* and */ in that line, and providing the inputs a = 2.0e−45 and b = 4.0e−45, one can actually make the computer print out the statement 0.00000/0.00000 = 0.00000! But this is not something that needs to be taken seriously.)

How satisfactory are the IEEE specifications that 0 0 = 0 *always* is an error? If we look upon this as a practical matter of making efficient calculations, then a universal rule of the kind that one has in current-day computing is *not* the most efficient. For example, in a practical situation, even if something is treated as non-representable, we might yet know that it is the *same* non-representable as one that was previously encountered. In that case, we may even want to apply the cancellation law to zero! We might want to say

$$\frac{2 \cdot 10^{46}}{4 \cdot 10^{46}} = \frac{1}{2}.$$

But this is a statement that the IEEE standard regards as erroneous for oats (real numbers represented in single precision), as the accompanying C program shows. According to that standard, the correct statement is:

$$\frac{2 \cdot 10^{46}}{4 \cdot 10^{46}} = \text{"Floating point error"}.$$

Accordingly, the computer treats the attempt to carry out the above calculation as erroneous, though anyone can see what the valid answer is. Thus, the attempt to eliminate one kind of absurdity (that might arise out of a wrong use of 0 0 = 0) leads to another kind of absurdity.

A machine cannot discriminate between a "legitimate" use of 0 0 = 0, and an "illegitimate" use: it cannot easily handle exceptional situations, it needs a universal rule, and this universal rule may lead to other absurdities. Though the IEEE has regarded the latter absurdity as more acceptable, this could change with circumstances. The conventions may change not only with who lays down the standard, but also with *who* performs the calculation: for human arithmetic, as distinct from machine arithmetic, we may use rules which permit exceptions. Possibly tomorrow's machines may be intelligent enough to make this kind of discrimination. This is exactly how Bhāskara II interprets Brahmagupta's rule while computing the value of x (= 44), given that

$$\frac{x \cdot 0 + \frac{x\ 0}{2}}{0} = 63.$$

This suggests that, when we go beyond the empirical, the "universal" may lie, as in a physical theory, in what Poincaré called "convenience". This criterion of "convenience" can have profound consequences as in the case of the theory of relativity: the constancy of the speed of light is not an empirical fact (though elementary physics texts usually misrepresent it as such), Poincaré defined the speed of light as a constant as a matter of "convenience". I see this criterion of "convenience" as more modest than the criterion of beauty which seeks to globalize a local sense of aesthetics.

V
CONCLUSIONS

1. Logic is not unique, and logic varies with culture. Proofs vary with logic. Formal mathematics having rejected the empirical, the choice of a logic can only be justified on cultural grounds. Accordingly, the theorems of present-day formal mathematics merely represent socially and culturally specific warrants for truth claims and are definitely not necessary or universal or even trans-cultural truths.

2. If logic is decided not on the basis of theology but on the basis of empirical facts, then it is far from certain that it would be two-valued or even truth-functional. Since empirical choices can only be justified inductively, and since social authority is more fallible than the empirical, whatever the technique used to justify the choice of logic, it follows that deduction will forever remain *more* fallible than induction, contrary to what has long been believed in Western philosophy.

3. Traditional Indian mathematics relies on *pramāṇa*, which uses the empirical and is, therefore, *more* certain and universal than mathematical proof.

4. The notion of number in present-day formal mathematics assumes the possibility of performing supertasks—something shown to be manifestly impossible with a computer, which cannot pretend to understand the idealist or formalist representation of number. Supertasks are not contemplated in the notion of number in traditional Indian mathematics.

5. Where mathematical constructs go beyond the empirical, it may be most appropriate to use Poincaré's criterion of "convenience".

APPENDIX 2.A
INDUSTRIAL CAPITALISM AND MATHEMATICAL AUTHORITY

Let me begin with the more general part of the thesis: the in uence of industrial capitalism on the belief in the universality of mathematics.

At the present moment, why does the state support mathematicians? Is this a matter of charity, or does the state derive (or expect to derive) some benefit from this? The logic is quite clear: mathematics is a key input to modern science, which is a key input to technology, which is the key to economic and physical domination.

Present-day mathematics has grown along with modern science under conditions of industrial capitalism. The substantial increases in profit come from technological innovation; consequently the scientist must have a single-minded focus on innovation useful for commercial production—when he is not working like von Neumann on designing new weapons like the atomic bomb, used to extract surplus by other means. Innovation has, thus, become a commodity, and specialization boosts the efficiency of production of commodified innovation; hence most scientists tend to be very specialized. One consequence of this is that scientists are not able to understand each other or communicate with each other. If a mathematician has to read a paper not exactly in his field, this process could easily take a determined effort lasting for a year or two. With such formidable difficulties in communication, scientists quickly start relying on authority. This is the first consequence of industrial capitalism: because it hopes to profit from specialization, it encourages reliance on authority.

Thus, the new standard of truth is this: if it is published by an important person in a respectable journal it must be true or, at any rate, very likely true (though there is still the possibility of a small error somewhere if one is speaking of the four-colour theorem, or Fermat's last theorem). The most pathetic example of this standard of truth is the grievous mathematical error[75] in a paper published by Einstein[76] in the *Annals of Mathematics*, in 1938, on the relativistic many-body problem, which exposes his fundamental lack of understanding of the special theory of relativity relative to Poincaré.

There is another reason why the prevalent social conditions systematically encourage the process of deciding truth by authority. Barring a few hundred relativists, and perhaps a few thousand people who might have some idea of it, most people in the world would be unable to judge for themselves the truth of the above example about Einstein. This state of affairs is not incidental. Commodified innovation is produced by scientists through a process of research; hence, the state is willing to invest resources into research facilities that scientists need to produce innovation. The state also does invest in the education of scientists, but only with the objective of reproducing the scientific labour power needed to produce innovation. It is well understood why, under conditions of industrial capitalism, there is systematically greater investment in production than in reproduction of the labour consumed in production. Hence, there is a systematic bias in the state support for science: more resources are invested in research facilities than in education. (In particular, the state is no longer interested in enabling people through education to understand the world around them. Not only has education been delinked from the needs of theology, but "understanding" is something that most scientists look down upon as "philosophy", since it consumes the time that could be more actively spent in the process of engineering useful innovations.) As a result of this systemic bias against education, in the state support for science, most people are scientifically illiterate, even in the developed countries.[77] In the interaction of illiterate patients with doctors one can easily see how illiterate persons are left with no option but to decide truth by relying on authority, whether that authority is conferred by the media or the state. This is the second consequence of industrial capitalism: it encourages reliance on authority by creating widespread scientific illiteracy or information poverty. (Spengler had already anticipated this widespread scientific illiteracy as a process contributing to the decline of the West.)

One can also enquire into the nature of this authority: what bearing does it have on truth? How reliable is authority, on an average? To continue the analogy, the illiterate patient has no option but to trust the doctor, but even to a casual observer it is obvious that a medical career is much sought after not out of a widespread desire to help out humanity at large, but to enable the person to lead a good life, as it is defined under industrial capitalism. The doctor's first concern usually is extraction of surplus rather than the health of the patient, and this is especially true if the patient is illiterate and hence not very important. Consequently, the doctor's prescription may suit the health of the pharmaceutical company more than that of the patient. Unlike doctors, scientists who are in authority are necessarily employed by, hence dependent upon, state and private capital.

Second, industrial capitalism is a great uniformizer, because standardization is essential for mass production. Once something becomes a standard, market logic tends to drive out others: a publisher will be more willing to publish a text in mathematics rather than a monograph on intuitionism. This process relies, like the market, on statistical effects, rather than any absolute prohibition: difference is not prohibited, but is made so disadvantageous that few people care to differ. Consequently, those in authority do not differ too much from each other. Thus, industrial capitalism encourages a process of uniformity and standardization in opinion.

The above processes lead to the remarkably widespread agreement that sociologists have observed among practitioners of mathematics and science. But this uniformity and standardization of opinion ought not to be mistaken for universality as it often is. In the context, uniformity of opinion does not make the opinion itself more reliable: if a variety of doctors prescribe the same drug this does not mean that that drug is most suited to one's health, it might simply mean that this is a drug being vigorously promoted.

To my mind it would be facile to set aside the above observations, regarding the determination of mathematical and scientific truth through authority, as concerning *practice* rather than *principle*. It is a myth that principles are insulated from practice. The very same practical and social considerations may infiltrate not only the allegedly universal and metaphysical "truths" of mathematics but also the very principles used to decide these truths—principles that have been and can only be formulated by authoritative mathematicians. If practical considerations can penetrate to the content of relativity, there is no reason why they cannot penetrate the content of the philosophy of science or mathematics. We will see this in greater detail below. Since these principles, as currently articulated in the formalistic philosophy of mathematics, have no external empirical anchor, it is all the more important to recognize the social processes within which mathematical authority is anchored.

Thus, authority decides mathematical truth—the veracity of mathematical theorems and the principles used to decide this veracity. The obvious point about authority as the standard of mathematical truth is that authority is socially conferred. It would, of course, be excessively naive (or religious) to imagine that social processes are such that they automatically (or by design) "select the fittest" and confer authority on those who seek truth through creative insights. Thus, it is not only in present-day India that knowledgeability and creativity have little relation to scientific authority. The primary interest under industrial capitalism is neither in understanding nor in the creative process of innovation, but in *control* of information or the *ownership* of the innovated commodity, as decided by patents, authorship of papers, etc.[78] As a clerk in the patent office, Einstein understood the subtler legalities of this process: that one may copy ideas if one does not copy the expression verbatim. A more recent example of this sort is Bill Gates, one of the richest men of all time, who legally won the claim of having innovated the windowing software that, despite its bugs, bears a striking resemblance to the earlier software of Apple Macintosh. The relative unimportance of the creative process is emphasized by the fact that no one has heard of the person who initially thought up the point-and-click concept behind the windowing software. Authority ows from ownership, and ownership, laws regarding ownership, and the principles on which these laws are based, are all rooted in social processes that it is not necessary to go into here.

To recapitulate, formal mathematics being divorced from the empirical, mathematical truth tends to depend upon social processes. Under industrial capitalism social processes tend to decide mathematical truth in two steps. (a) Overspecialization of scientists, and widespread scientific illiteracy of others, both, strongly encourage reliance on authority, and (b) authority devolves on those who are better able to manipulate social processes of deciding ownership of innovation rather than on those who are most knowledgeable or innovative—there is also a systematic decline of the best! The view of mathematics as a social construction results in the following irony: present-day formal mathematics is ultimately valued for its ability to promote inequity and injustice, though it claims to base itself on the *Elements*—written to promote equity and justice!

NOTES AND REFERENCES

1. Samuel P. Huntington, *The Clash of Civilizations and the Remaking of World Order*, Viking, New Delhi, 1997, p. 166.
2. *Proclus: A Commentary on the First Book of Euclid s Elements*, trans. Glenn R. Morrow, Princeton University Press, Princeton, 1970, p. 3.
3. Plato, *Republic*, Book VII, 526, trans. J. L. Davies and D. J. Vaughan, Wordsworth, Hertfordshire, 1997, p. 240. Jowett's translation reads "if geometry compels us to view being it concerns us; if becoming only, it does not concern us". *The Dialogues of Plato*, trans. B. Jowett, Encyclopaedia Britannica, Chicago, 1996, p. 394.
4. Rescher gives a very detailed account of the "Aristotelian" and "Diodorean" temporalized modalities and how these were interpreted by medieval European commentators. N. Rescher, "Truth and necessity in temporal perspective," in: R. M. Gale, *The Philosophy of Time*, Macmillan, 1962, pp. 183–220. Al Ghazālī, in his *Tahāfut al Falāsifā*, opposed the philosophers and rational theologians of Islam exactly on the grounds that *any* necessary component of the empirical world would restrict the powers of God, who continuously created the world; however, even al Ghazālī did not deny that God was compelled by (Aristotelian) *logical* necessity.
5. The notion of "truth" has of course had a variety of meanings in mathematics. Paul Ernest adopts the interesting terminology of "truth1" for the traditional European notion of mathematical truth, akin to naive realism (but without its empirical basis) prevalent until around the mid-nineteenth century, "truth2" for Tarski's notion of satisfiability, or true in a possible world, which presumably originated with Hilbert's work on geometry, and "truth3" for the notion of logical validity, which roughly corresponds to "true in all possible worlds". Paul Ernest, *Social Constructivism as a Philosophy of Mathematics*, SUNY, Albany, 1998, chapter 1. While I will not adopt this terminology explicitly, I hope the sense in which the word "true" is used will be clear from the context.
6. E.g., E. Mendelson, *Introduction to Mathematical Logic*, Van Nostrand, New York, 1964, p. 29.
7. C. K. Raju, *The Eleven Pictures of Time: The Physics, Philosophy, and Politics of Time Beliefs*, Sage, New Delhi, 2003.
8. Varāhamihīra, *Pañcasiddhāntika*, III.21, trans. G. Thibaut and Sudhakara Dwivedi [1888], reprint, Chowkhamba, Varanasi, 1968, p. 18. The context is the following. The *Vedāṅga Jyotiṣa* (ca. −1350 CE) locates the winter solstice at the beginning of Śraviṣṭha (Delphini) and summer solstice in the middle of Āśleṣā, while Varāhamihīra locates winter solstice at the end of the first quarter of Uttarāṣāḍha, and summer solstice at the end of three quarters of Punarvāsu. Today we would say that this happens because of the precession of the equinoxes (the earth precesses like a spinning top), which has a period of about 26000 years, so that the precession is about 1° in 72.2 years. In the roughly 1700 years between Varāhamihīra (who lived some 1500 years ago) and the *Vedāṅga Jyotiṣa*, there had been a precession of about $1\frac{3}{4}$ *nakṣatra* or about 23° 20′. The key point of concern here is that Varāhamihīra, Āryabhaṭa etc. are all willing to allow the *pratyakṣa* to override *śabda* or authority.
9. S. N. Sen and A. K. Bag, *The Śulbasūtras*, Indian National Science Academy, New Delhi, 1983.
10. K. S. Shukla, *Āryabhaṭīya of Āryabhaṭa*, Indian National Science Academy, New Delhi, 1976.
11. For the double quotation marks, see Chapter 1, or C. K. Raju, "How should Euclidean' geometry be taught", paper presented at the International Workshop on History of Science: Implications for Science Education, TIFR, Bombay, Feb. 1999. In: Nagarjuna G., ed., *History and Philosophy of Science: Implications for Science Education*, Homi Bhabha Centre, Bombay, 2001, pp. 241–260. Brie y, there are two reasons respectively for the two quotation marks. (1) Though the result was clearly known prior to Pythagoras, and Proclus regards the attribution to Pythagoras as a rumour, the attribution to Pythagoras has been sustained on the grounds that mathematics ought not to involve the empirical. (2) However, if that be the case, and we adopt Hilbert's synthetic approach, for consistency, then, as pointed out in Chapter 1, Proposition 1.47 of the *Elements* is no longer valid or syntactically acceptable, for it asserts "equality" in the sense of equal areas, and area, like length, is a metric notion, not available in Hilbert's synthetic approach, which substitutes Proclus' equality with congruence. This substitution does not apply to I.47, since the areas involved are non-congruent.
12. *Yuktibhāṣā*, Part I, ed. Ramavarma (Maru) Thampuran and A. R. Akhileswara Aiyer, Mangalodayam Ltd., Trichur, 1123 Malayalam Era, 1948 CE. Unpublished English translation by K. V. Sarma.
13. In Christian rational theology, the empirical world had to be contingent, since a necessary proposition was regarded as a proposition that had to be true for all time, or at least for all future time, or true in

all possible worlds. But, a world which existed for all time past, or all time future, would go against the respective doctrines of creation and apocalypse. This is in contrast to Buddhist thought which has no place for God or creation.

14. Proclus, cited earlier, p. 29.
15. D. Hilbert, *The Foundations of Geometry*, Open Court, La Salle, 1902.
16. B. Russell, *The Foundations of Geometry*, London, 1908.
17. School Mathematics Study Group, *Geometry*, Yale University Press, Yale, 1961.
18. Gödel's attack on Hilbert's program concerned Hilbert belief that theorems could be *mechanically* derived from axioms. Gödel's theorems do not challenge the notion of proof, which remains mechanical. That is, though it may be impossible to *generate* mechanically or recursively the proof of all theorems pertaining to the natural numbers, given a fully written-out proof, it is, in principle, possible to *check* its correctness *mechanically*.
19. G. D. Birkhoff, "A set of postulates for plane geometry (based on scale and protractor)", *Ann. Math.* **33** (1932). For an elementary elaboration of the difference between the various types of geometry, see E. Moise, *Elementary Geometry from an Advanced Standpoint*, Addison Wesley, Reading Mass, 1968.
20. Trans. Davies and Vaughan, cited earlier, p. 248. Jowett's translation reads: "—geometry and the like— they only dream about being, but never can they behold the waking reality so long as they leave the hypotheses which they use unexamined, and are unable to give an account of them. For when a man knows not his own first principle, and when the conclusion and intermediate step are also constructed out of he knows not what, how can he imagine that such a fabric of convention can ever become science?" B. Jowett, cited earlier, p. 397.
21. Proclus, cited earlier, p. 26.
22. Proclus, cited above, p. 33.
23. Proclus, cited earlier, p. 24.
24. Plato, *Republic* 527, trans. Davies and Vaughan, cited earlier, p. 240. Jowett's translation reads, "They have in view practice only, and are always speaking, in a narrow and ridiculous manner, of squaring and extending and applying and the like—they confuse the necessities of geometry with those of daily life; whereas knowledge is the real object of the whole science." B. Jowett, cited earlier, p. 394.
25. Jonathan Swift, *Gullivers Travels, Part III, A Voyage to Laputa...*, Wordsworth Editions, 1992, p. 125,
26. Suzan Rose Benedict, *A Comparative Study of the Early Treatises Introducing into Europe the Hindu Art of Reckoning*, Ph.D. Thesis, University of Michigan, April 1914, Rumford Press.
27. The victory of algorismus over abacus was depicted by a smiling Boethius using Indian numerals, and a glum Pythagoras to whom the abacus technique was attributed. This picture first appeared in the *Margarita Philosophica* of Gregor Reisch, 1503, and is reproduced e.g. in Karl Menninger, *Number Words and Number Symbols: A Cultural History of Numbers*, trans. Paul Broneer, MIT Press, Cambridge, Mass., 1970, p. 350.
28. More details, and quotations etc. may be found in C. K. Raju, "Kamāl or rāpalagai," paper presented at the Ninth Indo-Portuguese seminar on history, INSA, Dec. 1998. In: *Indo-Portuguese Encounters: Journeys in Science, Technology and Culture*, ed. Lotika Varadarajan, Indian National Science Academy, New Delhi, and Universidade Nova de Lisboa, Lisbon, 2006, vol. 2, pp. 483–504.
29. *The Principal Works of Simon Stevin*, vol. III, *Astronomy and Navigation*, ed. A. Pannekoek and Ernst Crone, Swets and Seitlinger, Amsterdam, 1961.
30. Christophori Clavii Bambergensis, *Tabulae Sinuum, Tangentium et Secantium ad partes radij 10,000,000...*, Ioannis Albini, 1607.
31. René Descartes, *The Geometry*, trans. David Eugene and Marcia L. Latham, Chicago, Encyclopaedia Britannica, 1990, Book 2, p. 544.
32. C. K. Raju, "Quantum mechanical time", chp. 6b in: *Time: Towards a Consistent Theory*, Kluwer, Dordrecht, 1994.
33. Stephen Hawking, personal communication of 16 December 1997, "Space and Time Warps by S. W. Hawking as at 18/10/95".
34. Immanuel Kant, *Critique of Pure Reason*, Preface to the Second Edition, 1787, trans. J. M. D. Meiklejohn, Encyclopaedia Britannica, Chicago, 1990, p. 5.
35. For some more details, see C. K. Raju, "Computers, mathematics education, and the alternative epistemology of the calculus in the Yuktibhāṣā", *Philosophy East and West*, **51** (3) 2001, pp. 325–62; "Mathematics and culture", in *History, Culture and Truth: Essays Presented to D. P. Chattopadhyaya*, ed. Daya Krishna and K. Satchidananda Murthy (eds), Kalki Prakash, New Delhi, 1999, pp. 179–193. Reprinted in *Philosophy of Mathematics Education* **11**. Available at http://www.people.ex.ac.uk/PErnest/pome11/art18.htm.

36. P. C. Mahalanobis, "The foundations of statistics (A study in Jaina logic)", *Dialectica* **8** (1954) pp. 95–111; reproduced in *Sankhya, Indian Journal of Statistics,* **18** (1957) pp. 183–194; reproduced in D. P. Chattopadhyaya, *History of Science and Technology in Ancient India*, Firma KLM, Calcutta, 1991, as Vol. 2, *Formation of the Theoretical Fundamentals of Natural Science,* Appendix IV B, pp. 417–432.
37. J. B. S. Haldane, "The *Syādavāda* system of predication", *Sankhya*, Indian Journal of Statistics, **18** (1957) p. 195; reproduced in D. P. Chattopadhyaya, cited in previous note, *Theoretical Fundamentals of Natural Science*, Appendix IV C, pp. 433–440.
38. D. S. Kothari, reproduced in D. P. Chattopadhyaya, cited above, *Theoretical Fundamentals of Natural Science,* Appendix IV D, pp. 441–448.
39. Sadly, Mahalanobis refers to the Buddhist doctrine of ux, in this context as "one well-known school of Buddhist philosophy which holds that reality consists of an infinite sequence of [atomistic] or completely independent [moments] which have no connexion with one another." Mahalanobis, cited above, pp. 424–25.
40. S. C. Vidyabhushan, *A History of Indian Logic*, Calcutta, 1921.
41. If he really was the brother of the astronomer Varāhamihīra, whose work on astronomy, cited in Chapter 1, note 24, is securely dated to the 6th c. CE.
42. D. S. Kothari (cited earlier) and his advisors seem to have been unaware of the work of Reichenbach, done decades earlier. For details of Reichenbach's work, and an exposition of three valued logic see Chapter 1 of C. K. Raju, *Time: Towards a Consistent Theory*, Kluwer Academic, Dordrecht, 1994. In view of the unsuccessfulness of this approach, it seems to me that using a three valued logic as the foundation of statistics, as suggested by Mahalanobis, leads neither to classical nor to quantum statistics!
43. The Jain units of time suggest that this time atom is linked to human perception of sound, which has a cutoff at 18KHz (or the next octave).
44. Maurice Walshe, cited earlier, pp. 78–79.
45. Maurice Walshe, cited earlier, p. 541, footnote 62 to Sutta 1.
46. Digha Nikaya, trans. Rahul Sānkrityāyana and Jagdish Kāshyapa, Paramamitra Prakashana, Delhi 2002.
47. *Dialogues of the Buddha*,vol. 1, trans. Rhys-Davids, cited earlier, p. 75.
48. B. M. Barua, D. Litt. Thesis, University of London, 1921, cited earlier. In Barua's view this was modified to a five-fold negation by Sañjaya Belaṭṭhaputta.
49. Retranslated from the Hindi by the author from Rahul Sānkrityāyana, trans. Note the corrections to the translation of *Dīgha Nikāya* by Maurice Walshe, *The Long Discourses of the Buddha*, cited earlier, p. 97.
50. Maurice Walshe, cited earlier, pp. 80–81
51. For this reason, I am doubtful of the translation of Nāgārjuna's *prasaṅg* into *reductio ad absurdum.* Though it is Nāgārjuna's objective to bring out the absurdity of certain beliefs, *reductio* has a specific meaning today (and in Euclid's *Elements*) in the context of two-valued logic.
52. E.g. Kenneth K. Inada, trans., Nāgārjuna, *Mūlamādhaymakakārikā*, Satguru Publications, Delhi 1993, 18.8. Also *Dīgha Nikāya (Long Discourses of the Buddha)* trans. Maurice Walshe, Wisdom Publications, Boston, 1995, pp. 80–81.
53. B. K. Matilal, *Logic, Language, and Reality*, Motilal Banarsidass, Delhi, 1985, p. 146
54. G. N. Ramachandran, Technical Report, Department of Mathematical Biology, Indian Institute of Science, Bangalore (198?).
55. This means that we have eight truth values, the negation of the first being the second, the negation of the second being the third, and so on, with the negation of the last being the first. For more details on cyclic negation, see the text of N. Rescher, *Many-Valued Logic*, 1967.
56. C. K. Raju, *Time: Towards a Consistent Theory*, cited earlier, Chapter VIB, and its appendix.
57. D. Chatterji (trans.), "*Hetucakranirṇaya*", *Indian Historical Quarterly* **9** (1933) pp. 511–514. Reproduced in full in, R. S. Y. Chi, *Buddhist Formal Logic*, The Royal Asiatic Society, London, 1969, reprint Motilal Banarsidass, Delhi 1984.
58. Diṅnāga clearly has the last word in S. C. Vidyabhushan's *Indian Logic*, cited earlier, p. 299 (and pull-out diagram annexed as the last page of the book)!
59. R. S. Y. Chi, *Buddhist Formal Logic*, cited above, p. 5. The claim is in the ellipsis which expands to read, "In fact, the so-called similar' and dissimilar' instances refer to the likeness to the major term but not to the middle term [reason, *hetu*]". See, however, Vidyabhushan, p. 291 (of reprint by Motilal Banarsidass, Delhi). In addition, there are some minor discrepancies which I cannot comment upon, since I do not know the Tibetan language.

60. Nothing can possibly be redundant in a text as brief as the *Hetucakra*, and the Sanskrit formulae of the *Nyāyavarttikā* clearly does not cover the last stanza of the *Hetucakra*, a point which Udyotkara also overlooks in his arguments against the Buddhist notion of instant of time. B. K. Matilal, *Logic, Language, and Reality*, Motilal Banarsidass, Delhi, 1985, p. 146, expresses the same opinion, "My own feeling is that to make sense of the use of negation in Buddhist philosophy in general, one needs to venture outside the perspective of the standard notion of negation." See also, H. Herzberger, "Double Negation in Buddhist Logic," *Journal of Indian Philosophy* **3** (1975) pp. 1–16.
61. E.g. A. N. Prior, *Past, Present, and Future*, Clarendon, Oxford, 1967.
62. E.g. E. Mendelson, *Introduction to Mathematical Logic*, The University Series in Undergraduate Mathematics, Van Nostrand Reinhold, New York, 1964.
63. To the above points, one could add the following. (3) Udyotkara's *Nyāyavarttikā* is implicitly, explicitly, and polemically against Buddhist philosophy; so I see no reason to regard Udyotkara's as the last word on Dignāga, especially since that last word is positioned at such a peculiar moment in the history of Buddhism in this country, when no Buddhist was left to respond to Udyotkara. (4) Diṅnāga's logic, in his *Pramāṇasamuccay*, cannot be instantly formalised, because he explicitly rejected tautological inferences as trivial, while Western logic admits only such inferences. Thus, to infer fire from smoke was a trivial inference. Nor from a smoky hill should one infer a fire on the hill (for the *connection* between fire and hill could not be inferred—the apparent connection between smoke and hill may be only an illusion). Hence, from a smoky hill one inferred a fiery hill—from an apparently smoky hill one inferred an apparently fiery hill.
64. Isn't socially approved knowledge the best that one can aspire for? That depends upon the nature of the society in question. Does social authority refer to unanimity or even to a democratically evolved consensus? As argued earlier, in industrial capitalist societies, for economic reasons, the social authority for scientific knowledge necessarily rests in certain specialists, and the social conferment of authority on these specialists often fully re ects the evils of these societies. Further, these specialists being under pressure to confirm, the agreement of many specialists is hardly a guarantee of secure knowledge. Thus mathematical knowledge in capitalist societies is exactly as insecure as the technology that arises from the capitalist way of getting quick practical results with the least resources. C. K. Raju, "Mathematics and Culture", cited earlier.
65. C. K. Raju, *Time: Towards a Consistent Theory*, Kluwer Academic, Dordrecht, 1994, Chapter 6b.
66. Ubiratan D'Ambrosio, *Socio-Cultural Bases for Mathematics Education*, Unicamp, 1985.
67. Paulus Gerdes, *Geometry from Africa: Mathematical and Educational Explorations*, Washington, Mathematical Association of America, 1999.
68. ANSI/IEEE standard 754 of 1986.
69. B. B. Dutta and A. N. Singh, *History of Hindu Mathematics, A Source Book*, Parts I and II, Asia Publishing House, Bombay [1935] 1962, p. 245, "Brahmagupta has made the incorrect statement that 0/0=0."
70. *Seminar on the Concept of Śūnya*, INSA and IGNCA, New Delhi, Dec. 1998.
71. W. Rudin, *Real and Complex Analysis*, Tata McGraw Hill, New Delhi, 1968, pp. 18–19.
72. Rudin, cited above.
73. C. K. Raju, "Mathematical Epistemology of śūnya", cited earlier.
74. Also, as we shall see in a later chapter, the Java computing language adopts the convention that the operation 1/0 has different meanings depending upon the *kind* of number "1" is regarded as: if "1" is regarded as the oating point number 1.0, the operation gives INF, but if 1 is regarded as an integer, the operation results in an error!
75. C. K. Raju, *Time: Towards a Consistent Theory*, Kluwer Academic, 1994, Chapter 5b. The error is that the essential history-dependence of the relativistic many-body problem has been wished away by using a Taylor expansion in powers of the delay to convert a retarded functional differential equation into an ordinary differential equation.
76. A. Einstein, L. Infeld, and B. Hoffman, *Ann. Math.* **39** (1938) 65.
77. Gerald Holton, *Science and Anti-Science*, Harvard University Press, Cambridge, Mass, 1994, p. 147.
78. This is re ected in the social phenomenon where many heads of scientific establishments routinely claim ownership of innovation by attaching their names to papers they may never have read, and may not even be able to understand or explain. Conversely, many younger scientists seek to gain authority by promoting this practice. The Darcy case demonstrated that this sort of thing is systematically true of leading institutions around the world. See, W. W. Stewart and N. Feder, "The integrity of scientific literature," *Nature* **325** (1987) pp. 207–214.

Part II

The Calculus in India

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CHAPTER 3

Infinite Series and π

The thousand-year background to infinite series in India and how they were derived

OVERVIEW

We have now seen the deficiencies in the present-day ("Hilbertian") notion of mathematical proof, and the illegitimate historical claims which gave it credence. We have also seen the advantages of an alternative epistemology of mathematics (as in *pramāṇa*). Accordingly, we are now prepared to examine the actual historical origin of the calculus without the mindless presupposition that has af icted previous authors that present-day formal mathematics (and particularly mathematical analysis) is the only possible way of doing things.

The background and derivation of the high-order "Taylor" series and "Gregory–Leibniz" series expansions used in 14th–15th c. CE in India has, in any case, never been fully and clearly explained. The calculus historically originated in India in the process of calculating precise trigonometric values, and the length of the circumference of a circle, using both infinite and indefinite series. The details are as follows.

Āryabhaṭa in *Gaṇita* 11 cursorily dismisses the clumsy geometric and algebraic method of computing trigonometric values, "using triangles and quadrilaterals". He goes on to state (*Gaṇita* 12) a finite difference method of computing sine values, which is exactly like an Euler solver for ordinary differential equations. (It should not be presumed that Euler derived this method independently, since Euler not only wrote an article on the use of the sidereal year in Indian astronomy, but diligently followed up the work of Fermat, whose challenge problem to European mathematicians is a solved example in Bhāskara II.) While the actual values in *Gītikā* 10/12 are values of the first sine differences, *Gaṇita* 12 applies to the *second* sine differences (as also noticed by Delambre), allowing us to compute both sine values and differences. Āryabhaṭa uses a computational notion of a function as a stored table of values/differences, along with a method of (linear) interpolation, which has epistemological advantages over the formal set theoretic definition, involving supertasks. Second differences are certainly used by Brahmagupta (*Khaṇḍakhādyaka* II.1.4) for quadratic interpolation. Vaṭeśvara (*Siddhānta* II.1.64–67) uses further "Stirling's" formula for quadratic interpolation, along with stored trigonometric values that are only 56 apart, to achieve a higher precision to the second (sexagesimal minute). Bhāskara II, who explicitly lists second differences, justifies the above interpolation formula using the notion of "instantaneous sine difference", closely related to his notion of instantaneous velocity (*tātkālika gati*) of a planet (*GrahaGaṇita* VII. 37–38).

This background combined with the *indefinite* fraction series expansion of Brahmagupta (*Brāhma Sphuṭa Siddhānta* 12.57) leads very naturally to the power series expansion for the sine function credited by the 1501 CE Nīlakaṇṭha to his predecessor Mādhava (1340 CE), and also found in the *TantrasaṅgrahaVyākhyā/Yuktidīpikā* (2.441–443), *Kriyākramakarī*, *Yuktibhāṣā*, etc. I also explain the basic principle of order counting and discarding of non-representables, used to obtain the sum of an *infinite* geometric series, as stated by Nīlakaṇṭha in his *ĀryabhaṭīyaBhāṣya*.

Some five centuries before Mādhava, Govindasvāmin (ca. 800) and then Udayadivākara (10th c.), of the Āryabhaṭa school in Kerala, tried to obtain trigonometric values accurate to the thirds (i.e., third sexagesimal minute), but their values were not accurate enough; Mādhava's trigonometric values *are* however accurate to the thirds. Hence, it is clear that a key input enabling the computation is the expression for the sum of the *vārasaṅkalitā* given by Nārāyaṇa Paṇḍit of Benares in his *Gaṇita Kaumudi*, to sum the intermediate series $\frac{1}{n^{k+1}} \ {}_{i=1}^{n} i^k$, for the non-elementary cases, k 4. (Fermat and Pascal's derivation of the area under "higher-order" parabolas similarly used higher order figurate numbers.) This shows, incidentally, that it is incorrect to attribute the entire development of the calculus to the "Kerala" school, since the development of the calculus involved key contributions from various parts of India, over a thousand year period, including Patna, Gujarat, Ujjain, and Benares, as much as from Cochin.

Āryabhaṭa's value of π, accurate to five decimal places, was perhaps derived by an earlier technique which combined geometric and algebraic methods, continuing the *śulba-sūtra* method (e.g. Apastamba 3.2) of cutting the corners of a square, but using, instead of the *śulba-sūtra* value of $\sqrt{2}$, a full algorithm for square-root extraction, stated in *Gaṇita* 4. This octagon-doubling method differs from the 13th–14th c. CE hexagon-doubling method used by both al Kashi and Yu-Chhin, but attributed (with insufficient reason) to "Archimedes" and to the 3rd c. CE Liu Hui, respectively.

For the 11th–12th order "Taylor" polynomials, computation of Mādhava's coefficients, accurate to the third minute, required a value of π accurate to at least 8 places, and the value of π accurate to 11 decimal places is attributed to Mādhava of Saṅgamagrāma by Nīlakaṇṭha in the *ĀryabhaṭīyaBhāṣya* (*Bhāṣya* on *Gaṇita* 17), and also credited to Mādhava in the commentary *Laghuvivṛti* on Nīlakaṇṭha's *Tantrasaṅgraha* (2.9.5). How was this value derived? Contrary to Srinivasiengar's assertion that summing the series must have involved a lot of labour, I explain how these series could be used to compute π accurately to 10 decimal places in a completely practical way with fewer than 100 oating point operations.

The sum of an infinite series was understood not as involving the supertask of adding together an infinite number of terms, but as that of summing the series to a finite number of terms beyond which the sum of the series became constant (up to non-representables). To actually compute the sum, in analogy with the indefinite series, used from long before, this infinite sum was expressed as the sum of a finite number of typical terms *plus* an exceptional or correction term. In the case of an indefinite series, the exceptional term made the successive sums (exactly) constant; to arrive at a similar situation (up to non-representables), in the case of an infinite series, the exceptional term was chosen to minimize the change in successive sums. The use of the correction term made it practicable to sum the infinite series, because the use of the correction term amounted to transforming the series to accelerate its convergence, especially important for the case of the slowly convergent "Gregory–Leibniz" series. I explain how the place-value notation for numbers was extended to represent polynomials and rational functions as expounded in the *Kriyākramakarī*, of Śaṅkara Vāriyar, which provides the most complete description of this correction/acceleration procedure. I also explain how the notion of the order of growth of a rational function in one variable (*rāśī*) was used to obtain the *saṁskāra* correction, and to improve it by computing its grossness (*sthaulya*), by a technique of iterative minimization that Youskevich and Hayashi et al. have missed. The computation explicitly resulted in the continued fraction expansion for π (related to the expansions used by Brouncker and Wallis).

I also point out the use of the traditional Indian technique of "zeroing" the insignificant or non-representable quantities in the above calculation. Any term, or terms, could be discarded or "zeroed" when insignificant or non-representable (*śūnya*) from a practical point of view. This zeroing is similar to rounding, but unlike rounding or chopping for oating point numbers, this zeroing was done in a non-mechanical way. This is clear, for example, from the slight difference in sine values between Āryabhaṭa I and Āryabhaṭa II. The consequence of this last factor is considered in greater detail in later chapters of this book. The zeroing of non-representables was understood as inevitable for numbers like π for which it was understood from very early times that an exact representation was impossible. Infinitesimals, that can be zeroed like non-representables, are a natural extension of this concept of zeroing, combined with the notion of order of growth of a polynomial or rational function. Finally, the *Yuktidīpikā* and *Yuktibhāṣa* derivation of these series does make use of the empirical in a variety of ways, even to the extent of using the atomic theory of the Naiyāyika-s to stop the subdivision of the circumference of the circle, when the subdivisions reach atomic or "indivisible" proportions.

Thus, in Indian tradition, there was a clear understanding of infinite series, and valid *pramāṇa* for the various derivations involved. (The point here is not the distinction between *pramāṇa* and proof, which we have already covered, but the contrast with the case of, say, Newton and Leibniz who could provide neither mathematical proof nor any coherent account to their contemporaries, of these very same series, imported into Europe about a century before them. In retrospect we can understand their lack of understanding: because they adopted (a) an all-rule-no-exception approach to these series, and, overlooking the exceptional or correction term, tried to sum an infinite number of terms; further, they (b) proceeded on an idealistic perspective that regarded mathematics as being perfect, so that the minutest quantity was not to be discarded. However, they obviously could not perform the required supertask of exactly summing a series with an infinite number of terms. Nor could they explain to their sceptical contemporaries, like Berkeley, the meaning of woolly concepts like " uxions" which had eventually to be abandoned in the interests of clarity. These matters are dealt with in a later chapter.)

I
INTRODUCTION

The importance of the calculus for the development of present-day science can hardly be overstressed. As already noted, all the mathematics needed for Newton's *Principia* (and for classical mechanics down to this day) is encapsulated in the so-called Taylor-series expansion, which is the pinnacle of the calculus. In the language of Arnol'd, "Newton invented Taylor series, the main instrument of analysis."[1] Taylor was Newton's pupil, and his work on it dates from 1715.[2] Though the 1671 work of James Gregory[3] predates both, Gregory made a small (almost inconsequential) error, which has been used to his discredit, so that the term "Gregory" series is often reserved for the series for arctan. A particular case of Gregory's series is what is today called the "Leibniz" series for π.

As we show below, the "Taylor" series, the "Gregory" series, and the "Leibniz" series are all found in Indian tradition. Also found are (a) **numerically efficient** rules for evaluating the sum of these series expansions, and (b) **accelerated convergence** methods of accelerating the convergence of slowly convergent series like the "Leibniz" series.

Gregory himself made no claim to originality, and many related series actually appear slightly earlier in Europe, by around 1630 with the work of Cavalieri, a Jesuati, and student of Galileo, whose access to Jesuit sources is very well documented.[4] In Europe, Cavalieri's approach using "indivisibles" was regarded as epistemologically insecure. Newton hence claimed that his uxions (which used the antithetical idea of the continuum) were "perfect", unlike Cavalieri's "approximations". And, though neither Newton nor Leibniz (nor even Taylor) made any fundamental epistemological advance from the perspective of idealistic mathematics (that had to await Dedekind, and the formalisation of set theory), and they were, in fact, unable to explain their ideas of the continuum in a coherent way to their sceptical contemporaries, like Berkeley, both commanded ample social authority which helped to make the calculus socially more acceptable in Europe.

This motivates us to consider two further issues. (c) **Epistemological continuity**. These infinite and indefinite series have an extensive background of a thousand years in Indian mathematics, predating their sudden (epistemologically discontinuous) appearance in Europe, a century after Europeans had established large settlements in the vicinity of the most active groups then working with these series in India, near Cochin. Considering that Europe was then still struggling to understand elementary algorithms for arithmetic, and notwithstanding claims that the calculus was invented by Newton and Leibniz, these series (and the calculus) naturally remained poorly understood in Europe, and could not be assimilated within the then-existing epistemological framework of Western mathematics.

There is also the issue of (d) **pramāṇa and proof**. In contrast to the situation in Europe, detailed derivations of the series expansion for arctan, sin, and cos are found in the *TantraSaṅgrahaVyākhyā*[5] also known as *Yuktidīpikā/ Laghuvivṛti*[6] of Śaṅkara Vāriyar (1500–1560 CE), the (ca. 1534) *Kriyākramakarī*[7] of Śaṅkara Vāriyar, and Nārāyaṇa, and the contemporary (ca. 1550 CE) *Yuktibhāṣā*[8] of Jyeṣṭhadeva. The *TantraSaṅgrahaVyākhyā*, as the name suggests, is an exposition of Nīlakaṇṭha's 1501 CE *TantraSaṅgraha*.[9] As its other name *Yuktidīpikā* suggests, this exposition throws light on the rationale or *yukti*, while the *Yuktibhāṣā*, as the name suggests, is a discourse on rationale in the *bhāsā* (= vernacular = Malayalam, naturally known to most Christian missionaries then in Cochin). The most complete (though somewhat neglected) work in this respect is the *Kriyākramakarī*, which could be important also from the viewpoint of transmission, since it is a commentary on Bhāskara's *Līlāvatī*, a popular and well-known work.

Though this rationale or *pramāṇa*, since it involves the empirical, does not constitute proof in the sense of Western mathematics, Western mathematical proofs involving idealized real numbers and supertasks do not constitute valid *pramāṇa* either. As seen in the previous chapter, this is *not* a purely relativistic position: Western mathematical proof, being devoid of any empirical basis, can never hope to be universally acceptable. Also, the practical value of mathematics derives from the ability to calculate, well adapted to *pramāṇa*, but not to idealistic mathematical proof involving impossible supertasks. Therefore, let us set aside this oft-repeated belief about Western mathematical proof as a theological superstition, shortly likely to become extinct.

The series expansions, themselves, are found also in various other books such as the *ĀryabhaṭīyaBhāṣya*[10] of Nīlakaṇṭha, or the anonymous *Karaṇapaddhati*.[11] The series expansion and the sine values are referred to as being "given by Mādhava", identified as the 14th

c. CE Mādhava of Saṅgamagrāma who first used them to derive a "table" of 24 very accurate trigonometric values. This table greatly improves upon the accuracy of a similar "table" of 24 values provided a thousand years earlier by Āryabhaṭa, and continuously improved upon since then by various people, including Bhāskara I and Vaṭeśvara. Where Āryabhaṭa's 24 sine values are accurate to the first sexagesimal minute, and Vaṭeśvara's 96 values are accurate to the second sexagesimal minute, Mādhava's 24 values are accurate to the third sexagesimal minute—about eight to nine decimal places.

II
THE SERIES EXPANSION FOR SINE AND COSINE

The "Taylor" series expansion for the sine is stated in a couple of verses, as follows.

> निहत्य चापवर्गेण चापं तत्तत्फलानि च ।
> हरेत् समूल्युग्वर्गैस्त्रिज्यावर्गहतैः क्रमात् ॥ ४४० ॥
> चापं फलानि चाधोऽधो न्यस्योपर्युपरित्यजेत् ।
> जीवाप्त्यै, संग्रहोऽस्यैव विद्वान् इत्यदिना क्रितः ॥ ४४१ ॥

The key passage[12] may be translated as follows.[13]

> Multiply the arc by the square of the arc, and take the result of repeating that [any number of times]. Divide [each of the above numerators] by the squares of successive even numbers increased by that number [lit. the root] and multiplied by the square of the radius. Place the arc and the successive results so obtained one below the other, and subtract each from the one above. These together give the *jīvā*, as collected together in the verse beginning with *"vidvān"* etc.

Jīvā relates to the sine function. Etymologically, the term sine derives from *sinus* (= fold), a Latin translation of the Arabic *jaib* (fold for pocket, as in a shirt). What the Oxford English Dictionary does not mention is that *jaib* (= pocket) is a misreading of the Arabic term *jībā* (both terms were written as *jb*, omitting the vowels). Mathematically, however, as is well known, Indian mathematics and astronomy (like European mathematics in the 16th and 17th c. CE) dealt not directly with present-day sines and cosines but with these quantities multiplied by the radius r of a standard circle. Thus, *jīvā* (earlier *jyā*) corresponds to $r \sin\theta$, and is sometimes called Rsine, while the *śara* corresponds to $r(1 - \cos\theta)$.

In present-day mathematical terminology, the above passage says the following. Let r denote the radius of the circle, let s denote the arc and let t_n denote the nth expression obtained by applying the rule cited above. The rule requires us to calculate as follows.

1. Numerator: multiply the arc s by its square s^2, this multiplication being repeated n times to obtain $s \cdot \prod_{1}^{n} s^2$.

2. Denominator: multiply the square of the radius, r^2, by $[(2\,k)^2 + 2\,k]$ ("the squares of successive even numbers increased by that number") for successive values of k, repeating this product n times to obtain $\prod_{k=1}^{n} r^2\,[(2k)^2 + 2k]$.

Thus, the nth iterate is obtained by

$$t_n = \frac{s^{2n} \cdot s}{(2^2 + 2) \cdot (4^2 + 4) \cdot \cdots \cdot [(2n)^2 + 2n] \cdot r^{2n}}. \tag{3.1}$$

The rule further says:

$$\text{jīvā} = s - t_1 + t_2 - t_3 + t_4 - t_5 + \ldots \tag{3.2}$$

$$= s - \frac{s^3}{r^2 \cdot (2^2+2)} + \frac{s^5}{r^4(2^2+2)(4^2+4)} - \cdots. \tag{3.3}$$

Substituting
(1) jīvā $= r \sin \theta$,
(2) $s = r\,\theta$, so that $s^{2n+1} / r^{2n} = r\,\theta^{2n+1}$, and noticing that
(3) $[(2k)^2 + 2k] = 2k \cdot (2k+1)$, so that
(4) $(2^2+2)(4^2+4)\cdots[(2n)^2 + 2n] = (2n+1)!$,
and cancelling r from both sides, we see that this is entirely equivalent to the well-known expression

$$\sin \theta = \theta - \frac{\theta^3}{3!} + \frac{\theta^5}{5!} - \frac{\theta^7}{7!} + \cdots. \tag{3.4}$$

A similar rule gives an iterative expression for *śara*. The passage[14] reads:

निहत्य चापवर्गेरा रूपं तत्तत्फलानि च ।
हरेद् विमूल्युग्वर्गैस्त्रिज्यावर्गहतैः क्रमात् ॥ ४४२॥
किन्तु व्यासदलेनैव द्विघ्नेनाद्यं विभज्यताम ।
फलान्यधोऽधः क्रमशो न्यस्योपर्युपरि त्यजेत् ॥ ४४३॥
शरापत्यै, संग्रहोस्यैव स्तेन स्त्रीत्यादिना क्रितः

This may be translated as follows.

> Multiply the square of the arc by the unit (= radius), and take the result of repeating that [any number of times]. Divide [each of the above numerators] by the squares of successive even numbers decreased by that number and multiplied by the square of the radius. But, the first term is [now] [the one which is] divided by twice the radius. Place the successive results so obtained one below the other and subtract [lit. remove] each from the one above. These together give the *śara*, as collected together in the verse beginning *"stena"*, *"strī"*, etc.

This amounts to

$$u_n = \frac{s^{2n} \cdot r}{(2^2-2)\cdot(4^2-4)\cdot \cdots \cdot [(2n)^2-2n] \cdot r^{2n}}, \tag{3.5}$$

and the rule further says that

$$\text{śara} = r(1-\cos\theta) = u_1 - u_2 + u_3 - u_4 + u_5 - \cdots, \tag{3.6}$$

$$= \frac{s^2 \cdot r}{r^2(2^2-2)} - \frac{s^4 \cdot r}{r^4(2^2-2)(4^2-4)} + \cdots. \tag{3.7}$$

Recalling that
(1) śara $= r(1-\cos\theta)$,
(2) $s = r\,\theta$, so that $s^{2n}r \quad r^{2n} = r\,\theta^{2n}$, and noticing that
(3) $[(2k)^2 - 2k] = 2k \cdot (2k-1)$, so that
(4) $(2^2-2)(4^2-4)\cdots[(2n)^2 - 2n] = (2n)\,!$,
we see that this is again equivalent to the well-known expression

$$\cos\theta = 1 - \frac{\theta^2}{2!} + \frac{\theta^4}{4!} - \frac{\theta^6}{6!} + \cdots. \tag{3.8}$$

Though a great achievement in itself, the actual numerical calculation of the sine and cosine values from here is far from trivial. If too few terms are taken, the results are inaccurate, especially for larger values of the arc. If too many terms are taken, the computations become impossibly unwieldy. Even with an ordinary calculator, calculating 20! is difficult, and an accurate value of 200! is non-trivial even on a computer. Further, as we shall see later, the value of the radius r is inextricably tied to the value of π, since the circle was traditionally taken as having a fixed circumference of 21,600 (= 3600 × 60). Finally, for $\theta > 1$ (radian), i.e., for angles larger than about 58°, the value of the powers of $s \quad r$ goes on increasing instead of decreasing. Therefore, a numerically efficient method was evolved, which could be used to calculate the desired sine values to high accuracy with 1 division, 6 multiplications, and 5 subtractions, or just 12 arithmetical operations in all, even by those who did not use the precise value of the radius.

This required a transformation of the above series. The series (3.3) was rewritten as

$$\text{jīvā} = s - \frac{s^3}{r^3}\cdot\frac{r}{(2^2+2)} + \frac{s^5}{r^5}\cdot\frac{r}{(2^2+2)(4^2+4)} + \cdots \tag{3.9}$$

$$= s - \frac{s^3}{c^3}\cdot\frac{r\left(\frac{\pi}{2}\right)^3}{(2^2+2)} + \frac{s^5}{c^5}\cdot\frac{r\left(\frac{\pi}{2}\right)^5}{(2^2+2)(4^2+4)} + \cdots, \tag{3.10}$$

where $c = 5400$ was a quarter of the circumference of the standard circle.

Thus, the actual calculation of sine values used a "ready-reckoner" stored "table" of numerical coefficients encapsulated in a single verse[15] of four lines beginning with *vidvān* etc.

विद्वांस् तुन्नबलः कवीशनिचयः सर्वार्थशीलस्थिरो
निर्विद्धाङ्गनरेन्द्ररुङ् निगदितेष्वेषु क्रमात् पञ्चसु ।
आधस्त्याद् गुणितादभीष्टधनुषः क्रित्या विहृत्यान्तिम-
स्याप्तं शोध्यमुपर्युपर्यथ घनेनैवं धनुष्यन्ततः ॥ ४३७ ॥

Here *vidvān, tunnabala, kavīśanicaya, sarvārthaśīlasthira, and nirviddhāṅganarendraru* are expressions for five numbers in the reverse *kaṭapayādi*-sexagesimal system (box 3.1).

Box 3.1. *Kaṭapayādi* system

This system is based on the letters of the Sanskrit alphabet. It was known to Āryabhaṭa who had a different system. The consonants (alphabets) in due succession denote the numerals, as in the following table.

1	क	ट	प	य
2	ख	ठ	फ	र
3	ग	ड	ब	ल
4	घ	ढ	भ	व
5	ङ	ण	म	श
6	च	त		ष
7	छ	थ		स
8	ज	द		ह
9	झ	ध		ळ
0	ञ	न		

1	*ka*	*ṭa*	*pa*	*ya*
2	*kha*	*ṭha*	*pha*	*ra*
3	*ga*	*ḍa*	*ba*	*la*
4	*gha*	*ḍha*	*bha*	*va*
5	*ṅa*	*ṇa*	*ma*	*śa*
6	*ca*	*ta*		*ṣa*
7	*cha*	*tha*		*sa*
8	*ja*	*da*		*ha*
9	*jha*	*dha*		*ḷa*
0	*ña*	*na*		

The vowels *standing by themselves* also denote 0.

Of two conjoint consonants, only the last has numerical significance.

The numerals may be in direct or reverse order. (The reverse order apparently found greater favour, according to the maxim: *aṅkānām vāmato gati.*)

E.g. *bhavatī* = *bha va tī* = 4 4 6 = 644 in reverse *kaṭapayādi.*

Chronograms may occasionally have an ordinary meaning. This additional meaning is regarded as an ornament (*śleṣa alaṁkāra*) to verse, and helps to memorize it.

In sexagesimal notation, a number is to be interpreted in terms of first (*kalā*), second (*vikalā*), and third (*tatparā*) minutes. Thus, *vidvān* = *vi dvā n* = 4 4 0 = 0 44 (for *dvā* use the conjoint consonant rule), *tunnabala* = *tu nna ba la* = 6 0 3 3 = 33 06 , *kavīśanicaya* = *ka vī śa ni ca ya* = 1 4 5 0 6 1 = 16 05 41 , *sarvārthaśīlasthira* = *sa rva rth śī la sthi ra* = 7 4 7 5 3 7 2 = 273 57 47 , *nirviddhaṅganarendraru* = *ni rvi ddha ṅga na re ndra ru* = 0 4 9 3 0 2 2 2 = 2220 39 40 .

The passage may now be translated.[16] The values of the Rsine, as collected together in the *śloka* beginning *vidvān* etc., are given by

> *vidvān, tunnabala*.... Successively multiply these five numbers in order by the square of the arc divided by the quarter of the circumference [i.e., 5400], and subtract from the next number. [Continue this process with the result so obtained and the next number.] Multiply [the final result] by the cube of the arc divided by quarter of the circumference, and subtract from the arc.

In present-day notation, if we denote these numbers, starting from *vidvān* respectively by a_{11}, a_9, a_7, a_5, a_3, then if s ($r\theta$) is the given arc in minutes, c is the length (= 5400) of a quadrant of the standard circle, and $jy\bar{a} = r \cdot \sin\theta$, then the verse corresponds to an iterative procedure. Starting with *vidvān* (a_{11}), multiply it by $(s\ \ c)^2$ and subtract it from the next number: *tunnabala* (a_9). Again multiply the result by $(s\ \ c)^2$ and subtract from the next number. Multiply the final result by $(s\ \ c)^3$ and subtract from the arc s, to obtain the *jyā*. Thus, the result my be expressed by the formula

$$r \sin\theta = s - \left(\frac{s}{c}\right)^3 \left(a_3 - \left(\frac{s}{c}\right)^2 \left(a_5 - \left(\frac{s}{c}\right)^2 \left(a_7 - \left(\frac{s}{c}\right)^2 \left(a_9 - \left(\frac{s}{c}\right)^2 a_{11}\right)\right)\right)\right). \tag{3.11}$$

This formula is a *numerically efficient* way to approximate the sine function by its "Taylor" polynomial of the 11th order.

There is a similar formula for cosine in the next verse[17] beginning with *stenah* etc., corresponding to numerically efficient approximation by its "Taylor" polynomial of the 12th order.

> स्तेनः स्त्रीपिशुनः सुगन्धिनगनुद् भद्राङ्गभव्यासनो
> मीनाङ्गोनरसिंह ऊनधनकृद्भूरेव षटस्वेषु तु ।
> आधस्त्याद् गुणितादभीष्टधनुषः कृत्या विहृत्यान्तिम-
> स्याप्तं शोध्यमुपर्युपर्यथ फलं स्यादुत्क्रमस्यान्त्यजम् ॥ ४३८ ॥

This may be translated:

> The six: *stena* [60 = 06], *strīpiśuna* [2150 = 05 12], *sugandhinaganud* [739030 = 03 09 37], *bhadrāṅgabhavyāsana* [4234170 = 071 43 24], *mināṅgonarasiṁha* [5030278 = 872 03 05], *ūnadhanakṛdbhūreva* [00901424 = 4241 09 00]. Multiply by the square of the arc divided by the quarter of the circumference, and subtract from the next number. [Continue with the result and the next number.] The final result will be the *utkramajyā* [R versed sine].

This corresponds to the formula

$$r\,(1 - \cos\theta) \;=\; \frac{s}{c}^{2} a_2 - \frac{s}{c}^{2} a_4 - \frac{s}{c}^{2} a_6 - \frac{s}{c}^{2} a_8 - \frac{s}{c}^{2} a_{10} - \frac{s}{c}^{2} a_{12} \qquad , \tag{3.12}$$

with a_k being the six numbers in the reverse *katapayādi* sexagesimal system, collected together in the verse beginning with *stena*. From (3.10), the above numbers correspond to

$$a_k = \frac{r}{k!}\;\frac{\pi}{2}^{k}, \tag{3.13}$$

where r is the radius of a circle of circumference 21,600 (= 3600 × 60) minutes ($r = \frac{10800}{\ }$). The actual calculation of a_k thus requires primarily the value of π. Nīlakaṇṭha, in his *ĀryabhaṭīyaBhaṣya,*[18] in his commentary on *Gaṇita* 10, described Mādhava's subtle value of π as follows:

सङ्गमग्रामजो माधवः पुनरत्यासन्नां परिधिसंख्यामुक्तवान् –

विबुधनेत्रगजाहिहुताशनत्रिगुणवेदभवारणबाहवः ।
नवनिखर्वमिते वृतिविस्तरे परिधिमानमिदं जगदुर्बुधाः ॥

The numbers in this verse are according to a different number system, known as the *bhūta saṁkhyā* system, which uses word numerals. Thus, *netra* means 2 because one has two eyes, *veda* = 4, *guṇa*=3, *tri*=3, etc.[19] The quantity *nikharva* = 10^{11}. Thus, the above verse may be translated:

> Mādhava of Saṅgamagrāma spoke the approximate [*āsanna*] number of the circumference of a circle: *vibudha* [33] *netra* [2] *gaja* [8] *ahi* [8] *hutāśana* [3] *tri* [3] *guṇa* [3] *veda* [4] *bhavāraṇa* [27] *bāhavaḥ* [28], i.e., [2,827,433,388,233] is the measure of a circle of diameter *nava* [9] *nikharva* [100,000,000,000].

This corresponds to $\pi = 3.141,592,653,5922\ldots$, accurate to 11 decimal places, with the 12th and 13th places (92 respectively) differing slightly from their accurate value (89). The term *nikharva* continues the series, *koṭi, arbuda, abja, kharva, nikharva*, then in common use for centuries.[20] (This decimal series coming from Vedic times is constant up to the term *koṭi* = crore, in current use. From *koṭi* onwards there are usually variations. Currently, of course, an *arbuda*, called "arab" is 100 crores, while a *kharva*, called "kharab" is 100 arabs.) The more common *kaṭapayādi*-sexagesimal expression for r is *Devo viśvasthalī bhṛguḥ*, corresponding (in reverse order) to 34374448 or 3437 44 48 , which is still substantially more accurate than Bhāskara I's figure of 3438 , or Vaṭeśvara's figure of 3437 44 .

According to Rajagopal and Rangachari, there is a significant discrepancy of -1 in the value of *mīnaṅgonarasiṁha*, "from its accurately rounded off [value]".[21] What they presumably mean is that if we use the "standard"[22] rounding procedure while applying formula (3.13), then the fourths turn out to be -30, while the fifths turn out to be -49. Thus, the "standard" rounding procedure would round -49 fifths to -1 fourths, and when this -1 is added to -30, we should get -31 fourths, which should then be rounded up to give another -1 thirds. Though this particular example is a bit stretched, and also involves negative numbers for which rounding conventions, even today, may vary significantly between mathematicians and computer scientists, we consider the details of the rounding procedure later on.

More to the point, the procedure used to calculate the coefficients explains the degree of accuracy actually needed for the value of π, to compute the coefficients accurately to the third minute. If it is accurate to only 7 places after the decimal, then there is an inaccuracy of 1 —in the coefficient *nirviddhāṅanarendraru*. Thus, an accuracy of at least 8 places after the decimal point is needed for the calculation of the coefficients. In actual fact, the value of π stated is accurate to 11 decimal places.

Using the series expansion and the stored coefficients, the actual sine values are computed. These are stated in the *ĀryabhaṭīyaBhāṣya*,[23] and also in a verse in the *Laghuvivṛti* commentary on the *Tantrasaṅgraha*.[24]

श्रेष्ठं नाम वरिष्ठानां हिमाद्रिर्वेदभावनः।
तपनो भानुसूक्तज्ञो मध्यमं विद्धि दोहनम्॥
धिगाज्यो नाशनं कष्टं छन्नभोगाशयाम्बिका।
म्रिगाहारो नरेशोऽयं वीरो रणजयोत्सुकः॥
मूलं विशुद्धं नाळस्य गानेषु विरळा नराः।
अशुद्धिगुप्ता चोरश्रीः शङ्कुकर्णो नगेश्वरः॥
तनुजो गर्भजो मित्रं श्रीमानत्र सुखी सखे! ।
शशी रात्रौ हिमाहारो वेगज्ञः पथि सिन्धुरः॥
छायालयो गजो नीलो निर्मलो नास्ति सत्कुले।
रात्रौ दर्पणमभ्राङ्गं नागस्तुङ्गनखो बली॥
धीरो युवा कथालोलः पूज्यो नारीजनैर्भगः।
कन्यागारे नागवल्ली देवो विश्वस्थली भृगुः॥
तत्परादिकलान्तास्तु महाज्या माधवोदिताः।
स्वस्वपूर्वविशुद्धे तु शिष्टास्तत्खण्डमौर्विकाः॥ २.९.५॥

The numbers here are again in the reverse sexagesimal *kaṭapayādi* notation, and give the minutes, seconds, and thirds for the 24 sine values. This passage may be translated and the resulting conversion to present-day notation is given in Table 3.1.

For the sake of comparison, the numbers have also been converted to decimals, and Table 3.2 gives the comparison with sine values. A computer program was used to generate the TEX output for this table, to avoid typing errors. It is clear that a minimum accuracy of as high as 7 places after the decimal point is maintained for *all* values, over the entire quadrant.

Table 3.1: Mādhava's sine table.

No.	Kaṭapayādi	kalā ( )	vikalā( )	tatparā( )
1	श्रेष्ठं नाम वरिष्ठानां	224	50	22
2	हिमाद्रिर्वेदभावनः	448	42	58
3	तपनो भानुसूक्तज्ञो	670	40	16
4	मध्यमं विद्धि दोहनम्	889	45	15
5	धिगाज्यो नाशनं कष्टं	1105	01	39
6	छन्नभोगाशयाम्बिका	1315	34	07
7	म्रिगाहारो नरेशोऽयं	1520	28	35
8	वीरो रणजयोत्सुकः	1718	52	24
9	मूलं विशुद्धं नाळस्य	1909	54	35
10	गानेषु विरळा नराः	2092	46	03
11	अशुद्धिगुप्ता चोरश्रीः	2266	39	50
12	शङ्कुकर्णो नगेश्वरः	2430	51	15
13	तनुजो गर्भजो मित्रं	2584	38	06
14	श्रीमानत्र सुखी सखे	2727	20	52
15	शशी रात्रौ हिमाहारो	2858	22	55
16	वेगज्ञः पथि सिन्धुरः	2977	10	34
17	छायालयो गजो नीलो	3083	13	17
18	निर्मलो नास्ति सत्कुले	3176	03	50
19	रात्रौ दर्पणमभ्राङ्गं	3255	18	22
20	नागस्तुङ्गनखो बली	3320	36	30
21	धीरो युवा कथालोलः	3371	41	29
22	पूज्यो नारीजनैर्भगः	3408	20	11
23	कन्यागारे नागवल्ली	3430	23	11
24	देवो विश्वस्थली भृगुः	3437	44	48

This raises various questions. How was the accurate value of π calculated? How were the power series obtained? Why was such a high level of accuracy required? etc. All these questions are addressed in the sequel.

Although the series expansion is clearly regarded as indefinite, the values of the coefficients, being given only to the nearest third minute, are tied to the assumption of an 11th/12th order polynomial. Hence, to achieve higher accuracy by using a higher-order polynomial, it would also be necessary to recompute the above coefficients to the desired level of accuracy. This was not done, except presumably for demonstration purposes, in the 19th c. CE *Sadratnamālā*, which computed the value of π to 17 decimal places. The limiting accuracy of the third sexagesimal minute is clearly set by the practical concerns of timekeeping, which are taken up in the next chapter, and related planetary models (not considered in detail in this book).

Table 3.2: Accuracy of Mādhava's sine table.

No.	Mādhava's sine value	Difference
1	0.0654031452	0.0000000160
2	0.1305262297	0.0000000375
3	0.1950903240	0.0000000020
4	0.2588190035	−0.0000000416
5	0.3214394797	0.0000000144
6	0.3826834083	−0.0000000241
7	0.4422886665	−0.0000000237
8	0.5000000000	0.0000000000
9	0.5555702346	0.0000000016
10	0.6087614077	−0.0000000213
11	0.6593458183	0.0000000032
12	0.7071068355	0.0000000543
13	0.7518398680	0.0000000605
14	0.7933533335	−0.0000000068
15	0.8314696287	0.0000000164
16	0.8660254521	0.0000000483
17	0.8968727739	0.0000000324
18	0.9238795632	0.0000000307
19	0.9469301920	0.0000000625
20	0.9659258390	0.0000000127
21	0.9807852980	0.0000000176
22	0.9914448967	0.0000000353
23	0.9978589819	0.0000000587
24	1.0000000000	0.0000000000

The context for the calculation of these trigonometric values within texts, such as the *Yuktibhāṣā*, is the calculation of the circumference of a circle *while avoiding the extraction of square roots*. We recollect that some 2000 years prior to the *Yuktibhāṣā*, in ca. −500 CE, the *śulba sūtra*-s had given an accurate value of $\sqrt{2}$, and that procedure inheres in the present-day term "surd" from the Latin term *surdus* (= deaf), which is a Latin translation of the Arabic mistranslation of the Sanskrit term *karaṇī* or *karṇa* (= diagonal), confused by Arabic translators with the other meaning of the word *karṇa* (= ear; hence "bad *karṇa*" = "bad ear" = "deaf"). This method of square-root extraction was used to compute the circumference of the circle, hence the value of π, starting with the octagon obtained by cutting the corners of a square. We recollect that Āryabhaṭa, who first stated a general algorithm for computing square roots, also probably used this octagon-doubling method to compute his value of the ratio today designated by π. The interesting thing about this octagon method is that it is distinct from the hexagon-doubling method widely used, from "Archimedes" to al Kāshī, to compute the circumference of a circle.[25] Historians have also failed to notice that

Āryabhaṭa clearly indicated his preference for numerical finite-difference techniques above these relatively clumsy geometric techniques. So let us look at that background first.

III
ĀRYABHAṬA'S TRIGONOMETRIC VALUES

Terminology, Notation, and the Role of the Historian as a Translator

The historian is perforce a translator, for, to make things comprehensible, he must necessarily translate from one cultural milieu at one time to another at a different time. In particular, to communicate with people at the present time—people who are typically trained in the Western mathematical tradition—it seems best to use present-day terminology. For this reason, in what follows, as in what preceded, we will, without fear of damaging the propositions advanced in Chapters 1 and 2, continue to use the language of present-day mathematics, with the understanding, of course, that the use of the current terms (such as π) is solely for communication, and does not re ect an acceptance of the underlying epistemology, or an implicit endorsement of the underlying history of science.

Area and the Value of π

Today one learns in school that the integral calculus concerns the integrals of "functions", and it is equally elementary that computing (definite) integrals is equivalent to calculating the area enclosed by a plane curve. But what is area? Unlike the case of Hilbert's interpretation of "Euclidean" geometry, which, as we have seen, stumbles on the question of defining area, Proclus' approximate contemporary Āryabhaṭa, in his *Aryabhaṭīya*,[26] defined the area of a general plane figure using triangulation. In the *Gaṇita* section, he first states (6a-b) that "The product of the perpendicular and half the base gives the area of a triangle." He then states (7a-b) that "half the circumference multiplied by half the diameter gives the area of a circle." He next states (8) that the area of a trapezium is obtained by "multiplying half the sum of the base and face by the height." He goes on to state that (9a-b) "for any plane region [find a way to fill it using rectangles or right triangles, and sum (half)] the product of the adjacent sides to obtain [surpass] the area." He then gives

> चतुरधिकं शतमष्टगुणं द्वाषष्टिस्तथा सहस्त्राणाम ।
> अयुतद्वय विश्कम्भस्यासन्नो व्रित्तपरिणाहः ॥ १० ॥

This may be translated:[27]

> (10) 4 more than 100, multiplied by 8, and added to 62,000: this is the approximate [*āsanna*] measure of the circumference of a circle whose diameter is 20,000.

This works out to

$$\pi = \frac{62832}{20000} = 3.1416. \tag{3.14}$$

Al Khwārizmī, in his *Algebra*, reproduces Āryabhaṭa's values in practically the same terminology

> The other method is used by the astronomers among them; it is this, that you multiply the diameter by sixty-two thousand eight hundred and thirty two and then divide the product by twenty thousand; the quotient is the periphery.

And this value is also cited by Stevin.

Surds, Roots, and Other Irrationals

Did Āryabhaṭa understand the "irrational nature of π"? There is a cultural disjuncture here: for, unlike Greek tradition, no special mystical significance was attached to ratios (or the corresponding musical harmonies), and irrational numbers like $\sqrt{2}$ are treated like other numbers from the days of the *śulba sūtra*. Of course, the difference is that a "number" like π could not (and still cannot) be completely specified.

Accordingly, in the *śulba sūtra* (ca. −500 CE), the term used for the value of $\sqrt{2}$ is *sa viśeṣa*[28]—meaning that there remains a small quantity in excess or deficit of the stated value.[29]

> प्रमाणं त्रितीयेन वर्धयेत्तच्च चतुर्थेनात्मचतुस्त्रिंशोनेन ।
> सविशेषः ॥ २.१२ ॥

This may be translated:[30]

> The measure is to be increased by its third and this (third) again by its own foruth less the thirtyfourth part (of that fourth); this is (the value of) the diagonal of a square (whose side is the measure), with something remaining.

That is,

$$\sqrt{2} \approx 1 + \frac{1}{3} + \frac{1}{3.4} - \frac{1}{3.4.34} = 1.4142156. \tag{3.15}$$

We note incidentally that the value is accurate to five places after the decimal point, or six decimal places in all ($\sqrt{2} = 1.4142135\ldots$).

Apastamba *śulba sūtra* 3.2 uses the term *sānitya*, interpreted as *sa anitya* = inexact, or impermanent. The same understanding applies to π, since the *śulbakāra*-s express the value of π using the value of $\sqrt{2}$ as follows.

चतुरश्रं मण्डलं चिकीर्षन मध्यात्कोट्यां निपातयेत ।
पार्श्वतः परिकृष्यातिशयतृतियेन सह मण्डलं परिलिखेत ॥
सानित्या मण्डलम ।
यावद्धीयते तावदागन्तु ॥ ३.२॥

The translation is as follows.

> If it is desired to transform a square into a circle, a cord is stretched from the centre (of the square) up to its corner (so as to measure out a length equal to half the diagonal). It is (then) stretched (from the centre) towards the (eastern) side. With one-third of the excess part (lying outside the eastern side) added (to the portion of the cord between the centre and the side), the (required) circle is drawn. This is the approximate circle for (almost) as much is added as is cut off (from the corners of the square).[31]

Thus, if $2a$ is the side of the square, and r is the radius of the desired circle, this corresponds to the formula

$$\begin{aligned} r &= a + \frac{1}{3}\,(a\sqrt{2} - a) \\ &= \frac{a}{3}(2 + \sqrt{2}). \end{aligned} \tag{3.16}$$

(The problem of squaring the circle arose in the *śulba sūtra* in the context of having altars with equal areas but of different shapes—what would today be called area or measure preserving transformations. This process already presumed the definition of the area of an arbitrary plane region, obtained by filling it with rectangular tiles, as explicitly stated by Āryabhaṭa.) Since the circle with the above radius r was required to have the same area as a square with side $2a$, from $\pi r^2 = 4a^2$ we get

$$\pi\left(\frac{1}{3}(2 + \sqrt{2})\right)^2 = 4. \tag{3.17}$$

corresponding to $\pi = 3.0883 \approx 3.1$.

Early Jain canonical works such as the *Sūrya prajñapati, sūtra* 20) also use the term *kiñcid viśeṣādhika* ("a little excess") in describing the value of π: "the diameter of the circle is 99640 *yojana*-s, the circumference is 315089", corresponding to a value of $\pi = 3.16227$. Likewise, about a thousand years later, Āryabhaṭa uses, for the measure of the circumference, the term *āsanna* (= near, proximate; *Gaṇita* verse 10, above). Almost exactly another thousand years later, Nīlakaṇṭha comments on the use of this term by Āryabhaṭa as follows:[32]

कुतः पुनर्वास्तवीं संख्यामुत्सृज्यासन्नैवेहोक्ता। उच्यते। तस्या वक्तुमशक्यत्वात्। कुतः। येन मानेन मीयमानो व्यासो निरवयवः स्यात्, तेनैव मीयमानः परिधिः पुनः सावयव एव स्यात् येन च मीयमानः परिधिर्निरवयवस्तेनैव मीयमानो व्यासोऽपि सावयव एव, इत्येकेनैव मानेन

मीयमानयोरुभयोः क्वापि न निरवयवत्वं स्यात्। महान्तमध्वानं गत्वाप्यल्पावयवत्वमेव लभ्यम्।
निरवयवत्वं तु क्वापि न लभ्यमिति भावः।

The translation is as follows.

> Why is the real value not given and the proximate [*āsanna* = near] value stated? I will tell. Because it is not possible to express that [the real value]. Why? By any measure [howsoever small] if the diameter is measured without a remainder, by the same measure the circumference [when measured] will leave a remainder most certainly. By any measure if the circumference is measured without remainder the diameter will leave a remainder. Whatever the measure there will always be a remainder. Though we may continue endlessly, we can only achieve smallness of the remainder, but never remainderlessness. That is the sense [of *āsanna*].

Āryabhaṭa s 24 Sine Values: Geometric Method

Āryabhaṭa not only gives the value of π, that value of π is embedded in the course of his derivation of the values of sine and cosines, conventionally done for 24 angles. Thus, he applies the above definition of area to state:

> (9c-d) The chord of one-sixth of the circumference is equal to half the diameter.

He goes on to state:

समवृत्तपरिधिपादं छिन्द्यात् त्रिभुजाच्चतुर्भुजाच्चैव।
समचापज्यार्धानि तु विष्कम्भार्धे यथेष्टानि॥ ११॥

This may be translated:

> (11) Divide the quadrant of a circle into equal parts, and pierce it with triangles and quadrilaterals, to find the corresponding *jyā*-s [Rsines] of equal arcs for any desired radius.

Even by Āryabhaṭa's standards of brevity, this is a very cursory and dismissive description of triangulation, and its cursoriness clearly indicates that the reader is assumed to be already familiar with the process described. As such, sine values must have been in use prior to Āryabhaṭa, and must have been computed using this geometric technique. The dismissive nature of the description suggests that Āryabhaṭa does not himself have a high opinion of this procedure. As we shall see, the *Āryabhaṭīya* emphasizes instead the finite difference technique in *Gaṇita* 12, and records only the sine differences in *Gītikā* 10/12.

The way the geometric process worked is explained with examples by Bhāskara I (and these examples are also worked out in detail by various persons, and, in particular, by Shukla and Sarma in their translation of the *Āryabhaṭīya*). In Bhāskara I's first example, one divides the quadrant of the circle into six equal parts. The corresponding sine values, at intervals of 15°, are today known to every school boy as: 0, $\frac{\sqrt{2-\sqrt{3}}}{2} = \frac{\sqrt{6}-\sqrt{2}}{4}$, $\frac{1}{2}$, $\frac{1}{\sqrt{2}}$, $\frac{\sqrt{3}}{2}$, $\frac{\sqrt{2+\sqrt{3}}}{2} = \frac{\sqrt{6}+\sqrt{2}}{4}$. The calculation nevertheless has certain points of interest: the value of R is explicitly used, and *the square roots are actually evaluated* instead of merely being indicated symbolically, as is done today.

Since the value of R is explicitly used, the question naturally arises: *what* value of R should one use? There is an interesting difference between Indian and Western tradition here. In Western tradition, the radius of the circle was usually taken to be given; the question was one of determining the length of the circumference. First there were the persistent doubts (pointed out in Chapter 1) whether measurement had anything to do with geometry. For practical purposes, of course, lengths had to be measured, and rigid rods were used for this purpose. Western philosophers assumed somewhat thoughtlessly that this was the "universal" or the only "right" way to measure length, so that those doubts about being able to measure length were incorporated in questions about the "rigidity" of the measuring rod. But on this prescription of using rigid rods, only straight lines could be measured, and the West assumed the ideal straight line to be the foundation of geometry. Since, it is obviously hard to measure the length of a circle, using a rod, there were even graver doubts whether the measure of the circumference could at all be expressed in terms of the radius—as we have seen, Descartes asserted that this was beyond the capacity of the human mind (p. 38)! Under the circumstances, it is understandable that the radius (which could be measured with a rigid rod) is assumed to be given, and the formula describes the circumference.

Indian geometric tradition, however, since the days of the *śulba sūtra*, used a flexible rope rather than a rigid measuring rod, so the length of a curved line could manifestly be measured. Therefore, at no stage did Indian tradition entertain the slightest doubt about the ability to measure the length of curved lines. And, it was the length or circumference of the circle that was usually taken as a standard, while it was the radius that was treated as the derived quantity.

From the earliest times, time, hence angles, have been measured sexagesimally (to base 60). Traditionally (since Vedic times), for example, a day has 60 *ghaṭī*-s or *nāḍikā*-s—each of some 24 minutes—instead of some 24 hours each of 60 minutes. Even today, the mathematical convention is that a circle has 360°, and if we take $1° = 60$, then the circle should have a length of $360 \times 60 = 21600$. Hence, the typical value of the standard length of the circle, from Āryabhaṭa onwards, is 21600. The larger figure enabled trigonometric values to be stated with greater precision, accurate to the first sexagesimal minute. There could have been further reasons related to the precision with which the orbit of the moon was to be calculated (the radius of the moon's orbit is expressed by Āryabhaṭa as 3600 × 60 yojana).

At any rate, with the figure of 21600 for the length of a standard circle, and using Āryabhaṭa's value of π, the value of the radius turns out to be $R = 3438$. We can now calculate the sine values.

First, one calculates the value of Rsin 30° (= $\frac{1}{2}$ chord 60°) as stated by verse 9. Verse 9 states that the chord of the sixth part of the circumference is equal to the radius. (In today's terminology, the angle subtended by that chord, at the centre of the circle is one-sixth of the circle, or 60°, so that the corresponding triangle is an equilateral triangle, since its apex angle is 60°, while it is evidently an isosceles triangle, since its two arms are both equal to the radius of the circle.) With the above value of R we can express Rsin 30° = R sin 30° = $\frac{1}{2}R = 1719$.

The derivation of other sine value from this is a straightforward (though tedious) process of applying the rule of three to similar triangles, and the diagonal rule to various "triangulations" to calculate the remaining sine values geometrically, as indicated in verse 11. The value of $R \sin 60°$ can be easily calculated by applying the diagonal rule: $R \sin 60° = \sqrt{R^2 - \left(\frac{R}{2}\right)^2} = \frac{\sqrt{3}}{2} R$. Explicit computation of the square root gives the fourth sine value as $R \sin 60° = 2978$.

Rounding

For a calculation done on a calculator, the exact value of $R \sin 60°$ comes out to be 2977.3953. This raises the very interesting question of exactly how rounding was done, for Āryabhaṭa had a definite algorithm for computing $\sqrt{3}$, which could hence be computed to any desired precision. However, the sine values given by Āryabhaṭa (implicitly through sine differences) are rounded off to the first minute. As we shall see later on, rounding was invariably done, for the sine values are always expressed in a whole number of minutes (or seconds, or thirds), but no simple mechanical rule was followed for rounding since the idea was a more goal-directed one of making a precise and practically useful calculation. (Āryabhaṭa II changes Āryabhaṭa I's sine value to 2977; but the value given by Āryabhaṭa I was surely not a mistake, for it remained unchanged by commentators over many centuries, though these very commentators, like Bhāskara I, naturally rejected many things that Āryabhaṭa I said.)

Square-Root Extraction

Apart from the question of rounding, there is another point worthy of note: the calculation of these sine values requires an actual method of computing square roots. This is hardly a trivial matter.

In this connection, we note that *Gaṇita* verse 3 defines square and cubes, both geometrically and numerically, while verses 4 and 5 explain respectively the method of extracting square roots and cube roots. This is the earliest known statement of an algorithm for extracting square roots—"Ptolemy", for instance, does not have such an algorithm. (Neither does he have sine values, and it is a great mystery how he even calculated his table of chords without such an algorithm for extracting square roots—in the purported text of the *Almagest* square-root extraction is done without comment, in Book I, e.g., $\sqrt{4500} = 67^p 4\ 55$. Perhaps the original Ptolemaic table of chords was a rough table that was updated by later authors, for the present-version of Ptolemy's text is not compatible with non-textual evidence such as the contemporary Roman calendar, which was hopelessly off the mark—just because the difficulties with fractions did not allow the Romans even to *articulate* the right length of the year. It is an even greater mystery why Western historians of mathematics nevertheless keep repeating uncritically that Āryabhaṭa's values are derived from Ptolemy's table of chords!) The fact is that Roman and Greek tradition had an obvious difficulty not only with fractions but also with multiplication and division (as Ptolemy states[33]), prior to the algorismus. It had an even greater difficulty with non-ratio numbers that are bound to arise in the process of square-root extraction. In contrast, as seen above, approximate values of $\sqrt{2}$, $\sqrt{3}$, etc. were known to Indian tradition from as early as the *śulba sūtra*-s (ca. −500 CE).

It is not known precisely how the *śulbakāra*-s obtained the value of $\sqrt{2}$ as precisely as they did, though various speculations have been made.[34] The basic idea in the *śulba sūtra* seems to have been a method of successive approximations as follows. To calculate $\sqrt{A}$, we first find a number a such that $a^2 \approx A$. Then, $\sqrt{A} = \sqrt{a^2 + c} = a + \frac{c}{2a+1} +$. This approximation can be understood using the rule of three (linear interpolation): $2a + 1$ is the difference between a^2 and $(a + 1)^2$. If the addition of $2a + 1$ increases the square root by one, by how much will the addition of c increase it? The addition of or a term such as *sa viśeṣa*, then indicates the understanding that this process is a quick approximation which can be improved. Alternatively, one may try to understand it in present-day algebraic terms, using $(a + b)^2 = a^2 + 2ab + b^2$, with $b = \frac{c}{2a}$. Continuing this process presumably led to the *sulba sūtra* approximation[35] noted earlier.

Howsoever good may be the *śulba sūtra* approximation, a knowledge of *some* technique of numerical approximation is one thing, and a knowledge of an easy and efficient algorithm for extracting square roots to *any* desired precision is another thing. We do not know for sure whether the *śulbakāra*-s had access to such an algorithm.

Āryabhaṭa, however, did have such a general algorithm for square-root extraction which went as follows (*Gaṇita* 4).

भागं हरेदवर्गान्नित्यं द्विगुणेन वर्गमूलेन ।
वर्गाद्वर्गे शुद्धे लब्धं स्थानान्तरे मूलम् ॥ ४ ॥

This may be translated as follows.

> Always divide the *avarga* [number in the even place] by twice the square root [previously obtained]. Then, having subtracted the square from the *varga* [number

in the odd place], transfer the quotient to the next place [to obtain the next digit of the square root]. This is the square root.

(Āryabhaṭa also gives an algorithm for cubing and extracting cube roots, but that does not concern us here.) This method of square-root extraction was probably not a very new method at the time of Āryabhaṭa, for this process of computing sine values using square roots was already being found to be cumbersome, and was in the process of being replaced by a superior technique.

Āryabhaṭa s 24 Sine Values: Finite Difference Method

While the above geometric method of computing trigonometric values can be continued, it soon becomes cumbersome[36] if the quadrant of the circle is divided into a large number of equal parts, such as the 24 parts for which Āryabhaṭa actually gives the sine differences. The sine values themselves are obtained from the tenth[37] *gītikā* of the *daśgītikā* section, which is as follows.

मखि भखि फखि धखि णखि ञखि
ङखि हस्झ स्ककि किष्ग श्घकि किघ्व ।
घ्लकि किग्र हक्य धकि किच
स्ग श्झ ङ्व क्ल प्त फ छ कलार्धज्या ॥ १२ ॥

The numbers involved here are expressed in Āryabhaṭa's novel notation explained in box 3.2.

Box 3.2. Āryabhaṭa s numerical notation

According to this notation, explained in the second verse at the beginning of the *Gītikā* section, the *varga* letters (classified letters, i.e., the letters from *k* to *m*) are to be used in the *varga* (odd) places. They, thus, have the value from 1 to 25 in alphabetical order. The *avarga* (unclassified letters, i.e., the letters *y, r, l, v, ś, ṣ, s, h*) are to be used in the *avarga* (even) places. They, thus, have the values 30, 40, 50, 60, 70, 80, 90, 100, respectively. The nine vowels *a, i, u, ṛ, lṛ, e, o, ai, au* respectively denote the eighteen places (lit. two nines of zeros) corresponding to 10^0 to 10^{17}, with each vowel occupying one *varga* and one *avarga* place: thus *a* denotes the place of 1 as well as 10, *i* denotes the place of 100 as well as 1000, etc. A consonant combined with a vowel denotes a number. When the vowel is combined with an *avarga* letter, it has a value 10 times what it has when combined with a *varga* letter. (The names for various powers of 10 given by Āryabhaṭa in *Gaṇita* 2, are a bit different from those given for the first 12 powers of 10 in the Yajurveda xvii.2, and also a bit different from those names in current use today, except up to *koṭi* = crore. Thus, for example, the present-day unit "arab" is 100 crores, whereas for Āryabhaṭa *arbudam* is only 10 *koṭi* [= 10 crores], while for commentators on the Yajurveda an *arbuda* is what we today call a crore.)

Thus, when combined with a vowel, a consonant acquires the place(s) of that vowel. (For the purposes of this rule, it is immaterial whether we use the short forms *a, i, u, ṛ*, or the longer forms *ā, ī, ū, ṝ*.) The system is very compact: thus *khyughṛ* = 4,320,000, since *kh* = 2, *y* = 30 , so that *khyu* = 320,000, while *gh* = 4, so that *ghṛ* = 4,000,000. It is also order-independent: thus *dhaki* = *kidh*. Despite its many virtues, the problem with this system is that the resulting words are often difficult to pronounce, e.g. *ṅiśibuṇlṛṣkhṛ* (= 1,582,237,500 = number of rotations of the earth in a *yuga*). Moreover, unlike the *bhūta saṅkhyā* system, the words need not be natural words which mean something. As such, they are difficult to recollect, and cannot be checked against meaning, which is what makes a mnemonic easy to remember. Further, it is difficult to use such number-words with the proper meter in a verse, since not much variation is possible.

In view of Āryabhaṭa's compact numerical notation, one might ask why his value of π was expressed in a prolix way ("4 more than 100 multiplied by 8 and 62 times 1000 . . ."). Perhaps he was simply restating a traditional *śloka*. More likely, this way of stating things enabled a play on words, allowing him to state indirectly that what he had done was extra clever. (The word *catur*, apart from meaning the number 4, also connotes cleverness by reference to one who has learnt all four Veda-s. "Clever" is the primary meaning of the word in derivative languages like Hindi.) Unfortunately, later interpreters seem to have lacked the sense of humour needed to appreciate this.

Thus, the verse may be translated:

> 225, 224, 222, 219, 215, 210, 205, 199, 191, 183, 174, 164, 154, 143, 131, 119, 106, 93, 79, 65, 51, 37, 22, 7—[these are the] Rsine differences [for the quadrant divided into as many equal parts, each part hence being 225] [in] minutes.

It is noteworthy that the above numbers give the 24 sine *differences* rather than sine values *per se*. (The first difference is taken to be equal to its value, since this is implicitly the difference from sin 0 which is obviously zero.) Combined with the cursory treatment of the geometric method of obtaining sines ("use triangles and quadrilaterals") this strongly suggests that the geometric method of computing sines was well known prior to Āryabhaṭa, and that he favours a change to the finite difference technique. It is strange that the significance of this point has gone largely unnoticed by earlier historians.

Calculating Sine Differences

How were the sine differences derived? In fact, Āryabhaṭa goes a step further, and gives a rule which can also be simultaneously used to derive the sine differences themselves.

प्रथमाच्चापज्यार्धाद्यैरूनं खरिडतं द्वितियार्धम् ।
तत्प्रथमज्यार्धांशैस्तैस्तैरूनानि शेषारिा ॥ १२ ॥

This rule, stated in *Gaṇita* verse 12, treats sine differences in a way which becomes the key to the origin of later infinitesimal techniques.

> (12) The Rsine of the first arc divided by itself and diminished gives the second Rsine difference. That same first Rsine, when it divides successive Rsines gives the remaining [Rsine differences].[38]

That is, if the quadrant of the circle is divided into, say, 24 equal parts, $R_1, R_2, \ldots, R_{24}$ are the 24 corresponding sine values, $_1\ (= R_1),\ _2, \ldots,\ _{24}$, are the corresponding sine differences, and $_i = R_i - R_{i-1}$, for $i \quad 2$, then Āryabhaṭa's rule consists of the following two parts:

$$_2 - {}_1 = -\frac{R_1}{R_1}, \tag{3.18}$$

$$_{n+1} - {}_n = -\frac{R_n}{R_1}. \tag{3.19}$$

Three key points are worth noticing here. The first is that Āryabhaṭa has here brought in the *second* difference. Today, we would rewrite the formula as

$$^{(2)}_n \qquad _{n+1} - {}_n = -\frac{R_n}{R_1}, \tag{3.20}$$

corresponding to the idea that that second difference/derivative of the sine is proportional to the sine itself. But with finite differences a little more detail is necessary, and Āryabhaṭa also specifies the constant of proportionality.

The above interpretation is also the one given by Nīlakaṇṭha in his *Āryabhaṭīyabhāṣya*, except that Nīlakaṇṭha makes it more precise, by stating it in the form

$$^{(2)}_n = - \frac{R_n}{R_1} ({}_1 - {}_2). \tag{3.21}$$

The difference here is that for Āryabhaṭa, working to the precision of minutes, $_1 - {}_2 = 225 - 224 = 1$, while this is no longer the case with Nīlakaṇṭha, working to the precision of thirds, who uses the earlier stated values, $R_1 = [224; 50; 22]$ and $R_2 = [448; 42; 58]$, so that $_2 = [223; 52; 36]$, and $_1 - {}_2 = [0; 57; 46]$.

There is a suggestion, in the above verse, *Gaṇita* 12, of a play on words, leading to two possible interpretations, both of which are correct. Thus, it is possible to interpret the above formula also as relating solely to first differences,

$$_{n+1} = R_1 - \frac{R_1 + R_2 + \cdots + R_n}{R_1}, \tag{3.22}$$

as has been done by Shukla and Sarma, based on the interpretation of a variety of earlier commentators, such as Prabhākara, Sūryadeva (b. 1191 CE), Yallaya (1480 CE), etc. It is even possible to interpret it as

$$_{n+1} = {}_{n} - \frac{_1 + {}_2 + \cdots + {}_n}{R_1}, \tag{3.23}$$

as has been done by Someśvara and Parameśvara. These different interpretations, however, give equivalent mathematical formulae, in the sense that we arrive at the same numerical values, whether we use first or second differences for the purpose of calculation.

My point is that it is quite possible that *both* interpretations are intended, and that the term "second sine difference" refers ambiguously, as in the English-language translation above, to either the quantity $_2$, or the quantity $^{(2)}_1$. In fact, if we further think of the term "unaṁ" as "negated" (in addition to its meaning as "diminished"), this provides a very neat and clear interpretation of the verse. The difficulty in interpreting this verse is thus perhaps because it has sought to incorporate an extraordinary level of cleverness.

This author is not the first in modern times to have translated *Gaṇita* 12 as involving the second difference. Delambre[39] long ago made the same observation, although he could not reconcile this observation with the local historical narrative within which he situated himself and the Indians. Accordingly, he falls back on that stock Western re ex: Āryabhaṭa might have noticed the relationship as a "mere empirical fact" (but was incapable of proving it; we have already seen the futility of this argument in Chapter 2).

Whether or not second sine differences were explicitly used by Āryabhaṭa is *not* a matter of very great consequence, from a historical perspective, since second differences were certainly used for interpolation, from Brahmagupta onwards, about a century later, and this use was continued subsequently by Vaṭeśvara, and Bhāskara II, as detailed below.

The formula (3.21) can be justified both algebraically and geometrically.[40] Algebraically, for example, Shukla provides the following elementary derivation:

$$\begin{aligned}
{}_n - {}_{n+1} &= R \sin nh - R \sin (n-1)h \\
&\quad - R \sin (n+1)h - R \sin nh \\
&= 2R \sin nh - R \sin (n+1)h - R \sin (n-1)h \\
&= 2R \sin nh - \frac{2R \sin nh \cdot R \cos h}{R} \\
&= 2R \sin nh \cdot \frac{R - R \cos h}{R} \\
&= 2R_n \cdot \frac{R - R \cos h}{R} .
\end{aligned} \tag{3.24}$$

Applying the last equation above, for the case $n = 1$, we see that

$$_1 - {}_2 = 2R_1 \cdot \frac{R - R\cos h}{R} \quad , \tag{3.25}$$

so that, (3.24) can be rewritten as

$$_n - {}_{n+1} = R_n \cdot \frac{_1 - {}_2}{R_1}, \tag{3.26}$$

which is the same as the earlier formula.

Computing Sine Values by the Finite Difference Method

The second key point about *Gaṇita* 12, and one which seems to have gone un-noticed so far is this: though Āryabhaṭa's formula admits some sort of an algebraic or geometric derivation, as has been suggested by Shukla, the equation cannot correctly be regarded as an algebraic equation. More precisely, it can be regarded as an algebraic equation for calculating the second difference, but not for its proper purpose, which is to calculate sine values. In view of the preceding algebraic derivation, this might seem a bit paradoxical, so let us take an example to illustrate what is meant. Thus, if we know R_n, we can calculate the second difference using (3.19); however, if we try to calculate R_n by multiplying (3.19) by R_1 to obtain $R_1 \times ({}_n - {}_{n+1})$, that would result in incorrect values, at least so far as Āryabhaṭa is concerned. For example, for $n = 23$, $_{23} = 22$, $_{24} = 7$, while $R_1 = 225$, so that we should have $R_{23} = ({}_{23} - {}_{24}) \times R_1 = 15 \times 225 = 3735$ while the 23rd sine value actually given in the *Sūrya Siddhānta* (or by Āryabhaṭa) is 3431, which is quite substantially different. In fact there will be a difference in *every* case, since none of the actual sine values is an integral multiple of 225. (Rounding is not involved here, for the difference involved is much too large.) Had the rule been intended for use as an algebraic equation, he would have, in *Gitika* 10, stated the sine differences as fractions, *without* rounding them. Since the differences are actually stated as whole numbers (of minutes), it follows that for the purpose of calculating sine values, the equation is intended as a special sort of equation: a finite difference equation.

Thus, Āryabhaṭa's rule for calculating sine values is the same thing as the recursive process ($n \quad 2$):

$$R_n = R_{n-1} + {}_n \tag{3.27}$$

$$_n = {}_{n-1} - \frac{R_{n-1}}{R_1} \tag{3.28}$$

with $R_0 = {}_0 = 0$ and $R_1 = {}_1 = \text{arc} = 225$, when 24 values are desired. Today we would immediately recognize the striking similarity of this with "Euler's method" of solving an ordinary differential equation, using finite differences, and this is discussed in more detail below.

Let us first see, with some examples, how this method of finite differences actually works, in the case of Āryabhaṭa. By convention, the standard circle is taken to be one which has a circumference of $3600 \times 60 = 21,600$, so that a quadrant of the circle has a length of 5400. As the first step in this calculation, we need the value of the radius of the circle. Using Āryabhaṭa's value of π, we can calculate the radius of this standard circle as $R = 3438$ (rounded to the minute).

To compute the 24 sine values, we divide the quadrant into 24 equal parts, so that each part corresponds to $\frac{5400'}{24} = 225$ or equivalently $\frac{90°}{24} = 3°45$. The first Rsine difference (and value) is taken equal to the arc. This, in fact, is the reason for choosing the number to be 24. As the commentator Sūryadeva Yajvan[41] explains:

> Now why should there be a rule that the number of *jyā*-s should be restricted to 24, when the quadrant of a circle can be divided into any number of parts?... the quadrant should be divided in such a manner that the first *jyā* and the corresponding arc are exactly equal. This is the case when the number of parts of the quadrant is 24.

Thus, $R_1 = 225$ corresponding to sin $3°45 = \frac{225}{3438} = 0.06544$, accurate to 4 places after the decimal point.

There are now two ways to proceed. The first way is to compute the values by using *Gītikā* 10. This is a straightforward matter of successive addition. Thus $R_2 = 225 + 224 = 249$, etc. This is so easy that it deserves no further comment.

It is more interesting, however, to examine the method of *Gaṇita* 12, which also permits one to simultaneously derive the sine differences themselves, as one goes along (and it noteworthy that this is the only method Āryabhaṭa has indicated of deriving those sine differences).

Accordingly, by Āryabhaṭa's formula for sine differences,

$$_2 = R_1 - \frac{R_1}{R_1} = 225 - 1 = 224\,. \tag{3.29}$$

By definition, $R_2 = R_1 + {}_2$, so that

$$R_2 = 449\,. \tag{3.30}$$

Similarly, ${}_3 = {}_2 - \frac{R_2}{R_1} = 224 - 449 \ 225 = 222$, so that $R_3 = R_2 + {}_3 = 671$. Further, ${}_4 = {}_3 - \frac{R_3}{R_1} = 219$, and $R_4 = 890$.

The calculation can obviously be continued. The resulting trigonometric values are also found in e.g. the *Sūrya Siddhānta*,[42] dated to a couple of centuries before Āryabhaṭa, but probably updated after him.

Rounding Again

A notable feature of the above calculation is the systematic (though implicit) way in which insignificant quantities are discarded or "zeroed", through rounding. The "general" rule for rounding was rounding to the nearest integer, so that a quantity greater than $\frac{1}{2}$ was rounded up to the next higher figure. But we have already seen an exception to this rule. In fact, as in Pāṇinī's grammar (or in the way traffic rules are still observed in smaller towns in India today) there are a very large number of exceptions! We find in Ranganatha[43] a comment about how to round the 24 sine values in exceptional cases which call for a departure from the rule.

> In the 21st, 20th, 6th, 15th, 7th, 12th, & 17th there is a difference ....

That is about 30% cases in which there is an exception!

In any case, it is clear that there was no *mechanical* rule in use for rounding, and that rounding as appropriate to ultimately greater precision was used. Thus, the numbers used in Indian mathematics, though very similar to oating point numbers, did not correspond *exactly* to any specific type of oating point numbers actually being used today, all of which involve mechanical rules for rounding.

There is a fundamental philosophical difference here, for it seems unlikely (and I believe it to be impossible) that one can at all reduce a purposive procedure to a mechanical (or causal) rule needed for routine numerical computing on a digital computer. To bring out this subtle philosophical difference between a purposive procedure, and a mechanical one, one can ask the question: would it be possible to design an expert system or an artificially intelligent computer which could mechanically reproduce such a purposive approach? This question is interesting because, as we have seen, Hilbert's vision of mathematics is so profoundly mechanical. This is too big a question to discuss here; however, I can summarize an answer that I have provided elsewhere:[44] a truly purposive procedure cannot, in principle, be mechanized. Thus, though Indian mathematics was computational, given these scarcely noticeable features, it may well be that there is a very fundamental philosophical difference between computation in Indian mathematics, and present-day rule-based computational mathematics.

The subtle difference may perhaps be more easily explained, in a non-technical way, by means of an analogy, readily comprehensible to those familiar with the difference between Indian and Western music. In Western music, the phenomenon known as the "Pythagorean comma" creates a problem analogous to the problem of rounding: starting from a given note, if one ascends 12 times by perfect fifths, then this is not the same as ascending by 7 octaves. (Alternatively, if one builds a scale of 12 notes by raising each note to a perfect fifth, and then reducing these 12 notes to the primary octave, then the 12th note in this scale will not be a perfect octave of the base note, so that these twelve notes will not form a perfect cycle—the musical cosmos fails to be exactly recurrent!) The failure of the musical cosmos to be recurrent is a catastrophe from the Pythagorean viewpoint. Since the perfect fifth (on the Pythagorean scale) is understood to have a frequency in the ratio of 3 2 to the frequency of the base note, and since an octave has double the frequency of the base note, the difference amounts to the ratio $\frac{(3/2)^{12}}{(2/1)^7} = \frac{531441}{524288} \approx 1.0136432$, which differs very slightly from 1. However, Western music presupposes that 12 perfect fifths are *exactly* equal to 7 octaves. No easy *mechanical* rule is available to settle the problem of the "Pythagorean comma". However, in the West, the common instruments for music, like the piano, are mechanical, in the sense that they are given, and not open to tuning by the player. Therefore, a mechanical rule was thought desirable. Hence, the actual solution, that is today in use, is called the equal-tempered scale, which attens each note by a small amount. (The notes on the equal tempered scale are obtained by ascending by $\sqrt[12]{2}$, so that the 12 notes fit into a perfect octave.) While this solves the problem of the Pythagorean comma, and also standardizes all instruments, it also has the disadvantage that it makes *every* note in Western music very slightly off key. Though scarcely noticeable except to a musically trained ear, this is a very unsatisfying consequence of marrying a mechanistic philosophy to something like music which seems intrinsically non-mechanical. With traditional Indian musical instruments, however, even a "fixed-pitch" instrument like a ute is so designed as to admit of substantial human adjustment during play. (Also, there is no compulsion to follow a pre-prepared musical score, which might have been composed by another person, using a different instrument.) Tonal problems, therefore, are left to be resolved by the expert player in real time without the need to degrade, even if ever so slightly, the quality of the music as a whole.

Finite Differences vs Square Roots

To return to the calculation of trigonometric values, it is evident that the numerical method is shockingly easy compared to the geometrical method using triangles and square-root extraction. Using the stored table of differences, which are themselves small numbers, only simple addition is required. Even if the differences themselves are to be computed, the multiplication in (3.28) involves relatively small numbers, and absolutely no square-root extraction is necessary. (Somehow this point seems to have been overlooked by more recent commentators on Āryabhaṭa.) These differences become all the more important when we take into account the rounding that must necessarily accompany actual square-root extraction in the geometrical method.

The geometric method, apart from being quite cumbersome when a large number of sine values are involved, especially when square roots have actually to be extracted (and not merely indicated symbolically), has a further disadvantage: the geometric method enables the computation of sine values only at a discrete set of points. This is the third key point to observe regarding the *Gaṇita* 12 rule: it facilitates interpolation.

Interpolation

This author is not aware of anyone who has commented on *why* Āryabhaṭa chose to give a table of *differences*, instead of a table of sine *values*. Presumably, this was done because Āryabhaṭa had observed that the tabulation of differences leads to the computation of sine values with greater economy. Thus storing a table of differences is more efficient, for differences are what are directly required by the interpolation procedure: from the sine differences one can directly compute the values for *any* desired arc, and not merely the 24 values. As stated in the preceding paragraph, this is another key difference from the geometric method: what one obtains with Āryabhaṭa's method are not just 24 values, as would have been obtained on the geometric method, but values for *any* desired angle. With the geometric method, the concept of a sine *function* is only implicit. With the computational method it becomes explicit, for there is a way to compute the value of the function at any point.

That is to say, Āryabhaṭa's notion of the sine function is exactly the notion one has today of a function in numerical computing: *a stored table of values together with an interpolation procedure.* From the computational point of view, as noted above, in the absence of such a technique for computing the values of the function, the notion of "the value of the function at a point" remains something of an impractical idealism.

This notion of function is not the set-theoretic formal definition, $f : R \to R$, used in present-day mathematics. Though the set-theoretic definition of a function involves various supertasks, and is not intended to be useful for any practical purpose, since it belongs to the domain of mathematics-as-proof, it is nevertheless regarded as somehow "superior" to the practical and computational concept of a function in mathematics-as-calculation. This, as we have already seen, is mere cultural prejudice.

The interpolation rule used in Āryabhaṭa's time is simple linear interpolation, between a set of equally spaced values, which corresponds to an application of the rule of three. In modern-day notation, if we divide the quadrant into N equal parts, and set $h = \frac{1}{2}\frac{R}{N}$, then, using $R_n = \text{Rsin}\, nh$, $n = 0, 1, 2, \ldots, N$, the definition $\ _n = R_n - R_{n-1}$, $n = 1, 2, \ldots, N$, amounts to

$$\ _n = \text{Rsin}\, nh - \text{Rsin}\,(n-1)h, \qquad (3.31)$$

and the customary formula for piecewise linear interpolation, as given by the rule of three, amounts to

$$\text{Rsin}\,(n + \ )h = \text{Rsin}\, nh + \ \ _n, \qquad 0 \quad\ < 1, \qquad n = 1, 2, \ldots, N. \qquad (3.32)$$

Thus, the unit change in the sine value (at the point n) is $\ _n$, so that the change in the sine value for the fraction $\ $ would be $\ \ _n$. A change of notation, putting $\ h = \theta$, and a slight rearrangement, allows us to rewrite the above formula in the form

$$\text{Rsin}\,(nh + \theta) = \text{Rsin}\, nh + \theta\, \frac{\ _n}{h}, \qquad 0 \quad \theta < h, \qquad n = 1, 2, \ldots, N. \qquad (3.33)$$

From the computational mathematics point of view, the difference quotient, $\frac{n}{h}$, that enters into the above rule for piecewise linear interpolation, is exactly the counterpart of the formal first derivative, and the interpolation formula is then the exact counterpart of "Taylor's" formula to the first order. It is noteworthy that in Europe it is the discrete version of the formula that appears first—in the correspondence of Gregory, as communicated to Newton through Collins.[45] We will see below how this was extended to quadratic, and then higher order interpolation in Indian tradition. The (generalized) formula has been called the Gregory–Newton interpolation formula.

Finite Differences vs Derivatives

Though in later times, because of the epistemological struggle in which it was involved, the calculus somehow got identified with the use of derivatives as limits, these limiting methods are not essential to the calculus as used even in present-day computation. Finite differences suffice for all practical computation. They are, practically speaking, also necessary for all but the simplest computations.

Secondly, the interpolation procedure links naturally to the recursive method of numerically calculating the values of the function.

That is, given the initial datum $\sin 0 = 0$, it is, of course, possible to derive the sine values proper, from a knowledge of sine differences, as in (3.28). Simply changing $0 \quad \theta < h$ to $0 \quad \theta \quad h$ extends the method of interpolation to a method which uses this technique to derive the Rsine values proper. (This suggests how Āryabhaṭa might have arrived at his method of difference equations.)

Translated in terms of present-day formalist techniques, this would correspond quite exactly to what is today known as Euler's method of solving ordinary differential equations. As pointed out earlier, such an interpretation is necessary, since Āryabhaṭa's rule simply does not make sense as purely an algebraic equation, especially when seen together with the table of differences he gives.

Euler's method is usually presented as follows. Given a differential equation $\frac{dy}{dx} = f(x, y)$, and an "initial" (or "final" or "intermediate") value $y(x_0) = y_0$, one uses the analogue of piecewise linear interpolation to calculate $y_1 = y_0 + (x_1 - x_0)f(x_0, y_0)$. From this value of $y(x_1) = y_1$, one proceeds to calculate y_2, etc. using $y_n = y_{n-1} + \quad x_n\, f(x_{n-1}, y_{n-1})$.

In the present context, things are different in three ways. First, there is no explicit dependence on x, so we are considering only the case of a simpler equation which would today be written in the form $d^2y \; dx^2 = -y(x)$. Second, we are, of course, already using here the finite difference in place of the derivative: $\quad y_n = f(y(x_n)) \quad x_n$, but there is no need for any intermediary in a "background" notion of a "continuous" derivative. (The word "continuous" is here used only in opposition to discrete, but those who are worried about Lipschitz conditions, etc., are referred to Chapter 10.) Third, using the idea earlier explained of a function as a table of values *plus* a method of interpolation, we are here specifying the derivative function using a table of values, $y_n = hf(y(x_n))$ for the differences. There is a fourth difference which is not so important: we are tabulating the values only on a mesh of *equal* intervals, as is commonly done, even today. The value of y_n is now built up from the value of $y_0 = 0$, and the values of y_n as already explained above: $y_n = y_{n-1} + h \quad y_n$.

The similarity with the Euler solver may not be obvious, since part of the beauty of Āryabhaṭa's formula is that it eliminates the explicit dependence on step size. So let us quickly see how it works. First, the second-order equation $y \quad = -y$ is converted to two first-order equations, by the well-known process: $y_1 = y_2$, and $y_2 = -y_1$. Euler's formulae are, then, $y_1(x_{n+1}) = y_1(x_n) + hy_2(x_n)$, and $y_2(x_{n+1}) = y_2(x_n) - hy_1(x_n)$. Replacing the derivative $y_2(x_n)$ by the finite difference, $y_1(x_n) \quad h$, cancelling h from both sides and converting to the earlier notation, we see that the first equation is just the same as $R_{n+1} = R_n + {}_{n+1}$, while the second equation gives ${}_{n+1} = {}_{n} - h^2 R_n$. Using this equation for $n = 1$, we can eliminate h^2 by using $h^2 = ({}_2 - {}_1) \quad R_1$, to obtain ${}_{n+1} = {}_{n} - (R_n \quad R_1)({}_2 - {}_1)$, which is Nīlakaṇṭha's form of Āryabhaṭa's formula, except that Āryabhaṭa has ${}_2 - {}_1 = 1$, accurate to the precision to which he works.

It should *not* be presumed that Euler arrived at his technique of solving differential equations independently of Āryabhaṭa, since Euler not only had access to Indian sources, but wrote an article around 1740 on how Indian astronomy texts used the sidereal year.[46] He also diligently followed up the work done by Fermat, who, as we shall see, was greatly interested in "ancient knowledge". As we shall also see, in a later chapter, there is strong circumstantial evidence that links Fermat (and his famous challenge problem) to a solved example in Bhāskara II, who, as pointed out below, makes some interesting observations on the use of second differences for quadratic interpolation. As we shall also see below, this is systematically extended to higher-order interpolation in the *Kriyākramakarī*, a Sanskrit commentary on the work of Bhāskara II, which gives a detailed exposition of the rationale, in places more detailed than the *Yuktibhāṣā*. Therefore, there is every possibility that the "Euler solver" was developed after a thoroughgoing study of the Indian procedures of computation.

Second Differences and Quadratic Interpolation

As regards the further development of the method, the use of the second difference is greatly furthered 130 years after Āryabhaṭa by Brahmagupta (629 CE), who was presumably dissatisfied with the accuracy of the method of piecewise linear interpolation, when the step sizes are large ($h = 15°$ or 900 apart). Brahmagupta improved the interpolation technique, using the second difference to enable greater numerical accuracy through quadratic interpolation, thus strengthening the foundations of the calculus. (This quadratic interpolation corresponded to using a second-order "Taylor" polynomial.)

He used second-order differences to propose a second-order interpolation formula, nowadays called "Stirling's formula". Brahmagupta's formula for quadratic interpolation is stated as follows.[47]

गतभोग्यखण्डकान्तरदलविकल वधाच्छतैर्नवभिराप्त्या ।
तद्युतिदलं युतोनं भोग्यादूनाधिकं भोग्यम्॥ ४ ॥

This has been translated (using Bhaṭṭotpala's 10th c. CE [Saka 888] commentary) as:

> Multiply the *Vikalā* by half the difference of the *Gatakhaṇḍa* and the *Bhogyakhaṇḍa* and divide the product by 900. Add the results to half the sum of the *Gatakhaṇḍa* and the *Bhogyakhaṇḍa*, if their half sum is less than the *Bhogyakhaṇḍa*; subtract, if greater. [The result in each case is the *Sphuṭabhogyakhaṇḍa* or correct "tabular" difference.]

Here, the underlying table is that calculated for *khaṇḍajyā*-s or sine differences for intervals that are spaced h apart, where it is assumed that $h = 15°$ or 900 . The *gatakhaṇḍa* or "past difference" (= $_n$) refers to the interval that has been crossed, and the *vikalā* (= θ) is the amount in minutes by which it has been crossed at the point at which we want to interpolate. The *bhogyakhaṇḍa* (= $_{n+1}$) is the one yet to come. Thus, the formula states:

$$\text{sphuṭabhogyakhaṇḍa} = \frac{_n + \ _{n+1}}{2} \quad \frac{\theta}{h} \quad \frac{_n - \ _{n+1}}{2} \tag{3.34}$$

$$\text{Rsin}\,(nh + \theta) - \text{Rsin}\,nh = \frac{\theta}{h} \times \text{sphuṭabhogyakhaṇḍa}. \tag{3.35}$$

This amounts to

$$\text{Rsin}\,(nh + \theta) = \text{Rsin}\,nh + \frac{\theta}{h} \quad \frac{_n + \ _{n+1}}{2} \quad \frac{\theta^2}{h^2} \quad \frac{_n - \ _{n+1}}{2}. \tag{3.36}$$

This formula is nowadays called Stirling's interpolation formula: just as linear interpolation leads to an Euler solver, so also quadratic interpolation easily extends to a second-order (Runge–Kutta) method of numerically solving an ordinary differential equation. (Indian tradition, of course, did not recognize differential equations, but it worked directly with difference equations from the time of Āryabhaṭa: this is still the way most differential equations are actually solved today, even though present-day mathematics pretends that differential equations are somehow superior to difference equations.)

Just as a Runge–Kutta method can take much larger steps than an Euler solver, while retaining the same level of accuracy, the higher accuracy of quadratic interpolation enabled Brahmagupta to work with values 900 apart.

But Vaṭeśvara (in 904 CE) works with arcs that are only 56 15 apart, and still uses quadratic interpolation, explicitly giving the second of the above formulae, among many others.[48]

विकलाच्चापाप्तदलं ज्यान्तरहतमृणधनं गुणे भुक्ते।
तद्धनुषाप्तं हीनं युक्तं विकलाहतेन विकलज्या॥ ६४॥
अगतातीतज्यान्तरदलं विकलहतं स्वधनुषाप्तयुतम्।
ज्यान्तरदलं तदूनो युक्तो भुक्तो गुणो भोज्यम्॥ ६५॥
धनुषाप्त-भुक्तजीवाघाते लब्धं सरूपकं दलितम्।
लब्धघ्नविवरहतं [च] संशोध्य नियोज्य विक[ल]ज्या॥ ६६॥

The translation goes as follows.[49]

> (II.1.64) Multiply one-half of what is obtained on dividing the residual arc (*vikalā* [= θ]) by the elemental arc (*cāpa* [= h]) by the difference between the (traversed and untraversed) Rsine-differences (*jyāntara*), and subtract that from or add that to the traversed Rsine-difference (*bhuktaguṇa*). That difference or sum divided by the residual arc (*vikalā*) gives the residual Rsine-difference (i.e. the Rsine-difference corresponding to the residual arc, *vikalajyā*).
>
> 65. Multiply half the difference between the traversed (*atīta*/past) and untraversed (*agata*/future) Rsine-differences (*agatātitajyāntaradala*) by the residual arc (*vikala*) and divide by the elemental arc (*dhanuṣa* or *cāpa*). Add that to half the difference between the (traversed and untraversed) Rsine-differences (*jyāntaradala*). Subtract that from or add that to the traversed Rsine-difference (bhuktaguṇa). Then is obtained the (instantaneous) Rsine-difference (*bhojya-guṇa*).
>
> 66. Add 1 to the *labdha* (i.e., to the result obtained on dividing the residual arc by the elemental arc), reduce it to half, and then multiply that by the product of the *labdha* and the *vivara* (*jyāntar*), i.e. the difference between the traversed and untraversed Rsine-differences). Subtract that from or add that to the product of the *labdha* (*dhanuṣāpta*) and the traversed Rsine-difference (*bhuktajyā*). Then is obtained the residual Rsine-difference (*vikalajyā*).

Here, the *cāpa* or *dhanuṣ* is the elemental arc (= h) which is 56 15 in Vaṭeśvara's case. The traversed (*atīta*=past) sine difference is $_{n-1}$ = Rsin nh − Rsin $(n-1)h$. The untraversed (*agata*= non-gone = future) sine difference is $_n$ = Rsin $(n+1)h$ − Rsin nh. The formula then states

$$\text{Rsin}\,(nh+\theta) - \text{Rsin}\,nh = \frac{\theta}{h}\ {}_{n-1}\quad \frac{\theta}{2h}\quad {}_{n} - {}_{n-1}\ , \tag{3.37}$$

with the positive or negative sign being chosen according to the order in which the sines are traversed. The above may be rewritten as

$$\text{Rsin}\,(nh+\theta) = \text{Rsin}\,nh + \frac{\theta}{h}\,{}_{n} - \frac{\theta}{h}\ \frac{\theta}{h}+1\ \ \frac{{}_{n} - {}_{n-1}}{2}. \tag{3.38}$$

As Shukla remarks, the usual interpolation formulae may be seen either as what is today called the Newton–Gauss forward difference formula, or as the Newton–Gauss backward difference formula. One of the variations of Vaṭeśvara's formula corresponds to a robust backward-differentiation technique of interpolation very useful also for the numerical solution of numerically stiff ordinary differential equations. It is quite clear that this quadratic interpolation is being used in the interests of greater accuracy, and that the precision of Āryabhaṭa's sine values is no longer satisfactory. Thus, Vaṭeśvara also expressed his dissatisfaction with the starting point of the procedure, with Brahmagupta having taken the Rsine of the 24th part of the quadrant (3° 45) as equal to the corresponding arc. He himself divided the quadrant into 96 equal parts, each equal to 56 15″, and stated that the 96th part of the quadrant was indeed as straight as a rod. (In modern terminology, one would say that for this small value of θ, $\sin\theta \approx \theta$, corresponding to the more formal statement that $\lim_{\ \ 0} \sin\theta \ \ \theta = 1$.)

Again, in the interests of greater accuracy, Vaṭeśvara also took the value of the radius R as 3437 44 (*nagaguṇavedāgnayo vedakṛtā*), instead of the value 3438 used in the above example, and, since the circumference in the quadrant is still taken to be 5400 , this corresponds to using a more accurate value of π. The upshot is that, compared to Āryabhaṭa's sine values that are accurate to the first sexagesimal minute, Vaṭeśvara's sine values are accurate to the *second* sexagesimal minute (while Mādhava's values are accurate to the *third* sexagesimal minute).

Instantaneous Velocity and Bhāskara II s Justi cation of Quadratic Interpolation

Bhāskara II's rationale for the above quadratic interpolation formula is interesting. The argument goes as follows. The Rsine-difference for the traversed elemental arc is ${}_{n-1}$, while the Rsine-difference for the untraversed elemental arc is ${}_{n}$. So the increase or decrease of the Rsine-difference is ${}_{n} - {}_{n-1}$. This increase takes place over an arc of length $2h$, which corresponds to $\frac{1}{2}(\ {}_{n} - {}_{n-1})$ for an arc of length h by a simple application of the rule of three. Now the Rsine-difference at the beginning of the arc is ${}_{n-1}$, and the increase or decrease of the Rsine-difference for an arc of length h is $\frac{1}{2}(\ {}_{n} - {}_{n-1})$; therefore, the Rsine-difference for an arc of length θ is ${}_{n-1} + \frac{}{h}\,\frac{1}{2}\left(\ {}_{n} - {}_{n-1}\right)$ by another application of the rule of three.

Hence,

$$\begin{aligned} \text{Rsin}\,(nh+\theta) &= \text{Rsin}\,nh + \frac{\theta}{h} \times \text{(instantaneous Rsine-difference)} \\ &= \text{Rsin}\,nh + \frac{\theta}{h}\,{}_{n} - \frac{\theta}{h}\;\;\frac{\theta}{h}+1\;\;\frac{{}_{n} - {}_{n-1}}{2}. \end{aligned} \qquad (3.39)$$

Bhāskara II offers a similar justification for the other formula stated by Vaṭeśvara. In this case the argument is that the past sine-difference being ${}_{n}$, and the future sine-difference being ${}_{n-1}$, the present or instantaneous sine-difference can be taken as the mean value $\frac{1}{2}(\ {}_{n} + {}_{n-1})$.

Bhāskara is here using the concept of *tātkālika bhogya khaṇḍa* or instantaneous sine-difference. This same notion is used in the notion of the *tātkālika gati* or instantaneous velocity of a planet,[50] since it is in that context that these interpolations were typically required. Bhāskara explains that the instantaneous velocity of a planet is obtained from the instantaneous sine difference, which is nothing but the cosine.

कोटीफलघ्नी मृदुकेन्द्रभुक्तिः स्त्रिज्योद्धृता कर्किमृगादिकेन्द्रे ।
तया युतोना ग्रहमध्यभुक्तिस्तात्कालिकी मन्दपरिस्फुटा स्यात् ॥ ३७ ॥

This may be translated as:

> Multiply the *koṭiphala* [i.e., $\frac{P \times R\cos(m-\alpha)}{360}$] by the rate of increase of the mean anomaly of the apsis and divide by the radius: the result taken as minutes of the arc applied positively and negatively in six signs of anomaly beginning from Cancer and Capricorn respectively to the mean motion of the planet will give the instantaneous (**तात्कालिक**) daily motion of the planet as affected by the apsis.[51]

The background to this is as follows. Indian planetary theory used an epicyclic model in which the mean longitude m and the true or *sphuṭa* longitude of a planet l (both measured from the first point of Aries) are related by

$$l = m \quad \frac{P \times \text{Rsin}\,(m-\alpha)}{360},$$

where α is the longitude of the apogee, and P is the periphery of the planet's epicycle of apsis. Indian planetary models typically used epicycles with *varying* radii. For example, in the case of the sun, the *Sūrya Siddhānta* provides for a radius which varies from 13° 40 to 14° (depending on the quadrant). The quantity P was the circumference of the corresponding epicyclic circle as expressed in units in which the larger circle was $360° = 21600$, and the radius $R = 3438$ or a similar number, as seen earlier.

Now, if l and l are the longitudes of the planet on two consecutive days, and n and n are the mean daily motions of the planet and its apogee, then

$$l = (m+n) \quad \frac{P}{360} R\sin \; (m+n) - (\alpha + n) \;,$$

so that

$$l - l = n \quad \frac{P}{360} \; \sin[(m-\alpha) + (n-n)] \; - \; \sin(m-\alpha)$$

$$(= n \quad \frac{P(n-n)}{360} \frac{R\Delta\sin(m-\alpha)}{h} \;)$$

$$= n \quad \frac{P(n-n)}{360}$$

$$\times \frac{1}{225} \times \text{ tabular difference of Rsines at } (m-\alpha). \qquad (3.40)$$

The last rule is found in the *Sūrya Siddhānta*, and a similar rule is mentioned by Varāhamihīra (who uses a value of h different from 225, since he works with a sine table with fewer than 24 values). Lalla attributes it to an unknown pupil of Āryabhaṭa.

It is in this context that Bhāskara's formula for the instantaneous velocity uses the cosine as the derivative of the sine. Certainly this formula was known earlier, and is explicitly found in Muñjala's *Laghumānasa*.[52] Nor was this was the only instance of a derivative that was worked out. Sengupta[53] provides examples of more complicated derivatives worked out by Brahmagupta[54] in the context of the corrected daily motion of a "star planet", such as Mars, for which the longitude would be written:

$$l \;=\; M + \tan^{-1}\frac{p\,\sin(\theta - M)}{R + p\,\cos(\theta - M)}. \tag{3.41}$$

Since the derivative for this function is evaluated, Brahmagupta too knew that sine differences are proportional to cosines. Bhāskara, however, explains the method used in Brahmagupta's calculation, and calls it the instantaneous velocity.

In the interests of complete clarity, it should be stated that just as Bhāskara used finite differences, so also Bhāskara's notion of time was essentially atomic. Just as linear measures built up the scale from the number of atoms in a dust particle, so also ordinary measures of time were built up from the smallest measure of time, known as a *truṭi*. In Vaṭeśvara's case,[55] a *truṭi* is $\frac{1}{112500}$ of a second, during which the motion (velocity) is treated as constant. Bhāskara takes a *truṭi* as $\frac{1}{33750}$ of a second. Time is today treated as a continuum (i.e., time is treated as having the topology of the real line), solely for the peculiar reason that the "laws" of physics are formulated using calculus, which has been seen to require the underlying notion of the continuum or the real number to make it compatible with Western theology! This is a strange unverifiable hypothesis to put at the base of physics. However, to the extent that the topology of time is re ected in the nature of logic[56] there is no real reason to suppose that this topology is like that of the real line! Furthermore, as reiterated several times earlier, it should *not* be automatically assumed that the continuum approach to the calculus is superior to the finite difference approach. On the contrary, with the finite difference approach there is no conceptual confusion here, as there is in Newton and Leibniz about the notion of an instant of time as a geometric point, which latter confusion is discussed in more detail in a subsequent chapter (and which notion of time requires a separate book in itself).

Bhāskara II s Use of Sine Values for Computation of Surface Area and Volume

Bhāskara II demonstrates an interesting use of sine values for computing areas and volumes—a typical application of present-day integral calculus. The volume of a sphere was first correctly expressed by Śrīdhara in his *Triśaṭikā*[57] 56. Bhāskara II provides a very interesting pedagogical demonstration.

> In order to make the point clear to the beginner, the teacher should demonstrate it on the surface of a sphere. Make a model of the earth in clay or wood, and suppose its circumference to contain as many units of length as there are minutes of the arc in a whole circle, i.e., 21600 units. Mark a point on the surface, with that point as the centre, and with 1 96 of the circumference as the "radius" [i.e., length of the cord stretched on the surface of the sphere] draw a circle. Again, with the same centre as before, and twice that thread, draw another circle; with three times that, another circle, and continue this operation till with 24 times that thread the 24th circle is described. Of these circles the radii [i.e. the radii in the plane of the circle] will be the *jyā*-s, viz. 225, 449, etc. From these, by proportion the lengths of the circle are obtained. Here, the length of the last circle is 21600 units, and its radius is 3438. If the *jyā*-s be multiplied by 21600 and divided by 3438 (or more correctly multiplied by 3927 and divided by 1250) we get the lengths of the circles. Between any two circles there is an [annular] figure and there are 24 such figures, more if more than 24 *jyā*-s are used. In each figure [if the net is stretched out the figure is a trapezium, so that] the larger, lower circle may be taken as the base and the upper smaller circle as the opposite side, while the perpendicular is 225. Hence, by the rule for the area of a trapezium, the area of each ring may be found. The sum of all these areas is the surface of half a sphere; twice that equals the surface of the whole sphere. This is equal to the product of the diameter and the circumference.[58]

An actual calculation brings up the following interesting discrepancy. Let A_i denote the areas of the various rings, and let R_i denote the ith *jyā*, or sine value, as before, and this is also now the radius of the ith circle. Then

$$A_1 = 225 \times \frac{\text{circumference of the 1st circle}}{2} \tag{3.42}$$

$$= \frac{225 \times 3927 \times 2R_1}{1250 \times 2} \tag{3.43}$$

$$= 225 \times \frac{62832}{10000} \times \frac{R_1}{2} \tag{3.44}$$

$$A_2 = 225 \times \frac{62832}{10000} \times \frac{R_1 + R_2}{2} \tag{3.45}$$

$$A_3 = 225 \times \frac{62832}{10000} \times \frac{R_2 + R_3}{2} \tag{3.46}$$

$$\vdots$$

$$A_{24} = 225 \times \frac{62832}{10000} \times \frac{R_{23} + R_{24}}{2}. \tag{3.47}$$

Hence, the surface of the hemisphere is

$$\sum_{i=1}^{24} A_i = 225 \times \frac{62832}{10000} \times \quad R_1 + R_2 + \cdots + R_{23} + \frac{R_{24}}{2} \tag{3.48}$$

$$= \frac{21600}{96} \times 6.23832 \times 52513 \tag{3.49}$$
$$= 21600 \times 3437. \tag{3.50}$$

Bhāskara well understood that the discrepancy arose because only 24 *jyā*-s were used. Bhāskara concludes:

> This is as I have said in my Arithmetic:[59] the area of a circle is equal to the product of the circumference by one-fourth of the diameter. That result multiplied by 4 gives the surface of the sphere, which is like the net surrounding a hand ball; the same (surface of a sphere) when multiplied by the diameter and divided by six becomes invariably the volume of the sphere.

These correct formulae and concepts for the the surface area and volume a sphere, are significant, since the correct formulae for the volume of a sphere was not known earlier. Thus Āryabhaṭa gave the incorrect value of $\sqrt{\pi}\pi r^3 \approx 1.47\pi r^3$, in *Gaṇita* 7, for the volume of a sphere. The error in Āryabhaṭa's formula for the volume of a sphere, thus, was probably due to the the particular numerical approximation he used.

Against this background of the use of infinitesimal methods to determine surface areas and volumes, four centuries before Europe, it is but natural that historians like Filliozat[60] felt insecure enough to feel compelled to describe as an "accident" and "no general method" the precise value of π derived by Āryabhaṭa, and they felt compelled to praise, in comparison, "general methods" like the formula $(a + b)^2 = a^2 + b^2 + 2ab$, implicit in the technique of squaring described by Āryabhaṭa!

The Widely Felt Need for Greater Accuracy

It is clear from the above example that there was a felt need for greater accuracy. This need for greater accuracy is found also in the earlier works of Govindasvāmin (ca. 800 CE) who, long before Mādhava, and even before Vaṭeśvara, first attempted to carry out Āryabhaṭa's calculation accurate to the third minute,[61] and gave a value for the radius as 3437 44 19 , to arrive at a value of π more accurate than that of Vaṭeśvara, but less accurate than that of Mādhava. The same value, written as 12375859 , is used by Udayadivākara.[62] This shows that from some five to six hundred years before Mādhava, there was a felt need for greater accuracy, to the third minute, in Āryabhaṭa's trigonometric values, and the value of π. A few centuries later, we find that this need for greater accuracy becomes widespread.

The idea of mathematics as a practical technique of calculation (rather than a religious instrument of spiritual progress, or a theological yardstick of correct argumentation) was also widespread in various other parts of the world, including China, Central and West Asia. Of course, these parts were hardly isolated from each other, and it is well known how mathematics and astronomy were transmitted from India to the Arab world via the algorismus, and the sind-hind tradition of astronomy. It is also well known how calendar-making in China was for centuries done by Indian Buddhists settled in China. The basis of these contacts was trade—since the mathematics in question was practically useful, and useful for commerce, it is not difficult to understand how this mathematics spread through commerce, in exactly the way the use of the algorismus spread to Europe through Florentine merchants.

From what we know, India, Africa, Arabia, and China formed a vast trading zone. From the archaeological evidence of ports in Harappan sites, it is evident that this trade stretched back to Harappan times. Since a good part of this trade was done by sea, the mathematics in question would also have spread through a sharing of celestial navigational techniques which obviously involved both mathematics and astronomy. In particular, celestial navigation involved both the stars and the globe. Measuring the globe involved a knowledge of the circle and the sphere. In particular, it required a knowledge of the ratio of the circumference of the circle to its diameter, a ratio today most easily identified as the number π. This knowledge was a widely felt requirement.

Now, according to Needham,[63] it so happens that about a century before Āryabhaṭa, in China,

> Liu Hui—by inscribing a polygon with 192 sides within a circle and calculating the polygon's perimeter,—obtained [π =] 157 50 or 3.14. Liu Hui also gave two other extreme values, and used a polygon of 3,072 sides for his best one, 3.14159—the Greeks had never achieved a value as accurate as this.

Around the time of Āryabhaṭa we find attributed to Tsu Chhung-Chih a value between 3.1415927 and 3.1415926, corresponding to the approximation 355 113, as actually stated and verified by about 1300 CE by Chao Yu-Chhin, using a polygon of up to 16,384 sides.

Now, the *Karaṇapaddhati* (VI, 7) which gives 31,415,926,536 as the circumference for a diameter of 10,000,000,000, also explains how the following approximations may be derived: $\frac{3}{1}, \frac{22}{7}, \frac{355}{113}, \frac{67783}{21576}, \frac{68138}{21689}, \frac{408473}{130021}$, etc. The interesting thing here is that the Chinese methods noted by Needham are purely geometric, while these rational representations of π in India arise naturally as part of a numerical calculation. Mādhava's approximate contemporary, al Kashi (d. 1429), the director of Ulugh Beg's Samarkand observatory, had calculated the value of $\pi = 3.141, 592, 653, 589, 793, 25$ accurate to 16 decimal places, in his *Risala al Muhutiyya* ("Treatise on the Circumference").

While increasing precision in the values of π is only a rough indicator of the overall mathematical sophistication, such precise values of π ultimately concern the origin of the integral and differential calculus, and one would like to understand how the calculus developed. It is clear that the questions being asked by contemporaries (give or take a century) in India, China, and Central Asia are roughly the same, that there is a widely-felt need for greater precision in numerical values, and that the numerical values being provided are also comparable. The differences therefore are only in the techniques.

What were the techniques used? Needham provides only a diagram from which one must guess the exact techniques used by Liu Hui and by Chao Yu-Chhin. Al Kashi pushed the earlier techniques to new limits. However, all these techniques were purely geometric, and hence had no future, as Āryabhaṭa understood long ago. Nevertheless, apart from the numerical technique of finite differences that he initiated, Āryabhaṭa also had access to a geometrical technique, which must have been prevalent from before his time. It is interesting that even this geometrical technique was different from any of the above geometrical techniques, and admitted a clearer understanding of the circle as a limit of polygons. This geometrical technique also led to a numerical algorithm which *could* be used to compute π to any desired degree of accuracy without excess labour.

IV
ĀRYABHAṬA'S GEOMETRICAL METHOD OF CALCULATING π

No other account has been given so far of this geometrical technique, at least not to my knowledge. Hence, I describe below the technique, as reconstructed from an unpublished draft translation of the *Yuktibhāṣā*. Unlike the numerical techniques, this technique requires the extraction of square roots (and the definition of area, both of which have been explicitly described earlier in the *Āryabhaṭīya*). This also is the technique described by Nīlakaṇṭha in his commentary on the *Aryabhaṭīya*. Therefore, this was a technique that was in use in Āryabhaṭa's school, hence was closely related to the original geometrical technique available to Āryabhaṭa. This is further reinforced by the fact that later-day techniques are given separately in the *Yuktibhāṣā* text, along with their advantages. In fact, the laboriousness of the geometrical techniques is used to motivate the later-day numerical techniques.

Moreover, the *Yuktibhāṣā* still is the earliest fully translated text from Āryabhaṭa's school which concerns an explanation of the rationale, and clearly the technique described here is one which had definitely been discarded by the school by the time of Mādhava. The more precise sine values and infinite series attributed to Mādhava clearly take off from the method of computing sines using finite differences which is described *next* by Āryabhaṭa. Also the technique is of independent interest; though the technique itself is partly geometric, it ultimately leads to a simple numerical algorithm, based on the method of square-root extraction certainly known to Āryabhaṭa.

"Archimedes" Method of Calculating π

By way of historical background, we recall that the "Greeks" knew of a way of approximating the circle by a polygon: inscribe a square in a circle, fill up the gaps by erecting an isosceles triangle on each side, and continue the process. We do not know from where the Greeks obtained this technique, nor whether the attributions to the Greeks are at all valid. In any case, an important variation of this technique is commonly attributed to Archimedes,[64] though I know of no serious evidence linking this technique to Archimedes. This method is described in more detail in Appendix 3.C.

Liu Hui s Method

Liu Hui's method of computing π was rather similar. He used only inscribed polygons and his method corresponds to the recursion formula

$$p_{2n} = \sqrt{\left(\frac{p_n}{2}\right)^2 + \left(R - \sqrt{R^2 - \left(\frac{p_n}{2}\right)^2}\right)^2},$$

where p_n is the side of the inscribed polygon, and R, the radius of the circumscribed circle, he took equal to 1. Liu, too, started with the hexagon, which is the natural thing to do, since in this case $p_n = 1$. Doubling to 12, 24, 48, and 96 sides he obtained his value of $\pi = 3.141024$. Apparently Liu continued this process up to a polygon of 3072 sides. Of course, it is not likely that Liu used the above recursion formula. Also, I have been unable to determine the exact method used by Liu Hui to compute square roots, which is the critical ingredient. As far as I know, no one prior to Āryabhaṭa states a general technique for extracting square roots.

Āryabhaṭa s Method

Āryabhaṭa, however, had an elegant method (essentially the current method) of extracting square roots, using the decimal place value. This method was applied to determine the value of π as follows. The geometrical idea here was to cut out a circle from a square (Fig. 3.1).

We reproduce the method in full from the *Yuktibhāṣā* commentary to bring out the avour of the techniques used, which have not before been explained. This process relies on octagons rather than the hexagons used by "Archimedes" and Liu. All calculations make repeated use of the "Pythagorean" "theorem", better renamed the sine rule, for the Indian tradition introduced and worked with sines rather than Ptolemy's chords, and the proposition in question is equivalent to the sine formula $R^2 \sin^2\theta + R^2\cos^2\theta = R^2$. Alternatively, for the sake of simplicity, it could be renamed the "diagonal rule" for in the *sulba sūtra* the rule is described by linking the square root of the diagonals of a rectangle to the square of the sides.

Step 1. Construct a square with sides equal to the diameter of the required circle.

Step 2. Draw the north–south and east–west lines to form four small squares. The required circle meets the square at the four cardinal points. Draw a line from the centre to the south-east corner.

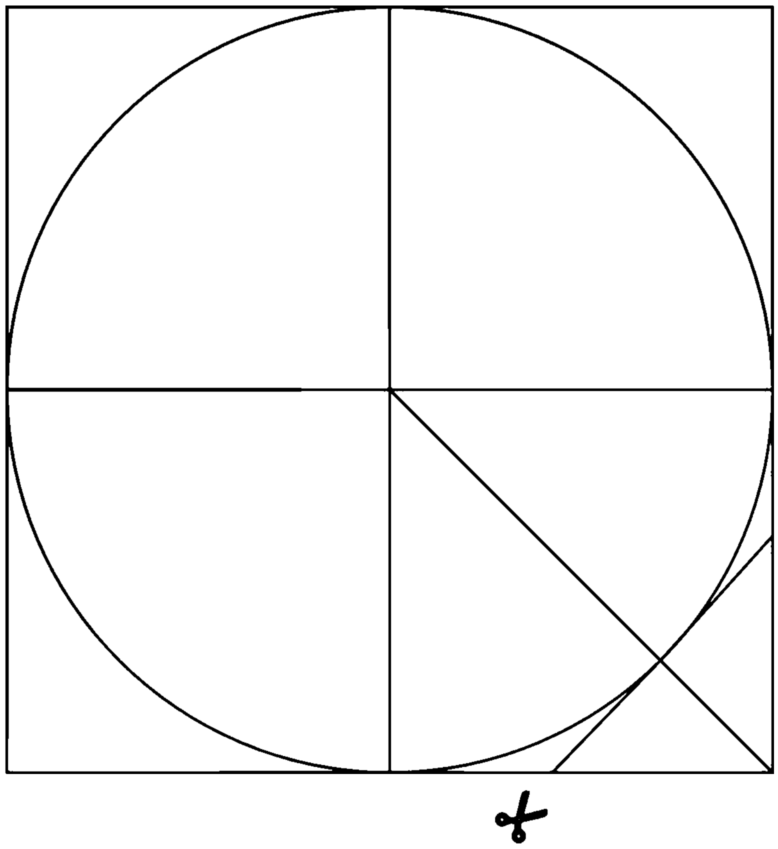

Figure 3.1: **Cutting corners.** The desired circle is the one inscribed in the polygon. At each stage one cuts off from the corner of the polygon an isosceles triangle by measuring out the sides, from the corner. The base of the triangle is tangential to the desired circle.

Step 3. The idea is to cut the south-east corner C along the line AB, and to repeat this process at the remaining 3 corners of the square. The requirement is that the resulting octagon (Fig. 3.2) should be equilateral. Alternatively, the requirement is that the line AB should be tangential to the required circle at the point where the circle intersects the line OC from the centre to the south-east corner.

Step 4. Let x be the side of the required octagon, and r be the radius of the required circle. Applying the sine rule to the right-angled isosceles triangle ABC with hypotenuse AB, we obtain the quadratic equation $x^2 = 2\left(r - \frac{x}{2}\right)^2$, with positive root $x = 2r(\sqrt{2} - 1) = 2(h - r)$, where $h = \sqrt{2}r$ is the diagonal of the smaller square.

Step 5. Since the triangle ESC is similar to triangle ABC, $\frac{h}{r} = \frac{x}{r - x/2}$, so by the rule of three $r - \frac{x}{2} = \frac{rx}{h}$. Measure out this last quantity ($= CA,\ CB$, Fig. 3.2) and cut the corner. (Observe that this quantity corresponds to an irrational number, that is being calculated and measured out, a process inconceivable in the synthetic reinterpretation of "Euclidean" geometry.)

Step 6. The first approximation to the circumference ($= 2\pi r$) is $8x$, and this gives $\pi \approx 3.313708$.

Step 7. (Fig. 3.3) The idea is to cut the corner B of the octagon, along the line B_1B_2, and to repeat this at the other seven corners, to get a 16-sided figure. Observe that the required circle meets each polygon tangentially at the mid-point of its sides. Thus, the line joining

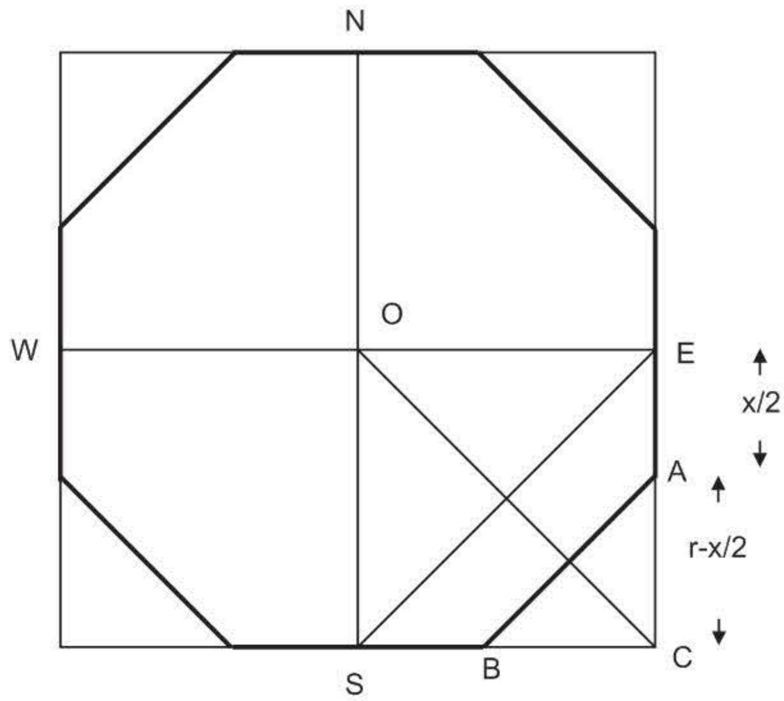

Figure 3.2: **The octagon method.** This method of calculating circumference or π starts with a square of side equal to the diameter of the desired circle, and proceeds by cutting off the corner of the square and of the successive polygons so obtained at each stage, to obtain the next equilateral polygon. This differs from the hexagon-doubling method attributed to Archimedes and Liu Hui.

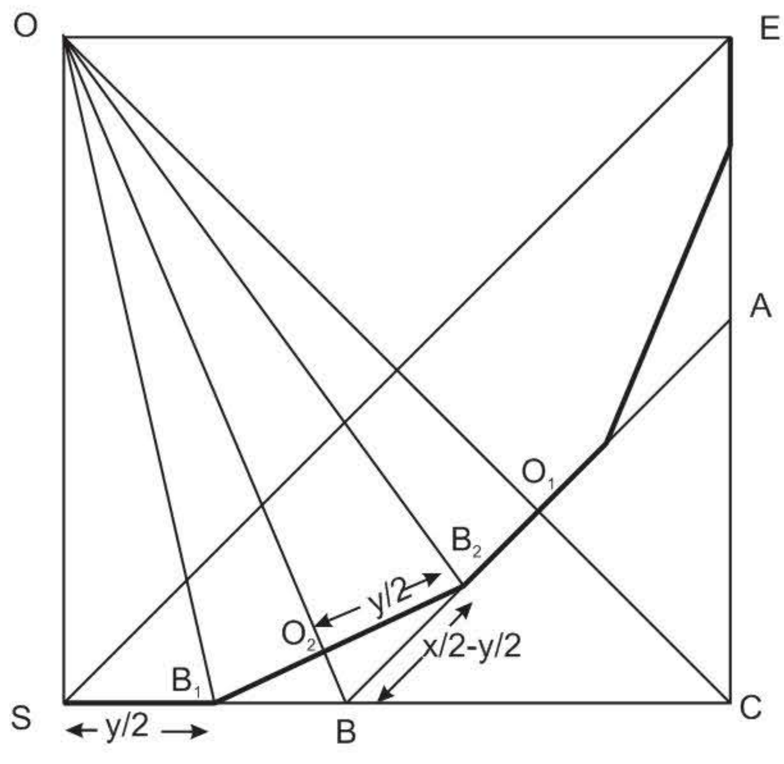

Figure 3.3: **Detail of the octagon-doubling method.** The figure shows the situation in the south-east square where the two corners of the octagon at B and A are cut by calculating and measuring out the sides of an isosceles triangle. The key to the recursion formula is that the required circle meets each such polygon tangentially at the mid-point of its sides.

the centre to the mid-point of the side of the octagon has length r. Solving the right-angled triangle OBO_1, gives $(OB)^2 = r^2 + \frac{x^2}{4}$, hence $BO_2 = OB - r$. But $BB_2 = \frac{x}{2} - \frac{y}{2}$, and $O_2B_2 = \frac{y}{2}$, so we can calculate y by applying the sine rule to the triangle BO_2B_2. In fact, this gives the formula $y = r\frac{a^2-k^2}{a}$, where $a = \sqrt{2} - 1$, $k = \sqrt{(a + a^2)} - 1$, and $\pi = 16\frac{k}{a}$.
Step 8. The method and calculations in the above step can be repeated indefinitely. Hence, we are led to the following numerical algorithm. Let

$$g(x) = \sqrt{(1+x^2)} - 1,$$

$$f(x) = \frac{g(x)}{x}.$$

The algorithm computes, to level n,

$$z_0 = a = (\sqrt{2} - 1),$$

$$z_i = f(z_{i-1}),$$

$$\pi \approx 2^{2+i+1} z_i.$$

It is clear that the algorithm involves computation of only squares and square roots, and Āryabhaṭa had already stated efficient algorithms for these, which use the decimal place value notation. We took a short cut, and wrote a computer program, using the intrinsic `sqrt` function in Turbo C. The results show that Āryabhaṭa used either the value $n = 5$, or the value $n = 6$, corresponding to a polygon with 512 sides or 1024 sides. In particular, Āryabhaṭa's octagon method could *not* have been the method used by Liu Hui, who clearly used a technique similar to that of "Archimedes", since $3072 = 3 \times 1024 = 3 \times 2^{10}$ is not a power of 2 but is a number that would be obtained on the hexagon-doubling method. The same method of hexagon-doubling must have been used by al-Kashi, since he used a polygon with 3×2^{28} sides.

V
THE DERIVATION OF THE SERIES EXPANSION

Computation of the Circumference

Having outlined the above procedure of calculating the circumference of the circle, using square roots, the *Yuktibhāṣā* now points out that it is possible to avoid the cumbersome computation of square roots, and proceeds to calculate the circumference using a series expansion. (This is closely analogous to the avoidance of square-root extraction while computing sine values.) Unlike the geometric technique of computing circumference which is restricted to the calculation of π, the infinitesimal techniques can be used also to calculate various trigonometric values. This provides an important link between the computation of the circumference ("π") and the computation of sine values proper, using Āryabhaṭa's finite

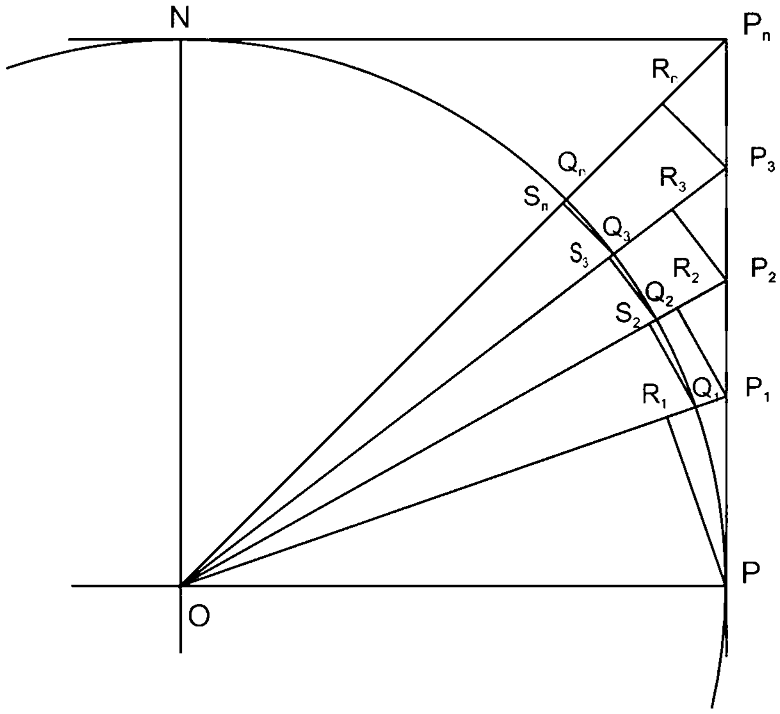

Figure 3.4: **The Yuktibhāṣā calculation of the circumference.** In this method, the circumference is calculated as the sum of the chords Q_iQ_{i+1}, as the number of divisions becomes infinite.

difference method explained earlier. In the following we give a detailed presentation of this process, since it has not been explained earlier in a satisfactory way, as also noted earlier by Srinivasiengar.

The following presentation relies mainly on the *TantrasaṅgrahaVyākhyā*, *Yuktidīpikā*, and *Kriyākramakarī*, of Śaṅkara Vāriyar, which are also the Sanskrit sources of the modern *Yuktibhāṣā* commentary in Malayalam, as noted earlier. This has the advantage of using a critically edited version prepared by a knowledgeable scholar using several manuscript sources. It also has the advantage of being readily available in printed form. This helps us to give a complete account[65] that is comprehensible from a contemporary perspective (though the methods are not those of contemporary formal mathematics). The process is as follows.

As usual, a circle of radius r is inscribed in a square of side $2r$. Attention is focussed on the first (north-east-east) octant.[66] The half-side of the square has length r, and this is divided into a number of small equal parts of length Δr, by marking off the points $P, P_1, P_2, P_3, \ldots, P_n$. These points are joined to the centre of the circle, O, by means of lines $OP_1, OP_2, \ldots, OP_n$, called *karṇa*-s.[67] These lines intersect the circle at the points $Q_1, Q_2, \ldots, Q_n$, dividing the circumference into a number of (unequal) parts $Q_i\ Q_{i+1}$. A perpendicular is dropped from each Q_i to the next *karṇa*, which it meets at the point S_{i+1} (Fig. 3.5). The idea of the calculation is to approximate the length of the arcs $Q_i\ Q_{i+1}$ by the length of the straight lines Q_iS_{i+1}, and then sum up these lengths, in the limit as $n \to \infty$. As an aid to calculate the lengths $Q_i\ S_i$, perpendiculars are dropped from the points P_i to the next *karṇa*, which they meet at the points R_{i+1}. The calculation now proceeds as follows.

The first case is a special one. The triangles OPP_1, and OPR_1 are similar (they are right-angled triangles with one additional angle $\_P_1OP$ in common). Hence, by the rule of three,

$$\frac{PR_1}{PP_1} = \frac{OP}{OP_1}, \tag{3.51}$$

or

$$PR_1 = \frac{PP_1 \cdot OP}{OP_1} = \frac{\Delta r \cdot r}{OP_1}. \tag{3.52}$$

The second case onwards is a typical one. The triangles $P_1R_2P_2$ and POP_2 are similar (since they are right-angled triangles with one additional angle $\_OP_2P_1$ in common). Hence, by the rule of three,

$$\frac{P_1R_2}{P_1P_2} = \frac{OP}{OP_2}, \tag{3.53}$$

so that

$$P_1R_2 = \frac{OP \cdot P_1P_2}{OP_2} = \frac{\Delta r \cdot r}{OP_2}. \tag{3.54}$$

Proceeding in this manner, we obtain

$$P_{n-1}R_n = \frac{\Delta r \cdot r}{OP_n}. \tag{3.55}$$

We can now calculate $Q_{n-1}S_n$ as follows. The triangles OP_1R_2 and OQ_1S_2 are similar (they are right-angled triangles with the additional angle $\_S_2OQ_1$ in common). Hence, from the rule of three,

$$\frac{Q_1S_2}{P_1R_2} = \frac{OQ_1}{OP_1}, \tag{3.56}$$

so that

$$Q_1S_2 = \frac{P_1R_2 \cdot OQ_1}{OP_1} = \frac{\Delta r \cdot r}{OP_2} \cdot \frac{r}{OP_1} \tag{3.57}$$

$$= r^2 \cdot \Delta r \cdot \frac{1}{OP_1 \cdot OP_2}. \tag{3.58}$$

Proceeding in this manner, we obtain

$$Q_{n-1}\,S_n = r^2 \cdot \Delta r \cdot \frac{1}{OP_{n-1} \cdot OP_n}. \tag{3.59}$$

The arc $PQ = \frac{\text{circumference}}{8}$ $\frac{2\ r}{8} = \frac{r}{4}$ can now be calculated.

$$PQ_n = PQ_1 + Q_1Q_2 + Q_2Q_3 + \cdots + Q_{n-1}Q_n \tag{3.60}$$

$$\text{i.e., } \frac{\pi r}{4} \approx PR_1 + Q_1S_2 + Q_2S_3 + \cdots + Q_{n-1}S_n \tag{3.61}$$

$$= \Delta r \cdot r^2 \; \frac{1}{OP \cdot OP_1} + \frac{1}{OP_1 \cdot OP_2} + \cdots + \frac{1}{OP_{n-1} \cdot OP_n} \;\; . \tag{3.62}$$

To evaluate the sum, it is simplified by neglecting certain quantities that are negligibly small when n is large. In present-day terminology we would say that as $n \rightarrow \infty$, the difference $OP_i - OP_{i+1} \rightarrow 0$; therefore this quantity can be neglected relative to quantities like OP_i or OP_{i+1}. The details of this argument are considered in more detail later on. But the basic idea of infinitesimal seems to have arisen as an extension of the technique of "zeroing" or rounding—as I have argued elsewhere, the term *śūnya* represents not merely the number 0, but also any quantity that was discarded or zeroed in the course of a calculation. For large values of n the difference $OP_i - OP_{i+1}$ is negligible, and can be zeroed or rounded off compared to quantities like OP_i or OP_{i+1}; the discarded quantity is negligibly small in the sense that it cannot even be *represented* relative to the quantity being retained. Ironically, from a computational point of view this immediately makes good sense just because the final answer to the calculation is being expressed to an arbitrary but limited precision, i.e., just because the formal continuum is *not* being used!

From $(OP_i - OP_{i+1})^2 + 2OP_iOP_{i+1} = OP_i^2 + OP_{i+1}^2$, since $(OP_i - OP_{i+1})^2 \quad 0$ (i.e., for large n this quantity is negligible compared to the other quantity on the same side of the equation), we can neglect it, so that $(OP_i^2 + OP_{i+1}^2) \quad 2OP_iOP_{i+1}$, and $(OP_i^2 + OP_{i+1}^2)^2 \quad 4OP_i^2OP_{i+1}^2$, so that

$$\frac{1}{OP_i \cdot OP_{i+1}} \approx \frac{2}{OP_i^2 + OP_{i+1}^2} \tag{3.63}$$

$$= \frac{2\left(OP_i^2 + OP_{i+1}^2\right)}{\left(OP_i^2 + OP_{i+1}^2\right)^2} \tag{3.64}$$

$$= \frac{2\left(OP_i^2 + OP_{i+1}^2\right)}{4OP_i^2OP_{i+1}^2}. \tag{3.65}$$

The upshot is that one may legitimately use

$$\frac{1}{OP_i \cdot OP_{i+1}} = \frac{1}{2} \quad \frac{1}{OP_i^2} + \frac{1}{OP_{i+1}^2} \quad \text{for sufficiently large } n, \tag{3.66}$$

so that the earlier approximation may be rewritten (neglecting some further insignificant quantities) as

$$\frac{\text{circumference}}{8} \qquad \frac{\pi r}{4} \approx r^2 \cdot \Delta r \cdot \sum_1^n \frac{1}{OP_i^2}. \tag{3.67}$$

(The neglected quantity is $\Delta r \cdot r^2 \quad \frac{1}{OP^2} - \frac{1}{OP_n^2} \quad = \Delta r \left(1 - \frac{1}{2}\right) = \frac{r}{2}$, which can evidently be zeroed, the first equality following since $OP_n^2 = OP^2 + PP_n^2 = 2OP^2 = 2r^2$.)

We can also keep track of the earlier neglected quantities if we like. Thus, the first of the above approximations (3.64) involved

$$\begin{aligned}
\frac{1}{OP_i \cdot OP_{i+1}} - \frac{2}{OP_i^2 + OP_{i+1}^2} &= \frac{OP_i^2 + OP_{i+1}^2 - 2OP_i \cdot OP_{i+1}}{(OP_i \cdot OP_{i+1})} \cdot \left(OP_i^2 + OP_{i+1}^2\right) \\
&= \frac{\left(OP_i^2 - OP_{i+1}^2\right)}{(OP_i \cdot OP_{i+1})} \cdot \left(OP_i^2 + OP_{i+1}^2\right) \\
&= \frac{(\Delta r)^2}{(OP_i \cdot OP_{i+1}) \cdot \left(OP_i^2 + OP_{i+1}^2\right)} \\
& \frac{(\Delta r)^2}{r^4}. \qquad (3.68)
\end{aligned}$$

Hence, the total quantity neglected is at most

$$r^2 \cdot \Delta r \cdot \sum_1^n \frac{(\Delta r)^2}{r^4} = \frac{(\Delta r)^2}{r^2} \sum_1^n \Delta r = \frac{(\Delta r)^2}{r^2}. \qquad (3.69)$$

Similarly, for the second approximation (3.65). As we shall see later on, this neglect of quantities is systematically based on order counting. Thus, $\Delta r = r\ n$ so that the discarded/neglected quantity is $O\ \frac{1}{n^2}$, and the principle is the following.

Principle: In comparison with a constant (*rūpa*), for large n, we may neglect any quantity which is $O\ \frac{1}{n}$.

Here the "order of growth", O, is decided not as is done today by an implicit appeal to limits, but is simply defined by order counting for any rational function, expressed using a novel place-value notation for rational functions, which we consider later. This principle is obviously valid for any calculation carried out to any arbitrary (but finite) precision. It is also evident that, for the class of functions ("quantities") to which it applies, this principle will lead to exactly the same results that are today obtained by using formal limits. Finally, it is evident that the principle can be (and was) extended in the obvious way to two rational functions which are respectively $O\ \frac{1}{n^j}$ and $O\ \frac{1}{n^k}$.

Thus, the whole issue of limits is neatly sidestepped because mathematics is not obliged to carry on its head the weight of a theological load by pretending to some imagined divine perfection, and instead takes into account the realities of non-representability.

Computation of Fractions and the Power Series

The next step uses the elementary identity

$$\frac{1}{b} = \frac{1}{c} - \frac{b-c}{bc} \qquad (3.70)$$

to iteratively evaluate 1 b by the series expansion

$$
\begin{aligned}
\frac{1}{b} &= \frac{1}{c} - \frac{b-c}{bc} \\
&= \frac{1}{c} - \frac{b-c}{c} \cdot \frac{1}{b} \\
&= \frac{1}{c} - \frac{b-c}{c} \cdot (\frac{1}{c} - \frac{b-c}{c} \cdot \frac{1}{b}) \\
&= \frac{1}{c} - \frac{(b-c)}{c^2} + \frac{(b-c)^2}{c^2} \cdot \frac{1}{b} \\
&= \frac{1}{c} - \frac{(b-c)}{c^2} + \frac{(b-c)^2}{c^3} \\
&\quad + \frac{(b-c)^3}{c^3} \cdot \frac{1}{b} \\
&= \cdots . \qquad (3.71)
\end{aligned}
$$

From an epistemological point of view, against the background of the problems with Leibniz's infinitesimals and Newton's uxions in Europe, and their amelioration by the – techniques of mathematical analysis, the key thing to note is that, as it stands, the above series expansion is *indefinite* rather than infinite. Thus, there are no difficulties about convergence. *Exact equality holds at each iterative stage*, and if c is appropriately chosen, the last term becomes smaller at each stage, and can eventually be neglected as non-representable in the usual way to yield a valid numerical answer to any desired degree of precision.

As we shall see later on, this interplay of infinite and indefinite series has a very important consequence: the exceptional term can be manipulated to accelerate the convergence of the corresponding infinite series, as was actually done in the *TantrasangrahaVyākhyā* and *Yuktidīpikā*. This point was overlooked by both Newton and Leibniz, who, like other European mathematicians, used the infinite series expansions in an intuitive way, overlooking the possibility of an exceptional term. So to say, they evidently believed in all rule and no exception!

In India, it was quite natural for the infinite series expansion to be understood in analogy with the indefinite series expansion. Thus, the *Yuktidīpikā* or *Yuktibhāṣā* is hardly the first to make use of this identity. This identity is found also a thousand years earlier in Brahmagupta's *Brāhma-Sphuṭa Siddhānta* (12.57)[68] as a technique for the computation of difficult fractions that was very much a part of the Indian mathematics preceding the algorismus. The verse states:

छेदेनेष्टयुतोनेनाप्तं भाज्यात् अनष्टम इष्टगुणाम्।
प्रकृतिस्थच्छेदहृतं लब्ध्या युत हीनकमनष्टम् ॥१२.५७॥

This may be translated:

> Divide the dividend by the divisor together with the desired [number] (*iṣṭa*), and indestructibly establish (*anaṣṭa*) the *āpta* (result) so obtained. Multiply by the desired, and divide by the natural divisor. What is so obtained should be added or subtracted from the indestructibly established āpta [depending upon whether the desired divisor is greater than or less than the original divisor].

Here the "indestructible" refers to the *pāṭi-gaṇita* (slate-arithmetic) procedure of erasing and writing over: "indestructible" means that it should be written in a place where there is no fear of erasing it, since it will be used repeatedly. Suppose $a\ \ b$ is the fraction to be evaluated. Take the desired number as h, so that we have to divide by $b + h$; therefore, the *āpta* which has to be indestructibly established is $a\ \ (b + h)$. The difference between the two is

$$a\ \ \frac{1}{b} - \frac{1}{b+h} \ = a\cdot\frac{h}{b(b+h)} = \frac{a}{b+h}\cdot\frac{h}{b}. \tag{3.72}$$

Thus, for the evaluation of a fraction of the form $a\ \ b$ we have the formula:

$$\frac{a}{b} = \frac{a}{b+h} + \quad \frac{a}{b+h} \ \cdot\frac{h}{b}. \tag{3.73}$$

As a well-known example of the use of this procedure in the algorismus evaluation of fractions, consider the case of the fraction $\frac{1920}{93}$. Using $h = 3$, this fraction can be evaluated as follows:

$$\frac{1920}{93} = \frac{1920}{96} + \frac{1920}{96}\cdot\frac{3}{93} = 20\frac{60}{93}.$$

As stated above, the key to the derivation of the various power series is the iterative application of the above formula:

$$\frac{1}{b} = \frac{1}{b+h} + \frac{1}{b+h}\cdot\frac{h}{b}, \tag{3.74}$$

to obtain the preceding formula with $c = b + h$. A similar formula is obtained, with alternating signs, if h is negative.

Applying the Fraction-Series Expansion

Applying the above procedure to the quantity $b = OP_1^2$, using $c = r^2$, and noticing that $b - c = OP_1^2 - r^2 = OP_1^2 - OP^2$
$= (\Delta r)^2$, we obtain

$$\frac{1}{OP_1^2} \ = \ \frac{1}{r^2} - \frac{(\Delta r)^2}{r^2}\cdot\frac{1}{OP_1^2} \tag{3.75}$$

$$= \ \frac{1}{r^2} - \frac{(\Delta r)^2}{r^2} \ \ \frac{1}{r^2} - \frac{(\Delta r)^2}{r^2} \ \ \frac{1}{OP_1^2} \tag{3.76}$$

$$= \frac{1}{r^2} - (\frac{(\Delta r)^2}{r^4} + \frac{(\Delta r)^4}{r^4} \quad \frac{1}{r^2} - \frac{(\Delta r)^2}{r^2} \quad \frac{1}{OP_1^2} \tag{3.77}$$

$$= \frac{1}{r^2} - \frac{(\Delta r)^2}{r^4} + \frac{(\Delta r)^4}{r^6}$$

$$- \frac{(\Delta r)^6}{r^6} \quad \frac{1}{r^2} - \frac{(\Delta r)^2}{r^2} \quad \frac{1}{OP_1^2} \tag{3.78}$$

$$= \cdots . \tag{3.79}$$

Hence,

$$\frac{r^2 \cdot \Delta r}{OP_1^2} = \Delta r \quad 1 - \frac{(\Delta r)^2}{r^2} + \frac{(\Delta r)^4}{r^4} + \cdots . \tag{3.80}$$

It should be clearly noted that the ellipsis here indicates an indefinite expansion and *not* an infinite expansion. When this process is applied to 1 OP_k^2, then we must use instead $OP_k^2 - r^2 = (k\Delta r)^2$. Thus, we obtain

$$\frac{r^2 \cdot \Delta r}{OP_k^2} = \Delta r \quad 1 - \frac{(k\Delta r)^2}{r^2} + \frac{(k\Delta r)^4}{r^4} + \ldots . \tag{3.81}$$

Hence, the original formula could be rewritten

$$\frac{\text{circumference}}{8} \quad \frac{\pi r}{4}$$

$$\approx r^2 \cdot \Delta r \cdot \sum_{1}^{n} \frac{1}{OP_i^2} \tag{3.82}$$

$$= \Delta r \quad 1 - \frac{(\Delta r)^2}{r^2} + \frac{(\Delta r)^4}{r^4} - \cdots$$

$$+ \Delta r \quad 1 - \frac{(2\Delta r)^2}{r^2} + \frac{(2\Delta r)^4}{r^4} - \cdots$$

$$+ \cdots$$

$$+ \Delta r \quad 1 - \frac{(n\Delta r)^2}{r^2} + \frac{(n\Delta r)^4}{r^4} - \cdots . \tag{3.83}$$

This gives, upon rearranging the terms,[69]

$$\frac{\text{circumference}}{8} \quad \frac{\pi r}{4}$$

$$\approx n\Delta r$$

$$- \frac{(\Delta r)^3}{r^2} \quad 1^2 + 2^2 + 3^2 + \cdots + n^2$$

$$+ \frac{(\Delta r)^5}{r^4} \quad 1^4 + 2^4 + 3^4 + \cdots + n^4$$

$$- \cdots . \tag{3.84}$$

The procedure now is to choose Δr as one unit (of length). Since the length r has been divided into n equal parts of length Δr, in modern language, this corresponds to substituting $r = n\Delta r$ in the above. Further, dividing both sides by r we obtain

$$\begin{aligned} \frac{\pi}{4} \approx\ & 1 \\ & - \frac{1}{n^3}\ 1^2 + 2^2 + 3^2 + \cdots + n^2 \\ & + \frac{1}{n^5}\ 1^4 + 2^4 + 3^4 + \cdots + n^4 \\ & - \cdots . \end{aligned} \tag{3.85}$$

Computing the Sum of the kth Powers: Step 1

It is now required to sum the various other series to which the sum of the original series has been reduced. The computation of $\ {}^{n}_{i=1}\ i^k$ for $k\ \ \ 3$ was known for quite a long time in Indian tradition, and predates Āryabhaṭa by about a thousand years.[70] In any case, the relevant formulae for these sums are given, for example, by Āryabhaṭa (*Gaṇita* 19, 22), Bhāskara, etc. Āryabhaṭa uses for these series the terms *citighana*, *vargacitighana*, and *ghanacitighana*. *Citighana* literally means the solid contents of a pile of units (grain?) in the shape of a pyramid with a triangular base. Each layer of the pyramid contains $1 + 2 + 3 + \cdots + r$ units, starting from the top layer which contains 1 unit. The term *vargacitighana* means the solid contents of a pyramid with a square base which has 1 unit in the topmost layer, 2^2 units in the next layer, and so on. Likewise, *ghanacitighana* means the solid contents of a pile of units (cuboidal bricks) in the shape of a pyramid having cuboidal layers, with 1 brick in the topmost layer, 2^3 bricks in the next layer, and so on. Bhāskara uses the term *saṅkalana* for the series, while Śridhar, in his *PāṭiGaṇita*, uses the terms *średhi* and *saṅkalitā* (*varga saṅkalitā*, *ghana saṅkalitā*, etc.). The term used in the *TantrasaṅgrahaVyākhyā*, *Yuktibhāṣā*, etc. is *saṅkalitā*.

The computation of $\ {}^{n}_{i=1}\ i^k$ for $k\ \ \ 4$ is, however, not exactly elementary: currently one typically uses the "Euler–Maclaurin" expansion (very similar to the "Taylor" expansion, and essentially equivalent to it) for this purpose. Since many historians may be unfamiliar with how this computation is carried out, and what the result is, this is explained in Appendix 3.B. Most texts in the history of mathematics wrongly state that this formula was first derived by Bernoulli.

This sum was computed in an altogether different way in Indian tradition, using triangular sums, first evaluated by Nārāyaṇa Paṇḍit of Benares in 1356 CE, centuries before Bernoulli. (As a matter of fact, we need to compute only the leading order term, which is a lot simpler. This is all that is required, since, according to the above calculation, we actually need to compute only $\frac{1}{n^{k+1}}\ \ {}^{n}_{i=0}\ i^k$ for large/infinite values of n.) This is not clearly explained in the *Yuktibhāṣā*. For example, Srinivasiengar laments,

> The result... [for the sum of the *k* th powers]... is not elementary, and its proof has not been indicated. [71]

This is a very important point, since it shows that the work that has been attributed in its entirety to the "Kerala school", depended critically on inputs from various other parts of India, not only from the time of Āryabhaṭa, Bhāskara I up to Bhāskara II, but even up to the mid-14th c. CE. (A well established trade route between north and south India existed up to this point of time, as is clear from the account of travellers like Ibn Battuta, for example, who, in the 14th c. CE, regarded the natural route from Delhi to China as going via Cochin.) In particular, let us recall the earlier work of Govindasvāmin and Udayadivākara, which unsuccessfully attempted (some five hundred years before Mādhava) what Mādhava achieved, viz. precision to the third sexagesimal minute. This was presumably the critical element responsible for their lack of success. Thus, Mādhava's achievement would not have been possible without the critical input of Nārāyaṇa Paṇḍit's formula for the *vārasaṅkalitā*.[72] Hence, also, it would be more appropriate to call the calculus the work of the Āryabhaṭa school, which is, in fact, how most of the persons involved viewed themselves.

The sums in (3.85) may be reduced to the triangular sums or the *vārasaṅkalitā* of Nārāyaṇa Paṇḍit of Benares, as follows.

Consider, first, the *mūla-saṅkalitā* or the series

$$s_1 = 1 + 2 + 3 + \cdots + n. \tag{3.86}$$

This case is well known, since expressions for the sum of this series were known long before Āryabhaṭa, and are given by almost everyone, including Āryabhaṭa, Śridhara, Mahāvīra, etc. The traditional derivation of this went as follows. If each of the terms in the above series were equal to n, then the sum of the series would be n^2, i.e.,

$$\underbrace{n + n + n + \cdots + n}_{(n \text{ times})} = n^2. \tag{3.87}$$

Write the first series (3.86) in reversed order, and subtract it from the second series (3.87)

$$\begin{array}{c} n + \cdots + n + n + n \\ -n - \cdots - 3 - 2 - 1 \end{array} = \begin{array}{c} n^2 \\ -s_1. \end{array} \tag{3.88}$$

Hence,

$$0 + 1 + 2 + \cdots + (n-1) = n^2 - s_1, \tag{3.89}$$

i.e.,

$$s_1 - n = n^2 - s_1, \tag{3.90}$$

so that

$$2 \cdot s_1 = n^2 + n, \tag{3.91}$$

or

$$s_1 = \frac{n \cdot (n+1)}{2}. \tag{3.92}$$

We are now in the situation where we need, in modern terminology, to proceed to the limit as $n \to \infty$. Recall that the above series was obtained by choosing the size of the division, Δr, as one unit. We now take this unit to be infinitesimal (lit. *aṇuparimāṇam* = of atomic dimension), i.e., we make the divisions of the side of the square, hence of the circumference, as fine as is *physically* conceivable. (In the Naiyāyika world view, the process of subdivision of the circumference would have *had* to terminate at the level of indivisible atoms.) In this case the number n of divisions is infinite (lit. *ananta*, or *asaṅkhya* = not countable). In this situation, we can simplify (3.92) to conclude that

$$\frac{s_1}{n^2} = \frac{1}{2} \quad (n \to \infty). \tag{3.93}$$

The neglected term is $1 \ (2n)$. What is happening here is, once again, that the term with the variable (*rāśī* = n) in the denominator is being discarded as non-representable relative to the constant (*rūpa*) term. This is in line with the principle noted above of discarding non-representables by order counting. This is a perfectly general and valid procedure, which is repeatedly used in the course of the derivation.

In present-day notation, if we are doing standard analysis over an Archimedean field like that of reals,[73] this same result for the sum of the arithmetic series would be expressed, in an equivalent form, as

$$\lim_n \frac{1 + 2 + 3 + \cdots + n}{n^2} = \frac{1}{2}. \tag{3.94}$$

However, there is no need to resort to formal limits, or even formal infinitesimals, required by Platonic idealism; one simply discards non-representable terms as usual. In fact, from the viewpoint of *śūnyavāda*, resorting to Platonic idealism, and invoking the existence of ideal limits in formal real numbers, that have no possibility of any real existence, would distinctly damage the argument. Thus, the above procedure is justified, whichever the way we look at it (so long as we do not mix the two opposing philosophies of *śūnyavāda* and Platonic idealism). In particular, it is justified according to the philosophy of non-representables (*śūnyavāda*).

In general, of course, one may need to allow for the possibility that a large number of discardable quantities may add up to a quantity that is not discardable. There is, however, no need to worry about that in the present context. Because of the interplay of indefinite and infinite series, at no stage does there arise a situation where we are required to consider "an infinite sum of infinitesimals". This logic may not have been entirely clear to the modern commentators of the *Yuktibhāṣā*, and is certainly absent from the literature in English on the subject since Whish.

That the tradition itself used infinite series in addition to indefinite series is clear from the way in which Nīlakaṇṭha, in the *AryabhaṭīyaBhāṣya*,[74] gives an expression for the sum of an infinite geometric series,

> एवं यस्तुल्यच्छेदपरभागपरम्पराया अनन्ताया अपि संयोगः
> तस्यानन्तानामपि कल्प्यमान्स्य योगस्याद्यावयविनः
> परम्परांशच्छेदादेकोनच्छेदांशसाम्यं सर्वत्रापि समानमेव।

which may be translated:[75]

> The sum of an infinite [*anantya*] series, whose later terms (after the first) are got by dividing the preceding one by the same divisor everywhere, is equal to the first term divided by one less than the common divisor.

(The divisor in question is assumed to be everywhere greater than 1, so that the common ratio is less than 1.) As pointed out earlier, such an understanding of infinitesimals, too, is a direct extension of the standard idea of non-representable built into the (non-idealist) number system.

Computing the Sum of the kth Powers: Vārasankalitā

Consider, next the *varga-saṅkalitā* or the series

$$s_2 = 1^2 + 2^2 + 3^2 + \cdots + n^2. \tag{3.95}$$

If each term in the *mūla-saṅkalitā* series (3.86) had been multiplied by n, then we would have obtained

$$1 \cdot n + 2 \cdot n + 3 \cdot n + \cdots + n \cdot n = s_1 \cdot n. \tag{3.96}$$

Write the *varga-saṅkalitā* (in the original order) under the above series, and subtract, to obtain

$$\begin{matrix} 1 \cdot n + 2 \cdot n + 3 \cdot n + \cdots + n \cdot n \\ -1 \cdot 1 - 2 \cdot 2 - 3 \cdot 3 - \cdots - n \cdot n \end{matrix} = \begin{matrix} n^2(n+1) \quad 2 \\ -s_2. \end{matrix} \tag{3.97}$$

That is,

$$(n-1) \cdot 1 + (n-2) \cdot 2 + (n-3) \cdot 3 + \cdots + (n-n) \cdot n = \frac{n^2(n+1)}{2} - s_2. \tag{3.98}$$

The series on the left of the above expression can be written as a triangular sum consisting of $(n-1)$ occurrences of 1, $(n-2)$ occurrences of 2, $(n-3)$ occurrences of 3, etc., each occurrence being stacked vertically:[76]

$$\begin{array}{l} 1 + 2 + 3 + \cdots + (n-3) + (n-2) + (n-1) \\ 1 + 2 + 3 + \cdots + (n-3) + (n-2) \\ 1 + 2 + 3 + \cdots + (n-3) \\ \vdots \\ 1 + 2 + 3 \\ 1 + 2 \\ 1. \end{array}$$

This is exactly the *vārasaṅkalitā*. It should be pointed out that the above triangular sum was long known to Indian tradition. To sum the above triangular sum by rows, we need to evaluate the sum $1 + (1+2) + (1+2+3) + \cdots$. This series was called *citighana* by Āryabhaṭa I, and an explicit value of the sum to n terms was given by Āryabhaṭa I (*Gaṇita* 21) in two ways as $\frac{n(n+1)(n+2)}{6}$, or $\frac{(n+1)^3-(n+1)}{6}$.

> Of the [arithmetic] series (*upaciti*) which has one for the first term and one for the common difference, take three terms in continuation of which the first is equal to the given number of terms, and find their continued product. That (product) or the number of terms plus one subtracted from the cube of that, divided by 6 gives the *citighana*.[77]

This series and its sum was, therefore, well known to the Āryabhaṭa school.

However, we now require to sum this series to all orders, and not merely the second. The general formula for the sum of such series is given by Nārāyaṇa Paṇḍit of Benares as follows.[78]

> एकाधिकवारमिताः पदादिरूपोत्तराः पृथक्तेंऽशाः।
> एकाद्येकचयहरा—स्तद्घातो वारसङ्कलितम्॥

This may be translated:

> The numbers beginning with the number of terms in the *vāra*, and increasing by one are the numerators. The [corresponding] denominators begin with 1 and increase by one. The product of these is the *vārasaṅkalitā*.[79]

That is, if there are r repeated summations, then the sum is given by

$$\frac{n}{1} \times \frac{n+1}{2} \times \frac{n+2}{3} \times \cdots \times \frac{n+r}{r+1} = \frac{n(n+1)(n+2)\cdots(n+r)}{(r+1)!}. \qquad (3.99)$$

In the *Gaṇita Kaumudi* a related formula[80] is used for example to calculate the total number of descendants of a cow after 20 years assuming that each cow calves every year beginning from the age of three.

Using Nārāyaṇa Paṇḍit's formula, the triangular sum above can be easily evaluated:

$$\sum_{k=1}^{n-1} \sum_{i=1}^{k} i = \frac{(n-1)n(n+1)}{1 \cdot 2 \cdot 3}. \tag{3.100}$$

where the double summation sign on the left has been used only as a notational convenience to save space and avoid rewriting the triangular sum all over again. Of course, this particular case could just as well have been evaluated by the formula in the *Āryabhaṭīya*, but the point is that Nārāyaṇa Paṇḍit's formula works to all orders.

Substituting the above value in (3.98), one obtains

$$\frac{(n-1)n(n+1)}{6} = \frac{n^2(n+1)}{2} - s_2, \tag{3.101}$$

whence

$$s_2 = \frac{n(n+1)(2n+1)}{6}, \tag{3.102}$$

and

$$\frac{s_2}{n^3} = \frac{1}{3} \quad (n \text{ sufficiently large}). \tag{3.103}$$

Again, for large n, only the leading order term (constant term) needs to be retained, and terms $O\ \frac{1}{n}$ or smaller can be discarded, as relatively non-representable for large n. This is a perfectly valid mathematical procedure, as noted earlier. However, in the notation of currently dominant (idealist) mathematical analysis, the last result would be rewritten as

$$\lim_{n} \frac{1^2 + 2^2 + 3^2 + \cdots + n^2}{n^3} = \frac{1}{3}. \tag{3.104}$$

From a knowledge of the sum (3.102) of the *varga-saṅkalitā*, one can compute the *ghana-saṅkalitā*. If we repeat the steps of the above derivation, we will run into a triangular sum of squares. The key point to notice is this: we have already, in the course of the above derivation, expressed the sum of squares using a *vārasaṅkalitā*. Hence, *a triangular sum of squares is nothing but a higher order vārasaṅkalitā*, which can be evaluated using Nārāyaṇa Paṇḍit's formula. The rest is a matter of elementary algebraic simplification. The algebra, too, is quite easy if we want to compute only the leading order term—adequate for the calculation to go through.

Computing the Sum of the kth Powers: Ghana-sankalitā

Explicitly, consider next the *ghana-saṅkalitā* or the series

$$s_3 = 1^3 + 2^3 + 3^3 + \cdots + n^3. \tag{3.105}$$

If each term in the *varga-saṅkalitā* series (3.95) had been multiplied by n, then we would have obtained

$$1^2 \cdot n + 2^2 \cdot n + 3^2 \cdot n + \cdots + n^2 \cdot n = s_2 \cdot n. \tag{3.106}$$

Write the *ghana-saṅkalitā* (in the original order) under the above series, and subtract, as before, to obtain

$$(n-1)\cdot 1^2 + (n-2)\cdot 2^2 + (n-3)\cdot 3^2 + \cdots + (n-n+1)\cdot(n-1)^2\cdot n = n\cdot s_2 - s_3. \quad (3.107)$$

The series on the left of the above equation can be written as a triangular sum consisting of $(n-1)$ occurrences of 1^2, $(n-2)$ occurrences of 2^2, $(n-3)$ occurrences of 3^2, etc., each occurrence being stacked vertically:

$$\begin{array}{l} 1^2 + 2^2 + 3^2 + \cdots + (n-3)^2 + (n-2)^2 + (n-1)^2 \\ 1^2 + 2^2 + 3^2 + \cdots + (n-3)^2 + (n-2)^2 \\ 1^2 + 2^2 + 3^2 + \cdots + (n-3)^2 \\ \vdots \\ 1^2 + 2^2 + 3^2 \\ 1^2 + 2^2 \\ 1^2. \end{array}$$

To express the remaining argument more compactly for a contemporary reader, we use a slight change of notation. The above can be rewritten as

$$n\cdot s_2(n) - s_3(n) = \sum_{j=1}^{n-1} s_2(j), \quad (3.108)$$

with the obvious notation that $s_2(n)$ is the sum of squares to n terms, etc. To evaluate the right-hand side, we need to evaluate the above triangular sum of squares. But, we have already expressed a sum of squares as a triangular sum of lower order:

$$s_2(n) = n\cdot s_1(n) - \sum_{k=1}^{n-1}\sum_{i=1}^{k} i \quad (3.109)$$

$$= n\cdot s_1(n) - \sum_{k=1}^{n-1} s_1(k) \quad (3.110)$$

$$= (n+1)\cdot s_1(n) - \sum_{k=1}^{n} s_1(k). \quad (3.111)$$

Further, we already have an expression for s_1, so that

$$(j+1)\cdot s_1(j) = \frac{j(j+1)^2}{2} \quad (3.112)$$

$$= \frac{1}{2}\left(j^3 + 2j^2 + j\right). \quad (3.113)$$

Hence, we obtain

$$
\begin{aligned}
n \cdot s_2(n) - s_3(n) &= \sum_{j=1}^{n-1} s_2(j) && (3.114)\\
&= \sum_{j=1}^{n-1} (j+1)s_1(j) - \sum_{j=1}^{n-1}\sum_{k=1}^{j} s_1(k) && (3.115)\\
&= \frac{1}{2}s_3(n-1) + s_2(n-1)\\
&\quad + \frac{1}{2}s_1(n-1) - \sum_{j=1}^{n-1}\sum_{k=1}^{j}\sum_{i=1}^{k} i. && (3.116)
\end{aligned}
$$

The last term on the right is just the *vārasaṅkalitā*, while the remaining terms are all known except for s_3 which can hence be evaluated. The actual evaluation is a now a simple but tedious matter of elementary algebra. But the tedium is considerably reduced if we retain only the leading order terms, and obtain

$$\frac{s_3}{n^4} = \frac{1}{4} \qquad (n \text{ sufficiently large}), \tag{3.117}$$

in the precise sense that the remaining terms are numerically non-representable or insignificant, or infinitesimal for n infinite (or as large as is physically possible). The fourth-order *varga-varga saṅkalitā*, the fifth-order *varga-ghana saṅkalitā*, and higher-order series can be evaluated in a way similar to the *ghana-saṅkalitā*, using the result for the preceding *saṅkalitā*. Expressed in present-day terminology, the conclusion is that, for large n,

$$\frac{1}{n^{k+1}} \sum_{1}^{n} i^k \approx \frac{1}{k+1}, \qquad k = 1,\ 2,\ 3, \cdots. \tag{3.118}$$

The Results

Substituting the results (3.118) in the earlier expression (3.84), and remembering that $n\Delta r = r$, we finally get the value of the circumference,

$$\frac{\text{circumference}}{8} = r - \frac{r}{3} + \frac{r}{5} - \frac{r}{7} + \frac{r}{9} - \cdots. \tag{3.119}$$

The basic series is expressed through the *śloka*[81]

व्यासे वारिधिनिहते रूपहृते व्याससागराभिहते ।
त्रिशरादिविषमसंख्याभक्तमृणां स्वं प्रिथक् क्रमात् कुर्यात् ॥ २.२७१ ॥

This may be translated as follows.[82]

> To the diameter multiplied by 4 alternately add and subtract in order the diameter multiplied by 4 and divided separately by the odd numbers 3, 5, etc.

This is described by the *Karaṇapaddhati* (VI, 1) as the accurate circumference. That is, if d is the diameter of the circle, then

$$\text{circumference} = 4d - \frac{4d}{3} + \frac{4d}{5} - \frac{4d}{7} + \cdots . \tag{3.120}$$

This corresponds to the value of π given by

$$\frac{\pi}{4} = 1 - \frac{1}{3} + \frac{1}{5} - \frac{1}{7} + \cdots . \tag{3.121}$$

This is the so-called Leibniz series. This series is *not* the best technique for calculating π, since the series (3.121) converges very slowly: some 10,000 terms are needed to obtain an accuracy of 4 decimal places. For an accuracy of four places after the decimal point, the above sum done on a computer needed to sum about 138,000 terms. Clearly, this sort of labour was impossible before digital computers, and, even with computers, one might have to pay some attention to the pile up of "rounding errors". (For 5 places after the decimals, a calculation done using double precision arithmetic is obviously good enough, since 10^5 oating point operations cannot propagate any "rounding errors" that far.)

This way of looking at things, however, overlooks some key points.

Deriving the Series Expansion for the Arctangent

First, once the idea was established, many other series expansions were obtained, and Whish has already recorded in 1832 a variety of fast-convergent expansions for π. In particular, Mādhava probably had obtained the series expansion for arctan, which involves only a slight extension of the above methods.

Referring back to Fig. 3.2, if Q is any point on the arc PQ_n, and if OQ is extended to meet the side square at P, then the *TantrasaṅgrahaVyākhyā/Yuktibhāṣā* states that an "equivalent argument" (*tulya nyāya*) shows that the arc PQ is given by replacing r, in the above expression (3.119), by PP. That is,

$$PQ = PP - \frac{PP}{3r^2} + \frac{PP}{5r^4} - \frac{PP}{7r^6} + \cdots . \tag{3.122}$$

If the arc PQ subtends an angle θ (= desired arc), and we use the notation $s = R\sin\theta$, $c = R\cos\theta$ (= *koṭijyā*), then we get from $PQ = r\theta$, and $PP = r\tan\theta = \frac{rs}{c}$, that

$$\text{arc } PQ = \frac{rs}{c} - \frac{rs}{3c}\cdot\frac{s^2}{c^2} + \frac{rs}{5c}\cdot\frac{s^4}{c^4} - \frac{rs}{7c}\cdot\frac{s^6}{c^6} + \cdots . \tag{3.123}$$

This is expressed by the *śloka*[83] for "arcification" of the sine:

इष्टज्यात्रिज्ययोर्घातात् कोट्याप्तं प्रथमं फलं॥ २.२०६॥

ज्यावर्गं गुणकं कृत्वा कोटिवर्गं च हारकम्।
प्रथमादिफलेभ्योऽथ नेया फलततिर्मुहुः॥ २०७॥

एकत्र्याद्योजसङ्ख्याभिर्भक्तेष्वेतेष्वनुक्रमात्।
ओजानां संयुतेस्त्यक्ते युग्मयोगे धनुर्भवेत्॥ २०८॥

दोः कोट्योरल्पमेवेष्टं कल्पनीयमिह स्मृतम्।

This may be translated:[84]

> The Rsine of the desired arc multiplied by the radius and divided by the Rcosine is the first result. Take the square of the Rsine as the multiplier, and the square of the Rcosine as the divisor, and multiply the first & etc. results to get the succeeding results. These are to be divided in order by the odd numbers, and the sum of the terms in even places is to be subtracted from the sum of the terms in the odd places. Remember to use the smaller of the two (Rsine and Rcosine) for this calculation.

It is clear that the expansion (3.123) is trivially equivalent to the more modern form

$$r\,\theta \;=\; r\tan\theta - \frac{r\tan^3\theta}{3} + \frac{r\tan^5\theta}{5} - \frac{r\tan^7\theta}{7} + \cdots, \qquad (3.124)$$

which, upon cancelling r, is the same as the "Gregory series" expansion for the arctan function:

$$\tan^{-1}\theta \;=\; \theta - \frac{\theta^3}{3} + \frac{\theta^5}{5} - \frac{\theta^7}{7} + \cdots. \qquad (3.125)$$

Deriving Rapidly Convergent Series for π

It is well known that the series (3.125) can be used to derive rapidly convergent expansions for π, using e.g. $\tan\frac{\pi}{6} = \frac{1}{\sqrt{3}}$, so that

$$\frac{\pi}{6} = \tan^{-1}\frac{1}{\sqrt{3}} = \frac{1}{\sqrt{3}}\left(1 - \frac{1}{3\cdot 3} + \frac{1}{5\cdot 3^2} - \frac{1}{7\cdot 3^3} + \cdots\right). \qquad (3.126)$$

This series requires only 9 terms for a precision of 4 decimal places. Small manipulations can be used to make the convergence even more rapid, and this was actually the way in which approximations to the value of π were calculated in Europe, by Sharp who in 1699 used "Gregory's" result to get 71 correct digits, by Machin who used a small improvement to get 100 correct digits, and whose method was used by de Lagny (1709, 112 digits), Vega (1789, 126 digits; 1799, 136 digits), Rutherford (1841, 152 digits; 1853, 440 digits), and Shanks (1873, 707 digits, of which 527 were correct). Indian mathematicians, however, being practical minded, computed π accurately to only the 11th decimal place, although 9 places were more than sufficient. (Much later, a mathematician of the Kerala school used exactly Mādhava's technique to calculate π to 17 decimal places, presumably as a demonstration.) Thus, the value of π is given by the *Karaṇapaddhati* (VI, 7), in the *kaṭapayādi* system which gives the circumference of a circle to be

चराडांशुचन्द्राधमकुंभिपाल

(31,415,926,536) for a radius of

अनूननून्नाननुन्ननित्यं

(10,000,000,000).

It is curious, however, that a similar figure of 10,000,000,000 is used for the radius by Christoph Clavius in his table of Rsines published at the beginning of the 17th c. CE, as is clear from its very title.

To complete this history, let us ask: exactly how well does the arctangent series (3.125) enable us to compute the value of π to the above precision?

Though the present-day answer to this question is quite elementary, and can be easily derived by any mathematician, this answer seems not to be properly known to many historians of mathematics, and may be especially difficult for those historians of mathematics who focus their expertise on languages and are unfamiliar with elementary numerical analysis. (Many such historians seem to exist today.) This elementary answer is explained in Appendix 3.B. One conclusion is that the value of π can be computed by hand to an accuracy of 10 places after the decimal, within an hour or so, using between 4 to 6 terms of the above series (3.125). This directly contradicts the conclusion of Srinivasiengar that the computation (of π) must have required a lot of labour: the point of the series expansion was to save labour, not expend it.

Secondly, though the treatment in Appendix 3.B builds on the method suggested in the calculus text of Lax et al., there is no great virtue to that method, except to illustrate what is required. Apart from elementary trigonometric identities, the key ingredient that goes into that method is an error estimate. The treatment in Appendix 3.B uses an error estimate that builds on the infinite sum of a geometric series. From the point of view of a contemporary text on calculus, like that of Lax et al., that is quite acceptable, since the sum of an infinite geometric series is today taught (though not explained) at quite an early stage (Std. 7 or 8). Indian mathematicians, also, long knew about geometric series, which they called more correctly as *gunottara saṅkalitā* or multiplicative series, and methods of summing the geometric series were a part of the elementary curriculum.[85] Even the use of infinite geometric series by Nīlakaṇṭha[86] has also long been known to historians. Of course, Indian mathematicians certainly knew how to carry out manipulations using elementary trigonometric identities. Therefore, the above approach could well have been used by some Indian mathematicians.

Nevertheless, for our immediate purposes, the error estimate used in Appendix 3.B is not the most appropriate. The reason is very simple. Our sources expound a different and more general method. While the above mentioned transformations of the series accelerate only the calculation of π, our sources describe a rather general technique to accelerate the convergence of a variety of slowly convergent series, and this general method of accelerating convergence could be used also for various other trigonometric computations. Some background is needed to understand this method of accelerating convergence.

In nite and Inde nite Series

As explained above, there were two sorts of series expansions in use: infinite and indefinite. An example of the infinite series expansion is Nīlakaṇṭha's expression for the sum of an infinite geometric series, or the computation of the sums of the above infinite serial, by computing the leading order terms, in which computation, the number of divisions of the circle are taken to be infinite. An example of indefinite series is the fraction series expansion, used by Brahmagupta. The idea of infinitesimal was a natural extension of the idea of rounding, using the additional notion of order of growth, and this is exactly how it is subsequently used: for infinite n the quantity $\frac{a}{n}$ is non-representable (*śūnya*) relative to b. This is quite similar to the statement (of non-standard analysis) that for n infinite, $\frac{a}{n}$ is infinitesimal, relative to b. It did not, however, require recourse to any of the complexities of non-standard analysis, since the operational definition of the equality of two numbers, with rounding arithmetic, took care of the rigour.

The sum of the indefinite series requires nothing special, since exact equality holds at each stage, until the exceptional term is dropped as non-representable. The precise meaning of the sum of an infinite series is found in the meaning assigned to $\frac{1}{n^{k+1}} \quad i^k$, which sum becomes constant for large n, when relatively non-representable terms are ignored (in the manner analogous to formal infinitesimals), based on order-counting. That is, operationally, one sums the series to n terms, and then discards (in relation to the *rūpa*, or constant term) the terms in the sum which have the *rāśi* n in the denominator. (Obviously, in all those cases the limit would exist, in the present-day sense.)

The Correction Term

However, the problem was not merely to prove that the series converged, but to calculate its sum. This was not a trivial task for a slowly convergent series like the "Leibniz" series. To actually calculate the sum, it was necessary to accelerate the convergence of the series, and this was done by adding to the infinite series, in analogy with the indefinite series, an exceptional or correction term. (It is interesting to notice how this rule-and-exception approach differs from the all-rule-no-exception approach used by Leibniz, for example, in thinking about the series.)

Since the basic series (3.121) is alternating, the exceptional or correction term (assumed positive) was hence to be added or subtracted according as the previous term was negative or positive. Thus, the full series actually looks as follows (after discarding the non-representable terms):

$$\frac{\pi}{4} = 1 - \frac{1}{3} + \frac{1}{5} - \frac{1}{7} + \cdots \quad \frac{1}{n} \quad f(n+1). \tag{3.127}$$

Some of the various forms of the correction term $f(n)$ that were tried were the following:

$$f_1(n) = \frac{1}{2n}, \tag{3.128}$$

$$f_2(n) = \frac{n}{2(n^2+1)} = \frac{\frac{1}{2}n}{n^2+1}, \tag{3.129}$$

$$f_3(n) = \frac{n^2+4}{2n(n^2+5)} = \frac{(\frac{1}{2}n)^2 + 1}{\frac{1}{2}n(n^2+4+1)}. \tag{3.130}$$

Thus, the quotation for (3.121) continues:[87]

व्यासे वारिधिनिहते रूपहृते व्याससागराभिहते।
त्रिशरादिविषमसंख्याभक्तमृणं स्वं प्रिथक् क्रमात् कुर्यात्॥ २.२७१॥
यत्संख्यात्र हरणे कृते निवृत्ता हृतिस्तु जामितया।
तस्या उर्ध्वगताया समसङ्ख्या तद्दलं गुणोऽन्ते स्यात्॥ २७२॥
तद्वर्गो रूपयुतो हारो व्यासाब्धि घाततः प्राग्वत्।
ताभ्यामाप्तं स्वमृणे कृते धने क्षेप एव करणीयः॥ २७३॥
लब्धः परिधिः सूक्ष्मो बहुकृत्वो हरणतोऽतिसूक्ष्मः स्यात्॥ २७४॥

This passage may be translated:

> To the diameter multiplied by 4 alternately add and subtract in order the diameter multiplied by 4 and divided separately by the odd numbers 3, 5, etc. That odd number at which this process ends, four times the diameter should be multiplied by the next even number, halved and [then] divided by one added to that [even] number squared. The result is to be added or subtracted according as the last term was subtracted or added. This gives the circumference more accurately than would be obtained by going on with that process.

$$\text{circumference} = \frac{4d}{1} - \frac{4d}{3} + \frac{4d}{5} - \frac{4d}{7} + \cdots \quad \frac{4d}{n} \quad 4d\frac{(n+1) \quad 2}{(n+1)^2+1}. \tag{3.131}$$

Similarly, we have[88]

एभ्यः सूक्ष्मतरोऽन्यो विलिख्यते कश्चनापि संस्कारः।
अन्ते समसंख्यादलवर्गः सैको गुणः स एव पुनः॥ २९५॥

युगगुणितो रूपयुतः समसंख्यादलहतो भवेद् हारः ।
त्रिशरादिविषमसंख्याहरणात् परमेतदेव वा कार्यम ॥ २९६ ॥

which may be translated as follows.

> A subtler method, with another correction. [Retain] the first procedure involving division of four times the diameter by the odd numbers, 3, 5, etc. [But] then add or subtract it [four times the diameter] multiplied by one added to the next even number halved and squared, and divided by one added to four times the preceding multiplier [with this] multiplied by the even number halved.

That is,

$$\text{circumference} = \frac{4d}{1} - \frac{4d}{3} + \frac{4d}{5} - \frac{4d}{7} + \cdots \frac{4d}{n} \\ 4d \frac{\left(\frac{n+1}{2}\right)^2 + 1}{\left(\frac{n+1}{2}\right)^2 + 1 \quad 4 + 1 \quad \left(\frac{n+1}{2}\right)}, \tag{3.132}$$

which simplifies to

$$\text{circumference} = \frac{4d}{1} - \frac{4d}{3} + \frac{4d}{5} - \frac{4d}{7} + \cdots \frac{4d}{n} \\ 4d \frac{\left(\frac{n+1}{2}\right)^2 + 1}{[(n+1)^2 + 4 + 1]\left(\frac{n+1}{2}\right)}. \tag{3.133}$$

Correction Term (Samskāra) and Acceleration of Convergence

In terms of present-day analysis, as described by Srinivasiengar,[89] the addition of such a correction term amounts to accelerating the convergence to a desired order (and appropriate terms can always be found to accelerate the convergence to any desired order). This analysis proceeds as follows.

Let $n = 4m + 1$, and let $S\left(\frac{n+1}{2}\right)$ and $S\left(\frac{n-1}{2}\right)$ denote the sums of the first $2m + 1$ and $2m$ terms of the uncorrected series, and let $T\left(\frac{n+1}{2}\right)$, $T\left(\frac{n-1}{2}\right)$ denote the corresponding corrected sums:

$$T \quad \frac{n+1}{2} = S \quad \frac{n+1}{2} - f(n+1) \tag{3.134}$$

$$T \quad \frac{n-1}{2} = S \quad \frac{n-1}{2} + f(n-1). \tag{3.135}$$

The corrected sums, $T\left(\frac{n+1}{2}\right)$, can be regarded as the partial sums of a series whose general term u_n is given by

$$u_n = T \quad \frac{n+1}{2} - T \quad \frac{n-1}{2} \quad . \tag{3.136}$$

That is,

$$u_n = \frac{1}{n} - f(n+1) - f(n-1). \tag{3.137}$$

Using the above for $n, n-2, n-4, \ldots, 3$, and alternately adding and subtracting, we obtain

$$-u_3 + u_5 - \cdots + u_n = -\frac{1}{3} + \frac{1}{5} - \cdots + \frac{1}{n} + f(2) - f(n+1), \tag{3.138}$$

so that one can just as well use u_n to obtain the value of the circumference, or, equivalently, the value of π:

$$\frac{\pi}{4} = 1 - f(2) - u_3 + u_5 - \cdots + u_n - \cdots . \tag{3.139}$$

However, this last series converges more rapidly, since, by choosing f appropriately, we can arrange things so that

$$u_n = O \quad \frac{1}{n^{2p+1}} \quad . \tag{3.140}$$

In contemporary mathematical language, we can easily understand as follows why this happens. To evaluate the right-hand side of (3.137) we momentarily suppose that the correction terms are functions of a real variable, and apply the "Taylor" series expansion to express both $f(n+1)$ and $f(n-1)$ in terms of the values of f and its derivatives at n. Then, we obtain

$$f(n+1) - f(n-1) = 2 \quad f(n) + \frac{f \ (n)}{2!} + \frac{f^{(4)}(n)}{4!} + \cdots \quad . \tag{3.141}$$

For the first correction term

$$2f(n) = \frac{1}{n}, \tag{3.142}$$

so that we have

$$u_n = \frac{1}{n} - f(n+1) - f(n-1) = -2 \quad \frac{f \ (n)}{2!} + \cdots \quad . \tag{3.143}$$

Since $2f(n) = \frac{1}{n}$,

$$u_n \approx f \ (n) \qquad O \quad \frac{1}{n^3} \quad . \tag{3.144}$$

To make it easier to carry out this calculation for the second term, we re-express it as

$$2f(n) = \frac{n}{(n^2+1)} = \frac{1}{n} - \frac{1}{n^3} + \frac{1}{n^5} - \frac{1}{n^7} + \cdots, \tag{3.145}$$

and assume that the (power) series may be differentiated term by term in its (annular) domain of convergence. Calculating the derivatives, putting them in (3.141), and substituting (3.141) in (3.137), we find

$$u_n \approx 2f^{(4)}(n) \qquad O \quad \frac{1}{n^5} \quad . \tag{3.146}$$

In this case, the $\frac{1}{n^3}$ term of $f\ \ (n)$ cancels with the $\frac{1}{n^3}$ term of $f(n)$, because of the way in which $f(n)$ has been chosen.

Similarly, we may re-express the third correction term as

$$2f(n) = \frac{n^2+4}{n(n^2+5)} = \frac{1}{n} - \frac{1}{n^3} + \frac{5}{n^5} - \frac{5^2}{n^7} + \cdots . \tag{3.147}$$

In this case all terms up to the fifth order cancel, and we are left with

$$O\,(u_n) = O \quad f^{(6)}(n) \quad = O \quad \frac{1}{n^7} \quad . \tag{3.148}$$

Obviously, these cancellations are not fortuitous—they depend upon the choice of $f(n)$—and we shall see later on how the correction terms were actually derived. We note that correction terms can again be applied to the modified series to further accelerate convergence, and Srinivasiengar provides examples of how this was actually done.

The Samskāra Term and Transformed Series

The modified series u_n, which are faster convergent, are explicitly worked out in various texts such as the *TantrasaṅgrahaVyākhyā* For example, the term (3.128) gives the value

$$u_n = -\frac{1}{n(n^2-1)}, \tag{3.149}$$

and the corresponding series is given by the *śloka* in the *TantrasaṅgrahaVyākhyā*[90] as:

व्यासाद् वारिधिनिहतात् पृथगाप्तं त्र्याद्ययुग्विमूलघनैः ।
त्रिघ्नव्यासे स्वमृणं क्रमशः कृत्वापि परिधिरानेयः ॥ २९० ॥

This may be translated:[91]

> Four times the diameter is divided by the cubes of [odd numbers] 3, etc., minus the numbers [lit. roots], to obtain separate quotients. To thrice the diameter, alternately add and subtract [the quotients], to obtain the circumference.

The corresponding series for π is

$$\frac{\pi-3}{4} = \frac{1}{3^3-3} - \frac{1}{5^3-5} - \frac{1}{7^3-7} + \cdots . \tag{3.150}$$

Similarly, the term (3.129) gives the value

$$u_n = \frac{4}{n\left[(n-1)^2 + 1\right]\left[(n+1)^2 + 1\right]}, \tag{3.151}$$

corresponding to the *śloka* in the *TantrasaṅgrahaVyākhyā*:[92]

समपञ्चा [हतयो या] रूपाद्ययुजां चतुःघ्नमूलयुताः ।
ताभिः षोडशगुणिताद् व्यासात् पृथगाहतेषु विषमयुतेः ॥ २८७ ॥

समफलयोगे त्यक्ते स्यादिष्टव्याससम्भवः परिधिः ॥ २८८ ॥

which may be translated as follows.[93]

> The fifth powers of 1, etc., plus four times the number; with that, divide 16 times the diameter separately for successive odd numbers and alternately add and subtract. The circumference is obtained for the desired diameter.

The corresponding series for π is

$$\frac{\pi}{16} = \frac{1}{1^5 + 4 \cdot 1} - \frac{1}{3^5 + 4 \cdot 3} + \frac{1}{5^5 + 4 \cdot 5} \cdots . \tag{3.152}$$

Various other manipulations of the basic series (3.121) all proceed similarly, such as

$$\frac{\frac{\ }{4} - \frac{4}{5}}{4} = -\frac{1}{3(2^2+1)(4^2+1)} + \frac{1}{5(4^2+1)(6^2+1)} + \cdots , \tag{3.153}$$

and a full catalogue of these series would take us too far afield.

Samskāra and Sthaulya

The use of the correction term implicitly or explicitly involved a "Taylor" series expansion, and the summation of this series for a variety of functions. This refutes a claim that Indian mathematics used these infinite series only for trigonometric functions.

The exact method of deriving the correction term (called *saṁskāra*) and its "error" (called *sthaulya* = grossness) is better explained in the *TantrasaṅgrahaVyākhyā/Yuktidīpikā*, and the *Kriyākramakarī*[94] both attributed to Śaṅkara Vāriyar, wherein the process has been attributed to "the teacher". A somewhat similar (but incomplete) explanation is also given in the editorial notes of Rama Varma and Akhilshwara Aiyar, in their edition of the *Yuktibhāṣā*, which used the same source. We will, however, refer to the original Sanskrit source rather than the more recent Malayalam commentary, for the reasons already indicated.

The Number of Terms to be Summed

Another way to look at the rate of convergence is in terms of the number of terms to be summed: the faster the series converges, the fewer the terms that are required to obtain its sum. How *many* terms of the series are to be summed? As explained in the *Kriyākramakarī*,[95] this is to be decided by the n for which the partial sums become constant, so that they satisfy

$$S(n) = S(n+1), \tag{3.154}$$

to the required level of precision. That is, $S(n) = S(n+1)$ with equality in the usual sense that non-representables are ignored. (In particular, as already stated, there is no question of trying to sum an infinite number of terms or trying to assign an idealized "meaning" to such a supertask.) We note also that the above practical criterion closely resembles the theoretical Cauchy criterion for convergence, although in present-day terminology, it would seem that the above constitutes only a necessary rather than sufficient condition for convergence, since the difference is considered only between *two* successive terms. The logic, as explained in the *Kriyākramakarī*, is that if the above equality holds for any two consecutive integers, it will hold thereafter. This justification shows that the real requirement is that the sum of the series should become constant, up to non-representable terms, as in the case of the geometric series. The argument—that if constancy holds for two terms it will hold thereafter—is certainly valid for the particular series that are considered. Though the "proof" of this argument has not been separately recorded, going through the above derivation of the series makes it clear why this should be so. The above criterion provides a means of fixing the correction term.

The Functional Equation

To understand how the correction term was originally derived, it helps to change the notation slightly, to bring it closer to the actual notation used in the text, as has also been done by Hayashi et al.,[96] and to rewrite the basic series as follows.

$$\begin{aligned}\frac{\pi}{4} &\approx 1 - \frac{1}{3} + \frac{1}{5} - \frac{1}{7} + \cdots + (-1)^{n-1}\frac{1}{2n-1} + (-1)^n F(n) \\ &= S(n) + (-1)^n F(n), \end{aligned} \tag{3.155}$$

where $S(n)$ denotes the sum of the first n terms of the series. Then the above requirement is equivalent to

$$F(n) + F(n+1) = \frac{1}{2n+1}. \tag{3.156}$$

This is the basic functional equation that must be solved. The correction terms, which approximately solve this functional equation, may be re-expressed rather more neatly in the new notation, in which n is twice what it was earlier, as:

$$F_1(n) = \frac{1}{4n}, \tag{3.157}$$

$$F_2(n) = \frac{n}{4n^2 + 1}, \tag{3.158}$$

$$F_3(n) = \frac{n^2+1}{n(4n^2 + 5)}. \tag{3.159}$$

The Continued Fraction Expansion

The functional equation (3.156) is actually solved in the *Kriyākramakarī* by means of a continued fraction expansion, and all three *saṁskāra* terms are actually derivable from the continued fraction expansion:[97]

$$F(n) = \frac{1}{4n+}\ \frac{1}{n+}\ \frac{1}{n+}\cdots. \tag{3.160}$$

The three *saṁskāra* terms given above are exactly the first three convergents to this continued fraction.

This point made also by Hayashi et al. is identical with what had been earlier explained, without textual support, by Rajagopal and Rangachari. In terms of the earlier notation, Rajagopal and Rangachari point out that the correction function $f(n)$, which renders exact the equation

$$\frac{\pi}{4} = 1 - \frac{1}{3} + \frac{1}{5} - \frac{1}{7} + \cdots \quad \frac{1}{n} - \quad f(n+1), \tag{3.161}$$

can be represented by means of the continued fraction

$$f(n) = \frac{1}{2} + \frac{1}{n+}\ \frac{1^2}{n+}\ \frac{2^2}{n+}\ \frac{3^2}{n+}\cdots. \tag{3.162}$$

Rajagopal and Rangachari unfortunately refer to this as "round off". The functions $f_1(n)$, $f_2(n)$, $f_3(n)$, considered earlier, are exactly the first three convergents to this continued fraction. Rajagopal and Rangachari attribute to D. T. Whiteside a numerical calculation of the first 10 terms of the series, with the correction term f_3, which gives a value differing from π only by 1 in the 8th place after the decimal, while a calculation of the first 25 terms, with the same correction term, gives a value that differs from π only in the 12th decimal place.

An interesting thing about the above continued fraction is that, for $n = 2$, it gives a value of π in terms of the continued fraction:

$$\frac{2}{4-\pi} = 2 + \frac{1^2}{2+}\ \frac{2^2}{2+}\ \frac{3^2}{2+}\cdots. \tag{3.163}$$

As Rajagopal and Rangachari point out, this continued fraction was used by William Brouncker in his 1654 reworking of John Wallis' related continued product. Likewise, through some minor modifications, one can obtain the expansion

$$\frac{\pi-2}{4} = \frac{1}{2}\cdot\cfrac{1}{1+\cfrac{1\cdot 2}{1+\cfrac{2\cdot 3}{1+\cfrac{3\cdot 4}{1+\cdots}}}} \tag{3.164}$$

used by Leonhard Euler in 1739, and published in 1750. This, however, is NOT anachronistic, as Rajagopal and Rangachari state, for, as already noted, Euler was well aware of Indian astronomical works. In particular, transmission in the case of both Wallis and Euler needs to be studied a lot more carefully.

How the Samskāra Term Was Obtained

There still remains the question of the way in which the *saṁskāra* correction term was originally obtained. Here, as already stated, the analysis uses the concept of *sthaulya*, or the grossness of the correction term. This is the difference $S(n) - S(n-1)$. The best choice of $F(n)$ is that which makes this difference zero. As we have already seen, the actual choice of the correction term corresponds to the continued fractions which are exactly the convergents to that best choice of $F(n)$.

How were these obtained? I do *not* think that the choice of $F(n)$ was arrived at by searching inductively for a general pattern, as has been suggested by Youskevich. The question of induction vs deduction, emphasized also by Hayashi et al., has already been exhaustively examined in Chapter 2, where we have already seen the incorrectness of asserting the superiority of deduction. More to the point is the distinction we have earlier drawn between a truly goal-directed procedure and a mechanical procedure. Practically speaking, as we have also seen, Indian mathematics sought practical rules rather than formal rules of ever greater generality, and there is no practical way to make the error zero.

The argument in the *Kriyākramakarī* proceeds as follows.

Step 1: If the difference $S(n) - S(n-1)$ is to be exactly zero, then we would have

> **अतो ययाकयाचिद् विषमसंख्यया लब्धेन फलेन तदुत्तरसंस्कारफलस्य पुर्वसंस्कारफलस्य च योगो यथा तुल्यो भवेत् तथा संस्कारः कर्तव्यः।**
>
> the quotient obtained by dividing one by any odd number should equal the sum of the earlier (*pūrva*) *saṁskāra* and later (*uttara*) *saṁskāra*: this is the duty of the *saṁskāra*.[98]

$$F(n) + F(n+1) = \frac{1}{2n+1}. \tag{3.165}$$

Thus, the ideal *saṁskāra* would satisfy $F(n) = F(n+1) = \frac{1}{2(2n+1)}$. But this is asserted to be not possible, for if the first *saṁskāra* were the reciprocal of twice an odd number, then the other *saṁskāra* must be the reciprocal of twice the *corresponding* odd number. Thus, the possibility of making the error (grossness) zero is rejected. This argument is not fully intelligible until it is pointed out that the tacit assumption here is that the *saṁskāra* $F(n)$ must be a rational function of the *rāśi* (n). We explain later on why this may be legitimately assumed.

If it is not practically possible to make the error zero, then one must choose that *saṁskāra* which "minimizes" the error or the grossness (*sthaulya*). But, how does one carry out this "minimization"?

Step 2: As a trial solution, close to the above "ideal" value, $\frac{1}{4n+2}$, we are, therefore, asked to consider

$$F(n) \;=\; \frac{1}{4n}. \tag{3.166}$$

To compute the *sthaulya*, a "novel" place-value notation is used in the *Kriyākramakarī* to express polynomials, and rational functions. As explained, each place denotes the successive powers such as "the square, cube, fourth, fifth and sixth powers". Thus, [1,0] means that the first power of the variable (*rāśi*) has a coefficient of 1, and the constant (*rūpa*) is zero, i.e., the polynomial $1 \cdot x \,+\, 0$, which is nothing but the *rāśi* itself.

$$[1,0] \qquad 1 \cdot x \,+\, 0 \,= x.$$

This is, of course, a straightforward extension of the usual place value notation for numerals where coefficients of the various powers of 10 are expressed by places, without stating explicitly the powers of 10. The novelty arises only from the training imparted in schools today, where students at an early stage are taught to put the symbol x and explicitly indicate its powers, to express a polynomial. Negative coefficients are denoted, as is customary, by putting a small 0, like a degree symbol on top of the number. This notation extends also to rational functions by using what we would today call a table with two rows. Thus, $\frac{[1\ \ 0\ \ 0]}{[0\ \ 1^{\circ}\ \ 0]}$ is the same thing as $\frac{x^2}{-x}$, which is the negative of the *rāśi*.

$$\frac{[1\ \ 0\ \ 0]}{[0\ \ 1^{\circ}\ \ 0]} \qquad \frac{x^2}{-\,x} \;=\; -x.$$

Incidentally, Hayashi et al. are completely wrong in maintaining that this notation cannot be used to express factors: for example, $(x+1)(x+2)$ could perfectly well be expressed as $[1,1] \;\times\; [1,2]$, etc., exactly as one expresses the multiplication of two numbers using positional notation. Though this is certainly not a limitation of the notation, which is perfectly general, it is another matter that the terms actually occurring in the *Kriyākramakarī* are all fully expanded, as was thought to be the proper way to express the final result whether an arithmetical one or an algebraic one.

The key point here is this: using this novel place-value system, not only were rational functions represented in a way analogous to rational numbers, but non-rational functions were also treated, like non-rational numbers, using a sequence of fractions or a continued fraction. Hence, the tacit assumption that the correction term must be given by a rational function is only the first step of the argument, and not a limitation to it.

Taking $m = 2n + 1$, so that $4n = 2m - 2$, $4(n+1) = 4n + 4 = 2m + 2$, and the above trial for $F(n)$, Śaṅkara explains that the teacher found that

$$F(n) = \frac{1}{2m-2} = \frac{2m^2 + 2m}{4m^3 - 4m}, \tag{3.167}$$

$$F(n+1) = \frac{1}{2m+2} = \frac{2m^2 - 2m}{4m^3 - 4m}, \tag{3.168}$$

$$\frac{1}{2n+1} = \frac{1}{m} = \frac{4m^2 - 4}{4m^3 - 4m}. \tag{3.169}$$

The last term, for instance, is expressed in the *Kriyākramakarī*[99] as $\frac{[4 \;\; 0 \;\; 4^\circ]}{[4 \;\; 0 \;\; 4^\circ \;\; 0]}$. The *sthaulya* or error is now readily computed as (4 times the diameter times)

$$F(n) + F(n+1) - \frac{1}{2n+1} = \frac{4}{4m^3 - 4m}. \tag{3.170}$$

The author of the *Kriyākramakarī* (Śaṅkara Vāriyar) now says that "on seeing this *sthaulya*, the teacher was not satisfied".

Step 3: The right-hand side of (3.170) above ought to have been zero, but it is positive. This error shows that the correction is a little in excess (*kincid adhik*), of what is required, and hence the correction $F(n)$ must be diminished, so that the denominator of $F(n)$ must be increased. Therefore, as a second trial solution, the teacher considered the possibility

$$F(n) = \frac{1}{4n+1}. \tag{3.171}$$

Proceeding exactly as before (and disregarding the baseless speculations about notation by Hayashi et al.) we have $m = 2n + 1$, so that $4n + 1 = 2m - 1$, $4(n+1) + 1 = 4n + 5 = 2m + 3$,

$$F(n) = \frac{1}{2m-1} = \frac{2m^2 + 3m}{m(2m-1)(2m+3)}, \tag{3.172}$$

$$F(n+1) = \frac{1}{2m+3} = \frac{2m^2 - 1m}{m(2m-1)(2m+3)}, \tag{3.173}$$

$$\frac{1}{2n+1} = \frac{1}{m} = \frac{4m^2 + 4m - 3}{m(2m-1)(2m+3)}, \tag{3.174}$$

and the *sthaulya* works out to be

$$F(n) + F(n+1) - \frac{1}{2n+1} = \frac{-2m + 3}{4m^3 + 4m^2 - 3m}. \tag{3.175}$$

Comparison of this error (r.h.s. of (3.175)) with the preceding one (r.h.s of (3.170)) requires a clear knowledge of the rate of growth of various rational functions.

Step 4: To this end, the *Kriyākramakarī* now makes a key observation. To understand this observation let us first note that the above error in (3.175) is negative. This indicates that the correction is in excess, and must be reduced. So the *Kriyākramakarī* explains the logic of the teacher by saying that "all of unity should not be added" to the previous trial correction, i.e., only a fraction must be added. However, nothing very much changes if we add a fraction like 1 2 or 1 3, or try, say, $F(n) = \frac{1}{4n+\frac{1}{5}}$. Why? The *Kriyākramakarī* explains this by saying that the numerator of the *sthaulya* term in (3.175) has now reached the "place of the *rāśi* (variable)", while the numerator of the earlier *sthaulya* in (3.170) had only the "place of the *rūpa* (constant)". This is the *Kriyākramakarī* way of stating that the numerator now grows faster, while the two denominators grow at the same rate, so that the error now grows faster.

Hayashi et al. have unfortunately missed the significance of this key observation, and have consequently lost the thread of the argument. They say that "Śaṅkara continues with an enigmatic expression", and proceeds to consider

$$F(n) = \frac{1}{4n + \dfrac{1}{n}}, \tag{3.176}$$

without offering any further explanation. Hayashi et al. then incorrectly accuse Śaṅkara of having resorted to "induction" after having tried and failed to provide a "deductive" approach. First, the author of the *Kriyākramakarī* is only trying to explain the logic used by his teacher, and this argument in the *Kriyākramakarī* certainly cannot be attributed to Śaṅkara, who comes later. Secondly, this "induction–deduction" dichotomy, as stated several times earlier, is an incorrect yardstick obsessively used by Western historians to try and establish Western superiority in mathematics, and is irrelevant to Indian tradition. More to the point: present-day mathematical proof is, in principle, addressed to a machine, and is expected to be so detailed that it can, in principle, be mechanically checked, without the application of intelligence. This was not the case in Indian tradition which aimed to be succinct and expected the student to exercise his or her intelligence.

To explain the argument in the present-day manner, it is clear from the above considerations that neither unity nor a constant fraction can be added to the denominator. Something, however, *must* be added to the denominator to reduce the error. Since that fraction cannot be a constant, it must be a variable (i.e., it must involve the *rāśi*). It is now obvious that what is being stated is that one needs to add a fraction with the *rāśi* in the denominator. Accordingly, no further explanation is given, since no further explanation is necessary, except to compute the error with the new trial function. As in Step 1 above, the *Kriyākramakarī* now proceeds to explain what happens if the fraction $\frac{4}{n}$ is added to the denominator:

$$F(n) = \frac{1}{4n + \dfrac{4}{n}}. \tag{3.177}$$

The error in this case is approximately computed. The key point of interest, from the present-day perspective, is the use of the relation, $2m \quad 2 \approx 2m$, which again involves a consideration of order of growth, this time quite explicitly.

Step 5: Since the *sthaulya* with this *saṁskāra*, too, is not satisfactory, the added fraction is reduced:

$$F(n) = \frac{1}{4n + \frac{1}{n}}. \tag{3.178}$$

Proceeding as above, the *sthaulya* in this case works out to be

$$\frac{1}{2n+1} - F(n) - F(n+1) = \frac{16}{4m^5 + 16m}, \tag{3.179}$$

which last quantity is expressed in the *Kriyākramakarī*[100] as $\frac{[16]}{[4\ 0\ 0\ 0\ 16\ 0]}$.

It is now amply clear how the minimization process proceeds iteratively, at each step, exploring the bounds by computing the error in two cases, one excess and one deficit, and adding a fraction (containing the variable [*rāśi*]) to the denominator. Any further explanation would invite the charge of prolixity, so no further explanation is considered necessary. It is also clear how the continued fraction is no artificial construct, but arises very naturally as a part of this iterative minimization process.

We note particularly, how the minimization was achieved by the simple process of order counting. Thus, **the grossness (*sthaulya*) of the correction (*samskāra*) is iteratively minimized by finding, at each stage, that largest continued fraction which gives the *lowest order* of growth for the difference** $S(n) - S(n-1)$. This analysis obviously remains unaffected by present-day definitions of convergence. It would still go through in much the same way.

VI
CONCLUSIONS

1. Finite differences and series expansions were in use in India since the time of Āryabhaṭa in the 5th c.

2. The numerical solution of difference equations ("Euler solver") was used (as a superior alternative to the "fundamental theorem of calculus") since the time of Āryabhaṭa, and it was through this process of numerical integration that the volume of a sphere was first accurately derived in India, as explained by Bhāskara II.

3. Differentiation was carried out for complicated functions, since Brahmagupta, and Bhāskara II relates this to the instantaneous velocity of the "planets" on the Indian planetary model.

4. By extending linear (Āryabhaṭa) to quadratic (Bhāskara I) to higher order interpolation (Mādhava), the series expansions in India developed over a thousand year period into a systematic method of interpolation via high order polynomials, which came to

be known as the "Taylor expansion" several centuries later (when these very same series expansions abruptly started appearing in Europe).

5. Although accuracy to the third minute was first attempted in the early ninth century, it was achieved only after another five centuries. Hence the above long-drawn process was the work of the Āryabhaṭa school, and continuously involved inputs from various regions across India up to the 14th c. CE.

6. From the time of the *śulba sūtra*-s there was a clear understanding of real numbers, which were understood realistically in Indian tradition (rather than metaphysically as Dedekind millennia later attempted to understand them).

7. Order counting and the usual discarding of non-representables was used to clearly comprehend and evaluate limiting values of a variety of rational functions, expressed using a novel place value notation. (This was in contrast to the Western idealistic tradition of mathematics which could not comprehend this process in terms of the perfection it attributed to mathematics, which purported perfection did not allow it to discard the smallest quantity.)

8. Infinite series, like the geometric series, were deemed summable, and summed, since the sum became constant up to non-representables. (This was in contrast to the Western tradition of mathematics, which, for long, saw the summing of infinite series as involving a supertask, and then regarded it as purely a matter of formal definition.)

9. The interplay of infinite with indefinite series led to the introduction of exceptional terms in infinite series. The introduction of these exceptional terms was equivalent to transforming the series to accelerate convergence. The transformed series were explicitly worked out. (This was in contrast to the all-rule-no-exception understanding of these same infinite series in Europe.)

10. The exceptional terms were derived by a technique of iterative minimization which has been overlooked in the Western historians' semi-religious obsession with the issue of induction vs deduction.

11. For the above understanding, there was valid *pramāṇa* at every step. (This was in contrast to Newton and Leibniz who ritualistically attempted proof, but could not provide a valid proof either by current standards or by the standards acceptable to their contemporaries.)

Hence, what developed in India was the calculus—epistemologically more secure than the half-digested proto calculus to which various European mathematicians of the 17th c. CE incorrectly laid claim.

APPENDIX 3.A
EULER–MACLAURIN SUM FORMULA

The "Euler-Maclaurin" expansion attributed to Euler[101] and Maclaurin,[102] gives a formula for approximating a definite integral, using the sum of the areas of quadrilaterals and a correction term, as explained, for example, in Whittaker and Robinson.[103] This formula is of some historical interest since Gregory[104] used a similar formula for numerical integration, with finite differences in place of derivatives. The basic idea remains that of approximating a function by a piecewise linear curve.

$$
\begin{aligned}
\frac{1}{w}\int_a^{a+rw} f(x)dx &= \frac{1}{2}f_0 + f_1 + f_2 + \cdots + f_{r-1} + \frac{1}{2}f_r \\
&\quad - \frac{B_1 w}{2!}(f_r - f_0) + \frac{B_2 w}{4!}(f_r - f_0) \\
&\quad - \frac{B_3 w^5}{6!}\ f_r^{(5)} - f_0^{(5)} \quad + \cdots
\end{aligned} \tag{3.180}
$$

Here, $f_i = f(a+iw)$, primes denote derivatives, and $f_i^{(k)} = f^{(k)}(a+iw)$, while the B_i are Bernoulli numbers, in old notation,

$$
\begin{aligned}
B_1 &= \frac{1}{6}, \quad B_2 = \frac{1}{30}, \quad B_3 = \frac{1}{42}, \\
B_4 &= \frac{1}{30}, \quad B_5 = \frac{5}{65}, \quad B_6 = \frac{691}{2730}, \ldots.
\end{aligned} \tag{3.181}
$$

Taking, $a = 0$, $w = 1$, $f(x) = x^p$, we get

$$
\begin{aligned}
\frac{r^{p+1}}{p+1} &= 1^p + 2^p + 3^p + \cdots + (r-1)^p + \frac{1}{2}r^p \\
&\quad - \frac{1}{12}p\,r^{p-1} + \frac{1}{720}p(p-1)(p-2)r^{p-3} \\
&\quad - \frac{1}{30240}p(p-1)(p-2)(p-3)(p-4)r^{p-4} + \cdots.
\end{aligned} \tag{3.182}
$$

Consequently,

$$
\begin{aligned}
1^p + 2^p &+ 3^p + \cdots + (r-1)^p + r^p = \frac{r^{p+1}}{p+1} + \frac{1}{2}r^p \\
&+ \frac{p}{12}r^{p-1} - \frac{p(p-1)(p-2)}{720}r^{p-3} \\
&+ \frac{p(p-1)(p-2)(p-3)(p-4)}{30240}r^{p-5} - \cdots.
\end{aligned} \tag{3.183}
$$

The series terminates with the last term in either r or r^2. Thus, for example,

$$
1^5 + 2^5 + 3^5 + \cdots + n^p = \frac{1}{6}n^6 + \frac{1}{2}n^5 + \frac{5}{12}n^4 - \frac{1}{12}n^2. \tag{3.184}
$$

As can be seen, the leading-order term is always $\frac{n^{p+1}}{p+1}$. The formula was first printed in James Bernoulli's work *Ars Conjectandi,* posthumously published in 1713 (p. 97). Bernoulli obtained his result for up to $p = 10$, using the figurate number triangle (very similar to Pascal's triangle).[105]

In present-day notation, Bernoulli numbers are defined a bit differently.

$$\frac{x}{e^x - 1} = \sum_{n=0} \frac{B_n x^n}{n!}, \tag{3.185}$$

so that B_n is the coefficient of $\frac{x^n}{n!}$ in the expansion of $\frac{x}{e^x-1}$. The relationship of the old and new notation is $B_{2n} = (-1)^n B_n$. We can also view the Bernoulli numbers as coefficients in the expansion of $\theta \cot \theta$ (for $-\pi \quad \theta \quad \pi$).

$$\theta \ \cot \theta = \sum_{n=0} (-1)^n B_{2n} \frac{\theta^{2n}}{(2n)!}. \tag{3.186}$$

This, incidentally, opens a possibility of a connection to the work of Ramanujam[106] who derived a number of curious relations between Bernoulli numbers using infinite series.

The new notation allows us to express the sum of $\quad i^p$ more neatly as follows.

$$\begin{aligned} \sum_{i=1}^{n} i^p &= \frac{1}{p+1} \sum_{k=1}^{p+1} (-1)^{(p+1-k)} \ \begin{matrix} p+1 \\ k \end{matrix} \ B_{p+1-k} n^k \\ &= \frac{n^{p+1}}{p+1} + \frac{1}{p+1} \sum_{k=1}^{p} (-1)^{(p+1-k)} \ \begin{matrix} p+1 \\ k \end{matrix} \ B_{p+1-k} n^k. \end{aligned} \tag{3.187}$$

We see that the leading term is always $\frac{n^{p+1}}{(p+1)}$.

APPENDIX 3.B
COMPUTATION OF ARCTANGENT USING THE SERIES: HOW ONE MIGHT DO IT TODAY

Step 1: Large y ($y > 1$). From the elementary identities $\sin\left(\frac{\pi}{2} - x\right) = \cos x$, and $\cos\left(\frac{\pi}{2} - x\right) = \sin x$, we have the relation $\tan\left(\frac{\pi}{2} - x\right) = \frac{1}{\tan x}$. Setting $y = \tan x$, and taking arctan of both sides, we have the elementary relation

$$\arctan y = \frac{\pi}{2} - \arctan\frac{1}{y}. \tag{3.188}$$

Thus, the computation of any value of arctan for $y > 1$ can always be reduced to a computation of $\arctan y$ for $y < 1$. For $y < 1$ the series (3.125) obviously converges faster, and this is the only case that it is necessary to examine.

Step 2: Small y ($0 < y < 0.1$). To estimate exactly how fast the series converges is an elementary matter. Starting from the identity

$$\arctan y = \int_0^y \frac{1}{1+x^2}\,dx, \tag{3.189}$$

we approximate the integrand by the *finite* geometric series

$$S_n(x) = \sum_{k=0}^{n}(-1)^k x^{2k} = 1 - x^2 + x^4 - \cdots + (-1)^n x^{2n}. \tag{3.190}$$

Clearly, integrating the finite series term by term,

$$\begin{aligned}\int_0^y S_n(x)dx &= y - \frac{y^3}{3} + \frac{y^5}{5} + \cdots + (-1)^n \frac{y^{2n+1}}{2n+1}\\ &= T_n(y).\end{aligned} \tag{3.191}$$

From the formula for the sum of a geometric series, we then have

$$\frac{1}{1+x^2} - S_n(x) = \sum_{k=n+1}(-1)^k x^{2k} = \frac{x^{2n+2}}{1+x^2}. \tag{3.192}$$

Hence,

$$\begin{aligned}\arctan y - T_n(y) & \int_0^y \frac{1}{1+x^2} - S_n(x)\;dx\\ &= \int_0^y \frac{x^{2n+2}}{1+x^2}dx\\ & \int_0^y x^{2n+2}dx = \frac{y^{2n+3}}{2n+3}.\end{aligned} \tag{3.193}$$

Thus, for $y = 0.1$, if the series (3.125) is used to approximate the arctangent, we can expect an accuracy of nearly 10 places after the decimal (error $< \frac{10^{-9}}{9}$) simply for $n = 3$.

That is, for $y = 0.1$, we can use the easily computed approximation

$$\arctan y \approx y - \frac{y^3}{3} + \frac{y^5}{5} - \frac{y^7}{7} \tag{3.194}$$

for an accuracy of nearly 10 places after the decimal point.

As is clear from the case of sine and cosine, the actual computations typically used the "Taylor" polynomials of the 11th or 12th order, so that we could add two more terms to the above,

$$\arctan y \approx y - \frac{y^3}{3} + \frac{y^5}{5} - \frac{y^7}{7} + \frac{y^9}{9} - \frac{y^{11}}{11}, \tag{3.195}$$

to ensure the required precision, allowing for rounding.

Step 3: Intermediate y ($0.1 < y \quad 1$). It remains to consider the case of a y (such as $y = \frac{1}{3}$) where we have $0.1 < y < 1$. This case can be easily reduced to the case in Step 2, as follows. From the addition formulae for the sine and cosine functions, so well known from the time of Āryabhaṭa, we can easily get the addition formula for the tangent function:

$$\tan(x - x\ ) = \frac{\tan x - \tan x}{1 + \tan x \quad \tan x}. \tag{3.196}$$

Putting $y = \tan x$, and $y \quad = \tan x$, and taking arctangent of both sides, we have

$$x \ - \ x \quad = \arctan \frac{y - y}{1 + yy}, \tag{3.197}$$

i.e.,

$$\arctan y = \arctan y \quad + \arctan \frac{y - y}{1 + yy}. \tag{3.198}$$

That is,

$$\arctan y = \arctan y \quad + \arctan y_1, \tag{3.199}$$

where

$$y_1 = \frac{y - y}{1 + yy}. \tag{3.200}$$

Taking $y \quad = 0.1$, we see that the computation of $\arctan y$ has been reduced to the case of the computation of $\arctan y_1$, where y_1 is clearly a number such that $y_1 < y - 0.1$. If it so happens that $y_1 < 0.1$, then we can compute $\arctan y_1$ by using the four-term series expansion of Step 2. Otherwise, we repeat the above procedure to obtain

$$\arctan y_1 = \arctan y \quad + \arctan y_2, \tag{3.201}$$

where

$$y_2 = \frac{y_1 - y}{1 + y_1 y}. \tag{3.202}$$

Clearly, $y_2 < y_1 - 0.1 < y - 0.2$. Proceeding in this manner, we see that in at most 9 steps we must arrive at a $y_n < 0.1$, which can be evaluated as in Step 2. This value would then be connected to the desired arctan value by

$$\arctan y \;=\; n \arctan y \;\; + \; \arctan y_n. \tag{3.203}$$

Thus, the case of a y between 1 and 0.1 requires only slightly more labour.

As an example, let us see how π can be computed by hand today, using the series expansion (3.125) and the above procedure. We know from the most basic table of six sine and cosine values that $\tan\left(\frac{\ }{6}\right) \;=\; \frac{1}{3}$, which gives

$$\pi \;=\; 6 \arctan \frac{1}{\sqrt{3}}. \tag{3.204}$$

Here, $y \;=\; \frac{1}{3} \;= 0.57735026918963$ (correct to 12 places after the decimal). Applying the above procedure, we need to compute

$$y_1 \;=\; \frac{y - 0.1}{1 + 0.1y} \;=\; 0.451294754396176, \tag{3.205}$$

$$y_2 \;=\; \frac{y_1 - 0.1}{1 + 0.1y_1} \;=\; 0.336125583146920, \tag{3.206}$$

$$y_3 \;=\; \frac{y_2 - 0.1}{1 + 0.1y_2} \;=\; 0.228446898450927, \tag{3.207}$$

$$y_4 \;=\; \frac{y_3 - 0.1}{1 + 0.1y_3} \;=\; 0.125578105577671, \tag{3.208}$$

$$y_5 \;=\; \frac{y_4 - 0.1}{1 + 0.1y_4} \;=\; 0.025260884179622. \tag{3.209}$$

This is followed by a computation of

$$\begin{aligned} a \;&=\; \arctan 0.1 \;\approx\; 0.1 - \frac{0.001}{3} + \frac{0.00001}{5}, \\ &- \frac{0.0000001}{7} + \frac{0.000000001}{9}, \\ &- \frac{0.00000000001}{11} = 0.099668652491154, \end{aligned} \tag{3.210}$$

and of

$$\begin{aligned} b \;&=\; \arctan y_5 \\ &\approx\; y_5 - \frac{y_5^3}{3} + \frac{y_5^5}{5}, \\ &- \frac{y_5^7}{7} + \frac{y_5^9}{9} - \frac{y_5^{11}}{11}, \\ &=\; 0.025255513142489. \end{aligned} \tag{3.211}$$

These simple calculations are enough to yield π correct to 10 decimal places, through the formula

$$\pi \approx 30a + 6b = 3.141592653589564. \tag{3.212}$$

The computation of each y_i requires 4 oating point operations, so that a total of 20 oating point operations are required to compute y_5. The computation of a and b requires some 10 oating point operations each (up to 7th order, or 15 oating point operations up to 11th order), so that the job can be accomplished in a total of 42 oating point operations (not counting the square-root extraction). Thus the entire job is quite do-able by hand, within an hour, or so, assuming an average speed of around 1 oating point operation per minute.

There is no great virtue to the exact procedure followed above, except that (1) the arctangent series converges very rapidly for small values of y, (2) for intermediate values of y one has to use some way to connect values of $\arctan y$ to its values for smaller y. Apart from the algebraic relation that has been used above, another possible way to do this would have been to use a finite difference technique ("Euler solver") as in the computation of the sine series.

APPENDIX 3.C
"ARCHIMEDES'" METHOD OF COMPUTING π

Our actual "knowledge" of the works of Archimedes is much worse than our knowledge of the *Elements*. One gets to hear endless stories about Archimedes, but of concrete evidence there is nothing. The earliest report of a work related to Archimedes is a report of a 13th c. translation by a high priest, William of Moerbeke, of the works of his commentator Eutocius. We know nothing whatsoever of the sources used in the translation, and have nothing better to go by than William of Moerbeke's word for it. In the appendix to chapter I we have already acquired some understanding of the process by which the author of the source might have been identified as an early Greek called Archimedes. In fact, given the character of the Inquisition, it is interesting to speculate what might have happened to the translator for contradicting the Bible which states that the value of π is 3! This Eutocius, whose work is believed to have been translated, is believed to have been a student of Ammonius, a student of Proclus, but we know nothing about Eutocius either, beyond the name. We have only faith piled on faith to go by, and nothing for those who lack faith.

Now, some 800 years are believed to have passed between Archimedes and Eutocius, and, of course, no one knows exactly what Archimedes wrote, or how Eutocius differed from him, but then, as one historian remarked, that is the way history is built—at least that is the sort of evidence on which the stories of present-day Western historians of science about Greeks are built. Finally, we don't actually have those 13th c. CE original translations either, but we do have copies of some of those. Western historians believe that the works of Archimedes were faithfully copied verbatim by later writers, so it should come as no surprise that the mythical Archimedes anticipated many of the things that were done later elsewhere in the 9th and 10th c.—a conclusion inferred from originals from perhaps the 15th c.!

Apart from this, there is a reportedly a very early (i.e., 10th c. CE) work of Archimedes that is reconstructed from a 13th c. CE palimpsest—a book that has been washed and reused to write another religious text on. The believed-to-be-10th c. "Archimedes" in this palimpsest was reconstructed by Heiberg. Now that the palimpsest is finally on display, there are numerous puzzling issues about the exact correspondence between the palimpsest and Heiberg's reconstruction. The natural interpretation of these discrepancies would be that where Heiberg ran out of imagination, he resorted to plain dishonesty, and misrepresented his source material. However, there would no doubt be those historians who would like to pile on the hypotheses in defence of Heiberg. Therefore, let us say that such discrepancies presumably arise because not all people can take the imaginative leaps to see what Heiberg's scholarship enabled him to see!

In any case, the very short text, attributed to Archimedes, starts by asserting that the ratio of the area of a circle to the square of its diameter is 11 14, corresponding to the well known approximation $\pi = 22\ 7$. It then goes on with two hexagons, one inscribed and one circumscribed, and continues this procedure by doubling the number of sides at each stage. Today this procedure is often described using a recursion formula to compute the perimeter, by alternately computing the harmonic and geometric means. If P_n and p_n respectively denote the semi-perimeters of the circumscribed and inscribed n-gons, then the formulae are

$$P_{2n} = \frac{2p_nP_n}{p_n + P_n}, \tag{3.213}$$

$$p_{2n} = \sqrt{p_nP_{2n}}. \tag{3.214}$$

The above is the standard way of presenting what Archimedes did, from the reports we have of it from at least some 1600 years later. In actual fact, the above neat algebraic formula is derived with techniques of trigonometry, and Archimedes had access to neither algebra nor trigonometry. Further, the second formula involves the computation of square roots, and Archimedes had no means of computing those. Further, like Eutocius in the Roman empire, the clumsy Roman numerals made ordinary addition, multiplication, and division so difficult that square-root extraction was surely a forbidding matter. The author of the "Archimedes" text had no particular algorithm for the extraction of square roots, and engages in lengthy estimates, eventually using 96-gons to estimate π as lying between 310 71 (= 223 71) = 3.1408 and 310 70 (= 22 7 = 3.1428).

APPENDIX 3.D
CHRONOLOGY OF INDIAN MATHEMATICIANS

ca. −1350 CE, *Vedāṅga Jyotiṣa*.

Baudhāyana, Katyāyana, Apastamba, ca. −500 CE, *Śulba sūtra*.

Pingala, ca. −3 rd c. CE, *Chandaḥsūtra*. (Binomial expansion, "Pascal's" triangle.)

ca. 3rd c. CE. *Sūrya Siddhānta*.

Āryabhaṭa, b. 476 CE, Kusumapura (identified with Pāṭaliputra by Bhāskara) = Patna, Bihar. Principal work *Āryabhaṭīya*, composed at age 23, as he describes in his chronogram. Manuscript sources and other details, in K. S. Shukla ed.

Bhāskara I, b. ca. 6th c., . 629 CE, Saurashtra? Asmaka (Nizamabad, Andhra Pradesh) *Mahā Bhāskarīya, Laghu Bhāskarīya, Āryabhaṭīya Bhāṣya*.

Varāhamih¯ra, 6th c. Ujjain (d. 587 CE), *Pañcasiddhāntikā*. Also attributed *BṛhatJātaka* (first Indian book on astrology).

Brahmagupta, ca. 628 CE, born Bhinmal (Gujarat, near Mt Abu) worked in Ujjain, *Khaṇḍakhādyaka, Brāhma Sphuṭa Siddhānta*.

Lallā, ca. 748 CE, Dasapura (Mandsaur), moved to the Kusumpura school, author of the well-known *Śiṣyadhīvṛddhida*.

Mahāv¯ra, ca. 850 CE, Karnataka, *GaṇitaSāra Saṁgraha*.

Śr¯dhara, 9th. c. CE, Bengal, *Pāṭīgaṇita*.

Vaṭeśvara, 904 CE, Anandapura/Nagar (Vadnagar, Gujarat), *Vaṭeśvara Siddhānta, Gola*.

Āryabhaṭa II, ca. 950 CE, *Mahāsiddhānta*.

Al B¯rūn¯, b. 976 CE, Afghanistan/North India (translated Vijay Nandi's *Karaṇa Tilak* of 966 CE) *Kitāb al Hind* contains a detailed account of the knowledge of contemporary Indian astronomy and mathematics that he gathered.

Bhāskara II, 12th c., *Lilāvatī, Bījagaṇita, Siddhāntaśiromaṇi*.

Nārāyaṇa Paṇḍit, 1350 CE, Benares, *GaṇitaKaumudī*.

Kamalākara, 1658 CE, Agra (Jehangir's court) *Siddhānta tattva viveka*.

Jai Singh, Delhi/Jaipur/Ujjain (Malwa), observatories built ca. 1730 CE. Commissioned the first translation of the *Elements* into Sanskrit (from Persian) as *Rekhāgaṇita* ("line mathematics") by Pt. Samrāta Jagannātha ca. 1723.

Āryabhaṭa school of mathematics and astronomy in Kerala (some key names and works)

Haridatta, ca. 650–700 (founder of Parāhita school of astronomy).

Govindasvāmin, ca. 800–850, *Bhāṣya on Mahābhāskarīya.*

Mādhava (of Saṅgamagrāma), ca. 1340–1425. Known works: *Venavaroha*, for true position of the moon every 36 minutes. *Mahājyānayanprakāra* (lost).

Parameśvara I (of Vaṭśrenī), ca. 1360–1455. Author of *Dṛgganita* (which revises and extends the Parāhita system), and *Goladīpikā*.

N¯lakaṇṭha Somayāji (of Trkkantiyur), 1444–1545, *Bhāṣya on Āryabhaṭīya*, *Tantrasaṅgraha*.

Jyeṣṭhadeva, 1500–1600, *Yuktibhāṣā*.

Śankara Vāriyar (of Trkkutaveli), ca. 1500–1560. Commentaries *Vyākhya* and *Laghuvivṛti* (1556) on *Tantrasaṅgraha*. Closely similar text called by the alternative name *Yuktidīpikā*. Author of part of *Kriyākramakarī* (a commentary on the *Lilāvatī*), which has some 400 verses in common with the *Yuktidīpikā*.

Nārāyaṇa I (ca. 1500–1575)? Brother of Śaṅkara Vāriyar. *Kriyākramakarī* (short commentary on *Lilāvatī*), and *Karmadīpikā* (long commentary).

Karaṇapaddhati (still in current use). Date unknown, possibly mid 16th c. CE.

Śankara Varma (Appu Tampuran) of Katattanat, 1800–1838, *Sadratnamāla* (1829).

Rama Varma (Maru) Tampuran, 1948. Commentary on *Yuktibhāṣā* with Akhileshwar Aiyar.

NOTES AND REFERENCES

1. V. I. Arnol'd, *Barrow and Huygens, Newton and Hooke*, trans. E. J. F. Primrose, Birkhauser Verlag, Basel, 1990, pp. 35–42.
2. Nowadays known as the Taylor theorem, this appeared as Proposition 7, Corollary 2, in Brook Taylor, *Methodus Incrementorum directa et inversa*, London, 1715. Translation in L. Feigenbaum, *Brook Taylor's "Methodus Incrementorum": A Translation with Mathematical and Historical Commentary*, Ph.D. Dissertation, Yale University, 1981. Apart from Newton, the series was anticipated by James Gregory. L. Feigenbaum, "Brook Taylor and the method of increments, " *Arch. Hist. Exact. Sci.* **34** (1) (1984) pp. 1–140.
3. In a letter of 15 Feb 1671 to John Collins, Gregory had supplied Collins with seven power series around 0, for arctan θ, tan θ, sec θ, log sec θ, etc., H. W. Turnbull, *James Gregory Tercentenary Memorial Volume*, London, 1939. Gregory's series, however, contained some minor errors in the calculation of the coefficient of the fifth-order term in the expansion.
4. W. Wallace, *Galileo and His Sources: The Heritage of the Collegio Romano in Galileo s Theory of Science*, Princeton University Press, 1984.
5. *TantraSaṅgrahaVyākhyā*, Palm Leaf MS No. 697 and its transcript No. T 1251, both of the Kerala University MS Library, Trivandrum. Also, transcript No. T-275 of the *TantraSa.ngrahaVyākhyā* at Trippunitra Sanskrit College Library, copied from a palm leaf manuscript of the Desa Mangalatta Mana.
6. *Tantrasaṅgraha* of Nīlakaṇṭha Somayaji, with *Yuktidīpikā* and *Laghuvivṛti* of Śaṅkara, ed. K. V. Sarma, VVBI, Hoshiarpur, 1977.
7. *Līlāvatī* of Bhāskaracarya with *Kriyākramkarī* of Śaṅkara and Nārāyaṇa, ed. K. V. Sarma, VVRI, Hoshiarpur, 1975.
8. *Yuktibhāṣā*, Part I, ed. Ramavarma (Maru) Thampuran and A. R. Akhileshwara Aiyar, Mangalodayam Ltd, Trichur, 1123 Malayalam Era, 1948. *The GaṇitaYuktibhāṣā of Jyeṣṭhadeva*, ed. and trans. K. V. Sarma (to appear).
9. *TantraSaṅgraha* of the Trivandrum Sanskrit Series. *Tantrasaṅgraha*, ed. S. K. Pillai, Trivandrum Sanskrit Series, 188, Trivandrum, 1958. *TantraSaṅgraha*, ed. K. V. Sarma, trans. V. S. Narasimhan in the *Indian Journal of History of Science*, (issue starting Vol. 33, No. 1, of March 1998).
10. *Āryabhaṭīya of Āryabhaṭācārya with the Bhāṣya of Nīlakaṇṭhasomasūtvan*, ed. K. Sambasiva Sastri, University of Kerala, Trivandrum, 1930, reprint 1970, commentary on Gaṇita 17, p. 151.
11. *Karaṇapaddhati*, Trivandrum Sanskrit Series 126, Chapter 6, verse 12–13 and sequel.
12. The key passage is quoted in the *Yuktibhāṣā* and attributed to the *Tantrasaṅgraha*. *Yuktibhāṣā*, Part I ed. Ramavarma and Akhileshwara Aiyar, cited above, p. 190. The passage is not to be found in the *TantraSaṅgraha* of the Trivandrum Sanskrit Series, cited above, or in the version of the *TantraSaṅgraha* (ed. K. V. Sarma, cited above), where they are found (p. 118) in Śaṅkara's *Yuktidīpikā* commentary (also known as the *TantrasaṅgrahaVyākhyā*, cited above) (2.440, 2.441) following *Tantrasaṅgraha* (2.14b, 2.15a). In his critical edition of *Tantrasaṅgraha*, K. V. Sarma has opined that this is a mistake of attribution by the authors of the modern *Yuktibhāṣā* commentary. I, of course, defer to his scholarship, but there is a small point worth noting. The authors of the modern *Yuktibhāṣā* commentary used a transcript of the MSS of the *TantraSaṅgrahavyākhyā* in the Desamangalatta Mana (house of Desamangala Namputiri), a well-known Namboodri household. Transcript No. T-275 of the *TantraSaṅgrahavyākhyā* at Trippunitra Sanskrit College Library is copied from a palm leaf manuscript of the Desamangalatta Mana; the missing verses are after 2.21a of the Trivandrum Sanskrit Series MS, and are found on pp. 68–69. This version of the *TantraSaṅgraha* is found also in the *TantraSaṅgrahavyākhyā*, Palm Leaf MS No. 697 and its transcript No. T-1251, both of the Kerala University MS Library, Trivandrum. It is worth noting that in PL-697/T-1251 the commentary in Malayalam is clearly separated from the Sanskrit text in Devanagari. Thus, the authors of the modern *Yuktibhāṣā* commentary seem to have been accurate in their attribution, though they could perhaps be faulted for relying on a single MS of the *Vyākhyā* available in the house of one of the authors, which also had an MS of the *Kriyākramakarī*. See, further, C. N. Srinivasiengar, *The History of Ancient Indian Mathematics*, Calcutta, The World Press, 1967. C. T. Rajagopal and M. S. Rangachari, *Archiv. Hist. Ex. Sci.* **18** (1978) pp. 89–102; C. T. Rajagopal and M. S. Rangachari, *Archiv. Hist. Ex. Sci.* **35** (1986) pp. 91–9. See also K. V. Sarma, *A History of the Kerala School of Astronomy (in perspective)*, Hoshiarpur, 1972, p. 17; A. K. Bag, "Mādhava's sine and cosine series," *Indian Journal of History of Science*, **11** (1976) pp. 54–57; T. A. Sarasvati Amma, *Geometry in Ancient and Medieval India*, Motilal Banarsidass, New Delhi, 1979, 2nd ed, 1999, pp. 184–190.

13. The Sanskrit translations in this book are mostly based on the translations in the above references, but modified as appropriate by the author. For this passage and the next cf. e.g. Rajagopal and Rangachari, and also Bag, cited above.
14. *Yuktidīpikā*, 2.442, 2.443. *Yuktibhāṣā* in the place cited earlier.
15. E.g. *Yuktidīpikā*, 2.437. Nīlakaṇṭha's *Āryabhaṭīyabhāṣya*, commentary on *Gaṇita* 17b, p. 151 and supercommentary on Bhāskara, has a slightly different reading, *kapīśanicayah* instead of *kavīśanicayah*. (However, in Devanagari, a carelessly written or read *va* can be easily confused for *pa*, as demonstrated in the paper by Rajagopal and Rangachari, and there can be no doubt about the correct reading.) Nīlakaṇṭha refers to this as "just so said Mādhava". *Āryabhaṭīya of Āryabhaṭacārya with the Bhāṣya of Nīlakaṇṭhasomasūtvan*, ed. K. Sambasiva Sastri, University of Kerala, Trivandrum, 1930, reprint 1970, p. 151.
16. cf. e.g., Rajagopal and Rangachari, cited above.
17. *Yuktidīpikā*, 2.438
18. *Āryabhaṭīya* with commentary of Nīlakaṇṭha (ed. K. Samabasiva Sastry, TSS 101), cited earlier, p. 56.
19. For more details, see B. B Datta and A. N. Singh, *History of Hindu Mathematics: A Source Book*, Part 1, pp. 53–63.
20. *Pāṭī Gaṇita*, 7. *PaṭīGaṇita of Śrīdharācarya*, ed. and trans. K. S. Shukla, Department of Mathematics and Astronomy, Lucknow University, 1959, p. 5.
21. Rajagopal and Rangachari, cited earlier, part 1, n. 15.
22. Formal mathematicians who tend to proceed authoritatively seem unaware that several rounding conventions are in use today. For example, a widely used compiler, the Microsoft Visual C++ compiler, will allow one to specify the rounding procedure used in oating point arithmetic in a variety of ways.
23. *ĀryabhaṭīyaBhāṣya*, cited earlier, commentary on *Gaṇita* 12, pp. 73–74.
24. *Tantrasaṅgraha* 2.9.5, Trivandrum Sanskrit Series, ed. S. K. Pillai, cited earlier, p. 19. The last line is not found in the *ĀryabhaṭīyaBhāṣya*.
25. C. K. Raju," Interaction between India, China, and Central Asia, ..." cited above.
26. *Āryabhaṭīya of Āryabhaṭa*, trans. K. S. Shukla and K. V. Sarma, INSA, New Delhi, 1976, pp. 38–45.
27. cf. Shukla and Sarma, cited above
28. Baudhāyana *śulba sūtra*, 2.12.
29. S. N. Sen and A. K. Bag, *The Śulbasūtras of Baudhāyana, Apastamba, Kātyāyana, and Mānava*, INSA, New Delhi, 1983, p. 169.
30. Sen and Bag, cited earlier, p. 80
31. Apastamba *śulba sūtra* 3.2; Sen and Bag, cited earlier, p. 103. The same thing is repeated in other *śulba sūtra*-s e.g. Katyāyana *śulba sūtra* 2.9 See also Baudhāyana *śulba sūtra* 2.9.
32. *Āryabhaṭīya*, Part I, *Gaṇitapāda*, ed. with the commentary of Nīlakaṇṭha Somasutvan, by K. Sambasiva Sastri, Kerala University, Trivandrum, 1930 (Trivandrum Sanskrit Series, 101), reprint, 1977, p. 56, commentary on Gaṇita verse 10.
33. "In general we shall use the sexagesimal system because of the difficulty of fractions, and we shall follow out the multiplications and divisions..." Ptolemy, *The Almagest*, trans. R. Catesby Taliaferro, Great Books of the Western World, vol. 15, Encyclopaedia Britannica, Chicago, 1996, p. 14. The need to justify the use of the sexagesimal system suggests a certain level of discomfort with it.
34. For a quick review see S. N. Sen and A. K. Bag, *The Śulbasūtras*, INSA, New Delhi, 1983, pp. 164–169.
35. Baudhāyana *śulba sūtra*, 2.12, Apastamba *śulba sūtra* 1.6, and Kātyāyana *śulba sūtra* 2.9
36. This is presumably the reason why in trigonometry, as taught in school today, children are provided with sine and cosine values for only six angles, the other sine values remaining a mystery.
37. This is traditionally referred to as the tenth *gītikā*, although it is the twelfth, the first two being invocations.
38. *Āryabhaṭīya*, trans. by the author based on the Hindi translation by Ramnivas Rai, INSA, New Delhi, 1976, pp. 42–43; cf. Shukla and Sarma, cited earlier, p. 51. This more literal translation is used also by the *Paitāmah Siddhānta*, although all translations give the same mathematical formula, expressed in different ways.
39. M. Delambre, *Histoire de l Astronomi Ancienne*, vol. I, Paris, 1817, p. 458. Cited by Bina Chatterjee, ed. and trans. *The Khaṇḍakhādyaka of Brahmagupta, with the Commentary of Bhaṭṭotpala*, vol. 1, Motilal Banarsidass, Delhi, 1970, Appendix VI, p. 200.
40. Shukla and Sarma, cited earlier, pp. 51–54, also provide the geometric arguments used by Nīlakaṇṭha.
41. *Āryabhaṭīya*, with the commentary of Sūryadeva Yajvan, ed. K. V. Sarma, INSA, New Delhi, 1976, commentary on *Gaṇita* 11, p. 50.

42. *The Sūrya Siddhānta*, trans. E. Burgess, ed. P. Gangooly, Motilal Banarsidass, Delhi [1860], reprint 1997, pp. 58–64. Though this text is earlier than the *Āryabhaṭīya*, being dated to about the 3rd or 4th c. CE, P. C. Sengupta has opined that the present version of the text is a composite, and that the sine table in the text actually derives from the *Āryabhaṭīya*. If that be the case, it is not clear what method was originally used in the *Sūrya Siddhānta*.
43. *Sūrya Siddhānta*, ed. with comm. *Gūdharthaprakasikā* of Ranganath, ed. Jivananda Bhattacharya, Calcutta, 1891.
44. C. K. Raju, "Time travel and the reality of spontaneity", *Found. Phys.* July 2006. http://doi.dx.org/10.1007/s10701-006-9056-x. Draft at http://philsci-archive.pitt.edu/archive/00002416/01/Time_Travel_and_the_Reality_of_Spontaneity.pdf. For an almost non-technical account, see C. K. Raju, "Time travel", *The Eleven Pictures of Time*, Sage, New Delhi, 2003, chp. 6. More precisely, the model for a "purposive procedure" that I have in mind is that of a process modelled by mixed-type functional differential equations. The claim then is that such a process cannot be mechanized, since it is impossible to predict it based on a knowledge of even the entire past. For those who do not understand the mathematics of functional differential equations, an easily visualizable example is that of time travel which cannot be mechanized.
45. James Gregory, letter to Collins of 23 Nov 1670, reproduced in Rigaud's *Correspondence of Scientific Men of the 17th Century*, vol. 2, p. 209, as cited by E. Whittaker and G. Robinson, p. 12n; the same note further cites Rigaud, vol. 2, p. 335 to the effect that "Collins was accustomed to send on to Newton the mathematical discoveries of Gregory."
46. Euler's paper on the "Hindu year" was an appendix to the *Historia Regni Grecorum Bactriani* by T. S. Bayer, as cited by G. R. Kaye, *Hindu Astronomy*, 1924, reprinted Cosmo Publications, New Delhi, 1981, p. 1.
47. Brahmagupta, *Khaṇḍakhādyaka*, with commentary of Bhaṭtotpala, ed. and trans. Bina Chatterjee, New Delhi, 1970, *Uttarakhaṇḍakhādyaka*, II.1.4, vol. II, p. 177.
48. *Vaṭeśvara Siddhānta and Gola of Vaṭeśvara*, ed. and trans. K. S. Shukla, Indian National Science Academy, New Delhi, 1976, II.1.63–82, part I, p. 96, and part II, p. xlvi.
49. *Vaṭeśvara Siddhānta*, trans. K. S. Shukla, cited above, part II, p. 173.
50. *Siddhānta Śiromaṇi*, *Spaṣṭādhikāra* 36–38, and accompanying auto-commentary *Vāsanābhāṣya*. For a detailed discussion, see Bapudev Sastri, "Bhāskara's knowledge of the differential calculus", *Journal of the Asiatic Society of Bengal*, **27** (1858) pp. 213–16 and P. C. Sengupta, "Infinitesimal calculus in Indian mathematics", *Journal of the Department of Letters* (Calcutta University) **22** (1932) pp. 1–17. *Siddhānta Śiromaṇi*, ed. Bapudeva Sastri, rev. by Ganapati Deva Sastri, Benares, 1929 (Kashi Sanskrit Series, No. 72) and ed. *GrahaGaṇitadhyāya* with *Vāsanābhāṣya*, pp. 52–53 and ed. *Golādhyāya* with *Vāsanābhāṣya* and com. Marici of Munisvara, Poona, 1939, and 1943, Anandashram Sanskrit Series, 110 and 122 respectively.
51. P. C. Sengupta, "Infinitesimal Calculus", cited earlier, p. 7.
52. Sengupta, cited above, p. 5.
53. P. C. Sengupta, cited above.
54. *Brāhma Sphuṭa Siddhānta*, II.41–42.
55. *Siddhānta*, 1.1.7–8.
56. E.g. W. H. Newton-Smith, *The Structure of Time*, Routledge and Keagan Paul, London, 1974. C. K. Raju, *Time: Towards a Consistent Theory*, Kluwer Academic, Doredrecht, 1994.
57. Śrīdhara, *Triśaṭikā*, 56, ed. Sudhakara Dwivedi, Benares, 1899. trans. N. Ramanaujachari and G. R. Kaye, *Bibliotheca Mathematica*, **13** (1912) pp. 203–17.
58. Bhāskara II, *Siddhānta Śiromaṇi*, trans. Sengupta, cited earlier.
59. Bhāskara II, *Lilāvatī*, cited earlier, rule 203.
60. Jean Fillozat, "Ancient Indian science", in *Ancient and Medieval Science*, ed. Rene Taton, Thomas and Hudson, London, 1963, p. 15.
61. Govindasvāmin, *Bhāṣya on the Mahā Bhaskarīya*, iv.22. [S. N. Sen, *A Bibliography of the Sanskrit works on Astronomy and Mathematics*, INSA, New Delhi, 1966, p. 78].
62. Udayadivākara, *Sundarī on the Laghu Bhāskarīya*, ii.3–6. INSA Bibliography, cited above, p. 280.
63. Joseph Needham, *The Shorter Science and Civilization in China*, abridged by Colin A. Ronan, Cambridge University Press, 1981, vol 2, p. 43.
64. Archimedes, *Measurement of a Circle*, trans. T. L. Heath, Encyclopaedia Britannica, Chicago, 1990, pp. 447–451.
65. Although the presentation in the *Yuktibhāṣā* is substantially similar, it is obscure on a couple of key points.

66. As a minor point of detail, it may be noted that this differs from the presentation of Saraswati Amma, where the diagram shows the north-north-east octant. T. A. Saraswati Amma, *Geometry in Ancient and Medieval India*, Motilal Banarsidass, New Delhi, 1979, p. 160 et seq.
67. not "in analogy with OP_n which is the diagonal", as maintained by Saraswati Amma, but because they are the hypotenuses of certain key right-angled triangles (which can be embedded in a square of which the *karṇa* would then be the diagonal). T. A. Saraswati Amma, cited above, p. 161
68. *Brāhma-Sphuṭa Siddhānta*, with *Vāsana*, *Vijnāna* and Hindi Commentaries, chief ed. and trans. Ram Swarup Sharma, New Delhi, 1966, vol. III, pp. 906–907.
69. All sums are finite at this stage, so there is no difficulty with rearrangement; this is merely the sum of terms in a column.
70. K. S. Shukla, *Āryabhaṭīya*, p. 65. For a discussion of arithmetic and geometric series in the Veda, see M. D. Pandit, *Mathematics as Known to the Vedic Saṁhitās*, Satguru Publications, Delhi 1993.
71. Srinivasiengar, *History of Ancient Indian Mathematics*, cited earlier, p. 154.
72. T. A. Saraswathi noted the formula for the *vārasaṅkalitā*, and its connection to the derivation of the infinite series, mentioned also in the commentaries. She did not, however, *explain* that connection either in that article or later, recalling Srinivasiengar's lament (p. 162). T. A. Saraswathi, "The development of mathematical series in India after Bhāskara II", *Bull. Nat. Inst. Sci.* **21** (1968) 320–343.
73. For some peculiarities of the number system in use in India, and its possible relations to a non-field of computational numbers, see my paper, "Mathematical epistemology of śūnya", cited earlier.
74. Nīlakanṭha, *AryabhaṭīyaBhāṣya*, cited earlier, commentary on *Gaṇita* 17, p. 142.
75. cf. K. V. Sarma, *A History of the Kerala School of Hindu Astronomy*, cited earlier, p. 19.
76. We recall that Fermat's derivation of the above result also used so-called figurate numbers. Incidentally, Fermat was much interested in ancient manuscripts, and his challenge problem to Wallis involved a problem explicitly found as a solved example in Bhāskara's *BījaGaṇita*. (This is the case $n = 61$ of "Pell's" equation, which has the solution $x = 1766319049$ $y = 226153980$.) No mathematician in Europe was able to solve this challenge problem for nearly a 100 years until Euler published a solution, so the probability of the same value of n occurring by chance seems rather small. See T. S. Bhanu Murthy, *Ancient Indian Mathematics*, Wiley Eastern, New Delhi, 1992, p. 121.
77. K. S. Shukla and K. V. Sarma, cited above, p. 64
78. Nārāyaṇa, *Gaṇita Kaumudi*, ed. Padmakara Dwivedi, Princess of Wales Sarasvati Bhavana Texts, No. 57, Benares, 1942, part 1, p. 123.
79. cf. T. A. Saraswathi, cited above.
80. T. A. Saraswathi, cited above.
81. *Yuktidīpikā*, 2.271, *Yuktibhāṣā*, cited earlier, p 99. .
82. cf. e.g. K. V. Sarma, *A History of the Kerala School of Hindu Astronomy*, Vishveshvaranand Institute, Hoshiarpur, 1972, p. 22.
83. *Yuktidīpikā*, 2.206–208. *Yuktibhāṣā*, cited earlier, p. 113.
84. cf. Rajagopal and Rangachari, cited above, p. 91.
85. Śrīdhara, *PāṭiGaṇita*, ed. and trans. K. S. Shukla, *The PāṭiGaṇita of Śrīdharācarya*, Department of Mathematics and Astronomy, Lucknow University, 1959, 94–95, trans. p. 75. This is a very common commercial problem found in a variety of other sources such as Mahāvīra, *GaṇitaSāra Saṁgraha*, (ed. and trans. L. C. Jain, Jaina Samskrti Samraksha Samgha, Sholapur, 1963, 2.94. See also, Āryabhaṭa II, *MahāSiddhānta*, 15.53, Bhāskara II, *Līlāvatī*, etc.
86. Nīlakanṭha, *Bhaṣya* on *Āryabhaṭīya*, comment on *Gaṇita* 22, Trivandrum SS 101, 1930, p. 106. Cited, e.g., by K. V. Sarma, *A History of the Kerala School of Hindu Astronomy*, Vishvesharanand Institute, Hoshiarpur, 1972, p. 19.
87. *Yuktidīpikā* , 2.271–74. *TantrasaṅgrahaVyākhyā*, R2505, Govt. Oriental Manuscript Library, Madras, I.1–1.6, pp. 109–10. All references to R2505, are as cited by C. T. Rajagopal and M. S. Rangachari, *Archive for History of Exact Science*, **18** (1978) pp. 89–101.
88. *Yuktidīpikā*, 2.295–96, *TantrasaṅgrahaVyākhyā*, R2505, II.5–8, p. 112.
89. C. N. Srinivasiengar, *The History of Ancient Indian Mathematics*, The World Press Pvt Ltd, Calcutta, 1967, pp. 148–52.
90. *Yuktidīpikā*, 2.290, *TantrasaṅgrahaVyākhyā*, R2505, II.5–4, p. 111.
91. cf. Rajagopal and Rangachari, cited above.
92. *Yuktidīpikā*, 2.287–88. *TantrasaṅgrahaVyākhyā*, R2505, II. 11–13, p. 111.
93. cf. Rajagopal and Rangachari, cited above.

94. Śaṅkara Vāriyar, *Kriyākramakarī* (a commentary on the Lilāvatī), ed. K. V. Sarma, Hoshiarpur, 1975.
95. *Yuktidīpikā* 2.220–221. *Kriyākramakarī* p. 386. This condition is actually proposed while testing the "subtlety" or fineness of the *saṁskāra* or correction term.
96. T. Hayashi, T. Kusuba, and M. Yano, "The correction of the Mādhava series for the circumference of a circle", *Centaurus*, **33**, 1990, pp. 149–74. An account by J. John, published in the proceedings of the Nīlkaṇṭha symposium, is too confused regarding the basic epistemological issues involved (e.g. induction vs deduction) to be given further consideration here. Further, John used the wrong source, since he relied on the modern Malayalam commentary on the *Yuktibhāṣa*, instead of the original Sanskrit source. See, further, Appendix 6.A.
97. *Kriyākramakarī*, p. 386, l. 17.
98. *Kriyākramakarī*, p. 386, line 18.
99. *Kriyākramakarī*, p. 388, l. 2
100. *Kriyākramakarī*, p. 389.
101. L. Euler, *Comm. Acad. Sci. Imp. Petrop.* **6** (1738) p. 68. (As cited by Whittaker and Robinson, cited below.)
102. Mclaurin, *Treatise on Fluxions* (1742), p. 672. (As cited by Whittaker and Robinson, cited below.)
103. E. T. Whittaker and G. Robinson, *Calculus of Observations*, 4th ed., Blackie & Sons, London, 1965, p. 135.
104. James Gregory, Letter of Gregory to Collins, 23 Nov 1670, in Rigand, *Correspondence*, 2, p. 209.
105. C. Boyer, *A History of Mathematics*, 1980, p. 85.
106. Ramanujam, *Ramanujam s Notebooks, Part IV*, ed. B. C. Berndt, Springer, Berlin, 1994, pp. 81–85.

CHAPTER 4

Time, Latitude, Longitude, and the Size of the Globe

Why precise trigonometric values were needed in India for determination of time, latitude, longitude, and the size of the earth

OVERVIEW

We saw in the previous chapter how the calculus developed through a continuous effort, spanning a thousand years, to obtain the numerical values of trigonometric functions precise to the first minute (*Sūrya Siddhānta* ca. 3rd c. CE, Āryabhaṭa, 499 CE), the second minute (Vaṭeśvara, 904 CE), and the third minute (Govindasvāmin 9th c. CE, Mādhava 14th c. CE). Why were trigonometric values needed? What social processes related to this drive for ever-increasing precision?

Trigonometric functions were introduced in the context of time measurement. (Accurate planetary models were developed for the same reason, but we will not examine that here.) Time-measurement and calendar-making had a special significance in the Indian context, since the economy relied (and still relies) on agriculture, and agriculture relied (and still relies) on the monsoons, so that a good calendar was (and still is) required to calculate the seasons and especially the rainy season. In contrast, in the West, the calendar was, for a long time, used mostly for ritual purposes, so that the role of the calendar as a key technology enabling monsoon-driven agriculture (and hence the prosperity and wealth of pre-colonial India) has not been appreciated.

An immediate illustration of the role of the calendar in agriculture is provided by the events of the preceding years. There was widespread panic, as evinced by newspaper headlines, because the monsoon was "delayed" according to the Gregorian calendar, which however lacks the very concept of a "rainy season". The matter was not confined to newspapers: some state governments geared up for drought relief, and requested and actually received vast sums of money from the central government, in anticipation of drought. The monsoon, however, came right on time according to the traditional Indian calendar. Nevertheless, there was partial crop failure due to the mistiming of agricultural operations, by those farmers who had been advised to go by the Gregorian calendar.

Agricultural activities in India were naturally tied to various festivals, and to a common calendrical tradition spread across India. Calculations related to the calendar were traditionally done for a single place—Ujjayinī, for at least the last 1500 years. The calendar was then recalibrated for the local place.

Because the common Indian culture was spread over so wide a geographical area, recalibration of the calendar required a knowledge of the local latitude and longitude, which was determined mainly by celestial observations, and accurate values of trigonometric functions. For example, as described by Bhāskara I, local latitude could be determined from a measurement of solar altitude at noon, using precise values of trigonometric functions (and supposing that the day of the equinox had been correctly identified on the calendar, unlike the Julian calendar).

Another essential input that went into the determination of local latitude and longitude was the size of the earth (assumed spherical). First, accurate angle measurements made locally were adequate to calculate the radius of the earth using precise trigonometric values, as documented by al Bīrūnī. Precision in trigonometric values was important because the earth is very large, so that even small imprecisions in trigonometric values would lead to large inaccuracies in calculating the radius of the earth. Secondly, the circumference of the earth needed to be calculated from its radius. This required a precise knowledge of the relation of circumference to radius, or a knowledge of the number today known as π. Again precision was important because of the large size of the earth: small imprecisions in the value of π would have led to large inaccuracies. Further, as we have already seen in the preceding chapter, increasingly precise knowledge of π was also needed as an input to calculating increasingly precise values of trigonometric functions.

The knowledge of ways of accurately determining local position, using celestial observations and calculations, was also useful for navigation. And navigation was a prerequisite for overseas trade—the other key source of early Indian wealth. The archaeological evidence of ports in Harappan sites shows that sea trade was already important in Harappan times. Sea trade routes to Alexandria certainly existed from pre-historic times (and were already so famous by the time of Alexander that he appointed an admiral, Nearchus to try to find the sea route to Alexandria from "India", i.e., present-day Afghanistan). Herodotus similarly expresses his conviction that Egyptians had practised navigation.[1] Around the 3rd or 4th c. CE, Indian trade with the Roman empire had expanded so tremendously that Roman historians were complaining about the significant loss of Roman wealth to high-priced goods imported from India. It was more efficient to transport heavier items like wood and elephants via sea, and some 120 ships sailed annually to Alexandria during Roman times. This trade with Alexandria was apart from the trade with Arabs, sub-Saharan Africa, and China. This kind of bulk and long-standing sea trade suggests secure routes, hence an established and reliable technique of navigation.

What technique of navigation did these vessels use? It is clear enough from early historical accounts like those of Fa-Hsien that, contrary to garbled Western histories of shipping and navigation, and contrary to the method followed by Nearchus, trading vessels did not creep along the coast, but navigated across the open sea, though they did *not* use charts and maps as in the West. Fa-Hsien states that his ship was unable to navigate when the sky became overcast, and this shows that celestial navigation techniques were used. Increasing volume of overseas trade (with Africa, Arabs, and China) required increasingly reliable trade routes, and hence increasingly reliable techniques of navigation. This, in turn, required increasingly precise trigonometric values. For example, longitude at sea could well be determined by Bhāskara I's method of determining longitude using a clepsydra, to measure the time difference between the time of observed phenomena and their calculated time for a reference longitude: typically the meridian of Ujjayinī, which then played a role similar to the present-day (and presumably derivative) notion of the meridian of Greenwich. These methods, however, could only be as accurate as the trigonometric values used, and hence a great interest in precise trigonometric values also characterized early European navigational theorists of the 16th c. CE like Nunes, Clavius, and Stevin.

The new finite difference method of computing precise values of trigonometric functions, expounded in the *Āryabhaṭīya*, breaking away from earlier geometric methods, was the key breakthrough enabling this precision, as we have already seen. Because of its great practical value, this knowledge was also vigorously pursued by Arabs and Moghuls (al Kāshi), and the Chinese.

I
TRIGONOMETRIC VALUES AND TIME MEASUREMENT

The calculus, as we saw in the previous chapter, developed in the process of deriving ever more accurate values of trigonometric functions. Trigonometric values are unknown to Hellenic tradition, howsoever broadly interpreted to include a big part of the African continent. Egyptians certainly developed astronomy—presumably for its practical value for navigation, for example. Some of this Egyptian knowledge of astronomy is presumably incorporated in the work of "Ptolemy of Alexandria", whose astronomical "observations" were reportedly carried out in the Great Library of Alexandria.[2] Nevertheless, even in the 11th c. *Almagest*, as Toomer remarks, "Ptolemy" used only the chord. As we have seen, "Ptolemy" had difficulty with multiplication and fractions, and did not even remotely have a technique of square-root extraction, so it is not clear how or why (or even whether) he actually derived his table of chords, in the form in which it is presented to us today through the diligent work of historians. Trigonometric values first appeared in Europe with Regiomontanus (1434–1476 CE), who undoubtedly obtained these values from unacknowledged Arab sources, using them in the context of astronomy, with a view to navigation, as in the lunar ephemerides that he published.

Twelve hundred years earlier, in India, trigonometric values were already in use in the *Sūrya Siddhānta*, and we saw how Āryabhaṭa I, in the 5th c. CE, modified the method of obtaining those values, shifting from earlier geometric techniques to a finite difference technique. Why were trigonometric values important? They are found in the context of *jyotiṣa*.

The Meaning of Jyotiṣa

What is *jyotiṣa*, and why was *jyotiṣa* important? In the typical character of Western histories of science, Western historians like Pingree have referred to *jyotiṣa* as "astral knowledge"—perhaps the sort of religious and astrological thing that Kepler and Newton[3] were deeply interested in. (Newton was not the only scientist to reconcile his belief in prophecy with science; scratch a physicist today, and he will tell you that the test/value of a physical theory relates not to refutability but to its ability to predict the future! As for Kepler, we will soon see the relevance of his profession of astrology.) The belief in prophecy has a long and deep seated cultural history in the West. A key objection to Islam, listed by Thomas Aquinas, was that its founder, Paigamber Mohammed, made no prophecy. In fact the Western belief in prophecy dates back to the time when the first oracle was introduced in Greece by a black woman abducted from Egypt.[4]

This belief in prophecy and divination related to the strong anti-scientific bias in Greek culture already noted: at his trial, Socrates was accused of not worshipping the moon as a god, but of regarding it as a clod of clay—for this offence he was to be punished with death. Socrates responded that his accuser was confounding him with Anaxagoras (who had earlier ed, after being found guilty of the same offence), and that he was innocent of any dangerous physical speculations, and that he believed in gods, since he believed in demigods, and Socrates went on to swear by Zeus to establish that he was not a disreputable and scientific atheist.[5]

It would, however, be a gross misrepresentation of early Indian tradition to attribute to it any such belief in astrology or prophecy, or any similar anti-scientific bias. Varāhamihīra (6th c. CE) is the earliest person to whom astrology in India is attributed. Irrespective of whether this attribution is correct, astrology in India postdates Varāhamihīra so, in all likelihood, astrology was transmitted into India.

This is in contrast to Indian astronomy which has a long history of some 3000 years of indigenous development, in response to local practical needs, from about −1350 CE[6] to about 1550 CE (*Yuktibhāṣā*) or, perhaps 1730 CE (Jai Singh). The *Vedānga Jyotiṣa* places timekeeping[7] at the head of all sciences (*śāstra-s*), describing it to be like the plume of the peacock or the crest-jewels of serpents.

Hence, also, unlike Greek tradition which abounds in oracles, no respectability is attached to astrology in early Indian tradition. Indeed, the Buddha states, quite categorically in the *Dīgha Nikāya*,[8] that *common people* regard fortune-telling as an unethical means of livelihood.

> "It is, monks, for elementary, inferior matters of ethical practice that the worldling would praise the Tathāgata. . . ." "Whereas some ascetics and Brahmins make their living by such base arts as predicting good or bad rainfall. . . the ascetic Gotama refrains from such base arts and wrong means of livelihood."

It may be noticed that the Buddha is here referring to the then common perceptions of ethical ways of earning a livelihood—and at the time of the Buddha, the common people were not already Buddhists, but were what Westerners would call "Hindus".

Jyotiṣa, as in the *Vedāṅga Jyotiṣa*, referred to timekeeping, which Western historians have purposely mis-translated as astrology, astral knowledge and the like. As I have repeatedly pointed out in other contexts,[9] the entire *Vedāṅga Jyotiṣa*[10] does not contain a single sentence relating to astrology or prophecy—it is entirely a practical manual of timekeeping relating the time measured by a clepsydra to solar and sidereal days etc., for example, as follows:[11]

> A vessel which holds (exactly) 50 *palas* of water is the measure called *āḍhaka*. From this is derived the *droṇa* measure (which is four times the *āḍhaka*). This lessened by three *kuḍava* measures (i.e., three-sixteenths of an *āḍhaka*) is the volume measured (in the clepsydra) for the length of one *nāḍikā* of time.

(As explained in (R-VJ 16) and (Y-VJ 38), 2 *nāḍikā*-s make a *muhūrta*, 30 *muhūrta*-s make a day, so *muhūrta* had exactly the same meaning as it has today, so that 60 *nāḍikā*-s make a [civil] day.)

Neither can we find a word of astrology in the works of Āryabhaṭa, Bhāskara, Varāhamihīra's *Pañcasiddhāntikā*, or in Vaṭeśvara, or Āryabhaṭa II, or Mādhava, or Jyeṣṭhadeva, which tradition of *jyotiṣa* collectively spans some 3000 years. This is in stark contrast to the Greek belief in oracles, and the related belief in the religious value of prophecy, which continues right down to Kepler who cast horoscopes and, quite naturally, extended his practice of deliberate fraud also to his scientific "observations".[12] The Indian approach to science was practical, not religious like that of Newton[13]—in whose religious beliefs prophecy was a key element.

To summarize, *jyotiṣa* mistranslated as astrology (*phalita jyotiṣa*) is of no concern to us or to any of the texts under consideration. In our contexts, *jyotiṣa* refers to its original sense of timekeeping, and to related questions about astronomy and mathematics. In India, unlike the case in the West, astrology did *not* precede astronomy, but came some 2000 years after it.

The Practical Importance of Timekeeping

From very ancient times, timekeeping has been done by observation of the sun, moon, and stars. Many ancient civilizations have left behind numerous evidences of their consequent interest in astronomy and mathematics. One would imagine that (if our ancestors were not utterly foolish, as Western historians have continuously sought to portray) all this effort would have had some practical purpose in mind.

Astronomy was used for timekeeping, but why was timekeeping important? In the European tradition, for a long time, the only function of timekeeping was ritualistic. Time meant the time for saying prayers. Notwithstanding the doctrine of progress, the Christian calendar regressed into a purely ritual calendar, as follows.

The word "calendar" derives from the Latin *calends*; in pre-Christian Rome, this meant the first day of the month, and especially the first day of March on the Julian calendar, from which the new year commenced, near the vernal equinox. (Prior to the Julian calendar there prevailed in the Roman territories only confusion regarding timekeeping—correctly, if unwittingly, characterized as "ultimate" confusion by Julius Caesar, who sought to end it by recourse to a year of 445 days!) The Roman year was primarily a civil year, with only a coarse correlation to astronomy—[14]and the calends seem to have been used mainly for accounting purposes.

This Roman civil year acquired a ritual significance for Christians. Prior to Constantine, the Christians in the Roman empire celebrated "Easter" (called *pascha* in Greek) along with the Jewish festival of Passover.[15] On this day a goat is sacrificed, as in the Islamic festival of Bakr-Id (Id-uz-Zuha, *Eidul Azha*, which is however celebrated in the 12th month). However, the Jewish calendar, like the Islamic calendar, is a lunar calendar, derived from Babylon (Iraq), and has some 354 days in a year, with an additional inter-calary month.[16] Consequently, the festival of the Passover which occurs after the 14th day of the first month after the vernal equinox, may occur on any day of the week.[17]

This situation was displeasing to a certain section of the Christians in the Roman empire, for they wanted their holy day to fall always on the Christian Sabbath, viz., Sunday, and this was decreed accordingly by the Council of Nicaea, regarded as the critical turning point in Christian history, which however had the sole agenda of fixing the date of Easter. (Easter was fixed as the first Sunday after the first full moon after the vernal equinox, provided it did not coincide with the Passover—in which case it was to be moved to the next Sunday.) The Easter festival relates to the Roman and Jewish new year, and what then was the Christian new year—it is obviously not possible to accept in any literal way the Christian myth that Easter commemorates the "historic" event of the resurrection of Christ.[18]

This reliance on the cycle of the week—7 civil days—to determine the key festival which marked the new year, shows how the Christian calendar came to be disconnected from astronomical phenomenon. This rift widened with the long-term drift between the tropical year and the Roman civil year of $365\frac{1}{4}$ civil days. Furthermore, while the date of Easter fixed by the Nicene council was determined relative to the first full moon (at Alexandria, after the vernal equinox), and hence was "moveable", later Christian festivals occurred on fixed days of the civil calendar, further alienating the calendar from any natural phenomenon.

Thus, in European tradition, only moneylenders and priests needed to know how to tell the time of the year: and the latter especially were little concerned with astronomical phenomena, and were too innumerate to handle it.[19] Thus, the Julian calendar came to be ritualised and completely divorced from both natural phenomenon and the process of economic *production*. This completely ritualistic approach to timekeeping is evident even today in the names for the times of the day itself, like noon, that are derived from the time of saying prayers. But this sort of totally ritualistic approach to time was exclusive to European tradition. It would be wrong to generalize this to other traditions, as some historians and sociologists have attempted to do.

The Calendar and Indian Agriculture

What was the practical importance of timekeeping? In Indian tradition there is a straightforward answer. The calendar was and is closely related to key aspects of economic production. The entire economy was (until recently) dependent upon agriculture (although in the last few years the contribution of agriculture has declined to a little less than 60%). And agriculture in India was (and to a substantial extent remains today) dependent upon the monsoons. Quite unlike the English notion of "a rainy day", in India the arrival of rain is widely celebrated, and this celebration has long been re ected in traditional songs, poems, literature, annual festivities, etc., all of which underline the great importance of the monsoons to Indian tradition. Hence also the importance of timekeeping: a calendar is required to know the timing of the seasons, and in particular the rainy season.

The successful practice of agriculture in India required a successful method of timekeeping, to synchronize agricultural activities with the start and end of the monsoons, for example. Consider, for example, what befell the European calendar (Julian calendar) when it first came to India, in Goa. The European calendar lacked (and still lacks) the concept of a rainy season. After the Christianization of Goa, through forcible mass conversions, destruction of all temples in Goa, etc.,[20] marriages had to be performed in church halls, and the church fixed an "appropriate" time for weddings, depending upon the convenience of the church in Europe. This created great consternation among the people in Goa, since this "appropriate time" happened to fall bang in the midst of the harvesting season, so the families themselves suffered great loss, and the guests were most reluctant to come. This led to a number of appeals to Rome to permit a revision in the allowable dates of marriage. That was eventually done, but the calendar (and later the Gregorian calendar) continued to lack a fixed date to mark the start or end of the rainy season. So, there may be some point after all in consulting a *jyotiṣī* (in the sense of timekeeper, not astrologer) for the appropriate date of marriage!

A calendar does not simply refer to that piece of paper which decorates the walls—pandering to the vanities of petty Roman dictators. In the Indian context, the calendar must be able to determine the rainy season just as the year (*varṣa*) relates to rain (*varṣā*) in the language. To this end, consider the more recent event of three years ago, which demonstrated the continuing contemporary importance of the calendar for purposes of Indian agriculture, and shows the havoc that can even today be caused by a "delayed monsoon" or a bad calendar. To bring out the avour of the events as they were experienced, the boxes 4.1 and 4.2 draw verbatim from articles written at that time.

Box 4.1. The not-too-soon monsoon of 2004

"Drought grips half the country: 274 of 524 Met Districts Get Deficient or Scanty Rainfall" screamed the top-left headline of *The Times of India* (New Delhi, Friday, 30 July 2004, Late City Edition). It is raining cats and dogs outside, and the *Hindustan Times, Bhopal HT Live* of the same day (31 July) points out on its front page that, after the recent heavy showers, only one district in MP remains classified as having scanty rainfall (−60% of average). The Met department has issued a warning of further heavy rains. The basis of the *Times of India* report is clear from the punny "Wither report" graphic which accompanies the headline, but is based on nine-day old data (as of July 21). Admittedly, it has been many years since I have thought of *The Times of India* when I was looking for an instance of responsible journalism; however, what is one to make of the fact that the MP government itself had already prepared a plan asking the centre for Rs 200 crores as drought relief? Obviously, the government could not have waited for the drought to become full blown. According to another report, the Central government has already released Rs 50 crores to MP by way of drought relief. However, with reports of oods from Assam to Mumbai, and various places in between, it might have been better to prepare for ood relief!

More seriously, although the *HT Bhopal Live* report tells us that the rains have arrived just in the nick of time to save the crop, the TOI in a related report (p. 8) sounds the sombre warning that normal rainfall now may not save the crop a significant proportion of which was sowed long ago.

Clearly, agricultural operations were significantly mistimed, and that can be potentially damaging to the crop. But what was the reason for this mistiming? To repeat the question I raised last year: was the monsoon delayed or is the calendar wrong?

India has officially recognized two calendars, and according to the traditional *pañcāṅga*, the current month is an *adhika māsa*—an intercalary month—it is an additional *Sāwan*, and, as any child knows, *Sāwan* and *Bhādon* are the months in which it rains. The second month of *Sāwan* commences on 17th Aug 2004, and since there are two *Sāwan*-s, Rakhi comes as "late" as 29 August. So the monsoon has arrived pretty much on time according to the traditional calendar, exactly as happened last year, when, too, the monsoon was declared to be delayed according to the Gregorian calendar, according to which the monsoon should have arrived long ago, by the first week of July. So who or what is to blame for the wrong timing of agricultural operations: the monsoons or the Gregorian calendar?

As an illustration of the old adage—that those who don't learn from history are condemned to repeat it—it should be pointed out that the same point had been made a year earlier, but it did not quite register with those in authority.

Box 4.2. Could India s "failed" monsoon have been predicted by the right calendar?

Agriculture traditionally was the mainstay of the Indian economy, and still remains vital to the Indian economy. Accordingly, a method of timekeeping in the form of a good calendar remains a critical technology in India. Traditional calendar-making techniques, calibrated over centuries, therefore, deserve serious consideration and evaluation, and should not be rejected in a cavalier manner.

Consider the current situation. This year [2003] the monsoon did not arrive for so long that there was a severe water crisis, and the government declared the state to be severely drought affected. Eventually the monsoon has arrived, after nearly a month of delay, and in Bhopal at least, the deficit has been wiped out, with oods in nearby rivers. (It is still raining heavily, but water is still being supplied only on alternate days!)

The question is this: was the monsoon delayed? or is the calendar wrong?

The background to this question is as follows. The traditional Indian calendar uses the sidereal year, while the Julian and Gregorian calendar uses the tropical year. The sidereal year is the time period in which the sun returns to the same position with respect to the stars—it is the orbital period of the earth around the sun—while the tropical year is defined as the time between two successive vernal equinoxes. The sidereal year involves the motion of the earth relative to the stars, and is MORE than 365.25 days (365.256363 days, approximately), while the tropical year involves the motion of the sun relative to the earth, and is LESS than 365.25 days (365.24219 days approximately, at the present epoch). The difference between the two types of years is approximately 20 minutes per year (1223 s), which can become substantial over long periods. The difference is attributed to the precession of the equinoxes: the axis of the earth is thought to precess like a top, so that it points to different points in the sky at different times along a cycle of some 26,000 years (i.e., Polaris was not the north-star a few thousand years ago, and will not be the north star a few thousand years from now). One sidereal year is roughly equal to $1 + \frac{1}{26000}$ or 1.000039 tropical years.

The Julian calendar was based on the tropical year or the equinoctial cycle; so is its corrected version—the Gregorian calendar (which is the calendar in current use).

The Gregorian calendar reform committee tried to consult Indian calendrical sources, as I have pointed out elsewhere—in connection with the transmission of the differential calculus from India to Europe. Christoph Clavius was the head of the Gregorian calendar reform committee, and just prior to the Gregorian calendar reform of 1582, Clavius' student, Matteo Ricci, was in India, in Cochin, searching for Indian calendrical manuals, after having been appropriately trained for this purpose. (I have a photocopy of Ricci's original handwritten letter.) Europe then lacked the knowledge needed for a precise determination of the length of either the tropical or the sidereal year.

The Gregorian calendar reform itself was initiated because the Julian calendar fixed the length of the year very crudely—in my opinion just because the Romans were not adept with fractions. Because of the error in the second decimal place (the Julian calendar took the year to be exactly 365.25 days) the Julian calendar slipped by about 1 day every 128 years or so ($365.25 - \frac{1}{128} = 365.24218$ days), and had, by 1582 CE, slipped about 10 days out of phase in the 1250 odd years since the Council of Nicaea fixed the date of Easter, by fixing the date of the vernal equinox on XII calends (21 March). Thus, towards the end of the 16th c. CE, the vernal equinox used to arrive around 11 March on the Julian calendar. The Gregorian calendar reform corrected that by (a) advancing the calendar by 10 days, and (b) by making every centennial year (e.g. 1700, 1800, etc.) not a leap year, except when divisible by 400 (e.g. 2000). Basically, by removing some 3 leap days in 400 years (or 1 day in 133 years) the Gregorian reform corresponded to a more accurate figure for the fractional part of the length of the tropical year, which it set at $365.25 - \frac{1}{133\,3} = 365.2425$ days. This correction of the calendar was needed for the very practical purpose of fixing latitude from observation of solar altitude at noon. (Navigation was, then, extremely important for Europe, which was then way behind the Indians and Arabs.) Although everything in Europe, including the mode of dress, required clerical approval, there could not have been any serious doctrinal considerations: the date of Easter was fixed at Nicaea more from a desire that Christians ought to differ from the Jews, and that objective would have been unaffected by a change in the date of the vernal equinox on the calendar. There was no doctrinal pressure from the Protestants for such a change—quite to the contrary they initially opposed the change, then later accepted it. Furthermore, the difference of ten days was too little to have had a visible effect on the seasons. But such a major step obviously had to have had a strong practical motive, which was why it was accorded religious approval.

The critical input needed for the reform of the Julian calendar was the exact length of the tropical year, sometimes called the problem of epacts in theological terminology. The Roman church had tried to find a solution to this problem since pope Hilarius in the 6th c. CE, but these attempts were unsuccessful, despite access to all works in the Roman empire, including obviously the works of "Claudius Ptolemy" of Alexandria—in the form in which they then existed, if they did. The length of the year was, however, very accurately known in India at least since about the 3rd c. CE.

Gregory's bull only mentions a book by one Alyosius Lilio brought to his attention by his brother Antonio Lilio, who apparently used the Alphonsine tables, and thus obtained this information from Arabic sources like Copernicus did. While this information from Arabic sources had been around for some time in Europe, Europeans lacked the means to verify it. Hence, quite possibly the critical input that the Jesuits in India provided was an "independent" confirmation of the validity of those figures, giving the green signal to Gregory.

The change of calendar did initially become a religious issue, since this changed also the date of Easter on the civil calendar (the sole point on the agenda at the Council of Nicaea, which hence practically defined the Nicene creed). Protestants, among others, opposed the papal bull. The reformed calendar was eventually accepted in Britain and in USA (then a British colony) only in 1752, by advancing the calendar by 11 days and implementing the rest of Clavius' recommendations.

Though neither calendar has changed significantly in the last 500 years, perceptions have. Therefore, ironically, after independence, the Indian calendar reform committee adopted the Gregorian calendar without much ado! In its report, the Indian calendar reform committee,[21] dominated by M. N. Saha (and N. C. Lahiri), simply stated that it is obvious that seasons depend on the tropical year.

> For calendarical purpose [sic], it is unmeaning to use the sidereal year... as then the dates would not correspond to seasons. The use of the tropical year is enjoined by the Hindu astronomical treatises like the *Sūrya Siddhānta* and the *PañcaSiddhāntikā*. But these passages have been misunderstood, and Indian calendar makers have been using the sidereal year with a somewhat wrong length since the fifth century AD.

If that is so, then the traditional Indian calendar ought to have slipped out of phase by around 21 days over the last 1500 years. Such a major failure should be pretty obvious, but is it? (Also, I don't see the part about "misunderstanding", since Āryabhaṭa, prior to Varāhamihīra and the *PañcaSiddhāntikā*, unambiguously advocates the sidereal year.)

Exactly how is it obvious that one must use the tropical year? While it is true that physically the sun is the main source of heat, one does not merely want to determine the hot and cold seasons—for the key feature of the calendar in India relates to the monsoons, which are the mainstay for agriculture. The monsoons depend upon the wind regime.

The wind regime or global circulation is not, however, decided solely by the position of the sun. Hot air rises at the equator, but it does not descend at the poles. Because of the so-called Coriolis force, due to the earth's rotation, the air is de ected and descends before the horse-latitudes.

The monsoons, thus, depend also upon the Coriolis force. The Coriolis force is an inertial force. The only possible inertial frame being a frame fixed relative to the distant stars, the Coriolis force hence relates to the sidereal motion of the earth. Thus it might be that the monsoons relate also to the sidereal year.

At any rate, the monsoons have arrived on time according to the Indian calendar, since Rakhi too was "very late" this time, and the current month is still *Srāvana*. (The calendar we are talking about was calibrated for Ujjain, about 150 km from Bhopal.) The monsoons, however, are delayed by a month according to the Gregorian calendar: or, to put it differently, the Gregorian calendar has given the time of the monsoons in a grossly incorrect way. If the monsoons depend only on the tropical year, then, because of the difference between the tropical and the sidereal year, it is the Indian calendar that ought to have been out of phase by three weeks (around 21 days).

Admittedly, the argument sketched above is no more than a conjecture at this stage, but it does show that there is no particular basis to the belief that the tropical year decides the periodicity of the monsoon. Actually solving the Navier–Stokes equations over a long period to ascertain what the periodicity of the monsoon depends upon is a supercomputing problem (still a "grand challenge problem" according to NASA). In the absence of an actual solution, the assertion that the monsoons should have a simple periodicity depending upon the tropical year is also not particularly credible, but is merely an article of belief. At any rate, one cannot consider as obvious that the seasons depend only on the tropical year, and that the traditional Indian calendar is hence wrong. Perhaps this is so, but there is nothing obvious about it, and a study at least is needed, to establish things either way. The tropical year might well work for the seasons in Europe, but the considerations in India are obviously different. (I may note in passing that what is required obviously is a causal rather than a statistical account.)

There could, of course, be other reasons why sidereal time was used in Indian astronomy. The rotation of the earth varies less than the apparent motion of the sun around the earth, so that the sidereal year provides a better method of timekeeping. It is better suited to planetary models, for the sidereal year is the "actual" time for the earth's orbit. It is also a more convenient method of timekeeping: for stellar transits are easy to observe, etc.

A sidereal day is 23 hours, 56 minutes, 4.09 seconds, about 4 minutes *less* than a tropical day (in contrast to the sidereal year), so that there are 366.2422 sidereal days in a tropical year, compared to 365.2422 tropical days.

If the matter of the traditional calendar is re-opened, it will be necessary, of course, also to summarize—if not sort out—the whole vexatious issue of the precession of equinoxes vs libration: whether or not the precession of the equinoxes is actually taken into account in the Indian astronomical literature.

(A similar story was repeated in 2006, but it is too late to include those details in this book.[22])

Accordingly, known methods of timekeeping date back to the *Vedāṅga Jyotiṣa* (ca. −1350 CE). (Doubtless, Harappans too had a calendar, but that script is yet to be deciphered, so there is nothing further to be said about that as of now.) There was and is no doubt a relation of the calendar to rituals: but the rituals related to festivals, and so many major festivals in India related (and still relate) to agriculture, or other productive activities. Underpinning the idea of rituals as a stratified, hence possibly degenerate, form of knowledge, those entrained into the rituals, like Holi, say, automatically carry out harvesting at the "right time", after which it might get hot, and the crop might be damaged.

Calendrical Recalibration and Determination of Latitude and Longitude

India is a large country. While political boundaries have obviously varied over time, the preceding sentence is also historically true in the sense that we find similar cultural practices spread across a wide geographical area. The similarity of cultural practices made it natural for a calendar made in one place to be used in another. But, just because India is so large, the calendrical calculation made for one place cannot be used directly in another place within India—they need to be recalibrated. Despite a profusion of local colour, the various *pañcāṅga*-s were based on essentially similar principles, so that a comparison of two calculations could also required recalibration. In any case, the practice evolved of doing the calendrical calculations for one place (Ujjayinī), and then recalibrating as appropriate for another place.

This recalibration of the calendar, however, is not a trivial matter. To do this recalibration, it is necessary to have an understanding, for example, of how the sun, the moon, and the stars will be seen from different parts of the earth. In particular, this requires (1) knowledge of the shape and size of the globe, and (2) the ability to determine the latitude and longitude of a given place, to be able to relate the astronomical observations made at one place with those made at another.

II
THE SPHERICAL EARTH IN INDIAN TRADITION

In Indian tradition, definitely from the time of the *Sūrya Siddhānta* and Āryabhaṭa, and probably from long before that, the earth was regarded as a sphere. As Āryabhaṭa describes it (*Āryabhaṭīya, Gola* 6–7):

> The globe of the Earth stands supportless in space. . . Just as the [spherical] bulb of a Kadamba ower is covered all around by blossoms, just so is the globe of the Earth surrounded by all creatures, terrestrial as well as aquatic.

While Āryabhaṭa does not feel the need to defend the idea of a round earth, later writers like Lalla (748 CE) do. Lalla, in the 20th chapter of his *Śiṣyadhīvṛddhida*[23] examines various false notions, and states that some people have the following false notions about the earth.

> (20.6) Some think that the earth is infinite; others that it is plane like a mirror. Again, others say that it extends to many *yojanas* and oats on water like a boat.

> (20.7) Some say that the earth is supported by a tortoise, a serpent, a boar, an elephant or by mountain ranges....

He then refutes the belief that the earth is plane through a variety of arguments, some of which are the following.[24]

> (20.31) The eclipse, the conjunction and rising of planets, the cusps of the Moon, and the length of the shadow (of the gnomon) at any time—the calculation of all these five depends upon the measurement of the earth, and agrees with the observed result.

> (20.35) Mathematicians say that one hundredth of the circumference of the earth appears to be plane.

> (20.36) If the earth is level, why cannot tall trees like the date palm, alas, be seen by man, though at a very great distance from the observer.

He separately refutes the belief that the earth is supported:[25]

> (20.39) Clay is destroyed by water, so it is not possible for the earth [made of clay] to remain in water or to oat on it like a boat.

> (20.40) If the heavy sphere of the earth can remain on water, which water stands supportless in space, why can the earth not remain in space?

> (20.41) If the earth is supported by a tortoise or other things, by whom are they supported in space? If they can remain in space [unsupported] what prevents the earth from remaining thus [unsupported]?

This idea is elaborated by Vaṭeśvara in his book also called *Gola* (meaning round or spherical, since this too deals with the same subject of spherics).[26]

> (V.2) Just as an iron ball surrounded by pieces of magnet does not fall through standing (supportless) in the sky, in the same way this Earth though supportless does not fall....

> (V.5) If the earth is supported by Sesa [serpent], tortoise, mountains, and elephants, etc. how do *they* stand supportless (in space)? If they are believed to be endowed with some power [to stand supportless], why is not the same power assigned to the Earth?

He also refutes the idea that the earth would fall down, on the grounds that "up" and "down" are decided by reference to the centre of the earth.

> (V.3) If you are inclined to believe that it falls down, say what is up and down for an object standing in space. The globe of the Earth... in what direction then should it fall?

> (V.7) As here in our locality a ame of fire goes aloft in the sky and a heavy mass falls towards the Earth, so is the case in every locality around the Earth. As there does not exist a lower surface (for the Earth to fall upon), where should it fall?

He goes on to comfort people who are afraid they might fall off the earth.

> (V.8) Just as a house lizard runs about on the surface of a pitcher [pot] lying in open space, so do the human beings move about comfortably all around the Earth.

Writers who precede Lalla and Vaṭeśvara, e.g. writers like Āryabhaṭa, or Bhāskara, or Brahmagupta, all invariably state that the earth is spherical, they state its dimensions etc., but they do not refute any such beliefs in a at earth. This suggests that the view was not seriously contested in their time.

(However, Āryabhaṭa's idea that the apparent movement of the celestial sphere is an illusion, "just as a man seated in a boat moving forward sees the stationary objects [on the river banks] moving backwards, just so are the stationary stars seen... as moving exactly west" (*Gola*, 9), is entirely his own, and is rejected by almost every one else in Indian tradition, including Varāhamihīra[27] and Vaṭeśvara, who regard the earth as stationary.)

Likewise, Varāhamihīra[28] and al Bīrūnī[29] not only stated this but they also arrived at fairly accurate estimates of the radius of the earth. (They erred in taking the earth to be a perfect sphere; but this was a legitimate approximation.) It is possible to compare the last estimate with current estimates, since the Arabic mile is accurately known in terms of current measures of length.

These estimates, like those of the 9th c. al Ma'mūn (accurate to within 1%) were far, far superior to the European estimates from Columbus to Newton: Columbus, for instance, hawked the theory that the distance from Portugal to the Chipangu (Japan) of Marco Polo was 2760 miles, when the actual distance is closer to 12,000 miles.[30] Thus his estimate was less than 25% of the actual value. (Newton's initial estimate was marginally better off at 40%.)

Being relatively isolated, the traditional navigational techniques of the Lakshadweep islands give us a fairly clear picture of the navigational techniques then used in the Arabian Sea. This knowledge of the round earth and its size was embodied in the traditional navigational practices: in the definition of the *zām* as the "distance from here to the horizon".[31]

Calendrical recomputation for a given place depended upon the location of the place, and Indian astronomers used a system of latitude and longitude to fix location on both the celestial and terrestrial spheres. For the determination of terrestrial longitude, they took as a standard the meridian through Ujjayinī (modern-day Ujjain). The present-day idea of the meridian of Greenwich may well be a direct (though unacknowledged) copy of this early Indian idea of the meridian of Ujjayinī as a time standard for longitude determination.

Āryabhaṭa and subsequent astronomers had a clear idea of how the sky is perceived from different parts of the globe. In particular, they had a clear idea of how drastically things vary with latitude. Thus, Āryabhaṭa asserts that day and night last for six months each at the poles (*Gola* 16–17):

> The gods living in the north at the Meru mountain (i.e., at the north pole) see the Sun, after it has risen, for half a solar year; so is done by the demons too [who live at the south pole].

Hence, in the computation of the duration of a Mahāyuga, in the Viṣṇu Purāṇa, there is the equation 1 year of mortals = 1 day and night of the gods, because it was believed to be literally true!

Calendrical recomputation required an answer to the famous *tripraśna* (three questions) about direction, place, and time. Hence, it required methods of determining the local latitude and longitude, and some of these methods of determining the local latitude and longitude had obvious applications to navigation.

III
EARLY NAVIGATION

Navigation did exist. Charts being central to Western techniques of navigation, Western accounts of the history of navigation have concluded with facility that the absence of charts indicates an absence of navigation. Perhaps one should not judge this erroneous conclusion too harshly, for at least an attempt has been made to provide evidence, which is a decided improvement over the usual fantasies substituted for history in the West.

It is very easy to see the untenability of the argument that navigation was impossible in the absence of charts, so that early navigators simply crept along the coast. Admittedly, this was the method adopted by Nearchus,[32] and Vasco da Gama, who was ultimately compelled to accept the advice that this technique could only lead him to the Red Sea and that he had to strike out across the ocean to get anywhere near the source of the spices that he sought. But, the "Guzerati Moor", Malemo Cana[33], who brought Vasco da Gama to Calicut, certainly understood how to navigate across the sea from Africa to India—and it is well-recorded that he navigated without the use of any charts. (Although it was Vasco da Gama who was doing the "creeping along the coast", it was he who is today regarded as navigator, while the actual navigator is called a "pilot"—one who only knows how to "creep along the coast".) A variety of islands were known: Mahal Dvīpa (Maldives) is found in every Arabic mariner's manual, though Arabs hardly used charts. Sailing out to these islands certainly involved sailing out of sight of land; hence it necessarily involved sharp problems of navigation, for small islands can easily be missed, as is recorded in European navigation manuals of even the 19th century. In view of this sort of clear evidence of the existence of navigation without charts, the argument linking the existence of charts to the existence of navigation can only be regarded as an attempt to falsify history in a crude sort of way.

If we discard such fabricated accounts of the history of navigation, it is clear that navigation has existed from the earliest pre-historic times. The size of Harappan docks suggests organized large scale trade, hence navigation. The same suggestion emerges from the earliest records, like those of the Buddhists and Jains. From the earliest recorded times, islands such as Lanka and Java were known. Sri Lanka is recorded in the Rāmāyaṇa, of course, but Ashoka also sent his daughter Sanghamitra there. One could hardly travel from India to Sri Lanka without sailing out of sight of land. Kautilya mentions the appropriate times for crossing the sea, suggesting that this was an established routine by his time.[34]

It has been alleged that though Indo-Arabic contacts stretch back to antiquity, the volume of sea traffic was small until Hippalus. The "discovery" of the monsoon winds by Hippalus seems inauthentic, and is perhaps a product of the historians' imagination like the "discovery" of India by Vasco da Gama. Perhaps this really was a discovery for the Romans, who learnt of navigation rather late.

The sea route also extended to China. As described by Fa-Hsien,[35] on his way back he stayed out at sea for a rather long time of ninety days. From Fa-Hsien's account it would appear that people could not navigate when the sky was overcast. This suggests the inference that the magnetic compass was not then in wide use, and that the navigational techniques in general use then were purely celestial. It should be pointed out here that navigational problems were particularly acute on the eastern coast of India due to the erratic monsoons, sudden shifts in wind direction, and a practically east–west course. There are also problems associated with uneven sea depth, sunken reefs, and magnetic anomalies. These acute navigational problems faced by the Cholas may have been part of the reason for wanting better sine values in astronomical techniques of navigation. Fa-Hsien's account is reproduced in box 4.3 as it is an early record which explicitly speaks of the celestial navigation techniques used in a long sea voyage.

Box 4.3. Fa-Hsien s description of a sea voyage

"The great ocean spreads out, a boundless expanse. There is no knowing east or west; only by observing the sun, moon, and stars was it possible to go forward. If the weather were dark and rainy, (the ship) went as she was carried by the wind, without any definite course. In the darkness of the night, only the great waves were to be seen, breaking on one another, and emitting a brightness like that of fire, with huge turtles and other monsters of the deep (all about). The merchants were full of terror, not knowing where they were going. The sea was deep and bottomless, and there was no place where they could drop anchor and stop. But when the sky became clear, they could tell east and west, and (the ship) again went forward in the right direction. If she had come on any hidden rock, there would have been no way of escape.

"After proceeding in this way for rather more than ninety days, they arrived at a country called Java-dvipa, where various forms of error and Brahmanism are ourishing, while Buddhism in it is not worth speaking of. After staying there for five months, (Fa-hien) again embarked in another large merchantman, which also had on board more than 200 men. They carried provisions for fifty days, and commenced the voyage on the sixteenth day of the fourth month.

"Fa-hien kept his retreat on board the ship. They took a course to the north-east, intending to fetch Kwang-chow. After more than a month, when the night-drum had sounded the second watch, they encountered a black wind [ta fung = the great wind = typhoon = toofan] and tempestuous rain, which threw the merchants and passengers into consternation.... After day-break, the Brahmans deliberated together and said, It is having this Sramana on board which has occasioned our misfortune and brought us this great and bitter suffering. Let us land the bhikshu and place him on some island-shore. We must not for the sake of one man allow ourselves to be exposed to such imminent peril.' A patron of Fa-hien, however, said to them, If you land the bhikshu, you must at the same time land me; and if you do not, then you must kill me. If you land this Sramana, when I get to the land of Han, I will go to the king, and inform against you. The king also reveres and believes the Law of Buddha, and honours the bhikshus.' The merchants hereupon were perplexed, and did not dare immediately to land (Fa-hien).

"At this time the sky continued very dark and gloomy, and the sailing- masters looked at one another and made mistakes. More than seventy days passed (from their leaving Java), and the provisions and water were nearly exhausted. They used the salt-water of the sea for cooking, and carefully divided the (fresh) water, each man getting two pints. Soon the whole was nearly gone, and the merchants took counsel and said, At the ordinary rate of sailing we ought to have reached Kwang-chow, and now the time is passed by many days;—must we not have held a wrong course?' Immediately they directed the ship to the north-west, looking out for land; and after sailing day and night for twelve days, they reached the shore on the south of mount Lao,...."[36]

Like calendrical recalibration, navigation required a way to answer the *tripraśna*, and determine latitude and longitude of a given place. By Fa-Hsien's time many such methods existed and were widely known.

IV
LATITUDE DETERMINATION

Latitude from the Pole Star

The latitude of a place on earth coincides with the altitude of the celestial pole. Since the pole star is approximately at the celestial pole, the simplest way to determine the latitude is by measuring the altitude of the pole star. This method was certainly well known to Indian tradition. While this method works for all of India, it is not so convenient to use at lower latitudes, on land, where the horizon may be obscured by trees etc. We take this up in the next chapter.

Latitude from the Equinoctial Midday Shadow

Another popular method was to determine the local latitude using the equinoctial midday shadow. This method is described by Bhāskara.

> On level ground erect a gnomon at the intersection of the direction lines (east–west and north–south lines), and test it for perpendicularity. Square the equinoctial midday shadow of the gnomon, and add to it the square of [the height] the gnomon. By this result divide the radius multiplied by [(a)] the gnomon, and [(b)] the shadow. This gives respectively the Rsines of the coaltitude and the latitude.[37]

That is, if
s = length of the equinoctial midday shadow,
g = height of the gnomon,
R = radius of the celestial sphere,
= latitude of the place, and
C = coaltitude of the place (= 90° −), then

$$R \sin C = \frac{g \times R}{\sqrt{g^2 + s^2}}, \tag{4.1}$$

and

$$R \sin \quad = \frac{s \times R}{\sqrt{(g^2 + s^2)}}. \tag{4.2}$$

The above corresponds to

$$\tan \quad = \frac{s}{g} \tag{4.3}$$

so that there is a very simple way to determine local latitude, *provided* one has, for example, an accurate way of calculating arctangents or arcifying sines.

Equinoctial Midday Shadow from Observations of the Pole Star

The equnioctial midday shadow can be determined directly by observations carried out at midday on equinox. But, of course, it is not necessary to wait until equinox to know the equinoctial midday shadow at a given place. One can determine the equinoctial midday shadow even by making observations on any night!

Thus, one can also find the equinoctial midday shadow by observations of the pole star, as is clear from the following verse of Vaṭeśvara.

> One should observe the Pole Star towards the north along the hypotenuse (*karṇa*) of the triangle-instrument, assuming its base to be equal to the gnomon; then the upright (of the triangle instrument), which lies between the line of vision and the base, will be equal to the equinoctial midday shadow.[38]

As Shukla elaborates,

> The triangle-instrument referred to here, is of the shape of a right-angled triangle. When it is held in the meridian plane towards the north with its base horizontal, its hypotenuse points to the Pole Star.

Since the angle between the sides meeting at the eye is equal to , the latitude of the place, and the base of the triangle has been assumed to be g, the size of the gnomon, therefore, the upright of the triangle instrument is equal to g tan , which by (4.3) is just the length of the equinoctial midday shadow.

Equinoctial Midday Shadow from Observations at Sunrise

The local latitude or the equinoctial midday shadow can also be determined in various other ways, for example through observations made at sunrise. For example, one can proceed as described in the *Vaṭeśvara Siddhānta* (3.1.12–14):

> 12. One should build an earthen platform which should be large, circular, as high as one's shoulders, with surface level with water, with circumference graduated with signs and degrees, and with well ascertained cardinal points.
>
> 13. Let a person, standing on the western side of that (platform) observe the rising Sun through the centre of the circle. Then the Rsine of the degrees of that point of the circle where he sees the rising Sun is the Sun's *agrā*.

> 14. The (Sun's) *agrā* multiplied by 12 and divided by the Rsine of the (Sun's) declination is the hypotenuse of the equinoctial midday shadow (*palasravana* or *palakarṇa*). By the difference between the hypotenuse of the equinoctial midday shadow and the gnomon multiply their sum and take the square root (of the product): the result is the equinoctial midday shadow (*akṣabhā* or *palabhā*).

There are a couple of implicit assumptions here. First, the circumference of the circular platform is supposed to be so graduated that the east mark is the zero. Hence, the point at which the Sun is observed to rise measures its actual angular deviation from the east. Accordingly, the Rsine of that angle is just the distance of the Sun's rising point from the east–west line. Second, the figure 12 comes from the assumption that the gnomon (*śaṅku*) is 12 *angula*-s (fingers) as usual.

The verse corresponds to the following. Let

a = the observed angular deviation of the Sun from the east,
Rsin a = *agrā*,
= Sun's declination,
h = hypotenuse of the Sun's equinoctial midday shadow,
s = the Sun's equinoctial midday shadow.
g = height of gnomon = 12.

Then

$$h = \frac{\text{Rsin}\, a \times g}{\text{Rsin}}, \tag{4.4}$$

and

$$s = \sqrt{(h-g)(h+g)} \quad = \sqrt{h^2 - g^2}\ . \tag{4.5}$$

The above relations (4.4), (4.5), when combined with (4.3), relate the local latitude to solar declination through observations made at sunrise.

This relation can be used in various ways. If the local latitude is known, it can be used to determine the solar declination on a given day. Alternatively, it can be used to fix local latitude as follows.

Ahargaṇa System, Declination, and Latitude

The Indian *ahargaṇa* system involved a simple day count from a fixed day. This *ahargaṇa* system is remarkably similar to what is today known as the Julian day-numbering system (except for its zero point). The Julian system is so named by Julian Scaliger, a contemporary of Clavius, who claimed to have "discovered" it just when he had ample opportunity to learn about the Indian *ahargaṇa* system.

Indian texts stated several algorithms which enabled ready computation of the *ahargaṇa* corresponding to the calendar date in question, and the *ahargaṇa* corresponding to the (nearest) equinox. That is, because the *ahargaṇa* system was prevalent, it was an easy matter to compute the number of days elapsed since equinox. (As we shall see, this was not possible with the Julian calendar.) From a knowledge of the number of days elapsed since the equinox, and the sun's maximum declination (taking it as approximately 24°), it is possible to approximately calculate the declination for any given day by a simple application of the rule of three.

Thus the local latitude (and the equinoctial midday shadow) could be readily calculated from a simple observation at sunrise, and a good calendar which embodied a knowledge of the precise date of the equinox.

Latitude Measurement from Solar Altitude at Noon

Similarly, local latitude could be determined by measuring solar altitude at noon. The advantage of this method is that it can be used anywhere (e.g. at sea) since it does not require a knowledge of the cardinal directions. (While cardinal directions are easy to determine on land, using the fish-figure, there may be a problem determining them at sea.) The (slight) disadvantage is that this method works best at lower latitudes. It is therefore a method complementary to the pole-star method, which works best at higher latitudes. It is also complementary in the sense that it is a method which works during the day while the pole-star method works at night.

The general relationship between solar altitude a, azimuth A and declination , at a place with local latitude , is the following:

$$\sin \quad = \sin \quad \sin a + \cos \quad \cos a \, \cos A. \tag{4.6}$$

At latitudes between the tropics, when the sun comes on the prime vertical, $\cos A = 0$, so that the relation simplifies to

$$\sin \quad = \sin \quad \sin a \tag{4.7}$$

when the sun is on the prime vertical.

This relation is described by Bhāskara I as follows.[39]

> The Rsine of the Sun's northern declination—when less than the Rsine of the latitude—multiplied by the radius should be divided by the Rsine of the latitude: the result is the Rsine of the altitude of the sun when it is on the prime vertical.
>
> The square root of the square of the radius diminished by the square of the Rsine of the Sun's altitude when multiplied by twelve and divided by the same Rsine of the Sun's altitude gives the shadow (of the gnomon corresponding to the Sun on the prime vertical.)

That is, when the sun is on the prime vertical, if a is its altitude, is its declination, and is the latitude of the local place, then

$$\text{Rsin}\, a = \frac{\text{Rsin} \quad \times R}{\text{Rsin}}, \tag{4.8}$$

which is the same as (4.7).

The next stanza gives the method of determining the solar altitude by observing the shadow s of the gnomon of length $g = 12$. It says,

$$s = \frac{g}{\text{Rsin}\, a} \quad R^2 - (\text{Rsin}\, a)^2 \quad ^{\frac{1}{2}}. \tag{4.9}$$

The above equation would be rewritten in present-day notation as

$$s = g \cot a. \tag{4.10}$$

Thus, using the knowledge available in widely circulated early 7th c. CE Indian texts, local latitude could be fixed at any location on land or sea, by day or night, either by observing the pole-star altitude, or, on any day of the year, by making simple observations at sunrise or midday or sunset, provided the solar declination for that day was known, i.e., provided the number of days elapsed since the equinox or solstice was precisely known.

V
LONGITUDE DETERMINATION

Time and Longitude

Present-day methods of navigation determine longitude using a chronometer. The basic idea is that the local time varies with the longitude, so by knowing the difference between the local time and the time at a fixed longitude, one can determine one's local longitude. This principle was long known to Indian astronomers. For example, Aryabhata has a clear idea of how time varies across the globe (*Gola* 13):

> When it is sunrise at Lanka, it is sunset at Siddhapura, midday at Yavakoti, and midnight at Romaka.

(The four names correspond to four imaginary cardinal points on the equator. In particular, Lanka is the point at which the meridian of Ujjayinī meets the equator, somewhat below the island known today as Sri Lanka. The other points are all 90° apart. Lanka, here, does not correspond exactly to the actual island of Sri Lanka any more than Romaka corresponds to Alexandria, or Siddhapura to Singhpur (Singapore).

Thus, this principle of time varying with the meridian was known to Aryabhata, Varāhamihīra, Brahmagupta, and hence to Arab astronomers in the Sind-Hind tradition.

The Prime Meridian

How was this principle of time varying with the longitude actually used? First, there was a concept of a prime meridian, as in the presumably derivative concept of the meridian of Greenwich that is in use today. The Indian prime meridian is described by various authors. For example, Bhāskara says,[40]

> The line which passes through Laṅkā, Vātsyapurā, Avantī, Sthāneśvara, and the "abode of the gods" [= Mount Meru = north pole] is the prime meridian [*deśāntara vidhāyanī*, lit. the prime meridian for longitude differences.]

(Here, Avantī is present-day Ujjain, and Sthāneśvara is present-day Thanesar in Pakistan; while there is some doubt about the precise location of Vātsyapurā, this is irrelevant for our purpose.)

Eclipses and Longitude

Various calculations were standardized for the prime meridian. In particular this included the time of the eclipses. This provides one method of telling the time difference between the local longitude and the prime meridian. This is indicated by Bhāskara as follows:[41]

> The difference between the computed and observed times of an eclipse is the longitude in terms of time.

The computed time here relates to the time of the eclipse as observed from the prime meridian, while the observed time relates to the time of the eclipse as observed at the local longitude.

Eclipses are occasional occurrences, so this method, of course, is suitable only for determining longitude on land.

Bhāskara s Use of a Clepsydra to Determine Longitude

How would this principle have been used to tell the time difference at sea? Though the mechanical clock, which could mechanically indicate the time of far-away places had not been invented, highly portable clepsydras (water clocks) were readily available. The simplest of these clepsydras was that very common thing found in every south Indian household: half of a coconut shell, with a hole in one of its natural eyes. More elaborate versions of this clepsydra were available in the form of a copper vessel with a minute hole at its bottom. One measured time by the duration that this shell (or a more elaborate copper vessel) took to sink. Longer periods were measured by immediately emptying the shell and repeating the process. Such a clepsydra, though highly portable, could not directly tell the time at a distant place. But, Bhāskara I explains how such a water clock may be used to determine the local longitude:

> On any day calculate the longitude of the Sun and the Moon for sunrise or sunset without applying the longitude correction, and therefrom find the time (since sunrise or sunset), in *ghatīs*, of rising or setting of the Moon; and having done this, note the corresponding time in *ghatīs* from the water clock. From the difference, knowledgeable astronomers can calculate the local longitude in time.[42]

That is, instead of measuring the time difference between the local place and the prime meridian by a mechanical clock or by the time difference between the observed (local) time and the calculated (prime meridian) time of an eclipse, Bhāskara recommends that one should use the time difference between observed and calculated time of a more frequent event like the rising or setting of the moon, relative to the rising and setting of the sun.

The method suggested by Bhāskara I was called the method of ephemeris time in Europe, and was first known to Europe through the work of Regiomontanus, who compiled a table of lunar ephemerides for navigational purposes, and specifically longitude determination. It could not be used for long in Europe because European lack of knowledge about various astronomical parameters made the ephemeris tables like those of Regiomontanus unreliable.

Solving the Longitude Triangle Using the Size of the Globe

Apart from the methods of eclipses and ephemeris, Bhāskara I lists a third method of determining the local longitude. Though Bhāskara I calls this method "gross", it seems to have been very popular, and is also mentioned by several other authors. This method involved solving the longitude triangle.

The longitude triangle was obtained as follows (Fig. 4.1). First one identified a nearby town on the prime meridian. The line joining the local place A to the identified town B was regarded as the hypotenuse AB of the longitude triangle. The perpendicular dropped from the local place on the prime meridian, and meeting the prime meridian at C gave the base AC of the triangle. The latitude difference, expressed as a distance, BC, was the upright of the triangle. The longitude triangle was then solved from a knowledge of the size of the globe, which enabled a calculation of BC from a knowledge of the latitudes of A and B and the distance AB "as known from common people".

This is described by Bhāskara as follows.[43]

> Subtract the degrees of the latitude of one of the towns mentioned above from the degrees of the [local] latitude, then multiply [the difference] by 3299 minus 8 25, and divide [the product] by the number of degrees in a circles [i.e., 360]. The resulting *yojana*-s constitute the *kotī* [upright of the right-angled "longitude" triangle]. The oblique distance from the local place and the town [on the prime meridian] chosen above, which is known in the world by the utterance of common people, is the hypotenuse. The square root of the difference between their

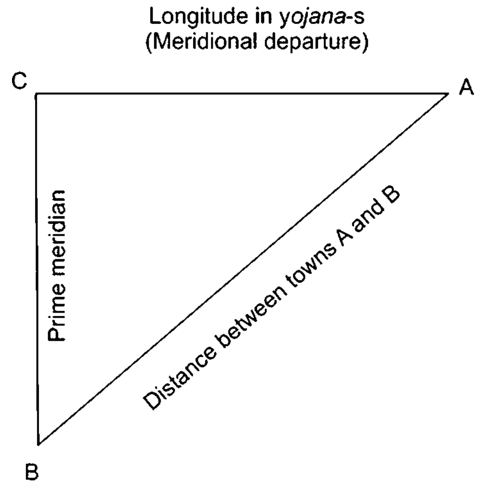

Figure 4.1: **Indian longitude triangle.** The longitude triangle used in India as a "gross" method of determining local longitude

> squares [i.e., between the square of the hypotenuse and the upright] is defined by some astronomers to be the distance [in *yojana*-s of the local place].

The only point which requires explanation in the above quote is the figure of $3299 - \frac{8}{25}$. Bhāskara takes the radius of the earth to be 1050 yojana-s,[44] and the value of π to be 3.1416, so that the circumference of the earth works out to $1050 \times 3.1416 = 3298.68 = 3299 - \frac{8}{25}$. When divided by 360° this gives the distance per degree latitude. So, what Bhāskara is saying is only that the difference (in degrees) of latitudes of A and B when multiplied by the distance per degree latitude gives the arm BC of the triangle. From a knowledge of the hypotenuse AB and the side BC one can evidently calculate the remaining side CA.

Longitude and Departure

Several possible objections may be raised to the third method of measuring longitudes suggested above. First, the above method yields what are called meridional departures rather than longitudes. Second, the method suggested above uses plane triangles, whereas accurate navigation would require spherical trigonometry.

These objections are easily met. As regards the first objection, it is quite true that departure differs from longitude, for the distance between meridians decreases from a maximum at the equator to zero at the poles. However, using accurate sine values and an accurate knowledge of the size of the earth, it is easy to convert between longitudes and departures. Bhāskara I states the rule explicitly.

> The *yojanas* (of the distance of the prime meridian) from the local place are obtained on multiplying the longitude in *ghatīs* by the local circumference of the Earth and dividing (the product) by 60.[45]

Here, *yojana* is a measure of distance, *ghaṭī* is a measure of time, and the concepts of departure and longitude are replaced respectively by "longitude measured in *yojana*-s" and "longitude measured in *ghaṭī*-s". The "local circumference of the Earth" refers to the circumference of the local circle of latitude; its value is given earlier by Bhāskara I:

> 3299 (*yojanas*) (the circumference of the Earth), multiplied by the Rsine of the coaltitude (of the local place), and divided by the radius (i.e., 3438') is known as the (Earth's) circumference at the local place.[46]

Furthermore, let us recognize that the reliance on charts for computing distance was an outcome of European reliance on the technique of dead reckoning for navigation. One need not be tied down to the European technique of dead reckoning for navigation, or to the use of charts for navigation, and for mechanically computing distance. So there is no reason why one should not simply use a coordinate grid consisting of latitude as one coordinate, and departure from a fixed meridian as the other coordinate.

Plane vs Spherical Triangles

The method of determining departures by solving a plane triangle was known to Arab navigators as a *tirfa* calculation. Some authoritative Western historians of navigation like Tibbets have given crude examples of actual *tirfa* calculations, to suggest that Arab navigators were unaware of elementary plane geometry in the 16th century CE. This is strange, considering the criticism of the use of plane triangles mentioned by Bhāskara I (629 CE) nearly a thousand years earlier.

Thus, after stating the above rule, Bhāskara hastens to add in the very next verse that the longitude so obtained is not particularly accurate, and that the resulting longitude

> ...has been stated to be incorrect by the disciples of (Ārya) bhaṭa ...on the grounds that the hypotenuse is gross... [and] on account of the sphericity of the earth....

That is, (a) distances obtained from accounts of common people need not be reliable, and (b) spherical triangles should be used instead of plane triangles.

Vaṭeśvara explicitly comments in his *Siddhānta* (904 CE) that "the Earth's surface being spherical, this [method of using plane triangles] is incorrect and unacceptable."[47] Spherical triangles should be used instead. Al Bīrūnī explicitly does this for longitude determination, using Brahmagupta's formula for cyclic quadrilaterals.

Finally, it is clear that the distance as obtained from common people need not be reliable. This point is belaboured by Śrīpati's commentator Makkibhaṭṭa, who states:[48]

> The above rule is incorrect because of the curvature of the Earth and because of the uncertainty of the distance in *yojana*-s depending on hearsay. No intelligent person has verified the popular [estimates of the distances in] *yojana*-s by actual measurement with the help of hand, staff, or rope. Therefore, in the face of plurality of popular estimates of distances, this rule is improper.

But the rule nevertheless seems to have remained popular, and is given by Brahmagupta,[49] Lalla,[50] etc., and long lists of towns on or close to the prime meridian have been provided by Lalla, Vaṭeśvara, Śrīpati, etc.

VI
SIZE OF THE GLOBE

The Longitude Problem of European Navigation

Before examining further details about Indian tradition, let us turn to the role played by this gross rule in European navigation a thousand years later. Prior to the invention of the mechanical clock, European navigators tended to rely on the disastrous method of "Dead Reckoning" (pun intended), which determined the departure (hence local longitude) by solving a plane triangle, in a somewhat similar manner.

The differences were as follows. Even the above gross way of determining longitude was not initially available to European navigators for a strange reason. The plausibility of Columbus' idea of sailing west to reach east rested on an erroneous belief in an earth much smaller than the accurate Arabic estimates from al-Ma'mūn's Mūsalī expedition. Presumably, Columbus de ated the accurate Arabic estimates, pretending that the earth was only $\frac{1}{4}$th its actual size, to help obtain funding for his project of reaching India by sailing West. Perhaps he genuinely believed it. In any case, Columbus' "success" lent weight to this wrong estimate for nearly two centuries: for example, Newton initially underestimated the size of the earth by some 60%, compared to the error of 0.25% in al Bīrūnī's estimate 6 centuries earlier. It was only after Picard's observations of 1671 that Newton revised his own estimates, and incorporated them in his later work.

The first consequence of Columbus's erroneous estimate was that the use of the globe for navigation by Europeans led to disasters, so that the carrying of globes aboard ships was banned by Portugal as early as 1500.[51] Therefore, even though we today retrospectively realize that Picard's estimate was accurate, this was not so clear to his contemporaries, and navigators in the 17th c. CE did not rush to change over to a new technique of navigation. The marine chronometer developed by the time (a century later) this revised estimate of the size of the earth became generally acceptable to European navigators, together with reliable and accurate sine tables. Moreover, there appears to have been a generalized cultural preference in the West for instrumentation that helps to avoid the mental exertion involved in a computation.

Since the European navigators of the 16th and 17th c. CE lacked a clear idea of the size of the globe, they could *not* correctly relate latitude differences to physical distances. They could however use the course angle to solve the same triangle. That is, they solved the longitude triangle from a knowledge of (a) course angle, and (b) distance travelled.

The other difference was that the input for the distance travelled was obtained by measurement all right, but the technique of measurement was the crude method of "heaving the log" and maintaining a continuous record of it in a log book. The more sophisticated form of this method was to use a rope with standardized knots, and measure the speed of the ship in "knots", which measured the length of the rope taken up, as the log oated out. It was well recognized, as we shall see in more detail in Chapter 7, that this resulted in very unreliable estimates of the speed of the ship, and hence of the distance travelled: estimates that were probably much worse than the estimates by common people of the distance between two towns. This led to the well-known problem that European navigators had with determining longitude at sea. The key part of the story that has not been told to date is that this was a problem peculiar to European navigation.

The irony is that the same plane triangle could have been solved from a knowledge of the course angle, the measured difference of latitudes between two points, and the distance per degree latitude, without having to rely on the distance.

Thus, if p is the departure, l is the difference of latitudes, d is the distance, and C is the course angle, in the plane sailing triangle (Fig. 4.2), the European method was to determine

$$p = d \times \sin C, \tag{4.11}$$

while the triangle could also have been solved by

$$p = a \times l \times \tan C, \tag{4.12}$$

where a, the distance per degree latitude, enables one to convert the measured latitude difference from degrees of the arc to physical distance. Having an accurate value of a is equivalent to having an accurate estimate of the size of the earth. Such an estimate of the size of the earth was also required to convert from departures to longitudes, as done by Bhāskara I.

However, during most of the 16th and 17th c. CE, European navigators could not use the second method, since Columbus promoted a wrong estimate of the size of the earth, so Europeans lacked an accurate value of the constant a. It was this blunder which led to the specifically European problem of an inability to determine the longitude at sea. This seems never to have been discussed earlier in the literature for curious historical reasons, although longitude could have been determined by this method without the need for any complicated

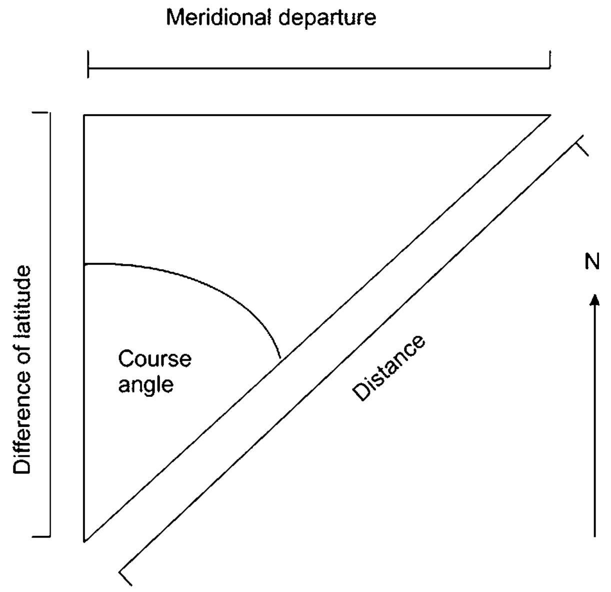

Figure 4.2: **Two ways to calculate departures.** The right-angled triangle shown above, also called the plane sailing triangle, can be solved from a knowledge of either (1) course angle and distance travelled, or (2) course angle and the difference of latitude. The first method was used by Europeans in dead-reckoning navigation. The second method requires an accurate estimate of the size of the earth: such an estimate was available to Indians from at least the 5th c. CE and to Arabs from at least the 9th c. CE, but not to Europeans until the late 17th c. CE. Hence, European navigators could not use the second method. This is what led to the famous problem of determining longitude at sea—a problem specific to European techniques of navigation.

instrumentation. Thus the European longitude problem was due to the difficulties that the Europeans had since they lacked an accurate knowledge of the size of the earth.

A trivial sort of objection that can be raised is about what would happen on the alternative method proposed above, in the case of "latitude sailing", when one travels on a course which is directly east–west, so that the difference of latitudes is not available, so that longitude cannot be determined. This objection cannot be countenanced once we have shifted our mathematical philosophy from the formalistic approach, which strives towards complete generality, to a computational approach which seeks efficient algorithmic solutions, possibly on a rule-and-exception basis.[52] A course due exactly east or west may be regarded as an exception—a set of probability/measure zero—something that would almost surely not arise. For any other course, one can obtain the departure (hence longitude) from a knowledge of latitudes and course angle, by solving the triangle of Fig. 4.2. (In the exceptional case of latitude sailing, the precise departure was not critically important in an age when time was not money.) So, in this exceptional case one could fall back on the use of less accurate methods based on estimating the distance travelled by other methods such as estimates of the speed.

To reiterate, glancing at Fig. 4.2, we see that departure calculation on the "Dead Reckoning" method solved the right-angled triangle from a knowledge of (a) course angle and (b) distance travelled. But the same right-angled triangle can also be solved from a knowledge of (a) the course angle, (b) the difference of latitudes, and (c) the *distance per degree latitude*.

Measuring the Size of the Globe

This last was a figure that was certainly known to Indian astronomers of the siddhantic period, but was known also to every Arabic astronomer from the time of Caliph al Ma'mūn (reigned 813–33 CE), at the very beginning of the Sind-Hind tradition. While the Indian units of distance (the yojana) cannot be readily related to contemporary units, Arabic units can be. Al Ma'mūn simply sent a team of surveyors to the Syrian desert: they physically travelled one degree north–south, and carefully measured out the distance so travelled. They arrived at a figure of $56\frac{2}{3}$ Arabic miles per degree of the meridian. There is the question of conversion from medieval to modern units. "This question was exhaustively investigated by Nallino 1892–3. He concluded that $56\frac{2}{3}$ Arabic miles is equivalent to 111.8 km per degree which is astonishingly close to the accurate value of 111.3."[53]

This estimate was no coincidence, and this accurate estimate of the size of the earth was confirmed by al Bīrūnī using the Indian method. All Indian astronomy texts state their differing estimates of the size of the earth, but do not document how they arrived at it—presumably since it involves very elementary geometry. Al Bīrūnī, who visited India and extensively studied and commented upon Indian astronomy, even translating an Indian text in astronomy, was, of course, very well conversant with the techniques of Indian astronomers. Hence, the method used by al-Bīrūnī is presumably the one used by Indian astronomers, as the reference to units in the following quotation further suggests. Since al-Bīrūnī's method is implicit also in the definition of the (fixed) *zām* (Chapter 5), as a unit of distance, it is worth recounting in detail. Al Bīrūnī described it as follows in his *Kitāb al Tahdīd*.

> You climb a mountain situated close to the sea or a level plain, and then observe the setting of the sun and find out the dip of the horizon. . . .[Then] find the value of the perpendicular of the mountain. You multiply this height into the sine of the complementary angle of the dip, and divide the total by the versed sine of this dip itself. Then multiply (twice) the quotient into 22 and divide the result by 7. You will get the. . . earth's circumference (in the same units) in which the height of the mountain has been found.[54]

That is, one measures the height h of a hill by measuring the angle α_1 subtended by the hill at a point, preferably at sea level. One then moves a known distance d towards the hill, on a level plane, and again measures the angle α_2 subtended by the hill at that point. The height of the hill is obtained by applying the elementary trigonometric formula:

$$h = \frac{d}{\cot \alpha_1 - \cot \alpha_2}.$$

Assuming that the earth is a perfect sphere, when one climbs the mountain, and measures the angle of dip of the horizon, the line of sight is tangential to the sphere, hence orthogonal to the radius, and a simple calculation (Appendix 4.A) gives the above formula for the radius of the earth, hence the circumference.

Remaining Questions: Instruments and Precise Trigonometric Values

The only questions that remain are this: a precise estimate of the size of the earth requires (1) an accurate instrument for angle measurement, and (2) accurate trigonometric values. The availability of precise trigonometric values we have already seen, in the previous chapter, and we move on to the question of an accurate but simple instrument for angle measurement—the *kamāl* or *rāpalagai*.

APPENDIX 4.A
CALCULATING THE SIZE OF THE EARTH AND THE VALUE OF THE FIXED *ZĀM*

We give below (Fig. 4.3) the simple geometry involved in al Bīrūnī's determination of the size of the earth, and the similar geometry involved in fixing the value of the *zām*.[55]

It immediately follows from the figure on the left, with θ as the angle of dip, that

$$\frac{r}{(r+h)} = \cos\theta, \tag{4.13}$$

so that

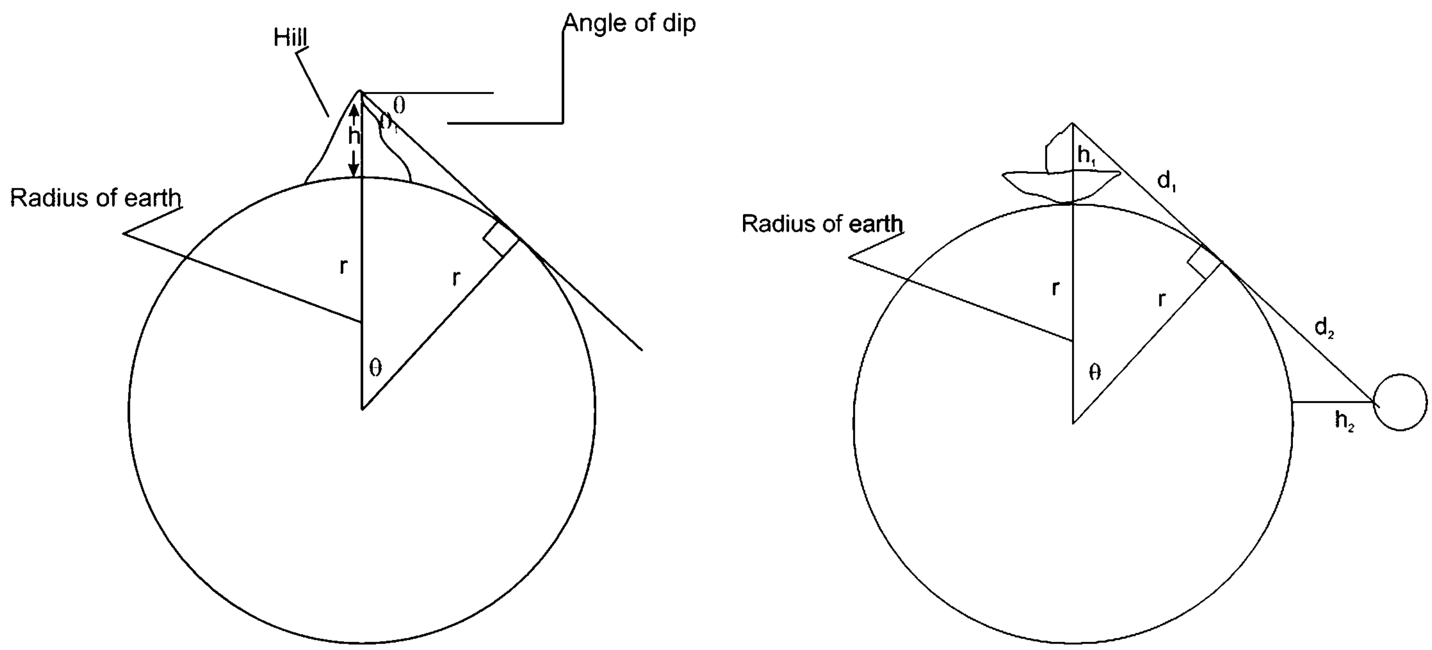

Figure 4.3: **The size of the earth and the value of the *zām*.** The figure on the left shows the geometry involved in the Indian method of determining the size of the earth, as documented by al Bīrūnī. The earth is assumed to be a perfect sphere, so that the line of sight which is tangential to the sphere at the horizon must hence be orthogonal to the radius at that point. The figure on the right gives the geometry involved in fixing the distance corresponding to one *zām*. The "horizon" now refers to the base of the tallest objects (e.g. tree tops) that are visible (i.e., are above the line of sight).

$$r = (r+h)\cos\theta,$$

or

$$r(1-\cos\theta) = h\cos\theta = h\sin\theta_1,$$

so that

$$r = \frac{h\sin\theta_1}{1-\cos\theta}.$$

The last expression is the same as al-Bīrūnī's formula, since versin $\theta = 1 - \cos\theta$.

To fix the value of a *zām*, we observe that the distance to the horizon may be approximated by the side d_1 of the above right-angled triangle. Clearly,

$$d_1 = \sqrt{(r+h_1)^2 - r^2} \approx \sqrt{2rh_1} = \sqrt{2r}\sqrt{h_1}. \tag{4.14}$$

Similarly, an object of height h_2 is first visible over the horizon, when it is at a distance $d_2 = \sqrt{2r}\sqrt{h_2}$, over the true horizon. The fixed *zām* or the "distance from here to the horizon" refers to the distance

$$d_1 + d_2 = \sqrt{2r}\,(\sqrt{h_1} + \sqrt{h_2}). \tag{4.15}$$

Taking the equatorial radius $r = 6.378 \times 10^6$ m, $h_1 = 5$ m (height of *odam*[56]), and $h_2 = 10$ m (height of coconut tree), we see that $d_1 + d_2 = 19.28$ km, approximately 12 miles. These figures are illustrative—I have not checked out the typical height of *odams* and coconut trees—but it seems fair to say that the unit of a *zām* incorporates within it a reasonably accurate estimate of the size of the spherical earth.

NOTES AND REFERENCES

1. Herodotus, *History, Euterpe*, 43, trans. G. Rawlinson, Encylopaedia Britannica, Chicago, 1990, p. 58.
2. R. R. Newton, *The Crime of Claudius Ptolemy*, Johns Hopkins University Press, Baltimore, 1977. William Broad and Nicholas Wade, *Betrayers of the Truth: Fraud and Deceit in the Halls of Science*, Simon and Schuster, New York, 1982.
3. Newton, a deeply religious person, specifically used the term "laws" (*lex*) in preference to "hypotheses" (*hypothesi*) to describe his calculus-based physics, because he thought the laws of God had been prophetically revealed to him. C. K. Raju, "Newton's secret", *The Eleven Pictures of Time*, Sage, New Delhi, 2003, chp. 4.
4. "[T]he oracle of Dodona... the most ancient oracle of Greece, and at that time there was no other." Herodotus, *History, Euterpe*, 54–58, Encyclopaedia Britannica, Chicago, 1990, pp. 68–72.
5. Plato, *Apology*, trans. Benjamin Jowett, Encyclopaedia Britannica, Chicago, 1990, pp. 201–05.
6. The date of the *Vedānga Jyotiṣa* is quite firmly fixed at ca. −1350 CE, for various reasons, including the reason already mentioned in Chapter 2. Varāhamihīra rejects the *Vedānga* (lit. an *anga* or limb of the Veda) on the ground that what it says differs from what is empirically manifest: since, for him, the empirically manifest overrides scriptural authority. To reiterate that example, the *Vedānga Jyotiṣa* locates the winter solstice at the beginning of Śraviṣṭa (Delphini) and summer solstice in the middle of Āśleṣā, while Varāhamihīra found winter solstice at the end of the first quarter of Uttarāṣaḍha, and summer solstice at the end of three quarters of Punarvāsu. Today we would say that this happens because of the precession of the equinoxes (the earth precesses like a spinning top), which has a period of about 26000 years, so that the precession is about 1° in 72.2 years. Since there had been a precession of about $1\frac{3}{4}$ *nakṣatra*, or about 23°20′, this would place the *Vedānga Jyotiṣa* 1680 years before Varāhamihīra (. 530 CE), i.e., at ca. −1150 CE. As Kuppunna Sastry adds, if by Śraviṣṭa, the reference is to the actual group of stars, which is about 3° from the beginning of the segment, we would get the date of −1350 CE. This is confirmed by the actual latitude and longitude of Śraviṣṭa given by the *Sūrya Siddhānta*, and the *Siddhānta Śiromaṇi*, which use the fixed zodiac beginning with the vernal equinox of ca. 550 CE the winter solstice of which is 270°, leading to a marginally different date of −1340 CE. Vedanga Jyotisa, cited below, p. 13. Varāhamihīra, *Pancsiddhāntika*, III.21, trans. G. Thibaut and Sudhakara Dwivedi, [1888], reprint, Chowkhamba, Varanasi, 1968, p. 18.
7. *Vedānga Jyotiṣa of Lagadha*, trans. T. S. Kuppanna Sastry, cr. ed. K. V. Sarma, Indian National Science Academy, New Delhi, 1985. Rgveda-Vedānga Jyotiṣa, 35.
8. *Dīgha Nikāya*, trans. adapted from Maurice Walshe, Wisdom Press, Boston, 1996, pp. 68–72.
9. "The emperor's new course", talk at the India International Centre, New Delhi, 1 Dec 2001, in the meeting on *Vedic Astrology in University Education: a Sound Decision?*
10. *Vedāṅga Jyotiṣa*, trans. T. S. Kuppanna Sastry, ed. K. V. Sarma, cited above.
11. *Yajurveda-Vedānga Jyotiṣa*, 24, similar to *RgVeda-Vedānga Jyotiṣa*, 17, trans. and ed. Kuppanna Sastry and K. V. Sarma, cited earlier, p. 37.
12. "Planet fakery exposed. Falsified data: Johannes Kepler". *The Times* (London) 25 January 1990, 31a. The article includes large excerpts from the article by William J. Broad, "After 400 years, a challenge to Kepler: he fabricated data, scholars say", *New York Times*, 23 January 1990, C1, 6. The key background article is William Donahue, "Kepler's fabricated figures: covering up the mess in the *New Astronomy*", *Journal for the History of Astronomy*, **19** (1988) pp. 217–37.
13. One example of how Newton's religious beliefs affected his scientific beliefs has already been provided in the note above, and further examples may be found in the same reference. This is not widely known since Newton's writings on the church have been carefully kept secret by Western historians for centuries because of their possible political repercussions. See C. K. Raju, *The Eleven Pictures of Time* cited above. We shall have more to say later on regarding the in uence of religious beliefs on the understanding of the calculus in Europe.
14. The correlation with astronomy, as regards (a) the length of the year in relation to the tropical year, and (b) the timing of the new year in relation to the vernal equinox, is so coarse as to cast very serious doubts on the alleged knowledge of astronomy in the Roman empire, a doubt reinforced by the difficulties Romans had with elementary arithmetic.
15. The Greek term *pascha* relates to the Aramaic form of the Hebrew *pesach* (transit, passover), and relates to the similar sounding names for it in various European languages, e.g. Latin *pascha*, Italian *pasqua*, Spanish

pascua, Dutch *paschen*, etc. The English term "Easter" derives from the goddess of dawn, Estre, according to Bede (*De temporum ratione*, I, v).

16. A further source of variation can arise from the fact that, as in the case of Id in contemporary India, the sighting of the crescent moon ("*Īd kā chānd*") has to be visually confirmed by the Imam, so also in the Jewish case, the sighting of the moon is traditionally required to be visually confirmed by the Sanhedrin. At the time of the Nicene council, this procedure could hardly have been adopted by the Christians, since there was then no recognized supreme theological authority in Christianity.
17. Moreover, it could occur on the evening of the 14th "day", since the Jewish calendar reckoned days from sunset to sunset.
18. In particular, 25th of December was not then celebrated, either as a shopping event or otherwise, as the date on which the Christ was born: although the pre-Christian pagans in the Roman empire did celebrate it as the date on which Bacchus/Dionysius was born of a virgin (exactly 9 months after the date of equinox, then taken as 25 March on the Roman calendar). In pre-Christian artefacts, the death of the same Bacchus (god of a more orgiastic kind of love) is depicted against the background of a cross. (For a picture of this artefact, see, e.g., Timothy Freke and Peter Gandy, *The Jesus Mysteries: Was the "Original Jesus" a Pagan God?*, HarperCollins, London, 1999.) The "pagan" interpretation, however, was rather more sophisticated than the literal one later promoted by the church—for the cross, especially when put into a circle, was, like the swastika, a symbol of reincarnation in the context of cyclic time. In particular, the new year celebration, related to the cycle of the year, which itself symbolized the cyclicity of time, hence reincarnation.
19. Because solar and lunar cycles are incommensurate, and neither involves an integral number of days, time measurement inevitably required some sophistication in calculation. At the minimum it needs the algorismus which was not available in the West until the second millennium CE, and not usually available to priests, until about 1570.
20. P. S. S. Pissurlencar, "Govyache Khristikarana", Shri Santadurga Quatercentenary Celebration Volume, Shaka 1488–1818, published by Durgarao Krishna Borkar, Bombay, 1966, pp. 91–122. English summary in *Bibliography of Dr Pissurlencar Collection*, part I, ed. B. S. Shastry and V. R. Navelkar, Goa University Publication Series, No. 3, pp. 67–69.
21. Govt of India, *Report of the Calendar Reform Committee*, CSIR, New Delhi, 1955, p. 158. The quote occurs in part C of the report on the "The History of the Calendar…", by M. N. Saha and N. C. Lahiri, published as a separate volume, under that title by CSIR, p. 158.
22. However, a key point to note is this: due to unusual weather (global warming perhaps), there was a lot of pre-monsoon rain in 2006—so much that it could easily be confounded with the monsoon. The qualitative difference between the steady rain of the monsoons and the downpours of thunder showers is clear enough, but it is not clear how this difference can be made statistically, from rainfall data. In particular, what is required is a causal rather than a statistical account.
23. Lalla, "False notions", *Śiṣyadhīvṛddhida Tantra of Lalla*, with the commentary of Mallikarjuna Suri, ed. and trans. Bina Chatterjee, INSA, New Delhi, Part II, chp. 20, p. 269.
24. Lalla, cited above, pp. 274–75.
25. Lalla, cited above, p. 276.
26. *Vaṭeśvara Siddhānta, and Gola of Vaṭeśvara*, ed. and trans. K. S. Shukla, part II, English translation and commentary, Indian National Science Academy, New Delhi, 1985, pp. 638–39. Emphasis added. Vaṭeśvara was well known as a critic of Brahmagupta. Vaṭeśvara's book (*Siddhānta*) was written in 904 CE, and is referred to by subsequent scholars such as al Bīrūnī (b. 973 CE) and Sripati (1039 CE).
27. Varāhamihīra accepts, of course, the roundness of the earth, but feels that it is the celestial sphere which rotates, and not the earth. His argument is that if the earth rotated, there would be a wind (aether wind) on the surface of the earth. Not finding this argument quite convincing (the aether near the surface may be dragged along with the earth—as in Stokes' mathematically implausible account, in favour of which the Michelson–Morley experiment concluded), he adds that eagles which y very high would be unable to return to their nests. Varāhamihīra, *Pañcasiddhāntikā*, trans. G. Thibaut and Sudhakara Dwivedi, reprint, Chowkhamba Sanskrit Series, Varanasi, 1968, 13.27 and 13.9–13, p. 72, p. 70.
28. The idea of a spherical earth was already incorporated in the Siddhantic literature translated into Arabic amongst others by al Bīrūnī himself who translated the *Pañcasiddhāntikā* of Varāhamihīra. The astrolabe was an important instrument which embodied a planispheric projection of the cosmic globe.
29. The method (presumably Indian) used and documented by al Bīrūnī is described in more detail later on. Al Bīrūnī's value of the radius of the earth was equal to 3938.77 English miles (using 1 Arabic mile = 1.225947 English miles). This compares favourably with the mean radius of the curvature of the reference

ellipsoid, at his latitude, which is 3947.80 miles. Al Bīrūnī translated the *Karaṇa Tilak*of one Vijaya Nandi of Benares, for Zij calculations, as the *Ghurrat-uz-Zijat,* the manuscript of which was discovered in 1959. The *Karaṇa Tilak* falls in between the *Khaṇḍakhādyaka* of the sixth-century Brahmagupta (who explicitly uses the idea that the first sine differences are proportional to cosines, and whose work al Bīrūnī had thoroughly studied) and the *Karaṇa Kutūhula* of the second Bhāskara. See S. S. H. Rizvi, "A newly discovered book of Al-Bīrūnī: Ghurrat-uz-Zijat', and Al-Bīrūnī's Measurements of Earth's Dimensions"'. In: *Al-Bīrūnī Commemorative Volume*, ed. H. M. Said, Hamdard Academy, Karachi, 1979, pp. 605–80.

30. *The Four Voyages of Christopher Columbus*, ed. and trans. J. M. Cohen, Penguin, 1969, pp. 13–16. Columbus seems to have persisted in this delusion till the end of his life believing even until his fourth voyage, that Costa Rica etc. were all part of Asia.
31. See Chapter 5.
32. Alexander, as the new king of Persia, came to Afghanistan at the behest of a border chieftain, Śaṣigupta, to settle a dispute Śaṣigupta and king Ambi had with a neighbouring ruler Puru. But, after their first real battle (Alexander's troops faced no serious resistance in Egypt or Persia), his troops mutinied at the thought of attacking India. Hoping to return with a larger army, and looking at the difficulties of transporting a large army across land, Alexander sought to discover the sea route to India. Nearchus' voyage is described in Arrian, *Indika,* cited by Saletore, p. 296. Nearchus availed of the services of local pilots who explained to him the meaning of phenomena such as whale spouts which terrified his army. This suggests that the local pilots, like Vasco da Gama's "pilot", even then went far out to sea, quite contrary to the idea that "pilots" merely were those who stayed near land. Arrian, *Anabasis Alexandri,* Book VIII (*Indica*), trans. Ilif Robson (1933) Ebook, at http://www.und.ac.za/und/classics/india/arrian.htm.
33. Not Ibn Majid. This information is quite self-consistent, for "Malemo" clearly comes from Mualim/Malmi (Navigator), and Cana/Kanha is a common name on the Diu coast, perhaps related to the port of Dwarka. At any rate, the earliest accounts by people who probably did not know what "Guzerat" was, could not have been expected to have concocted this, though no doubt some later Portuguese historians had ample motivation to concoct the story that it was the legendary Ibn Majid who took Vasco da Gama across.
34. Some confusion has been caused by Kautilya's use of the terms Ashadha and Kartika as the proper times for sailing out and in. By the time of the *Periplus,* it seems to have been an established procedure to take advantage of the monsoon winds by sailing out from Egypt around July, and sailing back around October–November. It has been suggested that Kautilya must have meant that people must sail out from foreign lands in Ashadha (June–July), and sail to foreign lands in Kartika (October–November). Perhaps Kautilya was talking about travel to China. Kautilya, *Arthaśāstra,* Book II, Ch. XXVIII, p. 142. Saletore, *ibid.*, pp. 281–284.
35. Fa-Hsien, *Record,* pp. 113–14, cited in Saletore, pp. 528–29. Also available as an Ebook at Project Gutenberg.
36. Fa-Hsien, *A Record of the Buddhistic Kingdoms,* trans. James Legge, Clarendon, Oxford, 1886, chp. 40, "after two years takes ship for China. Disastrous passage to Java; and thence to China;...". Reproduced here from the Etext prepared by John Bickers, jbickers@ihug.co.nz and Dagny, dagnyj@hotmail.com, and placed online at Project Gutenberg.
37. *Laghu Bhāskarīya,* III.2-3, ed. and trans. K. S. Shukla, Department of Mathematics and Astronomy, Lucknow, 1963, p. 42. Retranslated for clarity by the author.
38. *Vaṭeśvara Siddhānta,* III.26, ed. and trans. K. S. Shukla, INSA, New Delhi, 1985, p. 288.
39. *Laghu Bhāskarīya,* III.22–23.
40. *Laghu Bhāskarīya,* I.23.
41. *Laghu Bhāskarīya,* I.29.
42. *Mahā Bhāskarīya,* II.8, ed. and trans. K. S. Shukla, cited earlier, p. 53; translation adapted by the author.
43. *Mahā Bhāskarīya* II.3–4.
44. *Mahā Bhāskarīya* V.4
45. *Laghu Bhāskarīya,* I.32, ed. and trans. K. S. Shukla, cited earlier, p. 11.
46. *Laghu Bhāskarīya,* I.24, ed. and trans. K. S. Shukla, cited earlier, pp. 8–9. The accurate value of the earth's radius, according to Bhāskara I, is 1050 *yojana*-s, and he takes (equivalently) $\pi = 3.1416$ to obtain $1050 \times 3.1416 = 3298.68 = 3299 - 8/25$. RSine refers to the radius times the sine in current notation.
47. *Vaṭeśvara Siddhānta and Gola,* critically edited and translated by K. S. Shukla, INSA, New Delhi, 1985, p. 137.
48. Commentary on *Siddhānta Śekhara,* II.104.
49. *Brāhma Sphuṭa Siddhānta,* I.36.

50. *Śiṣydhīvṛddhida* I.57–58.
51. *Alguns Documentos da Torre do Tombo*, ed. J. Ramos Coelho, Lisbon, 1892, pp. 138–39, cited by W. G. L. Randles, "Pedro Nunes and the discovery of the loxodromic curve, or how, in the sixteenth century, navigating with a globe had failed to solve the difficulties encountered with the plane chart". *Revista da Universidade de Coimbra*, **35** (1989) pp. 119–30.
52. C. K. Raju, "The mathematical epistemology of śūnya," in: Proceedings of the Seminar on the Concept of Śūnya, INSA and IGNCA, New Delhi, 1997. In: *The Concept of Śūnya*, ed. A. K. Bag and S. R. Sarma, IGNCA, INSA and Aryan Books International, New Delhi, 2002, pp. 168–81.
53. E. S. Kennedy, *A Commentary upon Bīrūnī's Kitab Tahdid al-Amakin: An 11th Century Treatise on Mathematical Geography*, Beirut, 1973, p. 188.
54. Translation adapted from H. M. Said and A. Z. Khan, *Al-Bīrūnī: His Times, Life and Works*, Hamdard Academy, Karachi, 1981, p. 165.
55. "The distance from here to the horizon", see Chapter 5.
56. A traditional large boat used in the Lakshadweep islands.

CHAPTER 5

Navigation: *Kamāl* or *Rāpalagai*

Precise measurement of angles and the two-scale principle

OVERVIEW

THE techniques of navigation, prevalent in the Indian ocean, though they did not require any charts, assumed an accurate means of measuring angles. Instruments for accurate angle measurement were used by navigators from pre-Islamic times, for they definitely sailed out on the open sea, out of sight of land, for example, in sailing to small islands like Lakshadweep, or to larger islands like Sri Lanka, known from earliest recorded times, or as described in Fa-Hsien's travelogue. I describe one such instrument for angle measurement which I recovered from the Lakshadweep islands, and which is called the *kamāl* in Arabic and *rāpalagai* in Malayalam. This was definitely the instrument used by the Indian pilot who navigated Vasco da Gama across the Indian ocean from Melinde to Calicut, and Vasco Da Gama, who lacked the foggiest idea of its functioning, carried copies of the instrument back with him. (Based on a partial understanding of this instrument, many similar instruments were constructed in Europe in the 16th c. CE.) It was also probably the instrument used by al Bīrūnīin his record of Indian techniques of determination of the size of the earth. The curious thing is this: the actual instrument we obtained, called the *kamāl* or *rāpalagai*, though made simply of pieces of wood and string, uses the golden ratio, and a sophisticated two-scale principle, nowadays most commonly used in the instrument known by the name of Vernier (callipers), but earlier named in the West as Nonius after Pedro Nunes' use of this principle in another instrument to measure angles. Consequently, our *kamāl*, despite a huge overall range of 1500 miles north–south, has an accuracy of 11 miles at the lower end of the range, corresponding to an angular accuracy of 10 , needed to navigate to small coral islands.

I
BRIEF HISTORY

A little over five hundred years ago, Vasco da Gama, having rounded the cape, was creeping along the African coast, full of imaginary fears about the motives behind traditional African hospitality. Equally, he was afraid to strike out across the "uncharted" deep sea. Ultimately he accepted the advice to do just that if he wanted to proceed towards the land of spices. But he needed a pilot to bring him from Africa to India so that he could "discover" India. There is a controversy whether the pilot who brought Vasco da Gama from Melinde to Kozikhode (Calicut) was an Arab (the legendary Ibn Mājid) or a "Guzerati Moor", Malemo Cana, as earlier accounts called him.[1] (Vasco da Gama himself did not mention any nationality, for the obvious reason that he was unaware of Gujarat, and simply thought of all Muslims as Moors.) Tibbets[2] believes the latter is likely since Indians lack any sense of national identity. While agreeing with Tibbets' conclusion, and without needing to deny his irrelevant observation (which applies equally to Europeans), the connection between observation and conclusion is nevertheless far fetched, for the Arabs then tended to regard the Portuguese as barbarians. As is amply clear from the organized arrangements for traders that Vasco da Gama encountered in Calicut, sea trade between India, Arabs, Africa, and China was at that time carried out in a peaceful and honourable way.

In any case, everyone agrees that the pilot[3] (*Muālīm*, or *Mālmī*, or "Malemo") of that fateful voyage used the *kamāl*, a copy of which the mystified Vasco da Gama carried back with him. Vasco da Gama thought the pilot told the distance with his teeth! How did the pilot manage to do that?

Kamāl means complete, so *kamāl* denotes a complete instrument. *Rā* means night as in *rātri*, while *palagai* (usually spelt *palaka*) means a block of wood or instrument, so that *rāpalagai* means a night instrument.

It is now generally agreed that, during Vasco da Gama's time, the boat-building and navigational techniques existing in the Arabian Sea and the Indian Ocean were superior to those possessed by the Europeans. The Arabs then ridiculed the European method of using charts.[4] But things changed. According to Tibbets, by the mid-nineteenth century, pilots in the Arabian sea had abandoned the *kamāl* for the sextant. However, the navigational needs of the Lakshadweep islanders (excluding Minicoy) were limited to travel to the mainland and back. They travelled for barter, and not for commerce or adventure. So the Lakshadweep islanders continued using the *kamāl*, and shifted to the *kamān* (sextant) later.

In 1923, R. H. Ellis, a British officer, inspected the islands. He recommended[5] that schools should teach a course on modern navigation. The recommendation was intended to make the British government and its institutions more popular with the islanders. Eventually, a textbook called *Nāvik Shāstram* written in Malayalam, was published in 1939, and teaching of modern navigational techniques commenced at Amini. Today, no Amini islander recollects seeing the *rāpalagai* in use. I spoke to two of the oldest Amini-based navigators, Syed Bukhari (b. 1929), and Ahmed Pallechetta (also around 70 years at that time), who too learnt from Syed Bukhari's father; both used *Nāvik Shāstram* and "Noorie tables".

As regards the Arabic-sounding "Noorie", it should be clarified that the reference is to *Norie s Nautical Tables*, a book first published by Capt. James Norie, in 1803, which has remained in print continuously since then, though it has undergone numerous revisions. The enormous success of the book presumably enabled Capt. Norie to acquire a stake in a publishing company, which now publishes the tables. The Norie tables in the present *Rehmani* of Kunhi Kunhi Maestry of Kavaratti refer to the declination tables for the sun from the 1864 edition of *Norie s Tables*, which he consulted from the Kavaratti library. However, the idea of using solar altitude and declination to determine latitude is detailed in numerous Indian and Arabic astronomy books from the 5th century CE onwards. So this idea was already very much a part of the navigational traditions prevalent in the Indian ocean—but the sources have changed.

Contrary to what one might expect, *a priori*, the navigational traditions vary substantially between the islands: a knowledgeable navigator at Kavaratti may be quite unable to explain an instrument such as the *kolpalagai* used in Bitra. Similarly, though the Amini *mālmi*-s were quite unfamiliar with the *rāpalagai*, it was the Kiltan *mālmi*-s who were most knowledgeable about it. (The distance between Kiltan and Amini is around 30 km: Amini is adjacent to Kadmath, and there is a point in the sea between Kiltan and Kadmath from which one can simultaneously see both islands. Mr Abdullah Koya of Kiltan was able to supply us with a copy of the Arabic literature on the construction of the *kolpalagai*.)

Mr Ali Koya of Kiltan had a *kamāl* which he discarded for he had no use for it. Mr Harris, also of Kiltan, kindly constructed a model, but could not explain how the instrument was calibrated. The most knowledgeable person was Kazi Sirāj Koya of Kiltan. He could not offhand recollect the calculations used to calibrate the instrument, but referred to a book containing the calculations. Though Dr C. H. Koya had a copy of the book in Arabic-Malayalam he was unable to translate it for us.

Ultimately, a model of the *kamāl* was obtained from Mr Aboo Backer of Kavaratti, who had preserved it along with the *kamān* used by his father Mr Ahmed Malmi of Kavaratti.

The *rāpalagai* is clearly a lost tradition. None of the *malmī*-s I talked to, in the various islands, was able to explain the construction or use of the *rāpalagai*. One took the smaller piece in his mouth, and raised the knots above the block, as one might do with finger measurements. One divided the string into eight equal parts, but was unable to explain how to add five more equal parts he thought would be needed for Kavaratti at a lower latitude. One thought that the instrument was used to measure the speed of the boat in knots. One remembered only snatches of some mnemonic verses related to the *rāpalagai*.

II
THEORY

The theory given below is my reconstruction of the construction of the *rāpalagai*. I believe my reconstruction is valid, but I have no documentary support for this reconstruction. I do *not* regard documentary justification as critically important, and I leave it to others to search for the Arabic literature on the *kamāl*. Though James Prinsep was in a position to observe its use, his earlier article on the *kamāl* does not mention these details which are needed to be able to construct the *kamāl*.

Before the arrival of the Europeans, both the magnetic and stellar compass were known in the Arabian sea and the Indian ocean. (The word magnetic comes from the Arabic "magnethis".) Pilots like Ibn Mājid were aware of the limitations of the magnetic compass. But the principal limitation of the compass is that it indicates direction but not one's present location.

The pole star (called *kau* by the islander), however, is not only a directional star. The celestial sphere appears to rotate on a north-south axis through the celestial pole, very close to the pole-star. Therefore, unlike other stars, the altitude of the pole star essentially remains fixed throughout the night, and through the year at a given place.

But the altitude of the pole star varies with geographical latitude. Simplistically, at the north-pole it should seem to be vertically overhead, and at the equator it should be on the horizon. Indeed the latitude of a place is precisely the altitude of the pole star (strictly the celestial pole) at that place, although near the equator the pole star may be so close to the horizon that it ceases to be visible. (This happens at latitudes below the Maldives.)

For short-distance travel (a few hundred kilometres north–south) in mid-latitudes, each increase in the altitude of the pole star is proportional to the north-south distance travelled. Thus, the height of the pole star is a measure of one's latitudinal position, or of the north–south distance travelled. The height of the pole star can be measured using the angle α subtended at the observer's eye by the pole star and the horizon.

Finger Measurements

One can measure the angle α by blocking it off using the fingers of one hand held at a distance of one span, say, from the nose. No doubt the length of the span and the thickness of the fingers vary from person to person, just as the markings on a foot-rule will vary from foot-rule to foot-rule by thousands of Angströms. But someone employing a foot-rule is not interested in precision in Angströms; he does not worry about variations at that level, and the same argument applies to someone using finger measurements.

Finger measurements were converted into (north–south) distance. In the actual units used by the islander, each finger increase of *kau* corresponds to 8 *shāmams*. The islanders use the term *shāmam*, presumably derived from the Arabic *zām*, which, in turn, is derived from the Sanskrit *yāma*, corresponding to the Prakrit *jāma*, and the Telegu *jāmu/jhāmu,* one of the oldest time units in India. The *yāma*, more commonly known as *prahara* continued till quite recently as "so popular a unit in Indian time measurement that even the lay man expresses time in terms of *praharas*. . . ".[6] Day and night each were divided into 4 equal parts. Thus, afternoon is known as "do-prahara" or "dopahara", i.e., after two *prahara*-s.

This division was similar to the Babylonian division of the nycthemeron into 6 watches, and the medieval European ritualistic division into Matins, Prima, Tertia, Sexta, Nona, Vespers, and Compline. Since day and night are generally of unequal duration the *zām*-s were, to start with, of generally unequal duration, except during the equinoxes when they were of 3 hours each. The number of *zām*-s in a voyage might also vary with the wind and other conditions. Thus, Ibn Mājid declared: "from Somalia to Aden it is twenty zams, sometimes less in clearly easterly monsoon weather."[7]

Over a period of time, the unit of time came also to mean a unit of distance—the distance that a ship travelled during one *zām* of approximately 3 hours. With changes in ship-building technology, this created the inconvenience that the *zam* depended also on the kind of ship one was travelling in. The Arab navigators then started distinguishing between the fixed *zām* and the ordinary kind of *zām*. The fixed *zām*, as a unit of distance, has even been defined as corresponding to 1 8th part of the north–south distance leading to a one finger increase in the altitude of the pole star.

The islander's use of the term "shāmam", however, is quite general, unambiguous, and precise: a *shāmam* is "the distance from here to the horizon". On an open sea under clear visibility, this distance depends only on the size of the earth, on one's elevation above the sea,[8] and on the elevation of the object being sighted. The islanders typically take this distance to be 12 miles.

The disadvantage of finger measurements was that fractions of fingers were difficult to judge. But with one finger being 96 miles, fractions of fingers were required for sailing into small islands. (None of the Lakshadweep islands is more than some 3 miles wide, with the capital Kavaratti being 0.75 miles across at its widest.) Secondly, with fingers being held at some fixed distance, say arm's length, the range was limited to eight fingers. It is possible, of course, to measure larger elevations by reducing this distance, but the problem then is to how to measure this reduced distance accurately. Poo Koya Malmi of Androth island demonstrated a special sort of span: this involved the thumb and the index finger, with the thumb placed on the nose. But if one uses the span to judge the distance, one is reduced to using only the four fingers of one hand.

Rāpalagai

The *rāpalagai* overcomes these difficulties. Here angles are measured by holding a fixed board, at varying distances from the eye. The distance is measured by a string through the centre of the board, the other end of which is held between the observer's teeth. (Hence, the word *kau*, for the pole star, also means teeth, and this explains the origin of Vasco da Gama's confusion.) The string is graduated using knots.

The Relation

Finger measurements and *rāpalagai*, thus, represent two different methods of measuring angles. In one case the distance is held constant, and the height is varied; in the other case the height is held constant and the distance is varied (Fig. 5.1).

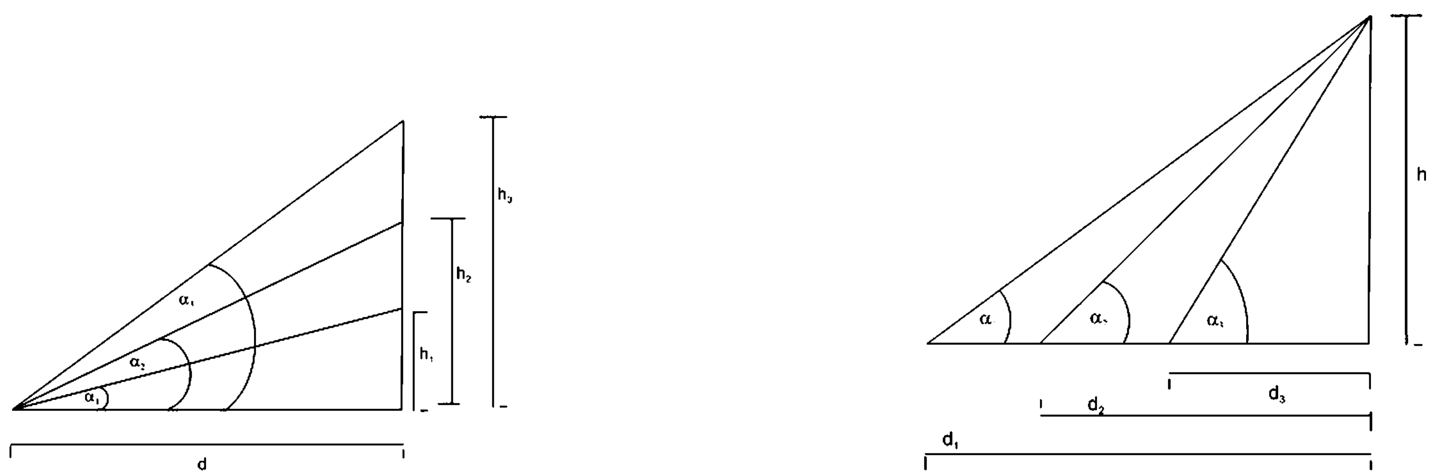

Figure 5.1: **Two ways of measuring angles.** Finger measurements (left) and *rāpalagai* (right). With finger measurements the distance from the eye is held constant, while the height is changed by changing the number of fingers. With the *rāpalagai* the height of the board is constant, and the distance from the eye is changed.

Referring to the figure, we see that, for finger measurements

$$\frac{h_i}{d} = \tan \alpha_i. \tag{5.1}$$

For the *rāpalagai*

$$\frac{h}{d_i} = \tan \alpha_i. \tag{5.2}$$

Take each knot to signify a 1 finger increase of altitude, or a fixed fraction thereof. (Each one-finger increase is assumed to correspond to equal distances of about 96 miles.) Then

$$h_{i+1} - h_i = \text{const.} = F = \text{one finger, or a multiple,} \tag{5.3}$$

so that

$$\tan \alpha_{i+1} - \tan \alpha_i = \frac{F}{d}, \tag{5.4}$$

where d is the constant distance (of one arm length or span) used for finger measurements in (5.1).

Now, from (5.2),

$$d_i = \frac{h}{\tan \alpha_i}, \tag{5.5}$$

so that

$$\frac{1}{d_{i+1}} - \frac{1}{d_i} = \frac{\tan \alpha_{i+1} - \tan \alpha_i}{h}, \tag{5.6}$$

or, finally, using (5.4),

$$\frac{1}{d_{i+1}} - \frac{1}{d_i} = \frac{F}{d\,h}. \tag{5.7}$$

We recall that, in (5.7), $F = 1$ finger (or a fixed multiple of it), $d = 1$ arm length or 1 span (or a fixed multiple of it), $h =$ height of the wooden piece, and $d_i =$ distance to the ith knot.

Hence, *the distance between the knots must be in harmonic progression.*

Moreover, the instrument may be constructed by measuring everything using fingers. That is, the distances d_i may be measured using fingers, if the span d and the height h of the board are measured using fingers.

The broader the board, the more sensitive the instrument, but the longer the string must be, and the bottom knot of the string decides the lowest latitude at which the *rāpalagai* can be used. Thus, the range for the size of the board is fixed by the latitude of the base island, and the length of the observer's arm.

Comparison of Theory with Instrument

Let us now compare this preliminary theory with the actual instrument we obtained (Fig. 5.2). Table 5.1 shows the results of this comparison for the 12 knots in the string attached to the larger piece. The distances between the knots were measured in $\frac{1}{16}$th of an inch and converted to decimal fractions. We used $\frac{F}{dh} = 0.011$ (in)$^{-1}$.

To the precision of the figures for the distance between the knots, the formula (5.7) fits quite exactly. One could try to go a step further, and find the value of $\frac{F}{dh}$ which minimizes the sums of squares of residues. There is not much point to this because of the following. If this value of $\frac{F}{dh} = 0.011$ (in)$^{-1}$ is used with the measured value of $h = 1.42$, we obtain the value $\frac{1}{64}$ for the dimensionless ratio $\frac{F}{d}$. At this stage, it is convenient to use new units: if we take $d = 8F_0$, then $\frac{F}{F_0} = \frac{1}{8}$, so that each knot represents $\frac{1}{8}$th of a unit increase in the altitude of *kau*, in units of F_0. Since there are 12 knots, the instrument can be used over a range of $1\frac{1}{2}$ units of *kau*.

However, the instrument we obtained has two pieces. To complete the comparison of theory with instrument, we applied this theory also to the second piece. The results are presented in Table 5.2, which used $\frac{F}{dh} = 0.04625$ (in)$^{-1}$.

Referring to Fig. 5.2 we see that in this case $h = 0.90157$, so that $\frac{F}{d} = 0.0417 \approx \frac{1}{24}$, so that each knot represents $\frac{1}{3}$rd of a unit change in the altitude of *kau*, in units of F_0, as used earlier. Since there are 8 knots attached to the smaller piece, the instrument has a range of $2\frac{2}{3}$ units of kau. The base of this instrument, incidentally, is set a little higher than the base of the other instrument.

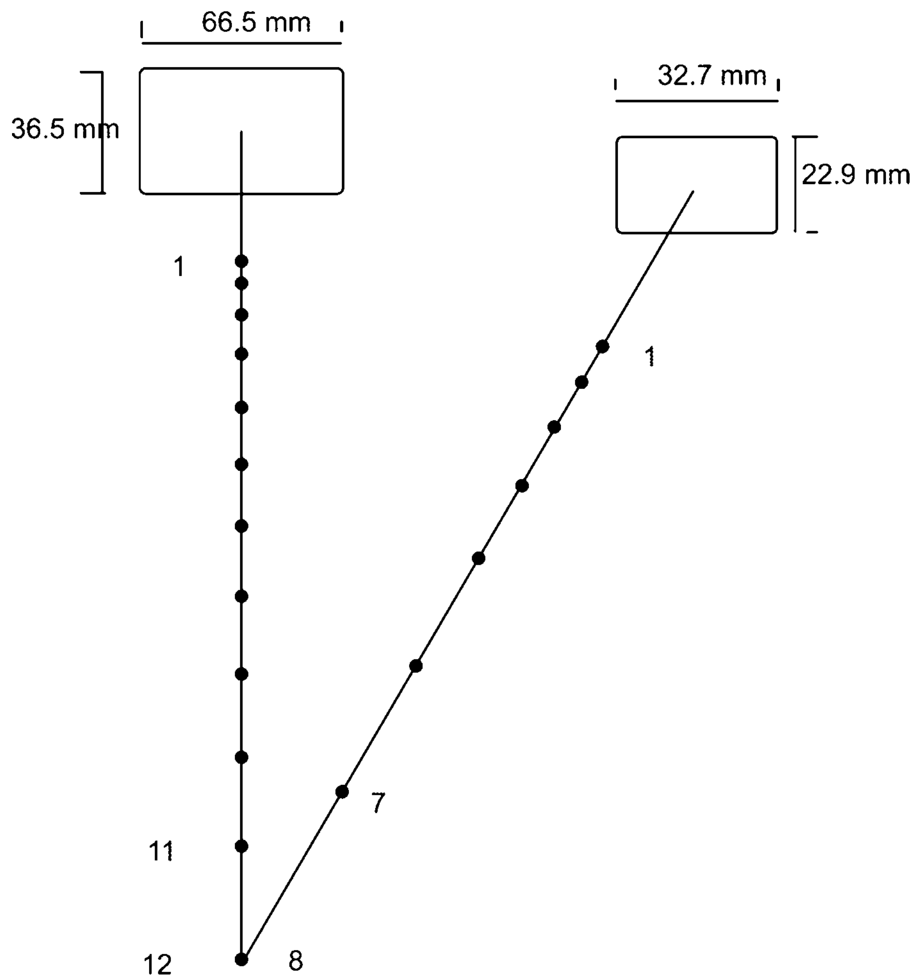

Figure 5.2: **The *kamāl*.** The instrument we obtained had two pieces. The string attached to the larger piece had 12 knots, while the string attached to the smaller piece had 8 knots. The distances shown in the figure are schematic and not to scale. The exact distances between the knots are given in Tables 5.1 and 5.2

No	d_i (in inches)	Residue: $\frac{1}{d_i} - \frac{1}{d_{i+1}} - \frac{F}{dh}$
1	6.0	0.003286
2	6.5625	0.00345
3	7.25	−5.309244e−05
4	7.875	0.000876
5	8.6875	0.000882
6	9.6875	−0.001892
7	10.6250	−0.000652
8	11.9375	0.001021
9	13.9375	−8.762292e−05
10	16.4375	−0.003144
11	18.875	−0.003604
12	21.9375	Average= 6.885132e−06

Table 5.1: **The knots with the larger piece**

No	d_i (in inches)	Residue: $\frac{1}{d_i} - \frac{1}{d_{i+1}} - \frac{F}{dh}$
1	2.625	0.054001
2	3.5625	0.029324
3	4.875	0.007935
4	6.625	0.002129
5	9.75	−0.025319
6	12.25	−0.033881
7	14.4375	−0.03454
8	17.3750	Average = −4.394772e−05

Table 5.2: **The knots with the smaller piece**

To summarize, we have two pieces and two harmonic scales. The smaller piece covers a range of $2\frac{2}{3}$ units, and has an accuracy of $\frac{1}{3}$ unit. The larger piece covers a range of $1\frac{1}{2}$ units, and has an accuracy of $\frac{1}{8}$ unit. In a subsequent section, we translate the above units into more familiar units of distance.

The Problem of Harmonic Interpolation

The theory developed above does not address some questions. For example, how should one interpolate values in-between the knots? The difficulty is that the knots are not in linear progression, but in harmonic progression. Hence, linear interpolation will not work: if the height of *kau* comes out half-way between two knots, that does not allow us to presume that our latitude is half-way between the latitudes corresponding to the two knots. There is clearly a practical problem here.

Perhaps there is a way to carry out this harmonic interpolation by using the two scales together. For example, can the two pieces be used in a way that applies some analogue of the "Vernier" principle to harmonic scales? The above theory treated the two instruments separately, ignoring any possible relation between them.

The Golden Ratio

The first thing to observe is this: the heights of the two pieces are in the ratio (36.5 mm)/(22.9 mm) = 1.593, which is remarkably close to 1.6 = $\frac{8}{5}$ a standard rational approximation to the golden ratio (= 1.618 ...). The lengths of the two strings are in the ratio (17.375 in)/(21.9375 in) = 0.792 which is remarkably close to $\frac{}{4}$ (= 0.785...). This suggests that the two pieces were intended to be used together. I don't know the significance of $\frac{}{4}$, but the two pieces might be used together as follows.

Interchanging the Scales

We assume that the heights are intended to be exactly in the ratio $\frac{8}{5}$, and that the (dimensionless) error of 0.007 is the error in constructing the wooden instrument, or an error of measurement. Then it is clear that the scale attached to the smaller piece can also be used (with only a slight inaccuracy) with the larger piece. The scale will, however, change, and each knot will now represent $\frac{1}{8} \times \frac{8}{5} = \frac{1}{5}$ units. Likewise, if the scale attached to the larger piece is used with the smaller piece, then each knot will represent $\frac{1}{3} \times \frac{5}{8} = \frac{5}{24} \approx \frac{1}{5}$ units.

Thus, the instrument actually represents *two* pairs of scales: the large piece corresponds to a $(\frac{1}{8}, \frac{1}{5})$ pair, and the small piece to a $(\frac{1}{3}, \frac{1}{5})$ pair. It is clear enough that using the $\frac{1}{5}$ scale instead of the $\frac{1}{8}$ scale helps to increase the range of the bigger piece, while diminishing its accuracy. Likewise, using the $\frac{1}{5}$ scale in place of the $\frac{1}{3}$ scale helps to increase the accuracy of the smaller piece while reducing its range.

Against this background, let us return to the interpolation problem for harmonic scales, where some method of interpolation is necessary, since visual judgment may obviously be an inadequate guide for interpolation. The problem is settled by observing that the two scales can be used together for harmonic interpolation.

The Two-Scale Principle

Recall the instrument known today as the Vernier calliper. This instrument uses two scales to interpolate, thereby increasing the accuracy of measurement tenfold. Can two harmonic scales be similarly used in the manner of two linear scales?

The instrument today called the Vernier calliper had its origins in an earlier instrument called the Nonius, after Pedro Nunes, and used to measure angles. Presumably, the instrument designed by Pedro Nunes was known to earlier Arabs, and was probably based on even earlier knowledge. Accordingly, we will simply refer to the underlying principle as the two-scale principle.

As the first step, we notice that the two-scale principle works perfectly well regardless of the use of the decimal division. Thus, suppose we have two (linear) scales, one which divides each unit into 3 equal parts (i.e. it has 3 notches for each unit), and the other which divides the same unit into 4 equal parts. Suppose now that we have a distance d which we cannot measure precisely with the first scale, since it comes out to be between the nth and $(n + 1)$th notch:

$$d = \frac{n}{3} + \quad . \tag{5.8}$$

We can use the second scale to interpolate and measure the value of   as follows. We position the zero point of the second scale at the tip of d (i.e., at $\frac{n}{3} + \quad$) and find a notch coincidence between the two scales positioned side by side. Let us say, the kth notch of the $\frac{1}{3}$ scale coincides (most closely) with the mth notch of the $\frac{1}{4}$ scale. Then, we obviously have

$$\frac{n}{3} + \quad + \frac{m}{4} = \frac{k}{3}, \tag{5.9}$$

from which we can calculate as

$$= \frac{k-n}{3} - \frac{m}{4} \tag{5.10}$$

or directly compute d,

$$d = \frac{k}{3} - \frac{m}{4}. \tag{5.11}$$

The least count is when $k = n + 1$, and $m = 1$, corresponding to $\quad = \frac{1}{12}$. Thus, using a $\frac{1}{3}$ scale together with a $\frac{1}{4}$ scale, amounts to having a $\frac{1}{12}$ scale. Clearly, there is no particular virtue to going by 3's and 4's, and the same thing will work perfectly well with any two relatively prime numbers p, q.

Theory of the Two-Scale Principle for Harmonic Scales

From the present-day viewpoint, the clue to interpolating in harmonic scales is the following. The theory developed above depends upon regarding harmonic scales as projections of linear scales. Since *projection preserves notch coincidence, the same principle of interpolation can be applied also to harmonic scales*. (This assumes that the two different scales are attached to the *same* piece of the kamāl, so that the same projection is used to derive the two harmonic scales from two linear scales.)

Let us now apply this to the *kamāl*. The smaller piece corresponds to a $(\frac{1}{3}, \frac{1}{5})$ pair of scales, as we have seen, so that using these two scales together can thus give an accuracy of $\frac{1}{15}$ of a unit. The larger piece corresponds to a $(\frac{1}{8}, \frac{1}{5})$ pair, so that using these two scales together can give an accuracy of $\frac{1}{40}$ of a unit. In the above calculation, for ease of exposition, we used the approximation $\frac{5}{24} \approx \frac{1}{5}$. If we do not use this approximation, we must suppose we are dealing with a $(\frac{1}{8}, \frac{5}{24})$ pair, i.e., a $(\frac{3}{24}, \frac{5}{24})$ pair for which the accuracy could be at best $\frac{1}{24}$ of a unit.

In practice, the interpolation can be carried out as follows. Find the exact length of the string corresponding to the angular elevation of the pole star, and suppose this length lies between two knots. Since projection preserves notch coincidence, line up the second piece with the identified length of the first string, and then find which two of the knots of the two strings are closest to each other. From this, one can interpolate as outlined in the previous section, using either pair of scales.

The Accuracy of the Kamāl

> I certainly imagined that nothing could be more primitive than my Maldive friend's *kamāl*..., when lo! here is something even less advanced in ingenuity!
>
> James Prinsep[9]

To express this accuracy in modern terms, we proceed as follows. A glance at Table 5.1 and Fig. 5.2 shows that the bigger piece has a range from

$$\tan^{-1} \frac{36.5}{21.9375 \times 25.4} = 3.747^\circ \tag{5.12}$$

to

$$\tan^{-1} \frac{36.5}{6.0 \times 25.4} = 13.45^\circ. \tag{5.13}$$

A 90° increase in the elevation of the pole star corresponds to the distance from the equator to the pole, i.e., $\frac{1}{4}$ of the earth's circumference, calculated using the polar radius. Thus, a 1° increase in the angular elevation of the pole star corresponds to $\frac{1}{360}$ of the polar circumference of the earth. This differs very slightly from the equatorial circumference, and using either gives us a figure of approximately around 69 English miles. This gives a total range of around 670 miles. Since this range has been divided into 12 equal parts, each knot of the *kamāl* corresponds to an average distance of around 55 miles. Thus, each knot of the *kamāl* represented approximately half a finger increase in the elevation of the pole star, so that the constant F_0, used earlier, corresponds approximately to 4 fingers. The larger piece was, thus, suitable for travel from Mahaladwipa (Maldives) to Mangalore.

The larger piece of the *kamāl* is also extremely precise at the local level. Thus, using the two scales together with the larger piece gives an accuracy which is five times better, so that the *kamāl* could actually be used to measure distances as small as some 11 miles, or better than one *shāmam* which is quite extraordinary. In practical terms, this accuracy meant that the *kamāl* could be used to navigate to a point within sighting distance of the target.

Such a level of accuracy was indeed needed to sail to small islands. Thus, 19th c. CE English sailing manuals mention the difficulty in navigating to small islands, and suggest that a good way to this would be to run into the latitude, and then adopt a course due east or west. If this sort of thing were to be done, an accuracy of better than one *shāmam* (the distance to the horizon) would be needed to ensure that one did not sail past the island without spotting it.

In terms of angular measure, if we regard the range of around 9.7° as divided into 12 equal parts, each knot measures an angle of around 0.8° or 48 . If the two scales are used together, the precision is improved by a factor of 5, so that the precision is around 10 of the arc.

Similar considerations apply to the smaller piece which covers a range from

$$\tan^{-1}\frac{32.7}{2.625 \times 25.4} = 26.168^\circ \tag{5.14}$$

to

$$\tan^{-1}\frac{32.7}{17.375 \times 25.4} = 4.237^\circ \tag{5.15}$$

divided into 8 knots, with each knot corresponding to 2.75° or around 189 (English) miles. The use of both scales would enable this instrument to do 5 times better and measure distances of around 40 miles. (If we use the figure $\frac{5}{24}$, the two scales together could do only 3 times better, so that last figure would be only around 63 miles.)

Note that the total range of the instrument is a little above 1500 miles north–south. The upper end of this scale corresponds to the latitude of Karachi. Thus, the instrument re ects the fact that at higher latitudes (after crossing the latitude of Mangalore, say), a very high level of accuracy was no longer critical since the coastline was near. This applied also to the eastern side, where sailors from Minicoy typically travelled as far as Singapore.

Thus, in totality, the *kamāl* is a remarkable instrument with a huge overall range of 1500 miles, together with a striking accuracy of 11 miles at the lower end of the range. The construction of the *kamāl* also shows how instruments can be built from simple materials to measure angles with an accuracy of 10 .

Clearly, it was James Prinsep who lacked the ingenuity needed to understand the construction of the instrument. Moreover, carried away by his sense of racist superiority he failed to exercise common sense and ask how the island-based navigators could have routinely managed to sail back to small islands with inaccurate techniques of navigation. It is also noticeable that since Prinsep's article was first published in 1836,[10] Western histories of the subject have simply repeated his account.

The Two-Scale Principle and the Size of the Earth

The use of the two-scale principle suggests how al Bīrūnī could well have constructed an accurate instrument for measuring angles, to measure the dip of the horizon, and hence estimate the size of the globe, as he recorded. This answers a question, raised by S. S. H. Rizvi,[11] as to the accuracy of al Bīrūnī's hand-made instrument. Rizvi speculated that al Bīrūnī's hand-made instrument could well have had an accuracy of 1° for him to have arrived at as accurate an estimate as he did. The *kamāl* shows how higher precision *by nearly an order of magnitude* is easily possible for a hand-made instrument. The reason for Rizvi's extra-conservative estimate is obviously a false history of science which wrongly suggests to us that this two-scale technique was invented by Vernier, though it has been known to Europe from at least the times of Pedro Nunes (who also used it in an instrument to measure angles).

Instrumental Accuracy and the Accuracy of Trigonometric Values

Such accurate instruments for angle measurements probably first came into widespread use with the rise of Arabic navigation, sometime between Brahmagupta and Vaṭeśvara, and that would explain very clearly why Vaṭeśvara found Brahmagupta's sine table very gross, and needed to alter it to a more precise sine table with stored values at intervals of 56 15 , together with a second-order procedure for interpolation. In fact, since the accuracy of the instrument is about ten times better, this would also explain very clearly why even Vaṭeśvara's sine values would have been found to be "too gross" by later authors, who would have needed even more accurate sine values, together with higher order interpolation procedures.

By the end of the 18th c. Europeans had picked up a lead in navigation. Just as the Arabs had earlier made fun of the European method of navigating by charts, the European now started ridiculing the "little pieces of wood and string" used by the Arabs. We see that "little pieces of wood and string" that the Europeans made fun of can make a formidable navigational instrument that can be used to determine latitude and longitude, especially when combined with an advanced knowledge of trigonometry (calculus), and the ability to carry out mental calculations. What the British actually achieved by teaching navigation in the Lakshadweep islands was to destroy the indigenous knowledge, without replacing it with something particularly better. On the contrary, whether deliberately or otherwise, what the British really succeeded in doing was to destroy the self-sufficiency of the islanders, and to make their way of life dependent on imported instruments and books manufactured in far away lands.

III
LONGITUDE DETERMINATION

While the *kamāl* is a very accurate instrument for measuring north–south distances, it does not enable the measurement of east–west distance. The Lakshadweep islands (barring Minicoy) are very small coral islands, and accurately navigating to small islands is a difficult matter, which requires the sort of precision that was not easily available to late 19th c. European navigators, as already noted.

Traditional Indian Methods of Longitude Determination

Therefore, it is worth recollecting the several traditional methods which enabled precise angle measurements, coupled with precise trigonometric values, to be used also in connection with the measurement of longitude at sea.

First, we recall that the principle of time varying with longitude was well known to Āryabhaṭa (*Gola* 13):

> When it is sunrise at Lanka, it is sunset at Siddhapura, midday at Yavakoti, and midnight at Romaka.

The four names refer to four equidistant imaginary cardinal points on the equator, with Lanka being the point at which the Indian prime meridian (Meridian of Ujjayinī) met the equator.

Secondly, the stock technique for determining longitude on land was to use the time difference between the local time of an eclipse and its calculated time on the prime meridian (*LaghuBhāskarīya*, I.29)

> The difference between the computed and observed times of an eclipse is the longitude in terms of time.

Thirdly, we recollect Bhāskara I's method of determining longitude by the method of ephemeris, using a water clock (*Mahā Bhāskarīya*, II.8):

> On any day calculate the longitude of the Sun and the Moon for sunrise or sunset without applying the longitude correction, and therefrom find the time (since sunrise or sunset), in *ghatīs*, of rising or setting of the Moon; and having done this, note the corresponding time in *ghatīs* from the water clock. From the difference, knowledgeable astronomers can calculate the local longitude in time.

Fourthly, we recall Bhāskara I's method of solving a plane "longitude" triangle (*Mahā Bhāskarīya* II.3–4):

> Subtract the degrees of the latitude of ... [a known point on the prime meridian] from the degrees of the [local] latitude, then multiply [the resulting difference of latitude] by 3299 minus 8 25 [the radius of the earth], and divide [the result] by the number of degrees in a circle [i.e., 360]. The resulting *yojana*-s constitute the *koṭī* [upright of the right-angled "longitude" triangle]. The oblique distance from the local place [to the point on the prime meridian chosen above], which is known... is the *karṇa* [hypotenuse]. The square root of the difference between the square of the *karṇa* [hypotenuse] and the *koṭī* [upright] is defined by some astronomers to be the distance [in *yojana*-s of the local place to the prime meridian].

We also recollect from Chapter 4 that the above Indian method uses the radius of the earth, or equivalently a knowledge of the distance per degree latitude, a, so that it is perfectly possible to solve the longitude triangle from a knowledge of the difference of latitude l and the course angle C, to obtain the departure p:

$$p = a \times l \times \tan C. \tag{5.16}$$

Furthermore, we recall that this Indian technique, available from before the 5th c., was *not* available to European navigators in the 16th and 17th c. CE, for the reason that Europeans lacked a precise knowledge of the size of the earth until the end of the 17th c. CE.

Finally, we recall that, knowing the size of the earth, it was an easy matter to convert distance from the prime meridian to longitude, and it was only necessary to invert a rule explicitly stated by Bhāskara I (*Laghu Bhāskarīya*, I.32), relating this distance to longitude:

> The *yojanas* (of the distance of the prime meridian) from the local place are obtained on multiplying the longitude in *ghatīs* by the local circumference of the Earth and dividing (the product) by 60.

Some Clari cations

The method of determining longitude/departures by solving a plane triangle was known to Arab navigators as a *tirfa* calculation. However, the examples of actual *tirfa* calculations given by Tibbets are rather crude, suggesting that Arab navigators were unaware of elementary plane geometry in the 16th century CE, and did not even know that two sides of a triangle are greater than the third.

Such historical depictions tend to raise a doubt. As we shall see later on, the real question is whether the slightest credibility is to be attached to Western accounts of history. For the time being, however, let us address this doubt. Could the techniques in the *Laghu Bhaskariya* have diffused to the islanders over a period of several centuries? Could the islanders have known about Mādhava's more precise sine tables? Clearly it would be inappropriate to assume that the average navigator was as knowledgeable as Bhāskara or al Bīrūnī. It would be equally inappropriate to assume that the average navigator on the Indian ocean or Arabian sea was as unskilled in astronomy and mathematics as Columbus or Vasco da Gama. There are two reasons for this.

First, navigational techniques here placed far greater reliance on celestial navigation. Unlike Columbus, therefore, Indian, Arabic, or Chinese navigators had to have *some* knowledge of astronomy. A modern-day analogy may help to explain the cultural difference: a semi-literate carpenter in India today is likely to be better at mental computations than a cash-register operator at a US supermarket, who has never done arithmetic without the aid of machine or paper. However, colonial historians found it galling to admit that the average navigator by the stars knew more than their own star navigators. How much knowledge of astronomy a navigator might have had, naturally depended on his competence, but given that his own life and the lives of many others depended on his knowledge, it would be a rare navigator who did not seek to expand his knowledge by acquiring at least the knowledge incorporated in the most popular texts in astronomy. Such a navigator is unlikely to have been much sought after.

Secondly, because of the monsoons, a navigator here could earn a living from navigation for at most some six months in a year. What did he do the rest of the time? Clearly, some, at least, of the navigators would have done exactly what Kepler did: use their knowledge of astronomy to make a living through astrology; others may have turned their attention to tasks such as calendar-making, etc. For this purpose too they would have had to consult the basic texts in astronomy. So it would hardly be too much to attribute to the average navigator the knowledge available in concise practical manuals of astronomy, such as the *Laghu Bhaskarīya* or the *KaranaPaddhati*, for the reason that

> both the *Maha-Bhāskarīya* and the *Laghu-Bhāskarīya* were popular works, having been studied in south India up to the end of the fifteenth century..., the latter being an excellent text-book for beginners in astronomy.[12]

To summarize, there is a difference between the knowledge required to derive and correct the rules, and the knowledge required simply to use these rules. One must attribute to the pre-colonial navigators at least the latter type of knowledge of astronomy.

On the Lakshadweep islands, Kunhi Kunhi Malmi, of Kavaratti for instance, made a living partly through astrology. His preoccupations are re ected in the fact that more than 50% of his *Rahmani* (released at the 10th Indo Portuguese Conference on History, INSA, New Delhi, 1998) is concerned with astrology. (Indeed Kunhi Kunhi made a good living and had two wives—as astonishing a thing in a matriarchal society as a woman with two husbands would be in a patriarchal society, and a definite indication of prosperity.) For the relatively simple needs of the Lakshadweep islanders, of course, spherical trigonometry was not required, and the solution in plane triangles, as in Fig. 5.3, was adequate.

Since some of the concerned texts, incorporating the requisite precise trigonometric values, are in Malayalam, in Kerala itself they enjoyed considerable circulation, as evidenced, for example, from the large number of copies of Jyeṣṭadeva's *Yuktibhāsā* which are still in existence, and the *KaraṇaPaddhati*, whose encapsulated rules continue to be very popular. (The relevant verses are also in the *KaraṇaPaddhati*.[13]) So why should the relevant sine values not have been known at least to some knowledgeable navigators on the island who knew something of the astronomical tradition in Kerala?

It is true that the islanders, like the Māpilā-s, spoke Arabic-Malayalam, and it is possible that they were hence regarded as illiterate by both Arabs and Malayalis! None of the *malmī*-s I spoke to was much educated in the Western tradition, but that did not prevent any of them from knowing about Norie's tables. Why, then, should the earlier *malmī*-s not have known about Mādhava's tables? The *tirfa* calculation done using these tables would indeed have made the *kamāl* a complete instrument which could be used to decide both latitude and transverse position at sea.

Thus, the name *kamāl* (= complete) was justified, since the instrument could be used across a wide range, was very accurate for navigation to small islands, and it was possible also to determine longitude at sea from a knowledge of the difference of latitudes.

Currently-Used Techniques of Longitude Determination

As opposed to this situation prevalent with traditional knowledge, currently the islanders use two techniques for longitude determination.

A watch (chronometer) is one technique used today by the islanders to decide longitude (though the figure commonly stated was 5 minutes per degree of longitude).

The principle behind using a watch to determine longitude is straightforward, and well known to all international travellers. Because of the diurnal rotation of the earth, as one travels east, one gains time—the sun seems to rise earlier. Consider an accurate watch set to local Bombay time, i.e., its hands read 12 o'clock when it is noon (the time of the shortest shadow) at Bombay. If this watch is carried to Calcutta, noon at Calcutta will seem a little early. In a complete circuit of 360° round the earth, the watch will appear to gain or lose 24 hours = 24×60 minutes, so that the watch will gain or lose 4 minutes per degree longitude.

The other technique the islanders currently use is a sand clock (*tappu kuppī*, lit. sand bottle) of 7 or 14 s and a log line (with the rope knotted at equal distances) to measure the speed of the boat. The speed of the boat can be used to calculate the distance travelled in a known period of time: this technique is known to be notoriously inaccurate. From a knowledge of the speed, and the duration for which the speed was maintained, one calculates the distance travelled. The course angle is known through a magnetic or stellar compass. Hence, the departure can be computed by resolving the problem into the solution of a plane triangle, as in Fig. 5.3 reproduced from Chapter 4. The solution itself was obtained using traverse tables from British sailing manuals.

IV
THE VALUE OF BRITISH EDUCATION

The islanders have evidently learnt this technique from the British efforts to "educate" them, as described earlier. This enables us to assess the value of British education in a microcosm. This is useful because, compared to mathematics education, which we will consider later on, the issues involved here are relatively simple.

First, the process of navigational education itself was initiated based on certain historical premises. It is worthwhile examining these historical premises: while distorted historical depictions of navigation history like that of Tibbets are amusing for the trained historian, the dissemination of false historical narratives at the popular level has had significantly mischievous political consequences.

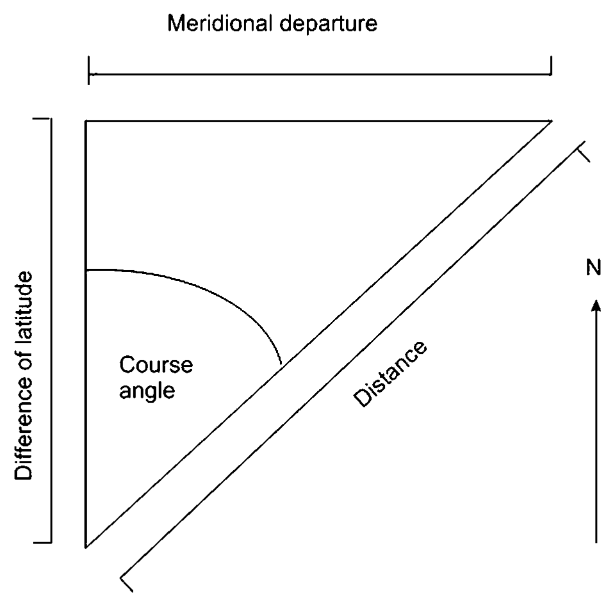

Figure 5.3: **Solving the nautical triangle.** The right-angled triangle shown above, also called the plane sailing triangle, can be solved from a knowledge of either (1) course angle and distance travelled, or (2) course angle and the difference of latitude. The first method was used by Europeans in dead-reckoning navigation. The second method requires an accurate estimate of the size of the earth: such an estimate was available to Indians from at least the 5th c. and Arabs from at least the 9th c. CE, but not to Europeans until the late 17th c. CE. Hence, European navigators could not use the second method. This is what led to the famous problem of determining longitude at sea—a problem specific to European techniques of navigation.

According to the grand historical narrative, the British were a great superpower, on account of their knowledge of navigation, while the islanders were "primitive" people, who lacked a knowledge of navigation. This sort of account of the "natives" is found most clearly in novels like *Coral Island* by R. M. Ballantyne.

Swept away by such fake historical narratives within which they situated themselves, the British seem not to have stopped to think how the islanders had survived if they did not have reliable techniques of navigation. This survival had a history going back to at least pre-Islamic times, considering that there are statues of the Buddha on the islands, which were subsequently defaced. Though these statues have not been dated to my knowledge, they could quite possibly go back a long time in the past. In fact, navigation no doubt existed also in the era when Ashoka's daughter, Sanghamitra, travelled to the island of Sri Lanka. At any rate the Lakshadweep islands were inhabited for over a thousand years before the British came to them. During all this period, how did the islanders solve the problem of navigating to small islands? (Recall that this was recognized as a difficult navigational problem by 19th c. CE British sailing manuals.)

Apparently, swept away by the military power of the British, the islanders too did not stop to think about it either, and the youth seem to have assumed that the navigation techniques taught to them by the British were intrinsically superior, just as youth today thoughtless tend to accept that Western ways are intrinsically superior. The point is that the islanders seem to have adopted the British techniques of navigation in a somewhat thoughtless way, and without having made a relative assessment of the two systems, just as youth today might adopt Western music in preference to Indian classical music without a clear understanding of the two systems. Although a technique of navigation is more directly relevant to survival than music, that the islanders' choice was not informed by any such relative assessment is confirmed by the fact that none of the islanders was able to tell me about the functioning of the *kamāl*.

The process of British education changed things in two significant ways. First, the islanders were taught about the sextant (*kamān*), but not about the *kamāl*, and as a direct consequence of this training they abandoned the *kamāl* in favour of the sextant. While stone sextants were used in Arab astronomical observatories from the 10th c. CE, the portable sextants used in navigation are made of steel. Since steel was not something they could make themselves, the islanders became dependent on far-off British engineering for their very survival. Merely to purchase appropriate instruments they would have needed to sail as far off as Bombay, and those who were most closely linked to the British were the one's best able to survive.

What advantages the sextant (*kamān*) had over the *kamāl* was obviously not discussed in the British text either, and the *kamāl* was never mentioned, just because the historical narrative in which the British situated themselves, assured them that the progress brought about by the march of science had made their knowledge superior to that of the "primitive" tribes of the world.

However, the sad fact is that the sextants actually used by the islanders typically had an accuracy of about 1°, and hence were a lot LESS accurate than the *kamāl*. Thus, the British, smug about their own superior techniques of navigation, ultimately ended up educating the islanders in inferior techniques of navigation! Noticeably, there was no colonial plot here, except an attempt to try and make the British empire more popular!

It is also a sad fact that the determination of longitude by using a sand clock and heaving the log also made the situation worse for the islanders: since the islanders did not rely on charts in the manner of the British, did not really use dead reckoning, and had no particular use for loxodromes, since they did not intend to sail to Europe by means of charts. The islanders would have done better by persisting with the traditional techniques of using ephemeris time or solving the longitude triangle in the manner of the *Laghu Bhāskarīya*, but they were taught instead the use of traverse tables as in British sailing manuals.

That the islanders became dependent also upon British sailing manuals is clear from the "Noorie" tables in the Rahmani of Kunhi Kunhi Koya. There was no way anyone on the island could have produced such tables. Thus, the islanders became consumers of knowledge that they could not themselves produce or even properly understand.

Thus British education systematically created a situation of dependency and inferiority as regards both knowledge and education. While the islanders could not earlier match

British violence and duplicity, this was not necessarily a matter of inferiority. From an ethical perspective this made them superior rather than inferior. However, after being educated by the British, the islanders actually became inferior, since their livelihood, which required navigational aids, became dependent upon the British, reducing them to a state of servility. Since the islanders never received enough education to make them producers of knowledge, they remained passive consumers of knowledge. Thus, education, instead of serving the purpose of liberation, became a means of bondage. Like a self-fulfilling prophecy, the fake historical narrative was thus turned into a distressing reality.

A Revised History of the European Longitude Problem

A brief examination of the actual sequence of historical events is also worthwhile, for our later purposes of understanding transmissions and diffusion from an epistemic perspective.

This dead-reckoning method was used extensively by early European navigators, who plotted the ship's course on charts to carry out the computation graphically. However, the method of estimating the ship's speed by "heaving the log" was well known to be extremely unreliable.

Early Portuguese navigators, however, had no alternative to dead reckoning, since they had not quite learnt the techniques of celestial navigation from the Arabs. In using the *kamāl*, the knots are counted by keeping the string between one's teeth; hence the name *kau* (=teeth) for the pole star. Vasco da Gama's men thought that the pilot (Malemo Cana) was telling the distance by his teeth!

Vasco da Gama carried back a copy of the instrument "to have it graduated in inches",[14] suggesting that he did not understand the difference between a linear scale and a harmonic scale. In fact, Europeans seem never to have quite understood the principle of harmonic interpolation used in the *kamāl*.

By the mid-16th century, the Portuguese had learnt some techniques of celestial navigation. What they learnt was, however, so inadequate compared to the tremendous economic importance of correct navigation, that in 1567 Philip II of Spain offered a big reward to anyone who could produce an accurate method of navigating at sea. One difficulty concerned latitude. From the time of Brahmagupta and the Sind-Hind tradition, it was known that latitude could be determined from solar altitude and declination (or the transits of circumpolar stars). The Europeans, however, had difficulties with this method, since they relied on an inaccurate ritual calendar that was partially corrected only in 1582. (Due to religious quarrels between Protestants and Catholics, even the corrected calendar was not uniformly adopted in all of Europe—Isaac Newton believed he was born on Christmas day, while many parts of Europe had already celebrated the New Year a few days before his birth.)

Correction of the calendar obviously was not enough to solve the navigational problem. The European technique of dead-reckoning had made navigation more complicated than it needed to be. So, from the European viewpoint, there remained

the problem of precise trigonometric values,

the problem of loxodromes,

the problem of the size of the globe, and

the problem of determining longitude.

Furthermore, the Europeans were culturally unaccustomed to mental calculation. Like the abacus, they wanted to be able to do the necessary calculations *mechanically*.

So the reward for an accurate technique of navigation was substantially increased in 1598. Galileo was one of the aspirants for the award for nearly 16 years, starting from 1616, though his method (using Jupiter's moons) was rejected as impractical. Later on he competed for the prize offered by the Dutch government in 1636.

In France, Colbert, following his predecessors Mazarin and Richelieu, sent personal invitations, offering vast sums of money, to Huygens, Leibniz, Roemer, Newton, Picard, ... for a solution of the longitude problem. From the reply, he selected 15 people to form the Académie Royale, with the specific objective "to improve maps, sailing charts, and advance the science of navigation".

By the late 17th century, "The Académie Royale des Sciences had solved the problem of longitude for places on land." The principle of the method of using eclipses had been stated succinctly by Bhāskara I, a thousand years earlier. The method was used in a slightly modified form some six centuries earlier by al Bīrūnī. The principle of the method is, first, that longitude corresponds to the local time. The difficulty is to measure the local time *simultaneously* at two localities. How should one synchronize the measurements in two separated places in the absence of radio or light signals? A lunar eclipse enables this synchronization: the two observers can each measure the local times of onset, totality, and end of the eclipse. A lunar eclipse is more suitable than a solar eclipse because the absence of parallax ensures simultaneity. Al Bīrūnī reported such a joint operation between him observing at Kāth (in Central Asia) and Abū al-Wafā' at Baghdad.[15]

The improvement by the Académie Royale came about through the availability of the telescope: they used instead the eclipses of the moons of Jupiter, which can be seen through a telescope.

The problem of determining longitude at sea remained. In 1707, because of bad navigation, four ships of the British Royal Navy sank off Scilly Isles, with some 2000 soldiers and Admiral Sir Clowdisley Shovel. There was an uproar, and the British Parliament established a committee before which Isaac Newton deposed.

> That, for determining the Longitude at Sea, there have been several Projects, true in theory, but difficult to execute. One is a Watch to keep time exactly, but, by reason of the motion of the Ship at Sea, the Variation of Heat and Cold, Wet and Dry, and the Difference of Gravity in different latitudes, such a Watch has not yet been made.[16]

By an Act passed in 1714, the British Government constituted a Board of Longitude, and offered a reward of £20,000 to any one devising a method of determining longitude at sea. (Newton's Fellowship offered him the considerable sum of £60 per annum.) Supported by the Board of Longitude, John Harrison (1693–1776), a carpenter from Yorkshire, developed such a watch—the Marine Chronometer—and competed for the award in 1757. The watch passed the test on a voyage to Jamaica in 1762, but Harrison was given only £2500, because the learned Board opined that the longitude of Jamaica was not well-enough known to decide whether the watch had cleared the test! (The mathematician Euler received a part of the award.)

That was hardly the end of the story. As late as 1864, practical measurement of longitude by European navigators was still so uncertain ("these instruments are liable to vary their rates") that Norie opined that a good way to make for small islands was to run into the latitude, and then sail due east or west to the island! The tables of 1864 elaborate on the practical problem encountered with the actual use of a chronometer.

Summary and Conclusions

Although knowledge of navigation existed in India, and Europeans in the 16th c. were well aware of it and carried it back with them to Europe, two facts stand out.

(1) Europeans could not directly use the Indian knowledge of navigation as it stood, since this knowledge was not consistent with other things they knew (or thought they knew), such as the size of the earth. (This is closely analogous to the epistemic divide in mathematics that we will come across later: Europeans, even after acquiring knowledge of the calculus from India, could not immediately use that knowledge because their understanding of it, involving infinities and infinitesimals, was not consistent with their ideas about mathematics.) Thus, Europeans failed to comprehend the Indian way of determining longitude at sea.

While a variety of European instruments were built in the 16th c. for latitude determination, copying instruments like the *kamāl,* I am not aware of any European instrument which used two scales for harmonic interpolation. In fact, the very principle of harmonic interpolation seems to have been unknown to European navigational instruments (as far as I know). Therefore, although Vasco da Gama carried back with him a copy of the *kamāl,* and although he had many persons in Cochin willing to advise him about it, somehow or the other he never managed to fully understand the construction of the *kamāl.*

(2) At no stage did European historians ever acknowledge in the straightforward way of Arab historians that they had obtained knowledge from India. The overarching in uence of the church ensured that they preferred to deny any pagan sources of knowledge to continue with the fake historical narrative which had been provided to them, by freely modifying historical facts as convenient. Like the numerous verbal covenants with local people that the Europeans broke, the temporal power of these historical lies is evident in the above sequence of events in which the Lakshadweep islanders swapped the better traditional technique of navigation that they had in favour of the inferior British technique taught by the Britishers. This swap made the islanders dependent upon the British, as consumers of British knowledge and navigational instruments, essential for their very survival, which British knowledge or instruments the islanders could not themselves produce.

The British education provided to the islanders was based on the historical narrative within which the Britishers situated themselves. Thus, the constant reiteration of a fake historical narrative became a key source of what is today called "soft power" and has played a far more important role in colonization than has been historically told to us till now.

NOTES AND REFERENCES

1. Ethel M. Pope, *India in Portuguese Literature*, Asian Educational Services, New Delhi, 1989. A Lopes Mendes, *A India Portugeza*, reprint. Frederick Charles Danvers, *The Portuguese in India*, Asian Educational Services, New Delhi, 1992. Cana suggests Kanha, a common enough name on the Gujarat coast, since tradition regards Dwarka as the home of Krishna. There has also been a suggestion that Cana relates to Kanaka, meaning astronomer.
2. G. H. Tibbets, *Arab Navigation in the Indian Ocean Before the Coming of the Portuguese* (Tr. of Ibn Majid's *Fawā id*), Royal Asiatic Society, London, 1971.
3. Strictly speaking, the term "pilot" is used for someone familiar with a particular port or coastline, and who does not know how to navigate at sea. In this case, it was Vasco da Gama who did not know how to navigate. But, in history written by Europeans, so strong is the tendency towards self-glorification and the disparagement of others that the tendency is to use disparaging terms about others, regardless of the facts. European historians have found it very galling to admit the truth that the real navigator was an Indian and not Vasco do Gama.
4. The Europeans used charts for "dead reckoning" since they lacked a reliable way to determine longitude at sea, until the middle of the 18th century, as discussed in more detail, later on.
5. Memorandum to the Presidency Port Officer No. 649 D/23-2, dated 11th April 1923, says: "Mr R. H. Ellis who inspected the Laccadive Islands is of the opinion that an elementary knowledge of Navigation would be of great use to the islanders and that the addition of this Subject to the curriculum of the school at Ameni where the Headmaster already knows the rudiments of navigation, would increase its popularity." The Memorandum is reproduced in *Nāvika Shāstram*, cited below.
6. Government of India, *Report of the Calendar Reform Committee* (M. N. Saha et al.), CSIR, New Delhi, 1955, p. 160.
7. As quoted in H. Grosset Grange and H. Rouquette, "Arabic nautical science", in: *Encyclopaedia of the History of Arabic Science*, vol. 1, ed. Roshdi Rashed and R. Morelon Routledge, London, 1996, p. 217.
8. The elevation is presumably standardized based on the height of a typical boat: the *odam*, and the height of the tallest coconut trees. The implied assumption of a spherical earth is discussed in more detail later on. On a strict usage of the term "horizon" an object at a height (such as a tree top) when it first appears above the line of sight may be located at a point below the horizon. Here, however, "distance from here to the horizon" refers to the distance from here to the base of that object.
9. "Note on the nautical instruments of the Arabs", *Introduction a LAstronomie Nautique Arabe*, ed. Gabriel Ferrand, Paul Geuthner, Paris, 1928, pp. 1–30 (p. 15).
10. *Journal of Asiatic Soc. of Bengal*, Dec, 1836, p. 784.
11. S. S. H. Rizvi, "A newly discovered book of Al-Bīrūnī: Ghurrat-uz-Zijat', and Al-Bīrūnī's Measurements of Earth's Dimensions"'. In: *Al-Bīrūnī Commemorative Volume*, ed. H. M. Said, Hamdard Academy, Karachi, 1979, pp. 605–80.
12. K. S. Shukla, *Laghu Bhāskarīya*, cited earlier, p. xix.
13. *Karaṇa-paddhati of Putumuna Somayāji*, ed. P. K. Koru, Astro Printing and Publishing Co., Cherp (Kerala), 1953, p. 203; *Karaṇa Paddhati of Putumna Somayāji*, ed. S. K. Nayar, Govt. Oriental Manuscript Library, Madras, 1956, pp. 189–193.
14. K. M. Mathew, *History of the Portuguese Navigation in India 1497–1600*, Mittal Publications, Delhi, 1985, p. 17.
15. E. S. Kennedy, *A Commentary upon Bīrūnīs Kitab Tahdid al-Amakin: An 11th Century Treatise on Mathematical Geography*, Beirut, 1973, p. 164.
16. Journals of the House of Commons, 11 June 1714, 677; cited in G. J. Whitrow, *Time in History*, Oxford University Press, 1989, p. 141.

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Part III

Transmission of the Calculus to Europe

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CHAPTER 6

Models of Information Transmission

General historiographic considerations and the nature and standards of evidence to decide transmission

OVERVIEW

EXPLICIT models of information transmission are needed, since the implicit models used so far by historians do not bear open scrutiny. Especially in the context of Toynbee's theory of "barbarian incursions", I propose a model in which not only wealth but information often ows *towards* military conquerors, as in the Roman conquest of Greece, or the Moghul conquest of Baghdad by Hulegu. What is being proposed is thus a causal link between Alexander's military conquests in Egypt and Iran, his enormous booty of Egyptian and Persian books, and Aristotle's scholarship, just as there is a causal link between Mahmood of Ghazni's conquests and al Bīrūnī's scholarship. This ow of information may long precede a military conquest, as in Herodotus' account of Greek traditions being but an imitation of the traditions of black Egyptians, or the ow of information from India to Baghdad that preceded Ghazni. A similar long-term ow of information into Europe took place for some 250 years after Vasco da Gama—during which Europeans repeatedly failed in their plans to conquer India and China by military force or religious conversion. This ow of information into Europe was a key cause of the rapid advances made by Europe in the 16th and 17th c. CE. Information may sometimes scatter in other directions following a military conquest, as when Buddhist ed to Tibet, after the sack of Nalanda by Muhammad-i-Bakhtiyar, or (Byzantine) Greek manuscripts, incorporating Arabic and Indian knowledge, through translations from Arabic to Greek, came in bulk to Europe after the fall of Istanbul to Mohammed the Conqueror in the 15th c. CE.

Apart from a model of information transmission, the other thing that is needed is an explicit standard of evidence for transmission. Before fixing the standard of evidence for transmission, let us first look at the past practice. Western historians have often claimed that most knowledge originated with the "Greeks" and was transmitted to other parts of the world. What standards of evidence were implicitly used to support these claims? To uncover these standards, it helps to put these past practices in their proper historical perspective.

The claim—that world knowledge was derived by transmission from the Greeks—historically originated as follows. In the late 11th c. CE, when Europe was still in its "Dark Age", and the "Islamic Golden Age" was coming to an end, the declining Arabic civilization experienced "barbarian incursions" from Europe. The very beginning of these Crusades is marked, as in the above theory, by an increased ow of information towards Europe. A major event here was the capture of Toledo and its library. The subsequent translation of hundreds of Toledan texts from Arabic to Latin, in the 12th c. CE, provided the primary corpus of texts for the first European universities. However, this massive ow of information into Europe generated two difficulties. First, during the Crusades, there was a sense of shame in learning from the Islamic enemy. Second, during the Inquisition, there was a fear that the Toledo library was a Trojan horse which would spread heresy, and thus undermine the power of the church.

The sense of shame was tackled by "Hellenization"—this was a simple trick by which a pure Greek origin was attributed to any incoming knowledge regarded as useful to Europeans. (The fear of heresy was tackled by "Christianization by reinterpretation": for example, the *Elements*, which first came to Europe via Toledo, was reinterpreted to strip it of its "Neoplatonic" philosophical concerns and make it consistent with Christian theology, as we have seen.)

Now the Arabic books at Toledo come from some 250 years after the formation of the House of Wisdom in Baghdad—where books were imported and translated from all over the world. The books imported at Baghdad are certainly known to have included many Indian books on mathematics and astronomy, for example. Some of the Indian books, like the *Pañcatantra* are known to have reached Baghdad not directly from India but indirectly from Jundishapur, and were translated from Pahlavi to Arabic. Jundishapur, in the 6th c. under Khusrow I, provided an earlier model of the Baghdad House of Wisdom, and had already imported also Indian astronomy and the game of chess, for example. (Where the *Pañcatantra* was used to teach justice, chess was used to teach strategy especially to kings.) Thus, the 11th c. CE Arabic books available at Toledo re ected an accumulation of world knowledge, certainly including much Indian knowledge.

Thus, the trick of Hellenization—attributing a "Hellenic" origin to all knowledge available in Arabic books at Toledo (and in subsequent Byzantine Greek texts)—appropriated to the West all the knowledge of world up to the 11th c. CE—especially knowledge of mathematics and astronomy. So, it is hardly a matter of surprise that the knowledge that Western historians hypothetically attribute to the early Greeks is all so remarkably similar to the world knowledge of the 11th c. on which this attribution is based: "Ptolemy's" *Almagest* begins (as natural for an 11th c. text) with what look like paraphrases of controversies from the history of Indian astronomy, "Aristotle's" syllogisms are remarkably similar to the Nyāya theory of syllogisms, "Aristotle" uses theories like those of "action by contact" and the same words like "aether" (= sky = *ākāśa*) long used in India, and his physics is as similar to Arabic physics as "Archimedes" is to 11th c. Arabic mathematics.

In support of the trick of Hellenization, it was argued that this similarity of "Greek knowledge" with 11th c. CE world knowledge was due not to incorrect attribution, but to transmission from the "Greeks". So what is the evidence for transmission?

On the face of it the standard of evidence involves similarity and precedence: if two texts are similar, then the later text is probably a copy of the earlier one. There is no major problem with this standard of evidence for transmission so long as there really are *two* texts. The problem arises when only one of the texts is real, and the existence of the earlier text is merely being conjectured from the later text. In this case, "similarity" becomes an empty tautology because all our knowledge of the purported earlier text is derived solely from speculations based on the later text. Priority also ceases to be meaningful: all that we really have is a Greek name of doubtful historicity, an untestable hypothesis that attributes authorship of a purely hypothetical early text to this name, and a speculative chronology attached to this name.

In the absence of serious evidence even to establish the validity of the attributions to "Greeks", it is understandable that nothing much was available by way of evidence for this "transmission from the Greeks". All that Western historians have had to offer is only a speculative chronology attached to Greek names, which chronology could well blend into history many mythical creatures of the imagination like "Euclid". Those familiar with how Biblical chronology was used by various noted European scholars, such as Sir John Lightfoot, Vice-Chancellor of the University of Cambridge, to fix the date and time of creation, with great precision, will immediately grasp the principle of the thing: chronology established the reality of the event of creation. In history, the aim of this competitive chronology was to establish the Greek priority in all forms of human knowledge, and hence to establish that all other knowledge derived by transmission from the Greeks.

This speculative Greek chronology derives entirely from stray remarks in various late texts. There has been a remarkable complacency towards the source material, ignoring both the chauvinism that accompanied the religious fervour of the Crusades, and the prevailing social circumstances of the Inquisition (and the preceding centuries of church terror against dissenters) all of which would naturally have encouraged the interpolation or forging of convenient remarks in the sources even by neutral scribes to save their skin. (We have already examined in depth one such remark in the case of "Euclid".) No attempt seems ever to have been made to compare this chronology with any non-textual evidence which might show such attributions to early Greeks as anachronistic. Nor was any attempt made to check the attributions against prevailing economic realities: in the days of papyrus technology, the survival of a text required investment. What led to the repeated long-term investments in the production and propagation of texts which seemed to have no particular relevance to the lives of the Greeks or the Romans? And why, despite this hypothetical investment, did Alexandrians remain unaware of Aristotle's syllogism and "Ptolemy's" *Almagest*?

Thus the standard of evidence for transmission *from* the Greeks was this: if a scholar could find some textual remark, real or contrived, to justify a competitively early chronology for a Greek name which could be attached to an Arabic or Byzantine Greek work of the 11th c. or later, then it was considered established that there had been transmission from the Greeks to others. It is this sort of principle of evidence that provides the basis for the oft-repeated claims of transmission from the probably mythical "Claudius Ptolemy" to Indian astronomy.

Now there certainly are some known and incontrovertible instances of transmission in the opposite direction: for example, the Indian *Pañcatantra*, translated from Sanskrit to Pahlavi in the 6th c., and then to Arabic in the 9th c., was indubitably translated from Arabic to Byzantine Greek latest by the mid-11th c. CE, and then to Latin, ca. 1251. There are other instances where such transmission by translation from Arabic to Greek to Latin seems very likely to have taken place. For example, Copernicus' model of the moon is identical to the earlier model of Ibn-as-Shātir of Damascus. It is quite likely that this model was translated from Arabic to Byzantine Greek, and came to Rome after the 1452 fall of Istanbul, along with other Byzantine Greek texts, and that Copernicus' key contribution was to translate it into Latin. Curiously, in this case, the noted historian Owen Gingerich asserts that it is possible that Copernicus had discovered his model independently. So, similarity and precedence (between two real texts) are *not* always regarded by Western scholars as conclusive proof of transmission. *A priori*, independent rediscovery is not impossible; it is just exceedingly improbable that, by some miracle, Copernicus independently rediscovered it just when he could have readily learnt of it by transmission.

In fact, Ashoka's rock edicts about the success of his mission of wise men sent to Alexandria at the time of Ptolemy II, and subsequent Roman and Alexandrian knowledge of India, show that Indian knowledge did go to Alexandria since Ptolemy II, and that it did have an impact, and that this impact persisted right up to the time of Porphyry. The similarity is noticeable. So it is conceivable that Indian knowledge of astronomy too could have travelled to early Alexandria, long before the time of "Claudius Ptolemy". However, the possibility of Indian knowledge having been transmitted in this way to Alexandria has been vehemently denied—primarily with a view to deny Indian in uence on early Christianity.

So, in practice, Western history has used *two* standards of evidence for transmission: one ultra-lax standard of evidence for transmission *from* "Greeks", and another ultra-strict standard for transmission *to* the West. For cases of alleged transmission from the Greeks, mere speculations—a speculative chronology combined with speculative attribution—are regarded as ample *evidence* of transmission. In the other direction, similarity with a real earlier work, by a real author, together with a clear channel of transmission, do not prove anything, for there is always the possibility of repeated miracles by which any number of people in the West may independently reinvent things just when they could be transmitted.

One might ask: why should there be two standards of evidence? For this, we need to understand the origins of racist history, in the systematic religious encouragement of violence.

The 15th c. CE Doctrine of Christian Discovery marked the culmination of a long-standing policy of using violence and state power against non-Christians, as this policy progressed through Constantius, Justinian, Charlemagne, the Crusades, and Inquisition to escalate to a truly genocidal crescendo. The Doctrine of Christian Discovery, which instigated the subsequent triple genocide in three continents of South and North America and later Australia—the only known successful cases of genocide in a literal sense—was explicitly proclaimed in papal bulls (Romanus Pontifex, 1454, and Inter Caetera 1493), which declared it the religious duty of Christians to kill and enslave all non-Christians. The first-hand descriptions of the genocide in the Americas provided by Las Casas (who accompanied Columbus) clearly show that it was religiously motivated, and that those engaged in the genocide thought they were doing their Christian duty by eliminating non-Christians and carrying out God's will here on earth as it would be in hell. However, unlike, say, Hitler, or Idi Amin whose violence is regarded as the epitome of immorality, in this case the instigators of genocide were also the self-appointed custodians of morality.

The moral justification for the violence created various problems. For example, some of the Africans enslaved in this process of colonial/Christian expansion, turned Christian. *Now* what was the justification for ill-treating them? How did *this* brutality further the doctrine of love? The theologians naturally understood the economic benefits of slavery: the enforced labour of the slaves was required to extract the wealth of the vacated continents; slaves were the key means of production. The categories of White and non-White were invented for this purpose, to morally justify the economic advantage deriving from genocide and slavery. Like a person's dress, during the Inquisition, the colour of a person's skin was an easy and sure visual way to identify those who were either non-Christians or were recent converts to Christianity. Like the Mozarabs of Toledo, or converted Jews during the Inquisition, recent converts to Christianity were not regarded as quite fully human. Many blacks have been Christians from the 16th c., but no pope has ever been black.

Hence, the fabrications of racist history aimed to explain the moral desirability of these crimes against humanity, by systematically denigrating all non-Whites, to portray them as somewhat less than human. This sort of religious racism coloured history even at Toledo: Gerard of Cremona is credited with having translated over 70 books from Arabic, although he knew no Arabic! Similarly, for the last five centuries, the Indian who brought Vasco da Gama from Africa to India is always described as a "pilot" (one who guides the ship near the land) and never a navigator, although the empirical fact was that it was Vasco who was creeping along the African coast, because he did not know enough to navigate across the ocean, and it was the Indian who took him across the ocean.

Not many people seem to know that the claim that Vasco "discovered" India has a technical meaning, deriving from the Doctrine of Christian Discovery—which asserts that any piece of land belongs to the first Christian to "discover" it. It is in this same sense that Columbus "discovered" America. This religious-technical meaning of "discovery", and the accompanying dehumanization of non-Christians, is institutionalized in legal questions of land ownership: according to the current US law, laid down by the US Supreme Court, it is for this reason of "discovery" that the original inhabitants of North America cannot claim any rights to their ancestral land. They lost that right on being "discovered" by Columbus, who also performed an appropriate Christian ritual—a little *pooja*—to take over the land in the name of his sovereign.

If non-Whites had no claim even to ownership of land, how could they claim ownership of knowledge? Therefore, in the same vein as this Doctrine of Christian Discovery, racist historians advanced the claim that no theologically incorrect part of the world had played any role in discovering anything worthwhile. Hence, they posited that anything worthwhile had either been invented in post-14th c. Europe, or had been earlier invented in (White) "Greece", or had been obtained from there by transmission. (Today no one any longer *says* that the concerned Alexandrian "Greeks" from Africa were White; they just put in an image of a person with Caucasian features—as in the latest Indian school texts—so that people get the picture right.) Just as the state-church preached the physical elimination of non-Christians, so also, European historians scrambled to write history with a view to eliminate any significant historical role for non-Christians and non-Whites: the church agenda of physical genocide was matched by the racist historians' agenda of cultural genocide. The agenda of physical appropriation of all land in the world was matched by the agenda of intellectual appropriation of the credit for all knowledge in the world.

The intrinsic absurdity of this historical proposition was no great difficulty for a church which had long been in the business of controlling large masses of people by making them believe all sorts of manifestly absurd propositions.

The enforced conformity prevailing in Europe made the task of racist historians easier. During the 16th c. CE, books started reaching Europe from all corners of the world, but the iron hand of the church made it impossible for European to acknowledge a "pagan" source of knowledge. It is easy to understand why people like Mercator, once arrested by the Inquisition, went to great lengths to hide their "pagan" sources—for revealing these sources would have invited a brutal and painful death by torture. On the other hand, people at the top of the religious hierarchy, like Clavius, could hardly be expected to truthfully acknowledge their non-Christian sources in public.

Even in countries like Britain, where there was no Inquisition, the slightest theological deviance was severely punished, a striking example being the way someone as prominent as Isaac Newton dared not publicly articulate his passionate theological deviance for 50 years, until his death. (His successor was quickly expelled for that reason; and Newton's work on the Bible remains suppressed to this day.) The only ones who could be acknowledged were the (early) "Greeks", regarded since Eusebius as the theologically-correct predecessors of Christianity. (Since they preceded Christianity, there was no possibility of any con ict.)

This belief that all knowledge in the world was due to Whites was further encouraged by the 15th c. in ux of Byzantine Greek sources, which set the basic tone of the narrative, despite the later-day rejection of these sources by some historians. Therefore, racist historians could, in a perverse sense, claim to be accurately describing the beliefs (i.e., myths and enforced superstitions) then prevailing in Europe: that all useful knowledge had to be attributed to either Christians in Europe or to their White predecessors in the "Greeks".

This peculiar form of history writing, or rather *en masse* fabrication, was supported by the writings and speeches of a huge standing army of priests maintained over the centuries by the church to promote its "soft" power.

One should not underestimate the force of this racist history. Even those who were not directly priests could hardly hope to escape the pervasive in uence of indoctrination via church-in uenced education. Bertrand Russell, for example, clearly freed himself from many aspects of religious indoctrination, but accepted uncritically the received historical narrative, and this directly in uenced his philosophy of mathematics. That philosophy, as we have seen, retrospectively reinforced the original racist history about White mathematics. Similarly, Newton, like Nietzsche, was bitterly opposed to the church, but could hardly escape its decisive in uence in his theoretical formulation of both physics and calculus.

Therefore, also, racist history, resulting from the genocidal church politics, ought not to be confounded with some implicit and subconscious "Eurocentrism" in colonial history. It is remarkable that exactly those historians who are blind to the decisive and long-term role of religious fanaticism in the development of mainstream Western history, since Eusebius, are the one's who rush to characterize opposition to it as being due to religious fanaticism. Perhaps adherence to this stereotype has something to do with the sources of their livelihood!

In view of these numerous farcical claims of transmission, motivated by the need to defend genocide and slavery with racist history, and supported by a blatant double standard of evidence, and institutionalized indoctrination, it is necessary to reconsider what ought to be proper standards of evidence for transmission. To begin with, I point out that possible contact, and precedence are *not* adequate grounds to establish transmission, as has been widely (but implicitly) assumed by historians so far. Thus, the naive meaning one tends to attach to the term "precedence" is quite different from the operational meaning it acquires in the context of a racist appropriation of credit for all knowledge in the name of "Greeks", using dubious chronology.

Under the circumstances, a more credible way to establish precedence is through **epistemological continuity**. For example, until just before the time of Alexander, the Greeks regarded any kind of scientific thought as an offence punishable by death, as is clear from the trials of Socrates and Anaxagoras, and the subsequent ight of Aristotle. How could any scientific thought have been produced in such an atmosphere? *Why* would it have been produced—to what economic processes did it relate? The absence of serious answers to these questions tends to corroborate that Aristotle was at best merely a translator of books looted during Alexander's conquests (and at worst merely a brand name used by later translators or scribes to increase the prices of their products). This point of view would also clearly explain why all the rest of "Hellenic" science could grow only on African soil!

Epistemological continuity relates also to non-textual sources. For example, the crudeness of the Greek and Roman calendar (compared to, say, the Indian calendar of the same period), and the related difficulty Greeks and Romans had in dealing with elementary arithmetic, is just not compatible with the astronomical knowledge attributed by historians to Claudius Ptolemy, suggesting a lack of epistemological continuity. This discontinuity suggests that the Arabic *al Majest* contains material unknown to Ptolemy that could well have been incomprehensible to all astronomers in the Roman empire. If we somehow deny the accretive nature of the scientific text in question, lack of epistemological continuity then suggests that Greeks and Romans had not even absorbed the knowledge that they were translating from Egyptian in Alexandria, just as 17th c. Europe had not quite absorbed the Indian knowledge translated by Jesuit priests in the 16th c. CE.

Epistemological continuity certainly relates also to social processes. Thus, consider the epicyclic model of planetary motion. Given the long-term Indian involvement with the calendar, related to agriculture, given the correlated development of mathematics, it is easy to understand how and why a sophisticated planetary model developed in India. But what practical requirements were there for such a model to develop in "Greece"? If there was a compelling social requirement, then a string of other persons prior to Ptolemy should have attempted to build planetary models. Where is the evidence for this? If there was indeed such a lost astronomical effort predating Ptolemy, why did not the corresponding arithmetic develop side by side? Thus epistemological continuity suggests that the "Ptolemaic" epicyclic model was obtained through transmissions from India, instead of the other way around as Western historians have maintained. (What India seems to have got in return was astrology, for Varāhamihīra, or the text attributed to him, represents an epistemically discontinuous boundary for astrology in India.)

Similarly, the table of chords in the *Almagest* needs a mysterious technique of square-root extraction, though the text itself asserts the difficulty in multiplication and division, suggesting an awareness of the Algorismus and a continuity with the early Arabic Zij of the 9th c.

CE. Thus, Ptolemy's table of chords should be situated against (a) transmissions arising from access to the long historical background of similar astronomical efforts in Egypt, combined with (b) transmissions from India (mainly to Arabs, from 8th c. onwards, but also possibly to Alexandria from Ptolemy II onwards), (c) the contribution of later-day Arabic authors, closer to the actual manuscript. This suggests that the *Almagest* is an accretive text, and that the actual "Greek" or "Hellenic" contribution to the present form of the *Almagest* is perhaps limited to the name "Ptolemy" (which name perhaps only indicated a tradition dating back to Ptolemaic times)! It is similarly absurd to speak of "Euclid's" division algorithm: for even supposing that this Euclid of Alexandria existed, he clearly lacked the concept of algorithm for multiplication, and division, which is impossible with Greek or Roman numerical notation adapted to the abacus.

Setting aside the mythical Euclids and Ptolemies, real contact could and often did result in the immediate transmission of knowledge in some cases, as in the case of the Greeks who acquired control of the books in the Great Library of Alexandria, or Vasco da Gama who carried back the *kamāl*. However, in all cases, there was a difficulty in understanding that newly acquired knowledge, an epistemological barrier, because of the underlying philosophical and cultural differences in the approach to that knowledge. (As a trivial example, Vasco accustomed to a linear scale found it difficult to understand a harmonic scale, etc.) Sometimes the underlying philosophical differences could be so large that the epistemological barriers were not scaled. Thus, it might also happen that despite prolonged contact there is no transmission of knowledge, and I examine such cases of contact *without* transmission (or with greatly delayed transmission) because of epistemological barriers. As a consequence of these epistemological barriers, knowledge currently regarded as superior was then seen as inferior and suspect.

Examples are the delayed acceptance of the *Elements*, by a stream of Indian tradition, because it was seen as having no practical value. Similarly, it took some five centuries for the algorismus to be accepted in Europe, because it was epistemologically discontinuous with the European tradition of the abacus. As we saw, even the *kamāl* was not fully understood, and Indian techniques of determining longitude at sea could not be incorporated by European navigators, just because they were incompatible with the (incorrect) prevalent European beliefs about the size of the earth. As we shall see, another example is the case of the calculus itself, which was not properly understood in Europe for centuries. Thus, epistemological discontinuities are as critical indicators of the transmission of knowledge as epistemological continuities are indicators of its indigenous creation. The long background of the calculus and its clear understanding in India show that the calculus was indigenous in origin, while the sudden arrival of the calculus and the difficulty in understanding it in Europe are indicators of its transmission.

Accordingly, I propose instead a legal standard of evidence for transmissions based on (a) opportunity, (b) motivation, (c) circumstantial and (d) documentary evidence, and

(e) epistemological evidence. The first three are standard features of current law, regarded as providing proof beyond reasonable doubt, adequate to convict a person of murder. The last point may be explained once again as follows by means of a mundane example: if two students give remarkably similar answers in an examination, where each had the opportunity to copy from the other, and if one of them cannot clearly explain his answers, he is the one likely to have copied from the other, i.e., there has been a transmission of information from one to the other. Furthermore, one can look at the background of past performance—the student who has a poorer performance in the past is more likely to have copied from the student who has a better record of past performance, than the other way around. The epistemological test can also be successfully applied to the ironically analogous transmission from India to Europe, in recent times, of this very thesis that the calculus was transmitted from India to Europe!

Apart from the far greater importance that needs to be attached to the epistemological evidence, I suggest also the downgrading of the value of documentary evidence which has been overrated because history was mostly done by historians within a scriptural and clerical tradition. Documentary evidence is not particularly reliable since it is very easy to manipulate. Thus, (a) there is the long tradition of forging documents, only some of which, like the Award of Constantine (on which the Vatican is founded), could be clearly established as forgeries. Likewise, there is (b) ample evidence for the way key documents (like Newton's writings summarizing his 50-year investigation into forgery in the scriptures themselves) have been deliberately suppressed for centuries, with a view to promote a deliberately false account of history. (For example, Gibbon tried to obtain those documents, but could not write Newton's real history because of the lack of documentary evidence.) Today, anyone familiar with the functioning of clerkdom, in a bureaucracy for example, will be well aware of how documentary evidence is routinely manipulated: numerous false documents are created everyday, and important documents are deliberately misplaced or "lost" or destroyed with the greatest of ease. The vast army of church clerics have been very adept at such manipulation. Thus, refusing to believe something due to lack of documentary evidence, sometimes becomes as farcical as refusing to convict a murderer or thief on the grounds that there is no signed confession of murder or theft. The interesting thing here is that the forgerers who manipulated the documentary evidence, also being clerics, implicitly relied on the same scriptural standard of evidence later used by the historian—namely that individual remarks and isolated paragraphs could be very weighty, as in the case of the passage about "Euclid" in the Monacensis text.

Thus, the value of both absence and presence of documentary evidence needs to be seriously downgraded. Epistemological continuities and discontinuities are far more important indicators for extracting the truth from such long-standing and systematic attempts to falsify history.

I
INTRODUCTION

Informationtransmission and information sharing are virtually synonymous with cultural interactions, so some conceptual framework is needed for information exchange—one needs a general model of information exchange. But despite the great interest in transmissions and diffusions, there is no explicitly stated theory of how information is exchanged between cultures in contact; and I do not find adequate the standard model of information transmission that is often implicitly assumed in most histories of mathematics and astronomy—this includes those non-Western histories of mathematics that have simply tried to reverse the direction of information ows, wherever possible, without seriously challenging the premises of the underlying model of information exchange. So I will begin by pulling out into the open the premises of this underlying model. I will also indicate alternative principles of information sharing and information ow that will help to understand, in a different way, what "interaction" means.

II
MODELS OF INFORMATION TRANSMISSION

The Direction of Information Transfer

The idea that information was transmitted, either unidirectionally, or more probably, from winners to losers in a military engagement sounds silly the moment it is explicitly articulated; it is as contrary to observation as Keynes's "trickle-down" model of development, which enables surplus to be sucked upwards. To have systematic warfare and military conquest (as distinct from feuds between neighbours) the economic conditions must be there for it: say, a long period of stable population growth followed by a sudden contraction in the available surplus, forcing people to change the lifestyle to which they had become accustomed.[1] There may be individual adventurers, but people usually do not run risks collectively except when there are compelling economic reasons to do so.

Hence, before capitalism, the aggressor would, often enough, have been the one with a *lower* state of development of productive forces, hence of science and culture.[2] At the start of the colonial project, it is manifest that Europe was extremely poor and technologically backward compared to India and China, say, or the Incas and the Mayas. Europe lacked the economic wherewithal to produce knowledge: whatever little knowledge was available in Europe was knowledge that had trickled down from the Arabs via their colonies in Europe.

Alexander s Booty of Books

Likewise, at the time of Alexander, the small Greek cities were constantly engaged in petty warfare, and lacked the economic base to support the production of knowledge. Iran (Persia), however, was an ancient civilization, and the Zoroastrian Book of Nativities records how Alexander had stolen ancient sciences from Persians for the Greeks:

> For when Alexander conquered the kingdom of Darius the king, he had all [the books] translated into the Greek language. Then he burnt all the original copies which were kept in the treasure-houses of Darius, and killed everyone whom he thought might be keeping any of them. Except that some books were saved through the protection of those who safeguarded them.[3]

Egypt was an even older civilization, with a history going back thousands of years before anything can be traced of Greek tradition. Egypt too had a huge fund of books, and the temples were also known as writing houses. Western historians have speculated that these books contained nothing more than administrative records. While temples must have kept some sort of administrative records, it is hard to believe that books kept in temples had no other function. We have seen that Herodotus records how Egyptian knowledge was being transmitted to Greeks long before Alexander. The temples were the repositories of Egyptian knowledge. The few Greek philosophers worthy of note prior to Alexander, like Pythagoras, had studied in Egypt, and Greek philosophy is so very similar to the Egyptian mysteries that the resemblance can hardly be put down to coincidence. This similarity with Egyptian mysteries is also strikingly evident in Anaxagoras' use of the doctrine of Nous. Alexander naturally would have wanted to continue this process of transmission.

Therefore, we must ask the key question that has not been asked by any Western historian so far: *where* did Alexander's loot of books go? Surely, Alexander was not so foolish as to have merely locked all these books in his treasury. Presumably, he passed on some of these books to the learned men in his kingdom. Aristotle, who happened to be Alexander's mentor, must therefore have got some of these books. This natural line of reasoning is corroborated by Strabo's statement[4] that "Aristotle... is the first man [Greek], so far as I know, to have collected books." By "man" Strabo presumably meant "Greek", for Egyptian temples certainly had libraries, while it is understandable that before Alexander there were not enough books in Greece to stock a library. Probably the major part of Alexander's booty of Persian books was initially stashed at Alexandria, perhaps because heavy loads were often more easily carried by sea. But there it stayed due to Alexander's sudden death.

The Contribution of Black Egypt: The Great Library of Alexandria

Almost all so-called Greek science comes from Alexandria, located in Egypt, where the Ptolemies had collected a library which by various accounts ran to some half a million books.

Where did the initial stock of books come from? This question, too, seems not to have been properly addressed earlier. Naturally, such a large collection of books could hardly have been produced in Greece, or by Greeks, and then transported to Alexandria—in any case it is a bit hard to imagine that Alexander's army was armed mostly with books, especially since there was no library in Greece before Aristotle! Even if we suppose, by a considerable stretch of the imagination, that as many as 10,000 books were brought to Alexandria by visitors from outside, including traders from places such as Persia, India, etc., the initial stock of these books would still have been very large, and of non-Greek origin.

The Greeks could hardly have written these books after occupying Alexandria. For, within 50 years of the death of Ptolemy I, Callimachus, the second librarian of Alexandria had already reportedly built a huge catalogue (*pinake*) of the holdings of the library. Since Ptolemy I spent most of his time fighting to establish his empire, and since his 4000 odd army of mostly illiterate people could hardly have been expected to have produced the books in question, most of the books had to have been sourced from outside Greece.

Thus there are two mysteries: (a) the fate of Alexander's loot of books, and (b) the source of the initial stock of books in the Great Library of Alexandria. Both mysteries have a simple common resolution. The Callimachus catalogue itself corroborates that there was a mass of books earlier lying uncatalogued as would be expected if the books had arrived as part of Alexander's loot.

Unlike the tiny Greek city states, the Egyptian economy, for example, was strong enough to support this sort of production of papyri, and Egypt had ourished long enough for the production of such a large mass of papyri.

That is, the major part of the initial stock of books in the Alexandrian library hence had to consist of Egyptian and Persian works, incorporating knowledge developed and accumulated over thousands of years. Some of these books were subsequently translated into the Greek language. Famous Greek names such as Eratosthenes were librarians of this library, and therefore had complete and unhindered access to its works. One can, therefore, well understand how they acquired a reputation for knowledgeability. According to various accounts, by a decree of Ptolemy II, the library of Alexandria also included all books brought into Alexandria by travellers, which books were forcibly confiscated, and copies returned to owners, while the originals remained in the library.

Science as a Criminal Offence in Athens

The absence of any Greek science prior to the time of Alexander is confirmed by Greek sources describing the trial of Socrates, as in Plato's *Apology*.[5] During this trial Socrates was accused of engaging in speculations about bodies like the moon, and he responded that his accusers had mixed him up with Anaxagoras (who had earlier been imprisoned, but had ed), and that he kept aloof from such speculations, and regarded the sun and moon as gods.

> . . . the simple truth is, O Athenians, that I have nothing to do with physical speculations.

> [Socrates:] "Do you [Meletus] mean that I do not believe in the godhead of the sun or moon, like other men?"
>
> [Meletus:] "I assure you, judges, that he does not: for he says that the sun is stone, and the moon earth."
>
> [Socrates:] "Friend Meletus, you think that you are accusing Anaxagoras, and you have but a bad opinion of the judges, if you fancy them illiterate to such a degree. . . "[6]

Military Conquests and Information Transmission

But even these very same Greeks who then regarded the study of the natural sciences as a serious offence punishable with death, regarded Alexander's Macedonians as barbarians. Considering that Socrates was a contemporary of Plato, and Plato was a contemporary of Aristotle, we find the situation radically changed within just a few years, with Aristotle claiming to be an author of several books on scientific subjects ranging from physics to biology. This claim to have produced numerous authoritative works from scratch is scarcely credible. But even if we accept a greatly watered down version of the claim, the only explanation for it is an in ux of knowledge from outside: not only did Aristotle's scholarship *follow* Alexander's military conquests, but it was causally dependent upon the in ux of knowledge brought in by the military conquest. Aristotle himself would not have had a great deal of time even to translate these books, for after Alexander died, the Athenians impelled by their anti-scientific ways chased Aristotle out of Athens like Anaxagoras.[7]

In these cases, certainly, information transmission related to military conquests very clearly took place in the reverse direction; the military victor learnt from the vanquished, though not presumably in the direct way that Rama learnt from Ravana, or Yudhisthira from Bhishma. Allowing for the possibility of some cases where military victory might have depended upon technological superiority, there is no doubt that the superior techniques would have been kept a closely guarded secret, drastically inhibiting information ows from the victor to the vanquished.

Studying the enemy would also have made sound military and diplomatic sense to those striving for conquest. Concrete examples are the people deputed by Alexander to gather knowledge for Aristotle, al-Bīrūnī deputed by Mahmūd of Ghazni to gather knowledge about India, and Adelard of Bath sent as a spy disguised as a Muslim student during the Crusades. While the ruled could maintain a distance, as in Egypt (or, for that matter, in any modern organization), to rule successfully the rulers had to learn about the foreign populace over whom they ruled. In short, those seeking to systematically extract surplus from foreign sources must first systematically extract information. This was true also of the European colonists, in India and China, and it continues to be true today as a general proposition.

Therefore, also, aggression has often led to the transmission of information mainly *towards* the aggressor.

In fact, this process of information transmission would normally have begun long before any actual military aggression. Thus, the Greeks were trading with Egypt for centuries before Alexander. Arabs were trading with India long before Ghazni, and the Europeans traded with India for two hundred and fifty years before the onset of colonialism.

Of course, following a military aggression knowledge may also scatter in other directions. For example, as al Bīrūnī recounts, many pundits ran away with their books, to escape from Mahmūd of Ghazni, to places "where our hand cannot reach them". Similarly, as recounted by the *Tabakkat-i-Nasirī*, some Buddhist monks escaped from Nalanda to Tibet to escape from the destruction of the University of Nalanda by Muhammad-i-Bakhtiyar-i-Khalji in 1198—which is why texts like those of Diṅnāga, cited earlier, are available only in Tibetan. Similarly, numerous Byzantine Greek texts came into Europe after the fall of Istanbul to Muhammad the Conqueror.

The only thing wrong with this simple and natural account of history is that it is against standard Western accounts of transmission. So let us next understand the origins of those "standard" accounts.

III
TRANSMISSION IN THE RACIST HISTORY OF SCIENCE

The above theory of knowledge often owing towards barbaric military conquerors applies very well also to the Crusades. Just as Egyptian knowledge started trickling into Greece from centuries before Alexander's military conquests, so also from the 9th c. onwards, Arab knowledge started trickling into Europe. John, Abbot of Gorze, travelled to Cordoba, at the time of Khalifa Abd-ar-Rahman III. Through him Arab knowledge came into the French Lorraine, and from there it spread to England, because King Knut preferred churchmen from the French Lorraine. Gerbert of Aurillac, took a deep interest in Arabic knowledge both before and after he became pope.

Just as Alexander's military conquests brought in a wealth of books, so also after the fall of Toledo, its famous library of Arabic books came under the control of the church. Just as the Greeks had earlier regarded scientific knowledge as profane, so also the church had a long history of burning books since the book-burning orders issued by the Roman emperors Jovian, Valens and Theodosius[8] (and the church was to continue relying on this method of book burning for centuries beyond Toledo). Just as Ptolemy I did not immediately know what to do with Alexander's booty of books, so also the church did little for the next forty years with the booty of books at Toledo. Eventually, like Ptolemy II who appointed Callimachus to tend to the Great Library, the church realized the value of the knowledge in the books, and instituted a translation factory, ca. 1125 CE, under Archbishop Raimundo, and Archdeacon Gundisalvi. Just as Ptolemy II financed the translations into Greek using the wealth of Egypt, so also the Toledo translations were financed by the gold obtained as the church's 1 4th share of the loot from the Crusades, and provided by Peter, Abbot of Cluny.

We know that hundreds of books were translated at Toledo, since a single translator, Gerard of Cremona, translated over seventy (some say 87) books from Latin to Arabic at Toledo. Naturally, the Toledo library included also later-day "copies" and reworkings of the earlier "translations" at Jundishapur and Baghdad, and these were now translated from Arabic into Latin. However, unlike the "translations" at Baghdad or at Jundishapur, many of the Toledo translations were extremely literal—since Gerard, for example, knew neither mathematics nor astronomy nor even Arabic but translated the *Elements* and the *Almagest* from Arabic to Latin! These books were the key source of learning for European universities for the next few centuries.

However, unlike Ptolemy, who was more than willing to turn into an Egyptian king, the church was not really ready to change its spots. Hence, the books at Toledo created two sorts of problems. Given that the church hype of the preceding centuries had just been upped during the Crusades, there was a strong sense of shame in having to learn from the Islamic enemy. This was articulated as follows by Daniel of Morley, one of the translators at Toledo.

> Let no one be shocked if, with reference to the creation of the world, I should invoke the testimony of pagan philosophers rather than the church fathers.... Let us then borrow from them and, with God's help and command, rob the pagan philosophers of their wisdom and eloquence. Let us take from the unfaithful so as to enrich ourselves faithfully with the spoils.[9]

As Daniel points out, his contemporaries were likely to be shocked that he should be learning from Islamic philosophers. Having first abused them with the derogatory term "pagan", he is unable even to use the correct word "learning" for this process by which Christians acquired knowledge from Arab sources, but speaks instead of "borrowing" from them, making one wonder what exactly it was that he intended to return! Finally, he justifies his actions by appealing to his god's command that Christians should rob all non-Christians, and goes on to claim that his god will assist in this robbery—as we shall see below, the church strongly encouraged this sort of thinking in papal bulls which remain valid to this day.

At this time of the Crusades, when the church was simultaneously running one of the biggest hate campaigns in history, against Islam, it felt a need to manage this sense of shame in learning from Islamic sources. This was hardly a difficult matter for the church: so, instead of representing this process as one of learning or "borrowing" or "robbing", the church misrepresented it as one of "recovery" by misrepresenting the credits for the knowledge. History was "Hellenized". The story went around that most (if not all) the useful, and secular knowledge obtained at Toledo was actually the handiwork of the early Greeks. On this story, this knowledge had merely been kept in safe custody by the Arabs. Thus, the Christians at

Toledo were only recovering their rightful Greek inheritance, albeit from Arabic books. All that the Muslims had done was to preserve this Greek inheritance for the Europeans.

There was nothing very new in such a project of fabricating a false history through abject lies—Eusebius commenced this tradition long ago. (And to this day, people believe the concocted stories of Christian martyrs in the Roman empire, when, in fact, as Gibbon[10] showed long ago, these stories were concoctions.)

Now Arabs did attribute various texts to Aristotle. For example, a key text translated at the Baghdad House of Wisdom was the *Uthulijiyya Aristutelis*, otherwise known as the *Theology of Aristotle*, "translated" by the philosopher al Kindi with the help of a Syrian Christian intermediary Abd'ul Masih ibn Na'imah al-Himsi. Another key religious work of Aristotle translated into Arabic was the *Kalam fi l mahd al-khair* ("The Theology of the Pure Good"), and came to be known as the "theology of Aristotle". However, these attributions by Arabs are today believed to be incorrect. The former text (today called the *Theologia*) is believed to be a paraphrase of the *Enneads* of Plotinus, together with the commentary of Porphyry. The latter text is today believed to be a paraphrase of thirty-two propositions of Proclus' *Elements of Theology*.[11]

Thus, there is a fundamental difference between the meaning of "Aristotle" in Arabic and in Latin. While it is natural enough to believe that the *falāsifā* that developed at the Baghdad Bayt al Hikma, under the in uence of Jundishapur, was deeply in uenced by "Neoplatonists", Aristotle's reputation among Arab scholars derived from just those aspects of *theology* attributed to him. In fact, the term "Aristotle" was used in Arabic more or less as a generic term for the "Shaykh al-Yunani" or "the Greek sage"—the way Pythagoreans used the name "Pythagoras". Its correspondence with the historical Aristotle is deeply problematic.

The difficulty, however, is that if some attributions to Aristotle are accepted as incorrect, others too may be. So, it is not as if European scholars were unaware of the possibility of wrong attributions—it is just that they have appealed to it selectively. So what is the criterion used to decide which attributions are correct and which not? The criterion that actually seems to have been used is the following.

Since these later Greek "Neoplatonists" were viciously persecuted by the Christians for over a century, and ultimately driven out of the Roman empire, it is clear that allowing such attributions would spoil the theologically correct image of Aristotle that has been built up in the West. In brief, the attributions have been corrected not to ensure historical accuracy, but to ensure the theological correctness of "Aristotle".

Another striking example of how theological correctness was critical to attribution is provided by the case of "Euclid". As Heath points out,[12]

> All our Greek texts of the *Elements* up to a century ago depended upon manuscripts containing Theon's recension of the work; these manuscripts purport, in

> their titles, to be either "from the edition of Theon"... or "from the lectures of Theon".

Further, Theon claimed to have *himself* proved certain results in the *Elements*. This is also quite in line with Proclus' exposition of the *Elements* as a "Neoplatonic" mystery text, which refutes point by point the revised Christian doctrine of the 4th c. CE. This is also corroborated by the archaeological evidence. Thus, there are but three Alexandrian Greek papyri which relate to anything that could be called scientific, and these three relate to geometry, but do not correspond to the received text of the *Elements*. This suggests that no standardized text of the *Elements* existed until the 4th c. CE—contrary to what one would expect had there really been a person like Euclid who had prepared a definitive geometry text at an early date. However, attributing the authorship of the *Elements* to Theon or to his daughter Hypatia would have been theologically incorrect, considering that Hypatia, for example, was raped and brutally murdered in a church by a mob organized by a Christian saint.

The attribution of the text to an unknown early Greek called "Euclid" was also very convenient for the process of Christianizing the text by reinterpreting it in a theologically correct way—unlike say Proclus, there were no known facts at all about Euclid, and thus no facts that could inconveniently get in the way. We saw how this happened: Proclus idea of the *Elements* as a mystery text was replaced by the idea of the *Elements* as a source of power through irrefragable argument. Ultimately this was secularized through the formalization of mathematics. This philosophy retrospectively acted back on the primary sources: all these available "Theonine" manuscripts were disregarded as inconsistent with the reinterpretation.

So even the sources were changed to align the sources to the re-interpretation, and the new definitive source of the *Elements* in the 20th c. CE was regarded as a single manuscript of uncertain ancestry just because it did not contain this statement about Theon, and supported the re-interpreted version, and was *hence* regarded as authentic! The average person today is unlikely to ever come in contact with a "Theonine" text. This shows the extent of the bias in favour of the theologically correct and how readily it can override and even replace all evidence to the contrary.

Ibn Abdun put the matter succinctly:

> they translate books of science and attribute authorship to their coreligionists.[13]

These were not the only known cases of false attribution. The *Fihrist* prepared by al Nadeem shows that by the late 10th c. a number of texts circulating in the Baghdad book bazaar were falsely attributed to various early authors to increase their market value. This seems to have been a fairly common technique, since ancient texts were regarded as more valuable.

Exactly how common these techniques were in those days, is brought out humorously by Adelard of Bath.[14]

> The present generation has this ingrained weakness, that it thinks that nothing discovered by the moderns is worthy to be received—the result of this is that if I wanted to publish anything of my own invention I should attribute it to someone else, and say, "Someone else said this, not I." Therefore (that I may not wholly be robbed of a hearing) it was a certain great man that discovered all my ideas, not I.

The truth underlying the humour is that deliberately false attributions could also have been made for reasons that involved other than religious, or pecuniary considerations.

Under these circumstances, where deliberately false attributions were common, it is not clear by what logic the veracity of an attribution is to be determined: apart from theological correctness, there seems no other reason why one should accept the veracity of the Peyrard manuscript on which the Heiberg "Euclid" is based.

More generally, this criterion of "theological correctness" led to the preferential attribution of various texts to *early* Greeks. The reason for this was that, unlike the later Alexandrian Greeks—like Porphyry, Hypatia, Proclus, etc.—who were marked opponents of the Christian church, there was no possibility of a con ict between the church and the early "Greeks" who preceded Christianity. Hence, as noted long ago by Eusebius, the early "Greeks" were theologically correct—one of the very few non-Christian people who could be so called, for the church waged war on all others.

Finally, the traditional Arab notion of attribution was quite different. For example, consider the case of the *Rahmani* of Kunhi Kunhi Maestry (mentioned in the previous chapter). This is attributed to the legendary Arab navigator Ibn Majid. Now certainly Kunhi Kunhi Maestry is well aware that many (or most) of the entries in the *Rahmani* are his own, or those of his father, and that he borrowed a British sailing manual from the Kavaratti library and copied out portions of it to include in his *Rahmani*. This is most natural—any book of practical value is bound to be accretive and constantly updated, else it loses its practical value. The knowledge in the book is, for him, a matter of life and death, the attribution is not, and is merely customary. So, the attribution to Ibn Majid does not mean for him that each and every sentence (or even the majority of sentences) in the book were written by Ibn Majid. It simply means that he heard from his father, who heard from *his* father, that this tradition of navigation had come down from the time of Ibn Majid. So the attribution to Ibn Majid is merely a part of folklore about the origins of this knowledge; it is an act of humility.

If we *do* interpret this attribution to mean that all or most of the sentences in the text were written by Ibn Majid, and that this was transmitted by blindly copying out that earlier work, we run into absurd anachronisms—that Ibn Majid had anticipated British sailing manuals. We would then be obliged to make the further absurd assertion that Ibn Majid's knowledge was somehow transmitted to British sailing manuals. But this is exactly what happened in the case of Greek texts, since Western scholars, for example, suppose that the historical Aristotle wrote most of the sentences in the *Organon* or *Physics*, and that for the next 1400 years, scribes kept copying out those sentences, and lacked the creative capacity to contribute to or update those texts. In fact, such a hypothesis is known to be false even for religious texts like the Bible. Indian texts of the Bible were so different from the European texts that in 1599, Archbishop Menezes of Goa called a meeting at Udayamperoor (Synod of Diamper) where he burnt copies of the earliest Aramaic Bible in India because they did not agree with his version of the Bible, and were regarded as being beyond repair.

Kunhi Kunhi's example generalizes quite easily. For scientific texts and texts that are practically useful, for the Arabs, the knowledge was important, attributions were not. This sort of thing can also be commonly observed to be the case with contemporary scientists and engineers: their interest often is in the way a problem is solved, not in its historical source. Hence, knowledge from one source could be casually attributed to another source. (Whittaker, for example, in his book on *Calculus of Observations*, corrects numerous historically incorrect attributions in numerical analysis.) Indian numerals are known to this day as "Arabic numerals", as part of European folklore. Another common example is provided by the *Hazār Afsāney*, a Pahlavi text from Jundishapur translated into Arabic at the Baghdad Bayt al Hikma: this Persian book is today known as the *Arabian Nights*, since this was the European folklore about it, and the interest is in the stories, and not the attributions. In particular, Arabs would not have hesitated to add the latest Indian knowledge to an astronomical text coming from Ptolemaic times—the historical accuracy of attributions in scientific texts was not a key concern. Western historians however stuck to the premise that each text was authored by a single individual, and that it had come down to the present time by a method of copying by scribes—whose key concern was to ensure accuracy of reproduction and not the propagation of useful knowledge—an assumption we have seen they well knew to be false.

Therefore, the thesis that early Greeks anticipated most of the knowledge of the 10th c., which was obtained by transmission from the Greeks, is an *a-priori* absurd thesis, contrary to elementary common sense.

This thesis of transmission by blind copying is certainly known to be factually false in the case of the "translations" carried out at the Baghdad House of Wisdom. Anyone with the slightest understanding of the Baghdad Bayt al Hikma knows that Khalifa al Ma'mūn's interest in starting it was to promote the Mu'tazilah or the *aql-ī-kalām*. The one thing that these people most utterly despised was blind copying or *naql*: they saw *aql* as the antonym of *naql* and accused the Islamic traditionalists of *naql*.[15] This is borne out by the fact that while Indian texts were "translated" at the Baghdad House of Wisdom, no single scientific or mathematical Indian text was literally translated or even paraphrased. Because the scholars at Baghdad *processed* knowledge, instead of *translating* individual books, the results were not invertible: from al Khwarizmi's work there is no way one can reconstruct any specific mathematical Indian text such as that of Brahmagupta or Mahāvī ra or Lalla. There is no reason to suppose that the situation was any different with Greek or Pahlavi texts. The "translations" at Baghdad cannot be compared with the translations at Toledo.

This aspect of the Mu'tazilah was certainly known to early medieval scholars like Adelard of Bath. Thus, Western historians of science have attributed creativity in a racist way they knew to be false. They supposed that the Greek authors were capable of making creative contributions while Arab authors were not.

The general theory of selection effects with mundane time tells us that it is perfectly possible to suppose to the contrary that Greeks had zero creativity in sciences, and that they merely translated books from Egypt, Babylon, and Persia, at Alexandria and preserved them for eventual re-translation back into Pahlavi and then Arabic, thus returning to Iranians and Arabs the heritage they had earlier looted, without, however, adding anything new to it—any more than Toledan translations added anything to the Arabic texts. Not being a racist, I would be ready to grant that quite probably the Greeks added something to what they learnt from others. But, in the absence of hard evidence of any specific original Greek contribution in science or mathematics, from sources close to their times, it is not possible to say what exactly their specific contribution was!

Secondly, the remarkable similarity between the conjectured knowledge of the early Greeks and the knowledge in 11th c. Arabic books at Toledo was tautological—since the conjectures about early Greek knowledge were entirely based on 11th c. CE Arabic books. However, there is more to the matter than meets the eye. The Arabic books at Toledo obviously incorporated and updated the knowledge that had accumulated earlier at Jundishapur and Baghdad. But that is well known to have included knowledge from various parts of the world—it certainly included Indian knowledge.

It is well known that Khusrau I (Noshirvan), following the earlier example of Alexandria, possibly under the in uence of the Alexandrian diaspora ejected from the Roman empire by Justinian, sent the famous physician Burzoe to India to fetch Sanskrit books to be translated into Pahlavi. Indian texts like *Pañcatantra* stories were first translated into Pahlavi as *Kelileh va Demneh*. Indian astronomy texts, too were imported and translated as the *zij-i-Shahryar*. Burzoe also brought back the game of chess.[16] Both the *Pañcatantra* and chess were regarded as practically useful for education, especially of kings, the one to teach them justice, and the other to teach them strategy. It is also well known that books from Jundishapur were translated at Baghdad, where, apart from Indian texts on mathematics and astronomy, the *Pañcatantra* was also translated from Pahlavi to Arabic.

Therefore, it is hardly a matter of surprise that there is much similarity between Indian knowledge, and knowledge that has been attributed to the early Greeks based on late Arabic texts: for example, the astronomical model attributed to "Ptolemy" is remarkably similar to Indian astronomical models, "Aristotle's" theory of action by contact, using aether (=sky=*ākāśa*) is as similar to the Nyāya theory as his syllogisms are to Nyāya syllogisms, etc. The natural thing would be to take this as evidence that authorship of this material has been wrongly attributed to the early Greeks, about whose alleged texts nothing is independently known from sources close to their time—since those books were all systematically burnt as heretical on orders of various Roman Christian kings. However, these similarities between the conjectured knowledge of the early Greeks and the actual knowledge in various other parts of the world was explained as arising not due to wrong attribution to the early Greeks but due to transmission of knowledge from the Greeks to other parts of the world. Speculation was piled on speculation, hypothesis on hypothesis to produce a miraculous and theologically correct end result, as is the norm in theology. Thus, by means of conjectured attribution to the early Greeks and claims of transmission, the entire knowledge of the world up to the 10th c. was appropriated to the West.

So far as I know, no one ever clearly articulated the mechanism of transmission by which the conjectured knowledge of the early Greeks was transmitted to the texts from which its existence was subsequently inferred. These were vaguely thought to have been due to the military conquests of Alexander—which is less plausible then the belief that the Crusaders spread European knowledge among the Arabs!

What, then, is the evidence for transmission? How do we decide between the following two possible ways to explain the similarity between Indian knowledge and alleged early Greek texts? (a) Indian knowledge transmitted to Arab texts was wrongly attributed to Greeks. (b) Knowledge in the conjectured early Greek texts was transmitted to India.

No Western scholars has apparently ever bothered to raise or answer this question. All that Western scholars did was to set up a competitive chronology for their heroes—real or imagined—who authored these conjectured Greek texts. Now chronology was a matter in which Western scholars had long experience. Thus, Augustine asserted long ago that the long time span of the cosmos (as in the *Viṣnu Purāṇa*, or similar beliefs among Alexandrian "Neoplatonists" or early Christians like Origen) was false, since "reckoning by the sacred writings, we find that not 6000 years have yet passed".[17] Bishop Ussher, in the 17th c. CE crowned the centuries of theological effort in chronology by putting the date of creation at −4004 CE, on Sunday, 23 October. With exquisite scholarship, the time of creation was further sharpened to 9 a.m. on that date by Sir John Lightfoot, Vice-Chancellor of the University of Cambridge.[18]

Therefore, all that Western scholars have had to offer by way of evidence for transmission is an elaborate chronology attached to a variety of Greek names—corresponding to persons both real and imagined. In the tradition of theological scholarship, this entire chronology is based on stray remarks here and there. We have seen as an example, how a chronology was attached to "Euclid", first by supposing "Euclid" to be the same as Euclid of Megara, and then using an inauthentic remark in the Monacensis text of Proclus. It has taken centuries to come round to considering questions about the authenticity of the identification or the remark. On the strength of this semi-mythical chronology, "Claudius Ptolemy" came prior to Āryabhaṭa and the *Sūrya Siddhānta*, so that the transmission must have taken place from

Ptolemy to India—although despite the alleged transmission, Āryabhaṭa seems unaware of the anticipation of his theory of the movement of the earth by Ptolemy, and also of the anticipation of future objections to his theory by Varāhamihīra and others, so succinctly summarized by the clairvoyant Claudius!

Setting aside, for the moment, the question of the *veracity* of the evidence offered, let us examine the *principle* of evidence used here. The principle is that if two texts articulate similar propositions then transmission must have taken place from the earlier author to the later.

The formula that "precedence + similarity = transmission" is quite acceptable (provided the precedence is real, and not merely conjectured from a later text). However, it is worth noting that this formula has not been applied consistently by Western scholars. Thus, consider the case of Copernicus. It is well known that his theory has a remarkable similarity to the earlier work of Ibn as Shātir of Damask. However, in this case the noted scholar Owen Gingerich has maintained that Copernicus might have discovered his work independently:

> Ibn al-Shatir's forgotten model was rediscovered in the late 1950's by E. S. Kennedy... In a preliminary work, the *Commentariolus*, he [Copernicus] employed an arrangement equivalent to Ibn al-Shatir's. Later, in *De revolutionibus*, he reverted to the use of eccentric orbits, adopting a model that was the sun-centered equivalent of the one developed at Maragha.
>
> Could Copernicus have been in uenced by the Maragha astronomers or by Ibn al-Shatir? ...some of the al-Tusi material is known to have reached Rome in the 15th century (many Greek manuscripts were carried west after the fall of Constantinople in 1453), but there is no evidence that Copernicus ever saw it.... I personally believe he could have invented the method independently.[19]

Now it is not impossible for Copernicus to have independently reinvented the model, it is just that it is exceedingly improbable that this independent rediscovery happened in Europe at just the time when the model could have been transmitted. We are, in fact, being asked to believe in a miracle, and we shall see a series of such miracles later on—miracles are all that are left to support the Western history of science.

Note also how the standard of evidence for transmission has changed. There is precedence, there is similarity (in fact, the two models are identical). Neither precedence nor similarity is doubted, but transmission is. Why? Because now there is a demand for new sorts of evidence of transmission. We must produce a manuscript in a language Copernicus could understand, we must produce proof that Copernicus saw it; only then can it be believed that transmission has been established.

In the many centuries, since Toledo, that Western historians have been talking of transmission from the Greeks, who ever produced a Sanskrit manuscript of Ptolemy? Who ever proved that Āryabhaṭa had seen such a Sanskrit manuscript? Yet every Western reference work on the subject asserts that Indian astronomy is transmitted from the Greeks. So is it the case that these reference works are all out of date, and that the standard of evidence for transmission has now changed? Does Owen Gingerich now deny transmission *from* the Greeks on the grounds that there is no evidence? Not at all; in the very same article he sticks to the entire fairy tale about transmission from the Greeks. So, it is not so much that the standards of evidence have changed, but that there are (even as of today) two simultaneous standards of evidence for transmission. One for transmission *to* the West, and another for purported transmission *from* the West. Not only is the judge biased, the very rules of evidence are biased!

As another example, consider the case of Ashoka's rock edicts, where he proclaimed the victory of Dhamma in the missions he had sent to various kings, including Ptolemy II of Alexandria.[20] In this case, the veracity of this rather solid piece of archaeological evidence coming from some 33 sites scattered across India, Pakistan, and Afghanistan is denied as follows by Rhys Davids: "It is quite likely that the Greek kings are only thrown in by way of makeweight as it were and that no emissaries had been actually sent there at all." (In fact, one of the Ashokan rock edicts found in Kandahar in Afghanistan is in Greek.) If this sort of archaeological evidence can be thus denied, and if the same standard is applied to Western history, forget about the "Greeks", it is unlikely that there is anything at all in Western history, even a single event, for which there is any evidence that can be regarded as reliable. But, of course, that was not Rhys Davids' intention: instead of changing all of Western history he wanted to preserve it by ordaining different rules of evidence for different people.

Despite Rhys-Davids' vehement denials, the fact is that there is this marked similarity between Indian thought about cosmology and that of "Neoplatonists", and early Christians like Origen, who believed in something very similar to *karma-saṃskāra*. This is suggestive of transmission. Certainly there is ample evidence that various "Neoplatonists" and early Christians in Alexandria well knew about Indian thought (Augustine even objected to Porphyry learning about the "mores and disciplines of Inde") and there was large scale commerce with India, and Indians even attended the lectures of Dio Chrysostom. So, there is similarity, there is precedence, there is ample opportunity for transmission over centuries. However, this transmission has been denied, mainly to deny Indian in uence on early Christianity. The denial has been done similarly by sharpening the standards of evidence to an unrealistic level.[21]

So, similarity and precedence do not always establish transmission. Whether or not they establish transmission depends upon the direction of transfer. Thus, in practice, there are two standards of evidence for transmission: an ultra-lax standard for transmission from Greeks, and an ultra-strict standard for transmission to the West.

Now why should there be this asymmetry? Why should there be two standards of evidence? We need to understand the deep seated religious motivations behind this.

The Doctrine of Christian Discovery

In support of the West's physical claim to the whole world, the Western history of science sought to establish an intellectual claim to all knowledge in the world, especially all scientific knowledge. To situate this claim in its proper perspective, we need to probe a little deeper to understand a bit of the unstated logic behind colonialism. According to the religious beliefs of the colonialists, such an intellectual claim of discovery, in turn, established the colonialist's moral claim to the whole world. It was these "moral" claims that distinguished colonialism from a simple project of robbing the world by physical force.

Let us try to understand the basis of these moral claims. The United States, for example, today occupies a continent from where the original inhabitants have been genetically deleted for all practical purposes. The fact is that a real genocide has manifestly taken place. However, unlike Hitler's genocidal attempt on the Jews, which is depicted as a brutal genocide, the American genocide is celebrated as a heroic feat. There is an entire genre of literature—"Western" films and comic books—devoted to celebrating this genocide. The in uence of this genre is evident: most American children have at some time or the other played the game of "cowboys and injuns". Therefore, the same American who regards Hitler's attempted genocide of the Jews as a shameful matter, is filled with pride at the thought of the genocide of the American Indian.

The genocide received support from the US supreme court, which has provided an interesting legal justification for the occupation of the American continent. The justification rests on the celebrated 1823 case of Johnson v. McIntosh (8 Wheat., 543).[22] On behalf of a court which unanimously sided with Johnson, Chief Justice John Marshall observed that Christian European nations had assumed "ultimate dominion" over the lands of America during the "Age of Discovery". After having been "discovered" by Christians the Indians had lost "their rights to complete sovereignty, as independent nations", and only retained a right of "occupancy" in their lands.[23]

> In other words, Indian nations were subject to the ultimate authority of the first nation of Christendom to claim possession of a given region of Indian lands.[24]

Marshall argued (pp. 587–89) that although this first Christian nation was Britain, the US had succeeded to the right of "discovery", and had acquired the power of "dominion" from Britain when it became independent of Britain in 1776.

Did Britain, a Protestant nation, subscribe to the doctrine of discovery promulgated by a Catholic pope? Addressing this implicit doubt, Marshall argued that British law had in it "complete recognition" of the doctrine of discovery: "As early as 1496", Marshall continued, "her [England's] monarch granted a commission to the Cabots, to discover countries then unknown to Christian people, and to take possession of them in the name of the king of England" (Johnson, pp. 576–77). Marshall summarized the charter given to the Cabots who were authorized to take possession of lands, "notwithstanding the occupancy of the natives, who were heathens, and, at the same time, admitting the prior title of any Christian people who may have made a previous discovery" (Johnson, p. 577).

Thus, the legal justification for the occupation of the United States, and for the inhumane treatment of its original inhabitants depends upon a religious principle, the "doctrine of discovery".

What is this "doctrine of Christian discovery", which gives so much power to "discovery" by Christians. The doctrine derives from papal edicts.[25] Thus, bull Romanus Pontifex, 1453, issued by Pope Nicholas V stated:[26]

> "[W]e bestow suitable favors and special graces on those Catholic kings and princes... intrepid champions of the Christian faith... to invade, search out, capture, vanquish, and subdue all Saracens and pagans whatsoever, and other enemies of Christ wheresoever placed, and ... to reduce their persons to perpetual slavery, and to apply and appropriate... possessions, and goods, and to convert them to... their use and profit."

This was later followed by the bull *Inter Caetera* of Pope Alexander of 3 May 1493, giving the rights to conquest and subjugation of one part of the globe to Spain, and the other part to Portugal.[27] These bulls were supported by numerous citations from the Bible (e.g. Psalm 2:8-9 N.I.V.,[28] and 149:6-9 N.I.V.[29]). This doctrine was used by Portugal, Spain, and later Britain as authoritative religious and moral sanction to grab all the land in the world, and kill or enslave the original inhabitants, as a matter of religious right. The doctrine naturally enjoined the corollary of genocide and slavery as the religious duty of a good Christian, and this was what subsequently happened. Here is first-hand account by Las Casas.[30]

> And the Christians, with their horses and swords and pikes began to carry out massacres and strange cruelties against them. They attacked the towns and spared neither the children nor the aged nor pregnant women nor women in childbed, not only stabbing them and dismembering them but cutting them to pieces as if dealing with sheep in the slaughter house. They laid bets as to who, with one stroke of the sword, could split a man in two or could cut off his head or spill out his entrails with a single stroke of the pike. They took infants from their mothers' breasts, snatching them by the legs and pitching them headfirst against the crags or snatched them by the arms and threw them into the rivers, roaring with laughter and saying as the babies fell into the water, "Boil there, you offspring of the devil!"... They made some low wide gallows on which the hanged victim's feet almost touched the ground, stringing up their victims in lots of thirteen, in memory of Our Redeemer and His twelve Apostles, then set burning wood at their feet and thus burned them alive. To others they attached

> straw or wrapped their whole bodies in straw and set them afire. With still others, all those they wanted to capture alive, they cut off their hands and hung them round the victim's neck, saying, "Go now, carry the message," meaning, Take the news to the Indians who have fled to the mountains. They usually dealt with the chieftains and nobles in the following way: they made a grid of rods which they placed on forked sticks, then lashed the victims to the grid and lighted a smoldering fire underneath, so that little by little, as those captives screamed in despair and torment, their souls would leave them.

The reference to terms like "offspring of the Devil", etc. shows that these were hate crimes instigated by religious belief that those perpetrating these crimes against innocent babies would be welcomed in heaven, while it was the victims of these crimes that would go to hell! Over a thousand years earlier, Augustine had so transformed Christianity that these crimes were legitimized. Those committing these horrible crimes thought they were performing holy deeds: for were they not only initiating in a small way the endless ordeal of physical torture that their God would continue to inflict for an eternity in hell, against those innocent newborns, for the crime of being non-Christian? Such notions of morality were a natural consequence of the doctrine of hate against all non-Christians that the priests of Christianity had been systematically propagating since the days of Constantine. The killings were on such a mass scale that they soon depopulated the entire continent, eliminating most of the original inhabitants. There was, obviously, no provocation, for the American Indians had welcomed the Spanish as messengers of the gods. Las Casas explains:

> and never have the Indians in all the Indies committed any act against the Spanish Christians, until those Christians have first and many times committed countless cruel aggressions against them or against neighboring nations. For in the beginning the Indians regarded the Spaniards as angels from Heaven. Only after the Spaniards had used violence against them, killing, robbing, torturing, did the Indians ever rise up against them....

Genocide, Slavery, and the Colour of the Skin

Further, as Las Casas' account shows, it was only later on that these murdered American Indians came to be described as "Red" Indians. Genocidal religious attitudes came to be related to the colour of the skin as follows. As seen above, slavery was religiously sanctioned by the same edicts of the pope which instigated genocide. Like genocide, slavery was also seen as an economic "necessity" in the interests of the state, since people were required to produce and extract agricultural wealth from the vast lands that had been "discovered" by Europeans. However, many of the slaves imported from Africa converted to Christianity. This created a moral problem: *now* what was the moral justification for ill-treating these people? Even Europeans who were otherwise quite comfortable with genocide and slavery, as being religiously sanctioned, now experienced a sense of unease. This moral unease, since it inhibited brutality, was seen as dangerous to the imperial objectives.

The categories "White" and "non-White"—red, black, brown, yellow—helped to resolve this moral unease! By the mid-16th c. CE, Inquisitioners had started developing a system of looking for the sort of evidence of pagan attitudes that could be easily spotted visually—like the dress. The colour of the skin provided such a simple visual yardstick, which could not be easily changed like dress, and which could help to identify those who were either non-Christian or were recent converts to Christianity. Skin colour became the index of religious beliefs. In defence of genocide and slavery it was now argued that not only was it morally correct for Christians to kill, ill-treat, and enslave non-Christians, but that it was morally permissible for Whites to kill, ill-treat, and enslave non-Whites—and it is well known that these attitudes persisted late into the 20th c. CE, and even retained legal sanction in South Africa until a few years ago.

But what was the justification for the belief that Whites could ill-treat non-Whites? To support the morality of Christian violence against non-Christians, it was easy enough to find numerous citations in the Bible, as illustrated above.[31] But new justification had to be invented for these new categories based on skin colour, not mentioned in the Bible.

This created the need to fabricate racist history—to systematically denigrate all non-Western cultures, to justify the White crimes against all non-Whites, on the grounds that non-Whites were somewhat less than human. Cultural genocide was used to morally justify the physical genocide that Europeans were engaged in.

Transmissions and the Racist Narrative of Greek Origins of All Knowledge

To this end of cultural genocide, the core narrative on the agenda of racist historians was simply this: all knowledge in the world was discovered by Whites—either by Christians, or before that by the Greeks. Since the aim was to establish White intellectual ownership of all knowledge, it was an unstated assumption that the Greeks in question had to be White. Since, as we have noted, "Greek" science actually comes from Alexandria, located in Africa, the validity of this assumption was not always clear. This is the amusing reason why, as noted earlier in Chapter 1, Thomas Heath was so concerned with negating the Arabic claim that "Archimedes was a short black man".

A not-so-amusing feature of this sort of racism is the way images of these "early Greeks" adorn the latest Indian school texts produced by the NCERT.[32] While I can point out to my child that photography did not exist in those days, so the pictures are obviously fake, even this input is not available to most children exposed to these texts. So these images have a dual purpose. First, they lend reality to unreal figures—a picture of "Euclid" is visual proof of his existence. Second, they indicate what cannot be explicitly stated today—that all these names are to be associated with Caucasian features, so that no one will take Archimedes to be a woolly-haired man. Since children tend to trust the first story they hear, all these Indian children are going to grow up indoctrinated with these racist beliefs.

Claims of transmission became the key instrument in the racist historian's agenda of cultural genocide: any evidence of knowledge in the non-White world perforce had to be explained as derived from the transmission of knowledge from Whites—hence all pre-Renaissance knowledge had to have come "from the Greeks". Conversely, the earliest finding of any knowledge in the White world was to be treated as "original" and not obtained by transmission, just as Columbus and Vasco da Gama were to be decreed the "original" discoverers of lands long occupied by others.

Accordingly, I would place the commencement of this project of fabricating racist history, much earlier than Bernal. By 1785 CE the project had entered a very virulent and blatant phase, with Europeans beginning to seize control in India and China, but the process commenced at Toledo and was strongly reinforced by the developments in the Americas in 16th c. CE itself.

Pagan Sources and the Inquisition

A couple of points regarding the development of this programme of racist history need some clarification.

First of all, just as Arabic traditions made it appropriate to acknowledge a famous early source, so also European traditions, especially those prevailing during the 16th and 17th c. CE, made it *inappropriate* to acknowledge any earlier source, especially an earlier non-Christian source.

A couple of illustrations will make the point clear. At the time of Copernicus, as already noted, the church was very much operating in the crusading mode of intense religious war. Copernicus, himself a priest, had connections high-up in the ecclesiastical hierarchy, who would certainly have been embarrassed had he acknowledged the non-Christian source of his astronomy, and their embarrassment would naturally have re ected on his own fortunes. Copernicus' fear of the church is clear from the fact that he waited until he was on his deathbed before he published his work. It is also manifest from the "grovelling" preface to his allegedly revolutionary book, in which he desperately seeks to have the authority of the church on his side. Under these circumstances he would naturally enough have preferred to hide any Islamic sources he used. The social circumstances of the Inquisition that compelled him to hide his heretical sources can be ignored only by those historians who deliberately wish to obfuscate the truth.

Similarly, Mercator was actually imprisoned by the Inquisition.[33] Revealing his pagan sources would have definitely been fatal to him. Naturally enough his sources have not been found. But the similarity of his maps to projections used in Chinese star maps of the 10th c. is well known. So summary and brutal were the ways of the Inquisition, and such was the atmosphere of terror created by it, that people were intensely afraid of being associated with anything that might even faintly be theologically incorrect, for any rival could have denounced them, leading to painful and fatal consequences. Thus, in the days of the Inquisition there was little likelihood that even an otherwise honest European would have acknowledged knowledge from any non-Christian sources. Similar remarks, with some slight modifications, would apply *a fortiori* to those high up in the church hierarchy like Clavius, Tycho Brahe, etc.

This tendency to hide pagan sources was compounded by historians—who tended to run-down non-Christian and non-White sources. This tendency persists down to the present time—for example, even today, the name of Regiomontanus is more emphatically associated with the stock history of trigonometry than that of Āryabhaṭa! We will see another example of this later on.

Knowledge as a Trade Secret

Secondly, apart from fear of the church, secrecy was also motivated by the monetary and social value of the knowledge. Knowledgeable navigators, for example, commanded a high price, and tended to keep their knowledge a secret in the manner of trade secrets of today. In fact, Portuguese navigators used to get the decks cleared before making observations, so that no one else should, by observing them closely, learn to navigate. Academics like Fermat acquired their reputation not by publishing in the manner of today's academics, but by *not* publishing and challenging others to solve problems they knew how to solve. Even Newton threatened to withhold publication of his *Principia*, to establish his priority. Thus, there was then also a general tendency in Europe to avoid altogether revealing any sources of knowledge, to the extent possible, because the society placed a high value on priority. This is in noticeable contrast to, say, early Indian tradition, where there was not a single known case of any priority dispute.

Byzantine Sources and the Narrative Bias

Finally, the Byzantine Greek manuscripts that poured into Europe, after the fall of Istanbul, in the latter half of the 15th c. CE made it a natural agenda to attribute all knowledge up to the 15th c. CE to an early Greek source! Given the earlier stories about Greeks it was natural to regard these as the "original Greek sources". Many people still consider these late Byzantine manuscripts as "original Greek sources". It is true that some later-day historians have questioned these sources, and rejected them as unreliable indicators of early Greek knowledge. But of what use is it today to question the historical authenticity of Jesus in the learned manner of Albert Schweitzer?[34] Once a certain critical mass of people have been indoctrinated and the historical narrative has been established, it acquires a life of its own, regardless of the evidence to the contrary, and certainly regardless of the criticism of the historical sources of the narrative. The tenacity with which people tend to cling to a narrative, especially one which they have acquired in childhood, is amazing—such is the power of narrative over facts. The most absurd propositions can and have been perpetuated in this manner.

In particular, the Byzantine Greek sources helped to reinforce the initial bias in favour of the narrative of early Greek origins of knowledge.

Example: Transmission of the Epicyclic Model to Ptolemy

An example might help to fix the above ideas. Consider the case of the epicyclic model of planetary motion. The initial Arab and Byzantine sources had set the bias in Europe: the Greek fount of astronomical knowledge was "Claudius Ptolemy", the supposed author of the Arabic *al Magest*. Today, all books, without any exception known to me, attribute the epicyclic model of planetary motion to Claudius Ptolemy. Now, apart from the Arabs, a similar, though somewhat more sophisticated, model is also found in Indian tradition. Accordingly, Western historians such as Pingree claim that the Indian planetary models were obtained by transmission from Ptolemy.

What exactly is the evidence for this claim of transmission? What is the evidence that the original model was developed by Ptolemy and that it was transmitted to India? Well, the *Sūrya Siddhānta* is dated to about the 3rd c. CE, while Claudius Ptolemy is dated to the 2nd c. CE, which is earlier. So the logic is that there is similarity and there is precedence, therefore there must have been transmission. There are some other arguments that are sometimes given. One is that the *Puliśa Siddhānta* mentioned by Varāhamihīra in his *Pañcasiddhāntikā* refers to Puliśa which Thibaut thought might be a distortion of "Paul". Such "evidence" is not even worth contesting, and I mention it only to put on display the sort of arguments on which the convictions of authoritative Western historians are based.

The alternative hypothesis proposed above was that while parts of the *Almagest* may be from Ptolemaic times, it is an accretive text (as any scientific text ought to be) the entire contents of which are today incorrectly attributed to a "Ptolemy" (whose historical existence is yet to be established). Indian knowledge of astronomy, which travelled to both Jundishapur and Baghdad, was used to accretively update an early Egyptian corpus, dating from Ptolemaic times, and this accretive text ultimately became the Arabic *Almagest*. Not only were significant portions of the *Almagest* text obtained through transmission from India, but the epicyclic model today attributed to "Ptolemy" was probably also obtained in this manner, and was but a simplification of the Indian epicyclic model.

A third hypothesis is possible: for apart from transmission via Jundishapur and Baghdad, there is a possibility that the epicyclic model could have been directly transmitted directly from India to Alexandria. After all, if a 11th c. CE Arabic text from Baghdad or Toledo or a later Greek text from Istanbul can be taken as evidence of the *exact* state of astronomical knowledge prevalent in another place, Alexandria, in another language, 8 centuries earlier, why can't the *Sūrya Siddhānta* be taken as representative of the astronomical knowledge prevailing in the same place, India, in the same language, a mere three centuries earlier? Thus, both the *Āryabhaṭīya* and the *Sūrya Siddhānta* simply take this epicyclic model for granted, suggesting that it was very widely known to tradition at that point of time, which would hardly have been the case if it had only recently been developed or imported from abroad. On the other hand, we do know that Indian trade with Egypt stretched back to times before Alexander, and many texts attest to a substantial presence of Indians in Alexandria. Certainly, an Indian navigator would have had good reason to have carried astronomical manuals with him for reference and study. So there is every likelihood that Indian knowledge of astronomy found its way into the libraries of Alexandria, from where Ptolemy, if there really was an actual person like him, may have translated or copied them out. While transmission from India *could* have taken place in both ways the plain evidence of the current text of the *Almagest* supports the first of the above two suggested routes, and this is the sole alternative we will consider in the sequel.

Thus, the two hypotheses before us are (a) that a certain Ptolemy of the 2nd c. wrote a definitive text on astronomy which was transmitted to India by the 3rd c. CE, and (b) that Indian knowledge of astronomy was transmitted to the Arabs in the late 8th and early 9th c., that this knowledge found its way into the accretive Arabic text of the *Almagest*, and was incorrectly attributed to a "Claudius Ptolemy" of the 2nd c.

Now how do we decide between the two hypotheses? First we need to decide whether the *Almagest* text is a single author work or an accretive work. Ptolemy has been "firmly" dated, as one might guess, on the strength of some passages in the text—and the assumption that the current form of the text is the work of exactly one author. The passages relate to observations of equinoxes and solstices reportedly made in the reign of the Roman king Antoninus.[35] People like Tycho Brahe, who actually made observations, realized long ago that these purported observations were all fabricated. Historians like Delambre reached the same conclusion. More recently, this was pointed out in a whole book by Newton:[36] the systematic error in the "observations" could not have been due to instrumental error, and conclusively fits the hypothesis that the stated times were back-calculated from the incorrect theory that the length of the year is 1 day in 300 less than $365\frac{1}{4}$. Similarly, the stellar "observations" all have a systematic error of about 1° in longitude, showing that the positions have been back-calculated using an incorrect theory of the precession of the equinoxes. In general, as pointed out by Newton, there is not a single reliable observation in the entire *Almagest*.

However, all this seems to me not so much evidence of a crime by Claudius Ptolemy (there is no evidence that he even existed), as evidence to show that the text is accretive: one author recorded the star charts, and some other author recorded the passage on the strength of which "Ptolemy" is dated. There is other internal evidence to show that the text is accretive. For example, the "Cyrus" to whom the text is addressed, probably locates one source of accretion in Iran. Similarly, it is not difficult to locate the time until which accretion was going on: Polaris leads the star catalogue in the *Almagest*, although there was no pole star in the epoch assigned to Ptolemy—at best it could have been Kochab.

Of course, if the *Almagest* is a multi-authored accretive text, then it is pointless to try to assign a precise date to it, as Western historians have been naively or mischievously doing for so long. Since the key evidence for the claim of transmission is the claim of precedence which derives from the date, the claim already falls apart. However, let us examine other aspects as well.

The second point to consider is whether the attribution of the knowledge in the text to the 2nd c. is anachronistic. When the Arabs first learnt Indian algorithms for arithmetic in the 9th c., they experienced difficulties in multiplication and division. Similar difficulties are explicitly referred to in the "Ptolemaic" text[37]

> In general, we shall use the sexagesimal system because of the difficulty of fractions, and we shall follow out the multiplications and divisions, aiming always at such *approximations* as shall leave no error worth considering as far as the accuracy of the senses is concerned. [Emphasis added.]

To get over these identical difficulties experienced by 9th c. Arabs, they prepared handy multiplication tables. While numerous such Arabic multiplication tables are available, most of them are only to a precision of the *second* sexagesimal minute, though some tables include thirds.

In India, as we have seen, Āryabhaṭa in the 5th c. derived his trigonometric values only to the precision of the first minute (which would require arithmetical calculations only to the second minute). It was only in the 9th c. CE that we find attempts to calculate these values correctly to the second and third minutes, which calculations, if done with tables, would normally have required multiplication tables to the fourth minute.

However, the *Almagest* states values of the chord to the third minute![38] Compared to the two-sexagesimal place Arab tables of the 9th c. CE, this required tables to the fifth sexagesimal place![39] Certain values, like the mean movement of the moon's anomaly in longitude are given to the *eighth* minute![40]

In the passage used to date Ptolemy, the author of the passage is struggling to fix the length of the year accurately to the second decimal place, through the admittedly crude device of looking at just a few pairs of (concocted) "observations" some 300 years apart—and even then he gets it wrong! This is the maximum level of accuracy that is consistent with our knowledge of the Roman calendar, which, despite earlier attempts at calendar reform, did not progress to second-decimal-place accuracy until the Gregorian calendar reform of 1582. (Even in 1582 Europeans were unable to fix the length of the year that accurately; *hence*

Protestant countries initially rejected the reform.) So, in the absence of accurate knowledge of even a simple parameter like the length of the year, where did the accuracy to the eighth minute come from? Why were chords needed accurately to the third minute?

These questions have simple answers if we locate the *Almagest* in the environment of post 9th–10th c. Arabs. The questions have no *straightforward* answers if we regard the *Almagest* as a 2nd c. Roman text. (The emphasis on "straightforward" is important, for, of course, it is a well-known principle in the philosophy of science, that any hypothesis can be made consistent with any facts by piling on more hypotheses.) Thus, the *Almagest* seems an accretive work, and attributing it entirely to the 2nd c. CE seems anachronistic.

It is instructive to compare the *Almagest* account of the earth, with the various arguments from Indian texts cited in Chapter 4. There is a remarkable similarity. Why doesn't the earth fall down? Vaṭeśvara asks the counter-question, "say what is up and down for an object standing in space?" and "Ptolemy" repeats[41] somewhat more unclearly, "For there is no above' and below' in the universe with respect to the earth, just as none could be conceived of in a sphere." Vaṭeśvara says, "Just as a ame of fire goes aloft in the sky and a heavy mass falls towards the earth, so is the case in every locality on the earth", and the *Almagest* repeats, with greater prolixity and less clarity, "And of the compound bodies in the universe to the extent of their proper and natural motion, the light and subtle one's are scattered in ames to the outside and to the circumference, and they seem to rush in the upward direction... but the heavy and the coarse bodies move to the centre and they seem to fall downwards."

One of the more interesting of these common features is the following argument where the rotation of the earth is denied in the *Almagest*: "Now some people... think... supposing, for instance, the heavens immobile and the earth as turning on the same axis from west to east very nearly one revolution a day...". The *Almagest* text goes on to paraphrase the arguments of Varāhamihīra about the aether wind (Chapter 4, p. 215, and note 27), although it changes the eagle to a falcon. Now in the Indian tradition we know the story, and it is understandable why Indian texts, after Āryabhaṭa consider it important to deny this possibility. It would also have been a very natural thing for a post 9th c. CE Arabic astronomer to have put things in this way, leaving the "some people" unspecified, for the relevant names would have communicated nothing. On the other hand, had there been transmission of any such text in the reverse direction to 3rd c. India, then Āryabhaṭa would have been compelled by tradition to address this argument (against the rotation of the earth) as a *pūrva pakṣa*. Therefore, this also supports the view that the *Almagest* is an accretive text incorporating Indian knowledge via post-9th c. CE Arabic astronomy.

Epistemological Continuity and Transmission

The racist double standard of evidence is often masked by an appeal to authority. Therefore, to resolve the issue of transmission, it is important to go beyond mere textual evidence (from late texts), and look at a variety of other criteria. One such criterion is that of epistemological continuity: a transmission is indicated by an epistemological discontinuity.

As an elementary illustration of this principle, consider a practical situation where two students turn in identical answer sheets (or projects). It helps to examine the background of the two students: if one student has long been performing well, it is likely that it is the other student who copied. That is, we do not look merely at the end result, but also look at the process by which that end result was obtained. Acquiring or generating knowledge is a process that takes time.

As another application of this principle, when there is a doubt about the dating of a text (or the authenticity of a claim of discovery) it helps to examine the continuity of a text (or discovery) with the knowledge exhibited by past and future texts from the same milieu. To avoid a situation where one speculation is supported only by other speculations, it helps to locate the text in the context of the non-textual evidence of what was definitely known at that time in that milieu. It also helps to ask about the social processes that supported the generation of the text, and the knowledge in it.

Thus, in India, the scientific interest in astronomy and timekeeping stretches back to at least the *Vedāṅga Jyotiśa* of ca. −1350 CE, for practical reasons, related to agriculture and economic production, as we have already seen. This provided a very long baseline of observations against which there was a need to invent, test and improve planetary models in an epistemologically continuous way. Since Indian astronomy was linked to the practical social requirement of agriculture, post-*Sūrya Siddhānta*, we find a series of astronomical texts right up to the 17th c., and, in fact, down to current times—for the traditional Indian calendar is still in use.

In Greece, on the other hand, there is no particular tradition of astronomy preceding "Ptolemy". We have already seen that any sort of scientific approach to astronomy was regarded as a crime up to the time of Plato and Aristotle. Likewise, Greeks at the time of Alexander knew nothing of navigation and had not made any serious sea voyages, as is clear from Arrian's account of Nearchus' voyage, and the way his soldiers got terrified on seeing the spout of a whale. So, till the time of Alexander, Greek knowledge of astronomy was virtually nil, and there were no social processes like agriculture or navigation with which it was entrained.

This ignorance of astronomy is re ected in Macedonian calendar which intercalated one new month for every two years. This was so crude a technique that the calendar gained about $3\frac{1}{2}$ days per year, so that there was no correlation even between the new moon on the Greek lunar calendar and the actual new moon! Naturally, wits mocked the Greek calendar using the term "Greek calends" to describe this state of chaos. (Nevertheless, it is the Greek Meton from this period to whom the "Metonic" cycle is attributed!) Thus, there was no social requirement for knowledge of astronomy among the Greeks.

Presumably, it was the input of African knowledge in Alexandria that brought about a change of Greek attitudes towards astronomy. The *Almagest* passage used to date Ptolemy (Book 3.1 on the length of the year) rejects the observations of earlier Greeks like the pupils of "Meton and Euctemon... as more or less taken in the rough, as Hipparchus also seems to have thought" (p. 81). It refers to Hipparchus carrying out observations, on a "bronze ring situated in what is called the Square Hall of Alexandria" (p. 78), presumably an Egyptian construction. Again, the *Almagest* itself (unlike present-day historians) does not find any other Greek astronomer worthy of mention in the next three centuries after Hipparchus. Thus, unlike the Indian tradition of astronomy which is a continuous tradition spanning three thousand years, "Ptolemy" is a singularity with no serious predecessors or successors: the only predecessor he acknowledges is a single individual who came some three centuries earlier. In placing the *Almagest* in the 2nd c. we are required to believe that Greek astronomy suddenly appeared with the definitive text of the *Almagest*.

Unlike the case in India where the planetary models were continually being refined, and their parameters adjusted, down to the 16th c. CE, there is no clear historical account of the process by which the parameters in the *Almagest* were obtained by "Ptolemy". In India, the move from precision of the first minute to the second minute took several centuries. So, did Ptolemy have any predecessors who did some less accurate calculations? Did he have a predecessor who perhaps calculated chords 1° apart? Unfortunately no: Ptolemy is a singularity who (by virtue of the chronology assigned to him) miraculously emerges with a full-blown model without any earlier mistakes or prototypes! Ptolemy's immediate predecessor in the Roman empire, Pliny, a man regarded as vastly learned, in his *Natural History*, put forward a planetary model with three suns and three moons! Pliny emphasized that the number of suns simultaneously observed has never exceeded three!

Now, how would Ptolemy have obtained the parameters of his model? Going by the key paragraph used to date him, his observational baseline is at most 285 years, on the basis of which he concludes that the tropical year is less than $365\frac{1}{4}$ by 1 day in 300 years. This is better than what one might expect with just two observations some 300 years apart, but this is nevertheless crude compared to Āryabhaṭa's estimate (of the sidereal year)—supposedly based on Ptolemy! If Ptolemy was satisfied with such crude observations and estimates, it is hard to see what were the theoretical or observational discrepancies to explain which Ptolemy would have needed precision to the thirds, for his table of chords, and a phenomenal precision to the eights for some of his other astronomical values.

Not only did Greek astronomy appear suddenly with a definitive text not preceded by anything, but, in a similar miraculous way, it disappeared with equal suddenness from the Roman empire! Not only were there no astronomers of note after the singular Ptolemy, the very knowledge of astronomy disappeared from the Roman empire. This is clear from the Hilarius evidence. The date of Easter was a major issue for the early Christian church, and several calendar reforms were attempted to this end. The Council of Nicaea which had this sole point on its agenda agreed to consult the Alexandrian astronomers about this. Alas! Despite the level of excitement that this council aroused, all the priests and all the king's men were unable to locate the magnificent work of this Roman citizen, "Claudius Ptolemy", which would have immediately settled the problem! At that time the Museum and the Serapeum in Alexandria had not been destroyed, and Christians were not yet burning books, so we must suppose that "Ptolemy's" *Almagest* voluntarily disappeared from the Roman empire by the early 4th c. This disappearance was permanent: Hilarius, as pope, again attempted calendar reforms, but he and his men too were unable to track down an accurate source of astronomy in the Roman empire. Consequently, the length of the year in the Roman calendar remained inaccurate in the second decimal place, for the next thousand years.[42]

In fact, the *Almagest*, in its present form, was certainly not available even in Jundishapur, where the Alexandrian diaspora congregated; and we know that Indian astronomy texts were imported and translated there. By the mid 6th c. the best that could be expected from Indian astronomy was obviously not too far beyond the precision achieved by Āryabhaṭa. Thus, the conjectured text of Ptolemy (in its present form) was not available even to the Alexandrian diaspora who were interested in astronomy, and would have known of any extant Greek texts in astronomy. So, if this knowledge was not with the state, and not with the refugees, one wonder where it was hiding.

If we do suppose that the *Almagest* text was playing hide-and-seek for so long, that creates another problem. One wonders how the text on papyrus managed to survive in hiding. One wonders how the text nevertheless manage to appear at a later time in Arabic.

A similar epistemological discontinuity applies to Ptolemy's use of the sexagesimal system and algorithms, which has neither any past nor future in the Roman empire. It is quite impossible to understand the sudden jump in arithmetical techniques from the integer arithmetic of the abacus to accuracy to the eights (about 15 places after the decimal point)! A new hypothesis is usually introduced to the effect that the sexagesimal system was imported from Babylon by Greek astronomers. There is not an iota of non-textual evidence that the Romans in the 2nd c. ever used the sexagesimal system or even understood how to deal with fractions or multiply numbers using algorithms—the place value system used in these algorithms is foreign to Roman numerals, and was not understood by the first Europeans to encounter it, like Pope Sylvester. We will see this in more detail later on. In any case, the key issue is not the sexagesimal system: it is one thing to use the sexagesimal system, and altogether another thing to have an accuracy to the eights. Finally, Western historians have overlooked that one more conjecture is needed to account for the fact, that despite the conjectured import of the sexagesimal system by the conjectured "Ptolemy", this use remained unknown to everyone else in the Roman empire. All the evidence we have comes from the late Arabic texts (or later Byzantine Greek texts), and these texts used the sexagesimal system because they learnt it from the Indian way of doing astronomy, along with the positional system of notation.

The list of questions is not exhausted. Who supported this Ptolemy in this effort to develop theoretical planetary models, and why? For what practical purpose did he need to develop them? (Clearly, the Greeks were satisfied with a crude calendar just because the calendar and astronomy were of little practical value to them.) What about the correlated apparatus of mathematics that he needed (square roots etc.), which was missing in the Roman empire?

Doubtless more speculations could be introduced to answer these questions too. However ,this method of piling speculation upon hard-to-believe speculation in the manner of theology—to lead to the premeditated conclusion—also means that the credibility of Western history of "Greek" science is little different from the credibility of theology: one can believe in it only if one has the requisite faith!

To summarize, the *Almagest* text in current circulation is epistemologically continuous with post-10th c. (and even 15th c.) Arab texts and is completely discontinuous with 2nd c. Roman knowledge of astronomy or arithmetic, and the related non-textual evidence, and social processes. Thus, attributing the current text of the *Almagest* to an author in the 2nd c. is unacceptable since it also requires us to to believe in a variety of things contrary to elementary common sense. Accordingly, we reject this hypothesis about an otherwise unknown "Claudius Ptolemy" who authored the *Almagest*, and regard the *Almagest* as an accretive text, perhaps coming down from Ptolemaic times, but repeatedly updated, at Jundishapur, Baghdad, and subsequently. The hypothesis (a) stands refuted (to the extent that it is refutable).

This illustrates how the criterion of epistemological continuity provides a check on the extravagant claims of racist history, supported by the authority of scholars guided by the iron hand of religion.

Continuation of Racist History to the Present

There is a belief that things have changed, that the racist model of history died of embarrassment when its naivette and designs started being exposed. But perhaps it was only hibernating while it renewed its thick skin, for it has returned to participate in the civilizational clashes proposed by Huntington[43] in his attempt to initiate cultural globalization, *à la* Toynbee.[44] An example from a recent history of astronomy is provided by North,[45] who, despite Bernal,[46] is still very keen to trace the source of all information ows back to a Greek fount—mathematics from Euclid and Archimedes, and astronomy from Ptolemy—and to dismiss everything else as mindless meandering or, at best, a matter of secondary importance.

And, as pointed out earlier, the racist model of history presents an immediate problem for it has returned to haunt the current Indian school texts.

Selection Effects

Whether or not it is dead, the racist model has left behind not only bogus theories of information transmission but also a legacy of selection effects. What is a selection effect? As an example, the most superficial observation shows how little is the space available for the non-West in "mainstream" academic conferences or journals devoted to history and philosophy of science, even though these conferences and journal refer to themselves as "international". How does this come about? One part of the story is that racist prejudices often lurk behind authority—the authority of the organizer of a conference or editor of a journal, for example, who are almost never from the non-West. But there is more to the matter than that.

A selection effect is a way of directing (or misdirecting) attention. If we focus attention on the stars in the sky at a small angular separation, they may seem related even though they are separated by vast tracts of space. If we pick stars at random from the sky, then any apparent relation between the stars in the sample is likely to be purely a figment of the imagination, an artefact, a consequence of the way our attention was focused. The problem is that the case for a relationship can always be argued, for ultimately we have no means of establishing whether the stars really are related or separated.[47]

By focussing attention selectively, a selection effect can also be used to manipulate credits. The typical Western history of trigonometry is likely to commence with Ptolemy and then take a great leap forward to Regiomontanus, with at best a passing mention of Āryabhaṭa.[48] Thus, historians[49] proclaimed triumphantly:

> Henceforth, Greek trigonometry was truly established. It was based on... tables rigorously computed. Its main object of study was always the sphere to which Menelaus' theorem applied particularly well. This theorem ... paved the way for the later appearance of the sine .... The main step had been taken, and the successors—Hindus, Arabs, Europeans—had simply to follow along the trail which the Greeks had blazed for them.

Even granting the myths about Ptolemy, the case for chronological precedence is shaky: for if a 15th c. source can be used to infer the state of knowledge in the 2nd c., in another place, there is no reason why the *Sūrya Siddhānta* and the *Āryabhaṭīya* should not be used to infer the state of knowledge prevalent a couple of centuries earlier—after all they *assume* knowledge of trigonometric functions, so that the knowledge of these functions can be safely assumed to have been widespread much before these texts. Therefore, while the *Sūrya Siddhānta* may postdate the conjectured date of Ptolemy, Indian knowledge of trigonometry very probably predates that conjectured date.

However, the real point of a selection effect is that chronological precedence is not critical to such an argument. It could always be argued that the "main" step was taken later. Even in the European context, the credit for the calculus was given to Newton and Leibniz in preference to Cavalieri, Fermat etc. exactly in this way.

On the face of it, the situation may be reminiscent of the fable of the four blind men and the elephant. However, just as racism should not be confounded with Eurocentrism, so also a selection effect should not be confounded with an inadvertently biased sample or judgement. It is better illustrated by the real story of the four learned men and the Indian elephant.

The Indian Elephant

The facts are as follows. A piece of Mayan architecture from Central America distinctly resembles an Indian (note Indian!) elephant.[50] Now, the American elephant became extinct some ten thousand years ago, whereas the roots of the Mayan civilization were not more than three thousand years deep. So one is naturally tempted to ask: "What induced the Maya to sculpt Indian elephants?" The similarity of the Egyptian and Mayan pyramids is well known, and is suggestive of organized navigation between Egypt and South America. Should one combine this with the known fact[51] that commerce between India and Egypt involved shipping the Indian elephant from India to Egypt?

But to the Western scholarly mind that is not the relevant question. Admitting such questions, like admitting questions about the similarity between indigenous African and North-American languages, would amount to admitting the possibility that the Europeans were not the first to sail across the Atlantic, and that would remove the last vestiges of any justification for the genocide in the Americas. Therefore, a more important issue must be settled first. What looks like an elephant to the untrained eye may or may not be an elephant—as is the case in more modern art. Here is a summary of the scholarly controversy that erupted in the well-known journal *Nature*.[52]

Professor Tozzer bases his views on the fact that the Maya also sculpted the macaw—a long-tailed, brightly coloured parrot that is native to South and Central America. Accordingly, he holds that a comparison of the "elephant" with the unmistakable sculpture of the macaw "shows that the two represent the same animal". What seems to be the elephant's trunk is no more or less than a stylized depiction of a macaw's beak.

Professor Elliot Smith suggests, "The accurate representation of the Indian elephant's profile, its trunk, tusk, and lower lip, the form of its ear, as well as the turbaned rider and his implement, no less than the distinctively Hindu artistic feeling in the modelling are entirely fatal to the macaw hypothesis."

Dr Eduard Seler's view is that the objects under discussion are tortoises. Disagreeing also with those who have favoured the tapir, Dr Spinden is quite definite: "That the hands with projecting snouts, used as architectural decorations, are connected with the concept of the snake rather than the elephant is easily proven by a study of homologous parts in a series of designs." The four learned men did not exhaust the possible interpretations of the piece of sculpture; presumably Erik von Daniken would interpret the object in question as a representation of an astronaut wearing a sulphur-dioxide mask!

Selection Effects and Transmissions

The selection effect operates for the learned men in much the same way as it does for the four blind men: by claiming disproportionate credit for a single feature. The difference is this: while the blind men were themselves misguided, the learned men often aim to misguide others!

The problem of localizing credit through precedence involves a picture of mundane time. As I have argued elsewhere,[53] in any social situation, there always is more than one actor, and there always is a chain of causes. What a selection effect does is to pick out one element in this chain and value it above all others. Although this is a political decision, the drastic consequences it can have on fact is clear from the "learned man" selection effect which reduces an elephant to a macaw!

IV
EPISTEMOLOGY AND NON-TRANSMISSION

Other Epistemological Issues

Another sort of selection effect operates by applying a standard epistemological filter to cloud alternative epistemologies. For example, the epistemological filter may be that of current-day socially dominant mathematics, which is used to exclude any other type of mathematics as non-mathematics. Hence, it is difficult to answer questions of information exchange about mathematics without reworking the entire epistemological foundations of traditional mathematics.

Non-Transmission of the Elements

Nevertheless, we have seen that in Western histories of science, a key reason for the interest in establishing transmissions has been the theological interest in glorification of the West to justify exploitation, and establish "pagan inferiority". If clerical apologetics for surplus extraction is not the goal, then it is clear that cases where information was *not* shared, despite extensive contact, are equally interesting. But this situation seems never before to have been studied in detail. There are many such cases where there was contact, but information was not transmitted.

One example is that of Jai Singh. He studied all the available systems, from the European to those of Ulugh Beg, but did not incorporate the knowledge of, for example, the telescope in his design of the Jantar Mantar in Jaipur. Jai Singh certainly knew about the telescope.

He had bought one at a cost of Rs 100, and had used it to observe "bright stars in broad daylight—say around the noon hour". He had observed "the planet Saturn...Jupiter", and knew that "the Sun rotates...on its axis...". He had recorded these observations in his *Zij Jadid Muhammad Shahi*.[54] However, he did not incorporate the telescope in his design of the Jantar Mantar, "since the telescope is not readily available to an average person".[55]

Jai Singh clearly seems to have regarded knowledge, in general, and the telescope, in particular, as only a means to an end that was partly pedagogical in this instance. Hence he rejected what would today be regarded as "superior" knowledge.

Jai Singh's case also demonstrates another sort of non-transmission for more mundane reasons. He financed the voyages of some Jesuit priests to Europe to fetch the latest knowledge of astronomy from Europe. Though the priests did visit Europe, they brought back out-of-date information—either because they were ignorant about the latest information, or simply too lazy to obtain it, or because they had no compunctions in deliberately deceiving a person whose patronage they willingly accepted.

There are other cases of non-transmission that are so long lasting that they cannot conceivably be put down to any individual aberration or idiosyncrasy. I will take up two such cases. The first concerns Euclidean geometry and the second concerns the calculus.

While there is considerable doubt whether "Euclidean" geometry is at all an original Greek tradition, there is no doubt that Euclidean geometry is not solely a Greek tradition. It was very much in vogue in the eastern parts of the Roman Empire, and among the Arabs and the Mughuls. Abul Fazl learnt Euclidean geometry, in India, presumably from Arabic and Persian sources, and mentions it in detail in the *Ain-i-Akbari*.[56] India had contacts with the Greeks and Alexandria certainly since before the time of Alexander. There were extensive trading contacts with the Roman Empire. Nevertheless, the in uence of Euclidean geometry is not traceable in the writings of non-Muslims in India until Kamalākara, Jehangir's court astronomer, long after the arrival of Jesuit priests in Akbar's court. Though mathematics, we are told, is one and universal, there were two streams of geometry simultaneously prevalent in India. Eventually, parts of the *Elements* were got translated into Sanskrit only in 1718 CE, by Jai Singh (Samrat Jagannath), from Persian, two centuries after the arrival of the Europeans, but before the beginning of colonialism in India.

A similarly negligent attitude towards Euclid prevailed among the Chinese whose geometry was tied to practical concerns, and did not pay much attention to the idea of theoretical demonstration or "proof" so popular with medieval European rational theologians and historians of science. The earlier rational theologians of Islam retained in this ideal of demonstration a Neoplatonic twist of equity, as we have seen, and, as expected on the abovementioned theory of transmissions, this Neoplatonic version travelled towards the aggressor from Mongolia, after the fall of Baghdad. One finds in Needham[57] that it was only after the 13th c. that:

> Yang Hui... proceeded to give a proof about parallelograms which is similar to the one in Euclid. If such proofs had been extended the Chinese might have developed an independent deductive geometry, and clearly some minds like Yang Hui were prepared to appreciate the Euclidean system. This is of great interest because there may have been at this time a translation into Chinese of Euclid's *Elements*, due to Chinese-Arabic contacts.

Why did these two older cultures not share the Western historian's enthusiasm for the *Elements* and the deductive method? This is a key question because of the central role of the *Elements* not only in the Western scheme of the history of mathematics, but also as a model for modern mathematics. The non-transmission of information about the *Elements* between the Arabs and the non-Muslim Indians thus emerges as a key fact which goes against the entire scheme of transmission in the Western history of mathematics, and also the current belief in the "universality" of mathematics used in the foundations of modern mathematics.

The reason why the *Elements* were not transmitted is quite simple. They were seen to be of no practical value, hence of no value at all!—at least to those who did not share the underlying religious beliefs. That is, there was an epistemological barrier to transmission.

Epistemological Barriers: Algorismus

Such epistemological barriers can also be seen in Europe, in the long time that it took Europeans to accept the algorismus. In this case, though the algorismus was seen to be of practical value, it did not fit into the existing theological scheme, i.e., the algorismus did not fit into the European idea of mathematics as certain knowledge; therefore it was regarded with suspicion for centuries. This is taken up in more detail in subsequent chapters.

Epistemological Barriers: Calculus

Similar suspicions attached to the calculus in Europe, for despite its obvious practical value it was seen as methodologically unacceptable, and we will argue later on that this epistemological barrier explains the delay in European acceptance of the calculus.

Physical Barriers to Transmission

Of course, all barriers need not be epistemological. There can well be other sorts of barriers. This brings us to final example of non-transmission which concerns the calculus. The immediate concern here is not with the question of its transmission to Europe, but with its non-transmission to other parts of India.

Perhaps there was a language barrier. But this is not an adequate explanation, since the *TantrasaṅgrahaVyākhyā* was anyway in Sanskrit, and the *Yuktibhāṣā* had already been translated into Sanskrit. In my view, this non-transmission indicates a physical disruption of the prevalent environment of information sharing, before it was finally dismantled. On the one hand, we know that right up to the time of Nārāyaṇa Paṇḍita, in the 14th c. CE there was extensive sharing of information between Cochin and Benares, for the formula for *vārasaṅkalitā* was immediately used by Mādhava to derive a result that had been sought in Kerala for centuries earlier, without success. From Ibn Battuta's account, we also know that the route from Delhi to China went across the sea from Calicut, for that is the route he took to carry Tughlak's presents for the Chinese emperor.

It is not very hard to understand the reasons for the eventual disruption of the channels of communication between North and South India. Right from the time of Mahmūd of Ghazni, al Bīrūnī described the situation in North India as follows:

> Mahmood utterly ruined the prosperity of the country...by which the Hindus became like atoms of dust scattered in all directions....Hindu sciences have retired far away from those parts of the country conquered by us, and have ed to places which our hand cannot yet reach, to Kashmir, Benares and other places.[58]

Over the next few centuries, conditions in North India remained very unsettled, and, from Timur to Tughlak, Delhi was twice emptied of its *entire* human population. As is clear from the description provided by Ibn Battuta, who set out from Tughlak's Delhi (Tughlak-abad) to China via the sea route from Calicut, the writ of the emperor of Delhi did not quite extend as far as Agra! On the other hand, conditions in the south were relatively settled during this period, because of the bulwark provided by the Vijaynagar empire, until the mid 16th c. CE. However, the wealth of the Vijaynagar empire attracted not only Vasco da Gama and the Portuguese, but also the nearby potentates like ies, and they were constantly warring with it, so that by the time the Mughul rule in Delhi had stabilized after Humayun's return, and Akbar consolidating his position in Agra, Hampi was in ruins.

To summarize, three kinds of the non-transmission of knowledge are thus visible across cultures. In the first kind, information owing in is critically evaluated and some or all of it is rejected or viewed with suspicion because of epistemological differences; an example here is the *Elements* in India or the algorismus in Europe. In the second kind of non-transmission, the traditional information-sharing network is disrupted, and eventually information preferentially ows out; the example here is the calculus and computations of the value of π. In the third kind of non-sharing, despite conscious efforts at information gathering, presumably to maintain secrecy, the information actually brought back is of such poor quality that it is rejected; the example here is Jai Singh and the information on European astronomy which he got from the Jesuits.

V
MISCELLANEOUS ASPECTS

Cooperative Versus Competitive Models of Information Sharing

Lastly, we must also examine the way in which information was shared within a culture, for this also decided the sort of information that could or could not easily be transmitted to others.

The model of information sharing current in our civil society today is a "competitive" one. The belief is that individuals (or small groups) create information.[59] The information so created is privately owned by the concerned individual, who as its creator acquires a right to "royalty" or "copyright" or "patent", i.e., a right to extract surplus from others with whom the information is shared. In principle, the society recognizes the creativity of the individual, and encourages it by enabling creative output to be swapped for a more dominant position in society. In case of conflicting claims, ownership is decided by priority, and in case of conflicting claims of priority, priority (hence ownership) is decided by authority, including judicial and historical authority. In modern industrial societies, ownership of information is highly valued, and so also is technological innovation (which can lead to dramatic increases in the efficiency of production).

This was not the situation in more traditional societies where, to give an analogy in terms of land-ownership patterns, there were large common spaces. Traditionally, creative activity was seen as the manifestation of an immanent God, quite distinct from Augustine's transcendent disciplinarian. Laws and traditions restricting the sharing of information related to the sharing of religious rather than secular information; these restrictions typically applied to whole groups (say castes, foreigners, etc.). There were some conventions of apprenticeship, such as the tradition of the *gurū-śiṣya* or the *ustād-shagird*. These regulated information flows in the manner of the religious techniques of initiation, rather than the commercial sale of property. Thus, while specialized information of immediate economic importance continued to be kept a secret within families and guilds, there were no laws governing its sharing and no priority disputes. Identifying oneself as the author of an innovation was not, therefore, terribly important as it was to Newton and Leibniz, who quarrelled so nastily[60] over priority for the calculus, which neither of them had. Value was attached to "authority" and the age of a tradition; tradition could be rejected in favour of a better system (as for example Varāhamihīra did, while updating the *Vedāṅga Jyotiṣa*, but innovativeness was not valued for its own sake.

Thus, in this "cooperative" model of information sharing, information might be held in secret for its economic value, and information might not be given out if the recipient of the information was not regarded as worthy enough to receive it. But information was not held in secret merely for the sake of establishing one's innovativeness to posterity—it was unimaginable that someone would threaten, as Newton threatened Hooke, to withhold publication to demonstrate priority. Indeed, if someone did make a small innovation which he regarded as valuable, it could well go in as an anonymous contribution to a book being copied out or commented upon. This was particularly true of Arabic traditions, where the numerous translations were never mechanical. Sailors manuals attributed to ancient sources could thus contain up-to-date information.[61]

A concrete model of this sort of information sharing can still be seen in remoter places such as the Lakshadweep islands. The result is quite striking. Though there is undoubtedly a common pool of information, there is also differentiation. Without any active attempt to keep anything secret, I found that islands which are only 30 km apart can have discernibly different traditions, and may be unfamiliar with some of each others' navigational instruments.[62]

To summarize, in this model of information sharing, it is neither possible nor important to try and trace each key development to an imagined unique source from which it diffused. To use an analogy, in locating the origin of agriculture in the Fertile Crescent or somewhere else, we are modelling information ows by a river which has a source. This may be true of some sorts of information ows; there are rivers, but there is also the sea—of shared information—for which it is futile to seek a source. In this case, it may be more interesting to look at currents and waves—individual peaks of localized information that only emphasize that it is the peaks that need an explanation rather than the at background of a very large shared base of common knowledge due to extensive contacts.

In India this cooperative model of information sharing was disrupted with the arrival of the Europeans, who systematically attempted to localize information by establishing asymmetric information ows towards themselves, in the manner of dams across rivers.

The Channels of Information Transmission

In contrast to models of information sharing, which have been neglected by scholars, *channels* of information transmission have been fairly well studied. Military or commercial exchanges created channels along which information could easily ow. India was connected to China, West Asia, and Africa through both land and sea routes.

The land routes have been extensively documented.[63] Trade routes have existed from before recorded history, among the most famous being, of course, the Silk Route. Aggressors with large empires who sought to extract larger volumes of surplus from far-off lands were forced to maintain the land routes. Examples are Alexander who had to link Greece to Afghanistan, Kanishka who linked Central Asia, West Asia, and North India, and the Mughul Empire in Baghdad which linked Central Asia and West Asia.

There are, however, three points that I would like to emphasize. The first concerns the bandwidth or the potential volume of information transmission. One would expect this to be proportional to the volume of trade, or the forcible surplus extraction, and this is usually underestimated. I would, therefore, like to draw attention to Pliny's complaint about the reverse extraction of the surplus that in no year did "India absorb less than five hundred and fifty million sesterces of our empire's wealth, sending back merchandise to be sold with us at a hundred times its prime cost."[64]

The second point that I would like to emphasize is that, analogous with the Internet (or a general packet-switched network), the route of information transmission need be neither unique nor the most direct one. As a concrete example, consider the *meru prastara* in Piṅgala's *Chandaḥsūtra*. It could have first travelled to China, through Buddhist travellers or traders, and from thence to Europe through Jesuit intermediaries, where it eventually came to be known as Pascal's triangle, giving the coefficients of the binomial expansion, nowadays attributed to Newton.

Thirdly, for the purposes of this book, the sea routes are relatively more interesting, for navigation involved the practical application of both astronomy and mathematics; it also provided a context in which information had to be shared, and tradition certainly would not have stood in the way of any technique which manifestly fetched results. (The sea routes were used to carry heavy cargo, like the Indian ebony that was exported to Rome.)

VI
STANDARD OF EVIDENCE OF INFORMATION TRANSMISSION

In speaking of information transmission, Western historians have had a fairly transparent racist agenda of establishing that the origin of everything important was somehow connected with Whites, and that the rest of the world contributed practically nothing. Accordingly, knowledge anywhere else in the world is claimed to have been derived by transmission, and in the past there have been far too many such claims of transmission. The evidence produced for these alleged cases of transmission is often farcical, as in Thibaut's claim that Ptolemaic astronomy was transmitted to India because Varāhamihīra's use of "Puliśa" suggests that it could have been derived from "Paul" (rather than Puliśa or Pulastya, one of the seven sages forming the constellation known as the Great Bear).

If this be the standard of evidence, there is nothing remaining to prove about the transmission of the calculus, for the works of Parameśvara, Mādhava, Nīlakaṇṭha, and Jyeṣṭhadeva, clearly precede those of Fermat, Pascal, Gregory, Wallis, Newton, and Leibniz, and India was clearly known (and actively linked) to Europe by the 16th c. CE.

However, we have also seen that the standard of evidence is not uniform, but varies with the claim being made. The standard of evidence required for an acceptable claim of transmission of knowledge from East to West is different from the standard of evidence required for a similar claim of transmission of knowledge from West to East! Thus, there always is the possibility that similar things could have been discovered independently, and that Western historians are still arguing about this, even in so obvious a case as that of Copernicus. Finally, we have seen that this racist double standard of evidence is not an incidental error, but is backed by centuries of racist tradition, religious exhortations by popes, and by legal interpretations authoritatively handed down by, say, the US supreme court.

Hence, to establish transmission we propose to adopt a legal standard of evidence good enough to hang a person for murder. Brie y, we propose that the case for any transmission must be established on the grounds of (1) motivation, (2) opportunity, (3) circumstantial evidence, and (4) documentary evidence. The importance of epistemology has been repeatedly stressed above: any such claim of transmission must also take into account (5) epistemological issues.

In the West documentary evidence is highly valued. However, this seems to be a purely cultural matter, specific to the West where a written scriptural tradition is regarded as important. However, documents can and have been easily forged—such as the forged "award of Constantine" used to grab the land on which the Vatican today stands. Such forgeries can operate in various ways, and false authorship may also relate to the case of someone who claims to have independently discovered something. On the other hand, documents, even vast quantities of them, can be suppressed for centuries, as the case of Newton shows, resulting in historians arriving at and maintaining wrong conclusions for centuries. Such conclusions, obviously, are linked to decisive political advantages. Accordingly, the value of documentary evidence needs to be downgraded, as a local cultural matter, and epistemological issues provide surer evidence of origins.

The importance of epistemological issues cannot be overstressed. The epistemological test is a simple one, and one which is routinely applied in everyday practice. Consider two students who turn in two identical (or nearly identical) answers or projects. Though it always remains a theoretical possibility, there is a level of similarity beyond which it is not practical to believe that these two answers had independent origins, for if there are too many "coincidences", the probability of an independent origin becomes too small to bother about. Under these circumstances, how does one decide who has copied from whom (or whether both have copied from a third common source)? The simple practical test, which I have often used is to call both students for an oral test. This sort of copying is made very easy only because excessive stress is laid on the value of documentary evidence, which is easy to manipulate. A similar manipulation is not so easily possible with an oral test. The fundamental weakness of documentary evidence is that in a documentary presentation, in contrast to an oral presentation, ignorance can be hidden far more easily.

What the oral test can test is understanding. Did the student fully understand what he wrote? The implicit belief, a robust one, is that creation presupposes some comprehension: one cannot create something that one does not clearly understand. Thus, on this test, sustained lack of understanding of the calculus in Europe, like the sustained lack of understanding of the algorismus, is a solid indication that it was transmitted. Europe could hardly have created a calculus it did not comprehend for centuries.

The last point is that of epistemological continuity. In the above example, if the students are not available for an oral test, one can check against the background of the students to see if the thinking and capabilities represented in the projects/papers are compatible with the thinking and capabilities suggested by their past background. Has the student consistently performed well earlier? or, has the student been caught cheating earlier? etc.

These everyday practical rules show why documentary evidence is far less important than epistemological evidence, though the application of epistemological evidence may not be a mechanical matter.

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2. This assertion goes well beyond what Toynbee has called "barbarian incursions". Arnold J. Toynbee, *A Study of History*, abridgement in 2 vols. by D. C. Somervell, Oxford University Press, 1957.
3. D. Gutas, *Greek Thought, Arabic Culture: the Graeco-Roman Translation Movement in Baghdad and Early Abbasid Society (2nd–4th/8th–10th Centuries)*, Routledge, London, 1998, p. 37.
4. Strabo, *Geography*, 13.1.54.
5. Plato, *Apology*, trans. Benjamin Jowett, Encyclopaedia Britannica, Chicago, 2nd edn. 1990, pp. 201–04.
6. Plato, *Apology*, pp. 201–04.
7. It is also not clear why, under these circumstances, Aristotle would not have translated any of the more religious or "spiritual" sorts of texts—though it is understandable why later-day historians would have been concerned with giving Aristotle a theologically correct image of being concerned exclusively with scientific works.
8. Clarence A. Forbes, "Books for the burning", *Transactions of the American Philological Society* **67** (1936) pp. 114–25.
9. Anthony Pym, *Negotiating the Frontier: Translators and Intercultures in Hispanic History*, Jerome Publishing, Manchester, 2000. Sourced from the draft at the website: http://www.fut.es/~apym/on-line/studies/toledo.html.
10. The Roman empire was not only tolerant when it was pagan, but (setting aside later-day forgeries) was not even aware of the existence of the Christians until the 3rd c. CE. As Gibbon remarks, "The total disregard of truth and probability in the representation of these primitive martyrdoms is occasioned by a very natural mistake. The ecclesiastical writers of the fourth or fifth centuries ascribed to the magistrates of Rome the same degree of implacable and unrelenting zeal which filled their own breasts against the heretics or the idolaters of their own time.... The learned Origen... declares, in the most express terms... that the number of martyrs was very inconsiderable. Dionysius..., in the immense city of Alexandria, and under the rigorous persecution of Decius, reckons only ten men and seven women who suffered for the profession of the Christian name." Gibbon goes on to add in a footnote that "The abbreviation MIL which may signify either soldiers or thousands is said to have occasioned some extraordinary mistakes" with estimates of "10,000 Christian soldiers crucified in one day". E. Gibbon, *Decline and Fall of the Roman Empire*, vol. 1, Encyclopaedia Britannica, Chicago, 1996, chp. 16, p. 217, and note 74, p. 731. Indeed, the simple fact is that the Roman empire, when pagan, tolerated Origen, who was then the spokesman of Christianity, while after turning fully Christian, the Roman empire under Justinian tolerated neither pagans nor even Origen! The lack of a serious response to Gibbon is evident from the fact that those unable to contest his arguments are compelled to fall back on allegations of an anti-Christian bias just because his history does not agree with church propaganda.
11. Richard C. Taylor, "A critical analysis of the Kalam fi'l mahd al-khair", *Neoplatonism and Islamic Thought*, ed. Parvez Morewedge, New York, 1992, pp. 11–40.
12. Thomas Heath, *A History of Greek Mathematics*, Dover, New York, 1981, p. 360.
13. Joseph F. O'Callaghan, *A History of Medieval Spain* Cornell University Press, Ithaca, 1975, p. 313.
14. Adelard of Bath, *Dodi Ve-Nechdi*, ed. and trans. H. Gollancz, London, Oxford University Press, 1920.
15. Here the term *aql*, commonly understood as intelligence or the mental faculty, does *not* relate to a *mechanical* process of deduction, as reasoning is today understood, but relates to a *creative* process of inference. Thus, this understanding of reason (*aql*) is also in contrast to the understanding of inference in the present-day notion of mathematical proof which was conceived by Hilbert, following Christian rational theology, as being necessarily of the sort that can, in principle, be checked *mechanically* by a machine which lacks intelligence. This creative understanding of reason is also clear from the fact that the Mu'tazilah grew from the *qādariyā*, i.e. those who believed in free will (*ikhtiyār*), suspension of divine will (*tafwīd*) or the human ability to create (*qudr, kudarat*), as opposed to those who believed in compulsion (*jabr*), i.e., that Allah could compel human actions. This belief in human creativity was a natural consequence of their belief in immanence—and not even traditionalists like al Ghazālī denied *that*.
16. *Encyclopaedia Britannica*, article on Khosrow I.

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25. For an eloquent account, see Steve Newcombe, "Five hundred years of injustice", http://ili.nativeweb.org/sdrm_art.html.
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27. Davenport, cited above, pp. 61–68.
28. "Ask of me, and I will make the nations your inheritance, the ends of the earth your possession. You will rule them with an iron scepter; you will dash them to pieces like pottery."
29. "May the praise of God be in their mouths and a double-edged sword in their hands, to in ict vengeance on the peoples, to bind their kings with fetters, their nobles with shackles of iron, to carry out the sentence written against them. This is the glory of all his saints. Praise the Lord."
30. As cited from Las Casas by Robert Francis, "Two kinds of beings: the Doctrine of Discovery and its implications for yesterday and today", web article at http://www.manataka.org/page94.html. The original work of Bartolomé de Las Casas, *A Brief Account of the Devastation of the Indies (1542/52)* can be found in various translations: e.g., *Bartolome'de las Casas—A Short Account of the Destruction of the Indies*, ed. and trans. Nigel Griffin, Penguin, 1992, and Bartolomé de Las Casas, *The Devastation of the Indies: A Brief Account*, trans. Herma Briffault, Johns Hopkins University Press, Baltimore, 1974.
31. Further numerous Biblical citations may be readily found in Ruth Hurmence Green, "Mass killings ordered, committed, or approved by God", *The Born Again Skeptic s Guide to the Bible*, 4th edn., Freedom from Religion Foundation, Madison, Wisconsin, 1999, chp. 5. It should be noted that these citations are all derived from the Latin Vulgate first prepared by Jerome, in the mid-5th c. CE. That Isaac Newton was right in supposing that the original version of the Bible was substantially different is confirmed by the meeting at Udayamperoor, "Synod of Diamper", in 1599, during which the Portuguese tricked the Indian bishops and burnt almost all early Aramaic versions of the Bible available in India. For some details about Newton's quest, see, C. K. Raju, "Newton's Secret", chp. 4 in *The Eleven Pictures of Time*, Sage, 2003. For details about the synod, see, e.g. K. R. N. Swamy, "Lost Aramaic Bible", Deccan Herald, Sunday, 11 April 2004.
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42. As a side effect of this Hilarius effort, Dionysius Exiguus, in the 6th c. CE adopted the cycle of 532 years, for the date of Easter to repeat, where $532 = 19 \times 4 \times 7$, 19 being the rough luni-solar cycle ("Metonic"

cycle), 4 being the cycle of leap years on the Julian calendar, and 7 being the cycle of the weeks. He set the beginning of the Easter calendar, or the beginning of the first cycle at 532 years before him, and he (or someone after him) seems to have identified the birth of Christ at 25 December, exactly nine months after equinox (25 March) at the beginning of calendar, thus creating the nomenclature AD and BC. The Hilarius evidence incidentally shows that it is false to claim that the church was disinterested in astronomical knowledge—the fact is that it tried and failed to obtain this knowledge in the 4th c. and again in the 6th c. CE.

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57. Joseph Needham, *The Shorter Science and Civilization in China*, abridged by Colin A. Ronan, Cambridge University Press, 1981, vol. 2, p. 43.
58. Al Bīrūnī, *Kitab al Hind*, trans. E. C. Sachau, *Alberuni s India*, reprint, Munshiram Manoharlal, New Delhi 1992, vol. 1, p. 22.
59. The church–state relationship based on Augustine's theology related to this principle of priority. The particular relation of the church and the state based on Augustinian theology required not only an *eternal* heaven and hell but also a principle by which priests could explain to people how their God would be able to decide whom to send where. This was the principle that causes could be localized within individuals. Hence the belief that, for any innovation, a causal analysis, extended backwards, would invariably terminate in single individual who had the priority. This is not an antiquated belief but decides the direction in which the Dunkel Draft has required us to modify our patent laws. More details may be found in C. K. Raju, "The curse on cyclic' time", *The Eleven Pictures of Time*, Sage, 2003, chp. 2.
60. For a short account of this quarrel, see the last but one paragraph of Stephen Hawking, *A Brief History of Time*, Bantam, New York, 1988.
61. For example, the *Rahmani* of Kunhi Kunhi Koya of Kavaratti, Lakshadweep, which contains tables from a British sailing manual, but is attributed to the legendary Ibn Majid.

62. C. K. Raju, "Kamal or Rapalagai", *Indo-Portuguese Encounters: Journeys in Science, Technology and Culture*, ed. Lotika Varadarajan, Indian National Science Academy, New Delhi, and Universidade Nova de Lisboa, Lisbon, 2006, vol. 2, pp. 483–504.
63. R. N. Saletore, *Early Indian Economic History*, Popular Prakashan, Bombay, 1993. Xinru Liu, *Ancient India and Ancient China: Trade and Religious Exchanges, AD 01-600*, Oxford University Press, Delhi, 1994.
64. Pliny, *Natural History*, II, Book VI, chp. 26, p. 63; cited in R.N. Saletore, ibid., p. 88. The book is also found online at urlhttp://www.perseus.tufts.edu/cgi-bin/ptext?lookup=Plin.+Nat.+6.26, and VI.26 there has the slightly different reading: "in no year does India drain our empire of less than five hundred and fifty millions of sesterces, giving back her own wares in exchange, which are sold among us at fully one hundred times their prime cost."

CHAPTER 7

How and Why the Calculus Was Imported into Europe

The European navigational problem and its solution available in Indian books easily accessible to Jesuits

OVERVIEW

MEDIEVAL Europe was extremely poor—spices commanded a high price because the little stored animal esh available to Europeans during long winters used to stink too nauseously to be eaten without spices. It is true that the church did nurse the military ambition of overcoming the reverses suffered during the Crusades by teaming up with "Prester John". However, during 1500–1700, Europe was far too weak and technologically backward to even attempt to conquer India except by religious conversion of the king, *à la* Constantine, attempted with Akbar in 1580. Thus European states turned to state-sponsored trade: the great European dream was to acquire wealth through direct trade with India in spices, bypassing the Arabs (and the Florentine merchants).

This required secure trade routes across the sea; hence, a good technique of navigation, but (exactly as in Toynbee's model of "barbarian incursions") Europe, being technologically backward in every department, was then ignorant also of navigation. The European navigational technique of "dead reckoning" required charts, which did not then exist since charts were not much used by Indo-Arabic navigators. Hence Vasco da Gama could not navigate across the Indian ocean and required the help of an Indian navigator Malemo Kanha—who used the technique of celestial navigation without maps already explained earlier. Thus, technologically backward Europeans had overwhelming motivation to learn about navigation and associated matters from India. During 1530–1761 various European governments recognized the European ignorance of navigation, and repeatedly offered huge prizes to anyone who could obtain or develop a reliable technique of navigation.

Latitude and Calendar

The first navigational problem was that of fixing latitude at sea. At the time of Vasco da Gama, Europeans, in fact, could not even determine latitude from observation of solar altitude at noon. One reason for this was that the European calendar (Julian calendar), till then used only for ritual religious purposes, had drifted way off the mark. The Julian calendar was erroneous because the Romans, lacking a good command over elementary arithmetic, found it difficult even to *articulate* the correct length of the (tropical) year, and had simplified matters to represent it by a nice rounded fraction: $365\frac{1}{4}$ days. This amounted to an error of 1 day in a century, which had accumulated to about 10 days by the 16th c. However, calendar reform affected the observance of key religious rituals like Easter—fixing the date of which was the sole point on the agenda of the Nicene council. Hence, changing the calendar was a tricky matter, especially in Europe in the days of the Protestant reformation and the Inquisition. Furthermore, the trickiest part was that Europeans did not know for sure what the exact length of the year ought to be: for Europe then lacked the observational base and the scientific knowledge of astronomy needed to determine that. The calendar reform was based on documents rather than replicable observations; *hence* Protestants remained unconvinced about the need for calendar reform, and this had to wait another 170 years, until 1752, for Protestant countries to accept it. Accordingly, this knowledge of the length of the year could only have come from outside, as the bull (fatwa) of Gregory states (though there may have been other sources that went unmentioned). The reform of the calendar only solved one part of the latitude problem, and precise trigonometric values were still needed to determine latitude from observations of solar altitude at noon, as described, for example, by the *Laghu Bhāskarīya* from a thousand years earlier.

The Jesuits in Cochin

The first batch of Catholic missionaries had arrived in Cochin in 1500. This happened because the Portuguese, lacking money for trade, abortively tried to muscle in on the long-established Arab and Florentine trade in Calicut, and were then forced to ee Calicut. They were guided to Cochin, then hostile to Calicut, by the Gujarati assistant assigned to the Portuguese by the Samudiri of Calicut. Here, the Catholic missionaries quickly established themselves with the help of the Raja of Cochin, and fanned out into the interior of Kerala, with the help of the substantial indigenous population of Syrian Christians, in the vicinity of Cochin. This was exactly in conformity with their pre-planned "Prester John" model of conducting religious war (Crusades) by establishing linkages with Christians living in or behind the "enemy" camp.

To further these linkages, the missionaries established their first college in Cochin, and this was taken over by the Jesuits in 1550, and is recorded to have been ourishing with a couple of hundred students, mostly Syrian Christians, by 1590. Outwardly, the Jesuits posed as holy men who were engaged in missionary activity: by the 1570's the Jesuits had established printing presses in local languages such as Tamil and Malayalam (with which, of course, they were very thoroughly conversant, even starting the first dictionaries in these languages). Their aim was to use the new technology to aggressively propagate their version of the Bible, translated from Latin, which differed substantially from the locally available Bible versions in or from Aramaic.

In reality, however, the missionary and the Jesuit differed radically from the then-prevailing Indian idea of a holy man—as one who had abandoned all worldly pursuits. Thus, the real aim of the Jesuits was to capture state power, and, though no one suspected them, they doubled as military spies, routinely sending back military intelligence in their despatches. It is well known that various earlier models of attaining and retaining state power by using religion were tried out in India: these included (1) the "Constantine model" (an attempt was made to conquer India by converting "the Grand Moghul", Akbar, in 1580), (2) the "Alexandrian model" (all temples were destroyed in Goa, 1523–1540), and (3) the Inquisition model of weeding out the disaffected (imposed in Goa in 1560). It is not so well known that (4) the "Toledo translation model" was also replicated in the Cochin college, where state power was supportive, and the Syrian Christians played the role of the Mozarab intermediaries of Toledo. Thus, the Jesuits were also actively collecting all possible locally available information in books, translating them, and despatching them to Europe, in factory mode, following the Toledo model.

However, while they had no difficulty in understanding the local languages, and in translating many of these books, they initially had a difficulty with the mathematics used in the Indian calendar, because the only mathematics that Jesuits studied was the mathematics of argument and proof found in the (European version of the) *Elements*, which was quite useless for this purpose of calculation. *For this specifically stated reason*—that Jesuits "were forced to fall silent" when matters related to astronomy and the calendar were raised in foreign lands—Christoph Clavius reformed the Jesuit syllabus at the Collegio Romano, including in it practical (as distinct from Platonic or Neoplatonic) mathematics. Among the first students of this modified syllabus was Matteo Ricci, who thereafter visited Coimbra to learn about navigation, and then travelled to India, and in particular Cochin. Matteo Ricci remained devoted lifelong to his teacher, Christoph Clavius, who also headed the Gregorian calendar reform committee. Shortly before the calendar reform, Ricci wrote saying that he was looking for an "intelligent Brahmin or an honest Moor" to explain the Indian methods of timekeeping.

Needless to say, the Indian infinite series were (and are) widely available in calendrical texts distributed around Cochin, and the authors of some of these texts, such as Śaṅkara Vāriyar, and his brother Narāyāṇa (part author of *Kriyākramakarī*), shared with the Portuguese a common patron in the Raja of Cochin. It was to these persons that the Jesuits would have turned for a knowledge of the Indian calendar, which knowledge they also needed simply to be able to operate in India, because the sophisticated Indian calendar worked on principles different from their own simple count of civil days, so that numerous Indian festivals were all on "moveable" dates on the Julian/Gregorian calendar. While it was not necessary to go out of Cochin to obtain the texts on Indian astronomy and the calculus, basically written in the context of the Indian tradition of *jyotiṣa*, or timekeeping, used for calendar-making, the Jesuits were by no means confined to Cochin: as already noted, the Catholic/Jesuit missionaries had also established deep inroads into the interiors of Kerala with the help of the indigenous Syrian Christians, with whom they were on the most cordial terms until about 1600 (when the Portuguese tricked the Syrian Christians, and burnt almost all copies of the Indian Bibles in Aramaic, because they disagreed so much with the Latin version). That Jesuits had been studying the Indian astronomy and calendrical texts for some time is clear from the polemic against the *Vedāṅga Jyotiṣa*, by the Jesuit de Nobili in ca. 1610.

Trigonometric Values, Loxodromes, and Mercator s Chart

Apart from the calendar, the second problem faced by European navigators was the lack of precise trigonometric values. These were needed for determining latitude from observations of solar altitude at noon, following e.g. the formula in the *Laghu Bhāskarīya*. They were needed also for calculating loxodromes. European navigators were accustomed to using charts and "dead reckoning". The high value of Mercator's chart arose from the fact that it showed loxodromes as straight lines, thus enabling a course to be set using dead reckoning. However, a precise table of secants, and something equivalent to the fundamental theorem of calculus was needed to calculate this chart. But the mysterious source of Mercator's precise trigonometric values, and his technique, remains unknown to this day. Mercator, who worked with Gemma Frisius at the Catholic University of Louvain, obviously had privileged access to information brought in by sailors and priests returning from India and China, via Antwerp. So it is hardly surprising that the "Mercator" projection is identical with a projection used in maps of the celestial globe from China from at least five centuries earlier—and the same principle could obviously be applied to the terrestrial globe. However, since Mercator was arrested by the Inquisition, and was lucky to escape with his life, it is also not surprising that he kept his "pagan" sources of information a closely guarded secret. The tables of trigonometric values published by Clavius, in 1608, used the Indian definition of sines and cosines, and the then common Indian value for the radius of the circle. Hence, these tables far exceeded in accuracy the "tables of secants" provided by earlier navigational theorists like Stevin for calculation of loxodromes, which were (at the accuracy of) Āryabhaṭa's values, known to the Arabs. It is hard to see how such accuracy (unprecedented for Europe) could even have been attempted without calculus techniques. Clavius, who authored the calendar reform proclaimed by pope Gregory, certainly had access to every bit of information brought in by the Jesuits, but could hardly be expected to be truthful enough to acknowledge *his* "pagan" sources. Since Clavius' tables were published several years before the first hint of the calculus "officially" appeared in Europe in the works of Kepler, and since Clavius provides no explanation of his method, it remains a mystery how these high-precision trigonometric values were calculated. The only reasonable explanation is that like his contemporaries, Tycho Brahe, who merely articulates Nīlakaṇṭha's astronomical model, or Scaliger, whose "Julian" day number system copies the Indian ahargaṇa system, Clavius obtained his trigonometric values from India.

Longitude and the Size of the Earth

The third problem faced by European navigators was the difficulty in determining longitude. Here, they had difficulty in adopting the Indian techniques, because these techniques for determination of longitude required (a) precise knowledge of the size of the earth, and (b) an ability to do mental calculations. Because Columbus had fudged the size of the earth, making it $\frac{1}{4}$th its actual size, and his fudged value acquired currency in Europe, Europeans lost the precise knowledge of the size of the earth, available to Arabs, and to Indians from at least a thousand years earlier. The wrong European estimate of the size of the earth led to navigational disasters, so that carrying of globes aboard ships was banned by Portugal in 1504. Picard's re-determination of the earth's size in 1671 was a long time in coming, and was not immediately accepted by European navigators, who remained at sea about the precise size of the earth during 1500–1700. Hence, Europeans could not use the Indian technique of longitude determination. Further, before Clavius, neither the algorismus nor practical mathematics were part of the curriculum in Europe, except among Florentine merchants who kept it a sort of trade secret, so there was the absence of training in mental calculation even among navigators. The situation was worse among common sailors who were rarely educated, given that living conditions on European ships were so harsh and filthy, and so very hazardous (with an over 30% rate of mortality per trip). However, these were the very people who would have had to navigate the ship if something happened to the navigator. Accordingly, there was a cultural expectation of a mechanical way to do the calculation for longitude. Hence also Europeans were unable to use the Indian techniques of longitude determination, and went in a different way, ultimately developing the marine chronometer in the latter half of the 18th c. CE. However, the idea of a prime meridian (of Greenwich) for measuring longitude differences obviously copied the Indian idea of the prime meridian of Ujjayinī.

From astronomy to technology and zoology, knowledge from India and China was pouring into Europe (although Europeans refused to acknowledge this as a matter of religious belief). However (as in the case of knowledge of longitude determination), not all the knowledge so obtained could be immediately used by the Europeans, since they failed to comprehend it or make it compatible with their then-existing epistemic frame. The trail of circumstantial evidence leads from Mercator to Clavius, Scaliger, and Tycho Brahe, to Kepler and onwards, suggesting that the trigonometric values of higher precision were understood first, followed by the Indian planetary models—for both of which precedents (via Arabs) were already available in Europe. Because of this difficulty of comprehension, it was about about a century after the formation of the Cochin college that the Indian infinite series explicitly appears in Europe in the works of Cavalieri, Fermat, Pascal, Gregory, etc., beginning 1630. (Cavalieri was a student of Galileo, whose access to the Jesuit sources in the Collegio Romano is well documented, and whose difficulties with the infinite are articulated in his correspondence with Cavalieri.) Fermat and Pascal use the earlier Indian method of summing these infinite series, and relate them to the calculation of area in the manner of Bhāskara II, while the Gregory series too related to the European calculations of the value of π. Fermat's challenge problem to European mathematicians is a solved exercise in Bhaskara II, and the large numbers involved make it clear that Fermat had access to Indian sources. None of these European mathematicians was able to explain the infinite series to their contemporaries, any more than Newton and Leibniz; this explanation had to await the formalisation of the real numbers within set theory, which was itself formalised only in the 1930's.

We compare the evidence of transmission with the standard previously stated: motivation, opportunity, circumstantial, documentary, and epistemological evidence. (To provide a self-contained and coherent account in one place, this chapter repeats some of the material that has already been covered in earlier chapters.)

I
INTRODUCTION

The calculus has played a key role in the development of the sciences, starting from the "Newtonian Revolution". According to the "standard" story, the calculus was invented independently by Leibniz and Newton. This story of indigenous development, *ab initio*, is now beginning to totter like the story of the "Copernican Revolution".[1]

The English-speaking world has known for over one and a half centuries[2] that "Taylor" series expansions for sine, cosine, and arctangent functions were found in Indian mathematics/astronomy/timekeeping (*jyotiṣa*) texts, and specifically in the works of Mādhava, Nīlakanṭha (*Tantrasaṅgraha*, 1501 CE), Śaṅkara Vāriyar (*TantrasaṅgrahaVyākhyā*), Jyeṣṭhadeva (*Yuktibhāṣā*, ca. 1530 CE), *Kriyākramakarī*, etc. A numerically efficient algorithm for computing with the series led to a 9 decimal-place precision table for the sine, cosine, and arctangent functions. These tables of sines and cosines, which make more precise Āryabhaṭa I's earlier table of 24 sines and cosines, are stated compactly in two verses using sexagesimal, *kaṭapayādi* notation. These verses are also found in various widely distributed texts like the *Karaṇapaddhati*[3] used to this day. By means of an accurate correction term, rapidly convergent versions of these infinite series were developed, and these were used to calculate accurately the value of π to 11 decimal places. These things are, by now, well known,[4] and we have already seen the details in Chapter 3.

No one else, however, has so far studied the connection of these Indian developments to European mathematics: what relation, if any, exists between the Indian infinite series, and the calculus development credited to Newton and Leibniz?

It is important to examine this relation on two planes: the epistemological and the historical. This accords with the idea that the history of mathematics without its philosophy is blind, just as much as the philosophy of mathematics without its history is lame. This is an idea particularly important for historians of mathematics in India, for they have taken the present-day formalist philosophy of mathematics as a given; they have slavishly accepted it as universal, across cultures and time, and have neglected the historical and geographical variations in the epistemology of mathematics. This leads to difficulties even in understanding basic notions of Indian mathematics, such as numbers and the concept of *śūnya*, as I have earlier[5] emphasized. Certainly it leads to difficulties in understanding Mādhava's infinite-series expansions, and in classifying this as calculus. These epistemological difficulties have already been comprehensively examined earlier[6] in Chapters 1, 2, and 3. As we shall see later on, it is only by addressing these epistemological issues that one can gain insight into the difficulties that accompanied the arrival of the calculus in Europe. Accordingly, the present chapter will focus on the historical dimension.

Historically, to relate Mādhava's sine, cosine, and arctan series expansion to the European use of the calculus, it is convenient to consider two stages: (1) the import of these infinite-series techniques into Europe, and (2) the dissemination of those techniques within Europe. This chapter will focus on the first stage. (The second stage, though interesting in its own right, is outside the scope of this book, and we will consider it only in so far as it has a bearing on the first stage.) Brie y, the import of the infinite-series techniques into Europe relates to the requirements of the European navigational problem, the foremost scientific and technological problem of the time in Europe. The navigational problem related to mathematics and astronomy via celestial navigation, and spherical trigonometry. In particular, precise trigonometric values were needed and used to calculate the three "ells": latitude, longitude, and loxodromes.

II
EUROPEAN NAVIGATION IN THE 16TH C. CE

Dead Reckoning and Charts

To start with, let us observe that navigational techniques in Europe, at the end of the 15th c. CE, were quite primitive compared to the then-prevalent state of the art. European navigational techniques at the end of the 15th c. CE were, in fact, confined to dead reckoning, a system peculiar to Europe, and adapted to the Mediterranean sea.

Dead reckoning (short for "deduced" reckoning[7]) is a system of navigation in which the position of a ship is estimated geometrically by using (a) a chart, (b) an estimate of the ship's speed, and (c) the course direction. A circle is drawn with centre at the last known position, and with radius given by the distance travelled. The distance travelled is estimated from the ship's speed and the time travelled. The ship's course is plotted on this chart, and the graphically calculated point of intersection is used as a new estimate of the ship's position. This cumbersome geometric method requires reliable charts, drawing instruments, a magnetic compass, a clock, a log, and a log book, to maintain a continuous record of speed and direction. Despite this impressive array of navigator's paraphernalia, the dead-reckoning method was excessively inaccurate for various reasons.

Though navigators naturally stressed the absence of reliable charts of "unexplored" regions, the unreliability of charts was not the sole reason for the unreliability of dead-reckoning. Each instrument introduced its own error: e.g. the magnetic compass could be unreliable because of imperfect suspension (especially in a ship which is rocking and rolling), because of magnetic variations and anomalies, and because of the deviations of the magnetic north from the true north. Contemporary Arabian sailing manuals recognized the unreliability of the magnetic compass.[8]

Heaving the Log, and the Log Book

Probably, the greatest error in the European technique of dead-reckoning was introduced by the crude technique of measuring the distance travelled. Initially this was reckoned in terms of the number of days of sail, analogous to the Arabic *zām*.[9]

Later on, distance travelled was calculated from the ship's speed. The speed itself was measured by tossing a log overboard, and measuring (a) in how much time it oated past the ship, or (b) how much rope went out in a given time, and then continuously recording this in what was naturally called a log book. The second method was standardized to "knots" by measuring the length of the rope using knots at regular intervals. Later the distance between knots was standardized at $47\frac{1}{4}$ feet (14.3 m). Time was measured using a sand-glass of 28 seconds which was inverted as soon as it emptied. This standardization took place only in the mid-17th c. (Richard Norwood recommended this in 1637), so that a speed of one "knot" came to 1 nautical mile (= 6076 feet, or 1853 m) per hour. This grossly inadequate method of measuring speed remained in use for some three-and-a-half centuries, even in to the mid-19th century (by which time all problems related to latitude and longitude determination were conceptually settled, according to historical accounts, and the patent log was slowly coming into use). As recorded by a European sailing manual of the mid-nineteenth century, numerous precautions were necessary because of the inaccuracies due to the log:[10]

> ... if the gale has not been the same during the whole hour, or time between heaving the log, or if there has been more sail set or handed, there must be an allowance made for it, according to the discretion of the officer. Sometimes, when the ship is before the wind and a great sea is setting after her, it will bring home the log; in such cases it is customary to allow one mile in ten, and less in proportion if the sea be not so great; a proper allowance ought also to be made if there be a head sea. In heaving the log, great care should be taken to veer out the line as fast as the log takes it; for if the log be left to turn the reel itself, it will come home, and give an erroneous distance.

In addition to variations in wind velocity, the sampling might be biased, the mean might have a large variance, and so on.

The Navigational Skills of Columbus and Vasco

In contrast, celestial navigation was the method of choice prevailing in the Indian ocean, and long used by Indian, Arabian, African, and Chinese seafarers. Though this method used no charts, and very little by way of instrumentation, it was a lot more reliable than dead-reckoning techniques, even up to the middle of the nineteenth century, by which time the unreliable European navigational instruments of the 16th c. CE had improved to the point that Europeans had started poking fun at the parsimony of instruments in the Indo-Arabic technique of navigation.

Both Columbus and Vasco da Gama used dead reckoning and were ignorant of celestial navigation.

> Now let us look at Columbus's ability at celestial sights....His first recorded attempt at using a quadrant to establish his latitude was on 2 November when he was off the northern shore of Cuba. This sadly erroneous sighting put him on the latitude of Cape Cod. Even so, Columbus failed to recognize this gross error and instead concluded that he was... on the mainland of Cathay.... [This] illustrates Columbus's serious incompetence in celestial navigation. Columbus tried the quadrant again on 20 November and came up with the same deplorable result of 42 degrees north latitude, but this time he realized that something was wrong and blamed it on the quadrant which he said was broken and needed repair. How can a quadrant be broken when it has only one moving part and that part is a string with a weight on the end?[11]

Vasco da Gama was not much better off. He observed the Indian pilot using the *kamāl*, a simple but sophisticated instrument which consists of a couple of pieces of wood and some string, and is used to measure local latitude, by measuring the altitude of the pole star. The instrument is held level with the eye, and the knots on the string are counted by keeping the string between one's teeth; hence the name *kau* for the pole star, for *kau* also means teeth. Vasco da Gama thought that the pilot (Malemo Cana) was telling the distance by his teeth![12]

Governmental Intervention

This ignorance of a proper technique of navigation was a very painful matter for Europe, since navigation was both strategically and economically the key to the prosperity of Europe of that time. Europe of that age was exceedingly poor—the most prosperous parts of it were Spain and Portugal, just emerging from Arab colonization. In the language of Toynbee, the Europeans were the "external barbarian" intruders in the civilized world of Indians, Arabs, Chinese, Africans, etc. The European dream of riches lay in trade via the sea route they had recently learnt about. The absence of a good technique of navigation made this trade very risky, and each sunken ship meant great loss of wealth. Accordingly, a good method of navigation was of great commercial importance to Europe in the 16th c. CE.

However, a peculiar and novel aspect of European trade and commerce was that it involved various governments in Europe. Though this may seem very natural to us today, this was then in stark contrast to the prevailing Indian, Arabic, Chinese, and African practice, where trade was traditionally carried out between individuals—and the state only assisted the process. Since the European states themselves were engaged in trade, this trade inevitably involved war or armed con ict of some sort. Naval force was used to attack competitors. European trade, thus, required troops to be moved across long distances over sea, and a sunken ship also meant more loss of life and wealth than in an actual con ict. Thus, a good method of navigation was also of very great strategic importance for Europe in the 16th c. CE. (The subsequent history of tiny Britain attests to the importance of naval skills in that era.)

Accordingly, various European governments had no hesitation in acknowledging their ignorance of navigation, while announcing huge rewards, from the 16th to the 18th c. CE, to anyone who developed an appropriate technique of navigation. These rewards spread over two and a half centuries from the appointment of Pedro Nunes as Royal Cosmographer in 1529, to the Spanish government's prize of 1567 through its revised prize of 1598, the Dutch prize of 1636, Mazarin's prize to Morin of 1645, the French offer (through Colbert) of 1666, and the British prize legislated in 1711, which was eventually claimed in 1762 by Harrison, and paid in 1773. Many key scientists of the time (Huygens, Galileo, etc.) were involved in these efforts: the navigational problem was the specific objective of the French Royal Academy, and a key concern for starting the British Royal Society. European governments were also in fierce competition with one another, and the above sequence of prizes accurately re ects the successive dominance of the Portuguese, the Spanish, the Dutch, the French, and then the British.

Thus, for over two and a half centuries, from the beginning of 16th to at least the middle of the 18th c. CE, the European method of dead reckoning remained unreliable, and the navigational problem remained one of the foremost scientific and technological problems in Europe. European governments, combatively engaged in trade, were acutely aware of this problem, and did everything possible to support the search for a solution.

III
LATITUDE AND CALENDAR VS LONGITUDE AND CHRONOMETER

Though celestial navigation (hence mathematics and astronomy) was the actual focus of the European attack on the navigational problem, the present-day depictions of these events culminate in a triumphant account of the development of yet another navigational instrument: the marine chronometer, in the mid-18th century, and its use in determination of longitude.[13] Though Harrison's 1760 chronometer may have been accurate, these historical accounts of the chronometer are inaccurate on two counts. First, the chronometers in general use remained somewhat unreliable, even until a century later. As a sailing manual of the mid-19th c. records the chronometer still had to be treated as a delicate and pampered pet:

> In winding up a chronometer that is going, great caution should be observed, not to give it a circular motion, which would alter its rate some seconds, or perhaps even stop its going; but when a chronometer... has once stopped, though for ever so short a period,... no reliance can be placed on its performance, until its rate be proved by subsequent observations.... A chronometer should be wound up regularly at the same time of the day, and great care taken not to give the key, first half a turn, then a whole turn, afterwards three quarters, and so on; for this irregular mode of winding up will sometimes very materially alter its rate, and should be as carefully avoided as circular motion.[14]

Secondly, these triumphant accounts of the chronometer and its use in longitude-determination have overlooked the following. Navigation required the determination of *both* latitude and longitude. But, in the 16th c. CE, European navigators did not know how to fix *either*. Though the longitude problem has recently been highlighted, this was preceded by a latitude problem, and the problem of loxodromes, both of which were key issues in 16th century Europe.

As already noted above, Vasco da Gama and Columbus knew only dead reckoning, and were ignorant of celestial navigation, even in the matter of determining latitude. (To measure latitude, Vasco da Gama carried an astrolabe that could be used only on land.) Naturally, the European navigators of the time could see that the superior navigational techniques of the Arabs and the Indians did not rely on charts, and so could not have used dead reckoning. Like Vasco da Gama, they tried hard to copy these techniques, to bring their own navigational technology up to date, even though they did not then understand the simple principles of these instruments. Vasco da Gama, for instance, carried back a copy of the *kamāl* to have it graduated in inches, oblivious to the fact that the instrument used a harmonic scale and hence could not be graduated according to a linear scale!

Latitude Measurement

How is latitude to be measured? One method was to use instruments like the *kamāl*, and the cross-staff or sextant to measure pole-star altitude. By about the mid-16th c. Europeans had learnt this method from Indo-Arabic navigators. However, to travel (by sea) from Europe to India one must cross the equator, and the pole star ceased to be visible well before that. Moreover, there is no similar star in the southern hemisphere. Furthermore, this method was applicable only at night.

Latitude Measurement in Day Time and the Solar Declination

How is latitude to be measured in day time or near the equator? The solution to the problem is described in traditional timekeeping texts, like the 7th c. CE *Laghu Bhaskarīya*,[15] and was known to Arabs from the 9th c. CE. This traditional Indian solution to latitude measurement involves measurement of the solar altitude at noon. Noon is relatively easily identified as the time of the day when the shadow is the shortest, or the time when the shadow just stops becoming shorter and starts lengthening. (This time can also be identified by drawing a circle around the gnomon and bisecting the angle formed between the lines joining the centre of the circle to the two points at which the shadow just touches the circle.) Likewise solar altitude may be measured by any instrument used to measure angles, such as the *kamāl*, a cross-staff, or a sextant, or any of the numerous European instruments that were devised in the 16th and 17th c. specifically for measuring solar altitude.

The situation aboard an English ship is described picturesquely by a traveller.[16]

> Every day, about the hour of noon, the Sun's altitude was infallibly observ'd, not onely [sic] by the Pilots, as the custom is in all ships, and the Captain..., but ...there was no day, but at that hour twenty or thirty mariners, masters, boys, young men, and of all sorts came upon the deck to make the same observations: some with Astrolabes, others with Cross-Staffs, and others with several other instruments, particularly with one ...lately invented by one David, and, for his name, called David's Staff [Davis Staff].

However, the key difficulty is this: the observed solar altitude at noon is a function of *two* variables: the local latitude *and* the solar declination. As described in the *Laghu Bhāskarīya* of Bhāskara I,[17] a widely circulated timekeeping manual,

$$\sin \quad = \sin \quad \sin a, \tag{7.1}$$

where is the declination, is the local latitude, and a is the solar altitude on the prime vertical (i.e., at noon).

Solar Declination, Equinoxes, and the Calendar

At a given place (i.e., holding fixed) the solar altitude at noon (i.e., a) keeps varying throughout the year as the sun's declination ( ) varies, as it is seen to move north, and then to the south, and then back. How is the solar declination to be determined? Roughly speaking, this varies sinusoidally with a periodicity of one year, and can today be easily calculated from the date. A simple possibility is to make a linear estimate. Since the maximum solar declination, of 23°27 during solstice, is known, hence the average solar declination can be calculated, and the solar declination on any given day can be estimated from a knowledge of the number of days elapsed since the equinox or solstice. (This simple method would not be accurate, since even Bhāskara I observes that the change in solar declination varies from day to day.) However, even this simple method of determining latitude was not available to the Europeans in the 16th c. Thus, to determine the latitude from a measurement of solar altitude at noon, it is necessary also to have a proper calendar which correctly states the days elapsed since equinox, and hence correctly identifies the days of equinox. This was readily possible in Indian tradition, for the same *Laghu Bhāskarīya*, for example, also (a) described various ways of determining the equinox, and (b) used a traditional system of day-count (*ahargaṇa*) which facilitated the counting of the days elapsed since the equinox.

The Erroneous Julian Calendar

However, at the beginning of the 16th c. the European calendar (Julian calendar) could not properly identify the dates of the equinox. The reason was that they had long been using a calendar with the wrong length of the year. The Romans with their clumsy mathematical notation (Roman numerals) could not do mathematical calculations easily. They relied on an abacus, and found it difficult to handle fractions. Accordingly, they had simplified the length of the (tropical) year to be $365\frac{1}{4}$ days, a figure that was incorrect in the second decimal place, and contrasted poorly with the contemporary 5th c. CE estimate of the (sidereal) year by Āryabhaṭa,[18] which was more than ten times more accurate. The erroneous Roman figure for the length of a year led to an error of one day in a century. By the 16th c. CE, this error had piled up to 10 days. From the above formula (7.1) relating solar declination to local latitude and solar altitude at noon, it is clear that an error of ten days in the calendar will lead to inaccuracies of about 3° in latitude determination, not counting any errors of measurement and calculation. Taking 1° as 60 nautical miles, this was an error of over 150 nautical miles! Hence, European navigational theorists soon realized that the solution of the latitude problem required a reformed calendar.

To summarize, in the 16th c. fixing latitude was a problem for European navigators. The time-measurement problem relating to latitude concerned the calendar, rather than the chronometer (a later development related to the longitude problem).

The European Focus on Mathematics and Astronomy

Traditional methods of calendrical timekeeping inevitably related to astronomy and mathematics, and this was true also of Europe. Accordingly, prior to the 18th c. marine chronometer, attacks on the European navigational problem in the 16th and 17th c. focused on mathematics and astronomy, which were (correctly) believed to hold the key to celestial navigation.

This led to a urry of scholarly activity, and numerous star charts were published in that period. The *Almagest* (*Syntaxis*), like the *Geographia*, attributed to Ptolemy, became very popular, as did the *Sphere* of "Proclus", and of Sacrobosco.[19] Further, it was widely (and correctly) believed by European navigational theorists and mathematicians (e.g. by Stevin[20] and Mersenne) that this knowledge of celestial navigation was to be found in the "knowledge of the ancients". This "knowledge of the ancients" very much included non-Greek sources, since Stevin, for example, repeats Herodotus' remarks that the Greeks were like children before the Egyptians. Stevin, incidentally, introduced Europe to the decimal system in 1585. In particular, while Europeans had a difficulty in *acknowledging* a theologically incorrect source of knowledge, they had no difficulty in *using* that knowledge after hiding the source, or crediting it to a theologically correct source. To give an analogy, this was exactly in accord with the idea of converting an existing temple into a church instead of demolishing it.

The Jesuits

While calendar reform for latitude measurement was high on the European agenda in the 16th c., and calendar reform required adequate knowledge of mathematics and astronomy to determine accurately the equinoxes, there was a further problem. A change in the dates of the equinoxes meant a change in the date of Easter, and changing the date of Easter was not a trivial matter in medieval Europe dominated by the Church. Indeed, the date of Easter practically signified the Nicene creed, for the sole point on the agenda of the Nicene Council (First Ecumenical Council) held in Constantine's court, was to fix the date of Easter. In medieval Europe, departure from the Nicene creed attracted charges of heresy, even among Protestants, and heresy meant social ostracism if not a painful death, so that even a Newton had to hide his heretical beliefs lifelong (and these views have largely remained hidden, even after his death, down to the present day). Accordingly, though dissatisfaction had been earlier voiced over the Julian calendar, since before Regiomontanus, the Julian calendar continued to be used, until reforming the calendar became a matter of overwhelming practical importance to the state. This happened just at the time that the Roman Catholic church had vigorously initiated the process of counter reformation to meet the emerging challenges to its authority. Eventually, a change in the date of Easter was authorized by the Council of Trent, which started in 1545, to spearhead the counter-reform.

This period saw the rise of the Jesuits, as part of the Church's program of counter-reformation. At that time, science had not yet championed the Protestant cause, even in the popular imagination, and the Jesuits, with their success in founding educational institutions, were at the forefront of European knowledge. A key Jesuit figure was Christoph Clavius who headed the Calendar Reform Committee of the church for the Gregorian calendar reform of 1582. Clavius studied in Coimbra under the mathematician, astronomer, and navigational theorist Pedro Nunes. Clavius lamented the Jesuit ignorance of mathematics and astronomy, and subsequently reformed the Jesuit mathematical syllabus at the Collegio Romano,[21] to change its orientation from spiritual mathematics towards practical mathematics. Clavius even wrote a text on practical mathematics.[22] Clavius, incidentally, remained in correspondence with his teacher Nunes during the period just prior to the calendar reform.

By this time, the Jesuits had established themselves in India, particularly in Cochin, and in Goa, where they had introduced the Inquisition by 1560. Because state and church were so closely intertwined, Jesuit priests often doubled as military spies, and sent back military information in their despatches. In any case, Jesuits were secretive, and many Jesuit documents remain a secret to this day. For Jesuits, conquest and conversion were related, and it is well known that in 1580 they sent a mission to Akbar's court, hoping to conquer India by converting the Moghul emperor, *à la* Constantine. Though the mission obviously failed, they maintained a continuing presence in the Moghul court. To this end of conquest through conversion, the Jesuits learnt the local languages with missionary zeal. Valignano declared that it was more important for Jesuits to know the local language than to know philosophy. By 1577 they had already started printing presses in Tamil and Malayalam, at Vapicota, with a view to translating and disseminating canonical literature in the local languages.

Cochin was (and still is) the centre closest to the various manuscript sources of infinite series, and Cochin was where the Jesuits had a very strong base. Cochin was a special focus of attention because of the presence in the vicinity of a large number of Syrian Christians whom they regarded (and still regard) as heretic Nestorians, but nevertheless saw them as their natural allies, like Prester John, or the Mozarabs. The Jesuits maintained a large army in Cochin and were involved in pearl-fishing. With their well-acclaimed acumen in setting up educational institutions, the Jesuits took over the Christian college in Cochin, and, according to the *Documenta Indica*, they were teaching Malayalam to the locals, at the latest by 1592.

By 1595 the Cochin college had a couple of hundred students, mostly "Thomas" Christians. These constituted a very useful source of local information for Jesuits.

To meet their objective of conquest through conversion, Jesuits naturally also studied and documented the local customs. They systematically collected and translated local manuscripts, and sent them back to Europe.[23]

> hardly seven years after the death of ... Francis Xavier the fathers obtained the translation of a great part of the 18 Puranas and sent it to Europe. A Brahmin spent eight years in translating the works of Veaso (Vyasa)... several Hindu books were got from Brahmin houses and brought to the Library of the Jesuit college.

The situation is reminiscent of the mass translations at Toledo. The Jesuits did study these translations, and even adapted their gospels accordingly to suit local customs and nomenclature. For example, the first book printed in Marathi, authored by Thomas Stephen (Thomas Estevao), in 1616, was called *The Christo Purāṇa*. The next was the *Purāṇa of St. Peter* by one Estevao de Cruz in 1629.[24]

The traditional Indian calendar must surely have puzzled the Jesuits, especially the way in which dates of local festivities were fixed. The Indian calendar has civil days as well as *tithī*-s. Festivals, however, relate to both lunar and solar cycles: Diwali, for example, is invariably on an *amāvasyā*, while Holi is always on a *pūrṇimā*. Since festivals relate to both lunar and solar cycles, *tithī*-s were (and still are) used for festivals, and this necessarily involves a system of intercalary days and months. Thus, it is non-trivial to correlate this calendar with the Julian calendar, which was a civil calendar, based solely on the solar cycle, having botched up the notion of "month", or a cycle of the moon, with various adjustments to suit the vanity of long-dead petty Roman despots.

For someone accustomed to one sort of calendar, it is very hard to understand the other sort of calendar. It is easy to get a feel for this difficulty: at least 9.9 out of 10 (elite) Indians today do not know *how* the date of a traditional festival is fixed, and will refer to the calendar to fix the dates of even important festivals such as Onam, or Diwali or Holi. Those being early days of mass-printing technology, mass-printed Indian calendars were not available so easily, and *pañcāṅga*-makers differed in their opinions! In any case, referring to a ready-made calendar is one thing, and understanding how it is made is quite another. For the Jesuits, seeking to understand local customs, it was surely important to understand how the calendar was made, especially after the Council of Trent in 1548 had declared the intent for calendar reform, making the calendar a hot topic of interest.

Calendar-making in India inevitably involved complex mathematics and astronomy, at least since the *Sūrya Siddhānta*. As already observed, the Jesuits were initially not sufficiently well-trained in mathematics and astronomy to understand how the *pañcāṅga* was made. After about 1575, however, Jesuits, like Matteo Ricci, who trained in mathematics and astronomy, under Clavius' new syllabus were sent to India. (Ricci also visited Coimbra and learnt navigation. He remained devoted to Clavius, and he later translated Clavius' books into Chinese.) In a 1581 letter to Petri Maffei, Ricci acknowledged that he was trying to understand local methods of timekeeping from "an intelligent Brahmin or an honest Moor".[25] Ricci had recently been in Cochin, close to Trichur, which was, then, the hub of mathematics and astronomy, since the Vijaynagar empire had sheltered it from the continuous onslaughts of raiders from the north. Language, as we have seen, was hardly a problem.

There was, however, another difficulty, quite similar to the difficulty that arose at Toledo. Unlike the Arabs who freely acknowledged what they had learnt from others, the medieval church was loath to acknowledge any reliance on "pagan" knowledge. This was a centuries-old tradition of the church, dating back to the days when it prosecuted "pagans" in the Roman empire. Augustine, for example, had chided Porphyry for studying the "mores and disciplines of Inde"—as if this was something offensive and sinful—and Indian thought (especially Śaṅkara's *Advaita Vedanta*, popular in South India) was rightly seen as similar to "pagan" (Neoplatonist) thought. Given this church policy of religious parochialism, the medieval church was very reluctant to admit publicly the value of "pagan" knowledge, especially in so sensitive a matter as fixing the date of Easter. It was, however, theologically acceptable to run down "pagan" knowledge. Accordingly, the persisting Jesuit interest in Indian astronomy is confirmed by de Nobili's 1610 polemic[26] against the *Vedāṅga Jyotiṣa*, the earliest Indian astronomical and timekeeping work that, because of its age (and the precession of the equinoxes), had been politely rejected as obsolete by Varāhamihīra,[27] a thousand years earlier. De Nobili's polemic is, thus, in the spirit of someone today running down the Bible as false on the ground that it gives an inaccurate value of π. However, De Nobili's polemic demonstrates two things: (a) that the Jesuit interest in Indian mathematics and astronomy persisted beyond the calendar reform, and (b) that by 1610 the Jesuits were confident enough about their knowledge of Indian mathematics and astronomy to write polemics against its older versions. As we have seen, from the case of Euler, for example, this interest persisted at least until the 18th c.

IV
NAVIGATION AND TRIGONOMETRIC VALUES

Latitude Determination and Precise Trigonometric Values

There was good reason for the continuing European interest in Indian books on mathematics and astronomy, even after the Gregorian Reform. The Gregorian Reform[28] of 1582 did not quite solve the latitude problem. While an improved calendar helped to determine latitude, in principle, from the observation of solar altitude at noon, the actual computation of latitude required also a knowledge of precise trigonometric values, in accordance with equation (7.1). In Europe of those days, common people did not even know how to add and multiply, without using counters.[29] The decimal system, long used in India, had just been introduced in Europe by Stevin in his *De Thiende* of 1585, translated as *La Disme*, and regarded as revolutionary. Therefore, the calculation of precise trigonometric values was a difficult matter. Hence, for latitude determination, from the measurement of solar altitude at noon, there remained the problem of computing precise trigonometric values (which were also needed for calculating loxodromes). How were precise trigonometric values to be obtained?

The first systematic use of sine and cosine values anywhere in the world is found in the treatise of Āryabhaṭa, which contains a table of 24 sine and cosine values.[30] Among European astronomers, trigonometry first appears with Regiomontanus, a thousand years after Āryabhaṭa: Regiomontanus presumably learnt of Āryabhaṭa's work through the Arabs.

Loxodromes, Mercator s Chart, and Precise Trigonometric Values

Trigonometric tables were used also to calculate loxodromes, which were the focus of efforts of navigational theorists like Nunes, Mercator, etc. A loxodrome or a rhumb line is the path followed by a ship which steers a constant course set by the magnetic compass or easily identifiable stars. The name derives from the Greek *loxos* (= oblique) and *dromos* (= curve). The word probably derives from the Dutch word *kromstrijk* (curved line) used by Stevin to describe the curves proposed by Nunes—as the result of following a constant rhumb line (on the globe)—with whose analysis Stevin disagreed. (The idea was to distinguish it from straight line sailing.)

A loxodrome intersects the meridians at constant angles. Though Nunes thought loxodromes were great circles,[31] this approximation is valid only for relatively short distances, like the Mediterranean. For large distances, in non-cardinal directions, a loxodrome is a curve which spirals towards the poles. This is artistically visualized in the artist Escher's painting "Sphere Surface with Fish".

We recollect that, in 1504, Portugal (King Manuel) had banned the use of the globe for navigation, because of the large error that it introduced. Though no cause was stated for banning the use of the globe, the reason was presumably because of the wrong size of the globe institutionalized by Columbus, and the poor understanding of spherics in Europe at that time. In fact, ships had been forbidden to carry globes of any sort,[32] and Pedro Nunes struggled in vain to defend the use of the globe for navigation.[33]

The big problem thus was to represent loxodromes on a plane map, as curves that the European navigator could easily understand. Mercator's map, or the Mercator projection, represents loxodromes by straight lines. This map was advertised as being of great value to mariners: its value chie y lies in the fact that for those using charts for navigation, this map can be used to set a course with a ruler, as European navigators were accustomed to doing in the Mediterranean.

As pointed out by Struik,[34] the problem of calculating loxodromes is exactly the problem of the fundamental theorem of calculus: to find a curve when the tangents to it are prescribed. How then did Mercator calculate loxodromes in 1568? This remains a long-standing mystery. In any case, Mercator's chart was not much used until the underlying principle was explained by Wright,[35] who taught mathematics at Cambridge. As we saw in Chapter 3, in India, difference equations were numerically solved instead of appealing to the fundamental theorem of calculus. These numerical techniques continue to be of distinctly greater practical value today than metaphysical theorems, and these were the techniques actually used by Wright, and those earlier European navigational theorists like Stevin who gave "tables of secants" for the purpose of calculating loxodromes.

Geometrically, however, the idea explained by Wright is to project the sphere on to a cylinder and then unroll it. However, the "Mercator" projection is not a straightforward cylindrical projection. In Wright's picturesque description, one should take a bladder, put it inside a cylinder, and in ate the bladder until the equator touches the cylinder. To get the positions of the other latitudes one should go on in ating the bladder, which now gets distorted and non-spherical, until that latitude touches the cylinder.

The precise mathematical formulae are

$$x = \lambda - \lambda_0 \tag{7.2}$$

$$\begin{aligned} y &= \quad_0 \sec x \, dx = \ln \quad \tan\left(\frac{\pi}{4} + \frac{\quad}{2}\right) \\ &= \sinh^{-1}(\tan \quad), \end{aligned} \tag{7.3}$$

where λ and are respectively the latitude and longitude of the point, and the x-axis of the plane map is at the equator, while the y-axis is at the longitude λ_0. The map greatly distorts areas near the poles ("Greenland effect"), while preserving angles at any point, i.e., it is a conformal map. The conformality of the map is of great importance in navigation.

The British naval supremacy has been attributed to their better understanding of this formula! Clearly, however, the developments in calculus and astronomy in the 17th c., and their contribution to navigation were of great importance in the dominance that Britain came to acquire in the 18th c. (Another factor of importance, remarked by a traveller, is that the Portuguese navigators kept their methods a secret, while the British openly shared their knowledge, thus allowing knowledge to grow at a much more rapid pace.[36])

Of course, the formula was not originally given in the above manner in which it is customarily given today. If we follow through with Wright's description, we see that each circle of latitude will be mapped to a circle of uniform radius on the cylinder. Since latitude circles on the earth shrink in length by a factor of cos , where is the latitude, this means that each circle must be stretched by the inverse factor of sec . If angles are to be preserved, this means that the distances between adjacent latitudes must also be stretched by the same amount, i.e., distances must be scaled by secants as the above formula shows. Wright accomplished this by carrying out the integral numerically. But this required an accurate table of secants, hence precise trigonometric values. Before Wright, navigational theorists like Stevin were equally concerned with the calculation of meridional parts, hence with precise secant values and trigonometric values generally. It was presumably these concerns which led Clavius to the publication of his table of precise trigonometric values at the turn of the century. Clavius fails to provide any explanation of how those tables were obtained.

We have seen that calculation of loxodromes involved the solution of a problem equivalent to the fundamental theorem of calculus. But that theorem was unknown to Europeans in the 16th c. How, then, did Mercator draw the chart? The abiding nature of the Mercator mystery is due to the fact that it cannot be appropriately solved within the framework of the Western historical narrative about the calculus. The mystery can be resolved by changing that narrative. It is hard to believe that Mercator drew his chart through sheer skill in draftsmanship. It is rather more likely that he had access to information from India or China, which he kept a secret. That this information was adequate to enable the calculation of loxodromes is evident from the fact that loxodromes were earlier used to map the zodiac, and a Chinese [Dunhuang] star map from ca. 950 follows the very same principle of isogonal cylindrical projection that has come to be known as the "Mercator" projection. This chart is reproduced in Needham's volume.[37] How did the Chinese draw the chart?

Clearly, the principles of using a finite difference technique for numerical integration, in the manner of Āryabhaṭa's computation of sine values, reproduced in Chapter 3, were certainly known to the Chinese by the 10th c. CE. Clearly, also, by this time, they were well aware of the principles of spherical trigonometry as found in e.g. Vaṭeśvara's work. The Chinese, therefore, had the necessary mathematical equipment to draw the map. Presumably, Mercator did likewise.

European Interest in Trigonometric Values

That 16th c. Europeans were greatly interested in precise trigonometric values is shown for example by Nunes, Stevin, Clavius, etc. all of whom published lengthy tables of accurate trigonometric values. The Indian connection is manifest. Stevin mentions Āryabhaṭa's value of π, which was widely known in the Arabic world.[38] Therefore, European navigational theorists were certainly aware of the prevailing Indian and Arabic techniques of computing trigonometric values, at least up to the time of Āryabhaṭa, as known to Arabs, which diffused into Europe from the time of Regiomontanus. Clavius further used the Indian definition of the sine, also called the Rsine, as is clear from the very title of his work.[39] The "coincidence" is all the more striking when we observe that the (large) value of R is the same as that found in Indian texts, and that *all* Jesuits in India (not Matteo Ricci alone) would have reported back to Clavius, anything of significant interest in mathematics and astronomy.

Presumably, other European scholars were also well aware of the real source of these trigonometric values, in Indian texts, and were interested in establishing a direct contact with the Indian source, as in the case of spices. For example, Bombelli (d. 1572) mentions in the preface to his *Algebra*[40] that the Greek text of Diophantus of Alexandria makes several references to Indian sources:

> a Greek work on this discipline has been discovered in the Library of our Lord in the Vatican, composed by a certain Diophantus of Alexandria, a Greek author, who lived at the time of Antoninus Pius. When it had been shown to me by Master Antonio Maria Pazzi, from Reggio, public lecturer in mathematics in Rome... (we) set ourselves to translate it... in this work we have found that he cites Indian authors many times, and thus I have been made aware that this discipline belonged to the Indians before the Arabs.

Fermat's interest in this particular text of Diophantus is well known; so mathematicians like Fermat, and certainly all those who read Bombelli, were aware of this Indian connection.

To reiterate, Mādhava's trigonometric tables, which improved Āryabhaṭa's trigonometric tables, using the series expansion for the sine, cosine, and arctan functions, were then the most accurate trigonometric values available, and the coefficients needed to calculate these values, in a numerically efficient way, were encapsulated, as we have seen, in a couple of verses in various widely distributed mathematics/astronomy/timekeeping (*jyotiṣa*) texts, including the *Karaṇapaddhati*. Texts like the *Yuktibhāṣā* were in contemporary Malayalam which the Jesuits could well understand,[41] while various other texts were in Sanskrit which they had got translated at the latest by 1600 and were then reading in the Sanskrit as is clear from de Nobili's polemic of ca. 1610 against the *Vedāṅga Jyotiṣa*. The key manuscripts were housed in a place less than a hundred kilometres from Cochin. Any attempt to acquire and understand Āryabhaṭa's texts or Bhāskara's texts, in the locality of Cochin, would have led in a natural way to these texts and to some of the authors like Śaṅkara, author of the *Tantrasaṅgraha Vyākhyā*. (Nīlakaṇṭha's *Āryabhaṭiyabhāṣya* was a commentary on Āryabhaṭa's work, while Śaṅkara's *Kriyākramakarī* was a commentary on Bhāskara's work.)

Jesuits, of course, were not the only ones bringing these Indian books into Europe. As we have seen, many people in Europe believed that the answer to the navigation problem lay in the "knowledge of the ancients", so that there were many other collectors of Oriental books and manuscripts in Europe of that period. Since these books were regarded as valuable, every itinerant, sailor, and merchant who could lay hands on them carried them back, either to keep as memorabilia, or to sell off at a good price.

Longitude Determination, Trigonometry, and the Size of the Globe

As we have seen in Chapters 4 and 5, longitude calculation can also be done by solving a triangle, if one has access to precise trigonometric values. This could be done in different ways. For example, from a knowledge of (a) the latitude difference, (b) the fact that latitudes and longitudes are orthogonal, and (c) either the course angle, or (c) the distance, one could solve a triangle to compute the longitude difference. For short distances (like the Mediterranean sea) this could be a plane triangle. Solving such a triangle required precise trigonometric values.

We have also seen that the early 7th c. Bhāskara I states the criticism that this method is faulty, and spherical triangles should be used instead. For slightly larger distances, and for sailing in non-cardinal directions, one could solve a spherical triangle, e.g., using the well-known formula

$$\cos D = \sin a \sin b + \cos a \cos b \cos P, \tag{7.4}$$

where D is the angular distance between points A and B, a is the latitude of point A, b is the latitude of point B, P is the longitudinal difference between points A and B. (In applying the above formula, south latitudes and west longitudes are treated as negative angles.) While the distance travelled could be used as a measure of the great-circle distance, the difficulty was in converting the physical distance d on the surface of the earth to the angular distance D between the two points A and B. This conversion requires a precise knowledge of the radius of the earth, or the length of one degree of the arc. This knowledge was also required in the case of plane navigation. (This technique is still used in Mercator sailing and great-circle sailing.) Thus, in addition to precise trigonometric values, it was necessary to know the radius of the earth.

Now the length of one degree of the arc, or the radius of the earth, was known to Āryabhaṭa, Bhāskara, et al. The trigonometrical method they used is not documented, but must be similar to the method documented by Al Bīrūnī who had studied India, and documented Indian mathematics and astronomy, in great detail, apart from translating Indian mathematics and astronomy texts. The method uses triangulation to measure the height of a hill; then one climbs the hill and measures the dip of the horizon.[42](For al Bīrūnī this was clearly a new method, and, while carrying out his observations in "India", his stated aim was to check the estimate so obtained against Caliph al Ma'mūn's measurement of one degree of the arc obtained by sending an expedition in the desert to measure this out physically. This triangulation method of measuring the size of the earth is implicit in the definition of a *shāmam*, or *yāma*, or *zām*, as the distance from here to the horizon, still used by Lakshadweep islanders.[43] All of these figures are remarkably accurate, though they err in assuming the earth to be perfectly spherical.)

These Indo-Arabic figures for the size of the earth were presumably available to the Portuguese. However, Columbus, systematically underestimated the size of the earth, supposing it to be only some $\frac{1}{4}$th of its actual size. One can understand that such systematic underestimation of the size of the earth would have facilitated the funds for Columbus' scheme of sailing West! Obviously enough, Columbus' "success" was unrelated to this estimate: he was simply aimed in a direction where there was so huge a land mass that he could hardly have missed it! However, Columbus' success gave credence to his estimate of the size of the earth, and this estimate came to be accepted in Europe, and went unchallenged for another one and a half centuries. Accordingly, navigating by the globe fell into deep disrepute. Columbus' incorrect estimate was corrected, in Europe, only towards the end of the 17th c. CE, after Picard's measurements. However, by the time this figure came to be generally accepted, Europeans had evolved a different technique of navigation.

This state of affairs is further circumstantial evidence for the import of the calculus into Europe, since it suggests that, although Clavius et al. stated very high-precision trigonometric values (which could have been obtained only by calculus techniques, thus suggesting an advanced knowledge of trigonometry), they simultaneously lacked the grasp of elementary trigonometry needed to use these values for practical matters like determining the size of the earth accurately.

The other way to fix the longitude of a place was by telling the difference of time from a reference longitude.

How was this done prior to the mechanical clock? Prior to the mechanical clock, it was possible to fix longitude by telling the time difference using a clepsydra! This is the method suggested by Bhāskara I. Naturally, a clepsydra cannot be set to the time of a reference longitude: however, one can use the clepsydra to measure the time difference between sunset and an event such as moonrise. One can now determine the time difference between the local time and time at the reference longitude by the simple process of comparing the observed time with the calculated (theoretical) time of the same event at the reference longitude.

> On any day calculate the longitude of the Sun and the Moon for sunrise or sunset without applying the longitude correction, and therefrom find the time (since sunrise or sunset), in *ghaṭīs*, of rising or setting of the Moon; and having done this, note the corresponding time in *ghaṭīs* from the water clock. From the difference, knowledgeable astronomers can calculate the local longitude in time.[44]

But, unlike Indian astronomers who long ago agreed on the meridian through Ujjain as the reference longitude, 16th and 17th c. Europe lacked the concept of a generally agreed reference longitude (though the idea was later copied as the meridian of Greenwich). Though Regiomontanus had learnt these techniques of triangulation, presumably from Arab rather than Indian sources, and had compiled ephemerides, his table were very unreliable. Reliable and standardized lunar ephemerides started being produced in Europe only around the mid-18th c. CE.

But Europeans encountered another difficulty in using these two methods of determining longitude. Both the above techniques require extensive calculations, and European navigators of the 16th and 17th c., being more accustomed to graphical and geometrical methods, were not well-enough versed in the algorismus to be able to do the required calculation.

Thus, even after precise trigonometric values and a precise knowledge of the globe had become available, they continued to rely on the older techniques, and preferred a simpler mechanical way of determining longitude. This goal of enabling a common sailor to navigate is also articulated in a contemporary poem concerning Gresham college, and its efforts regarding navigation, which contributed to the founding of the Royal Society.[45]

V
EVIDENCE FOR TRANSMISSION

We have now seen (1) the overwhelming importance of navigation for Europe, in the 16th c. CE; (2) the overwhelming importance of precise trigonometric values for navigation, in determining latitude, longitude, and loxodromes; (3) the easy availability of Indian texts giving these precise trigonometric values; (4) the systematic search for local texts by Jesuits in South India and the Toledo model of translating these texts and sending them back to Europe for further study; (5) the Jesuit's special interest in mathematics and astronomy; and (6) their location in close proximity to a major repository of the key texts. Under these circumstances, it would have been a very odd thing indeed if this Indian knowledge was not passed on to other mathematicians in Europe.

However, we had earlier proposed that in view of racist history, and its double standards of evidence regarding transmission, it is necessary to adopt fresh criteria to judge transmission. Let us summarize the evidence in terms of those criteria.

Navigation and Motivation

In the 16th c. navigation was of overwhelming importance to Europe. This is quite objectively demonstrated by the large rewards offered by various European governments for a good technique of navigation.

Accurate trigonometric values were critical for navigation: both for celestial navigation (for determining latitude and longitude by celestial observations), and for navigation by charts (for determining the loxodromes required to construct charts, for navigation according to the European technique).

This relationship between navigation and trigonometric values was well known to European navigational theorists such as Stevin, and key Jesuit leaders such as Christoph Clavius. Hence there was a motivation to acquire texts related to precise trigonometric values.

This motivation must be combined with their strong motivation arising also from the needs of the Gregorian calendar reform, authored by Clavius, which had become a major religious issue in Europe.

The changes in the Jesuit mathematical syllabus, brought about by Clavius, provide further evidence of the Jesuit motivation to learn about mathematics and astronomy, and the calendar.

Finally, to carry out their agenda of converting people to (their brand of) Christianity, the Jesuits were strongly motivated to learn about local customs, festivals, and hence the local calendar. There are numerous Indian festivals, and not a single one of them then appeared on a fixed day according to the Julian/Gregorian (civil) calendar. The Jesuits would have had motivation to consult and interact with appropriate local calendar experts for this purpose.

Thus, we have established very strong motivation, beyond all reasonable doubt, that the Europeans, in general, and the Jesuits, in particular, would have wanted to consult and study the Indian mathematical and astronomical texts.

Cochin and Opportunity

Impelled by the above motivation, more than a hundred years were available to Christian missionaries and then Jesuits in close proximity of the above texts, in Cochin. Furthermore, in Cochin, the Jesuits shared a common patron in the Raja of Cochin with the very authors of these texts, like Śaṅkara Vāriyar. Cochin was only a short distance from Trichur which was then one of the most well-known centre of astronomical learning in South India. The opportunity to acquire the Indian texts was enhanced by the active cooperation of the local Syrian Christian community, which cooperated with the Catholic missionaries for nearly a century, before falling apart with them.

We outline the story since it may not be fully known. It should be emphasized that Cochin was the first base of the Portuguese, starting 1500, well before Goa came within their control.

This happened for a peculiar reason. Vasco da Gama first landed in Calicut, where he was well received by the Samudiri (Zamorin) of Calicut, who (as his name suggests) facilitated sea trade, welcoming all traders by sea. When Pedro Alvarez Cabral arrived in Calicut in 1500, he was given a place to locate his factory, and a Gujarati merchant to instruct him in the local customs. But not having any money to buy the spices that he wanted to take home, and not finding any customers for the shabby commodities he had brought with him to exchange for spices, he tried spreading false rumours and then tried to seize the spices forcibly. In the resulting con ict, the Portuguese factory was razed, and Cabral and his men had to ee. Ten days later, around 26 November 1500, they arrived in Cochin, where the Gujarati merchant, who had accompanied and guided them, explained to the king of Cochin what had happened. As expected, they were welcomed, and given a place to locate their factory, because the king of Cochin was hostile to the Samudiri of Calicut. Five of the eight Franciscan friars (who had accompanied Cabral for missionary work) settled in Cochin. While there had been trade with Europe for many centuries earlier, the mission in Cochin was the first organized Catholic mission in India.

> They were followed by large contingents of zealous missionaries, who worked from the city of Cochin as a centre. The harvest of souls was rich, the Christians

> multiplied along the coast and in the interior, and in course of time a bishop was assigned to them.[46]

Language did not at all restrict the opportunity to acquire local texts. The missionaries naturally communicated with the local people in the local language, and not in Portuguese or Latin. However, from the beginning, the Portuguese had very few language problems. Calicut had exported spices to Europe from centuries before Vasco da Gama. The Venetian merchants who were involved in this spice trade were not only well aware of the source of spices in India, but, since it was a major source of wealth for them, they tried their best to stop the Portuguese by sending an ambassador to the Samudiri of Calicut, in 1501, through the Sultan of Cairo,[47] asking the Samudiri not to trade with the Portuguese. In fact, when Vasco da Gama landed in Calicut, he found there two merchants from Tunis who were there to trade, and were able to converse with him in Castilian and Genoese languages.[48] It was but natural that European languages were spoken in Calicut, which was a cosmopolitan place visited by merchants from China, Arabia, and Europe. In particular, because Portugal had emerged from Arabic rule only a few years earlier, Vasco da Gama had no difficulty in exchanging treaties with the Samudiri of Calicut in Arabic, which was translated for the Samudiri.

In Cochin, the Portuguese also had ample support from a section of the local population. In Cochin, Cabral immediately established contact with the Syrian Christians (whom the Catholics called Thomas or Nestorian Christians). After Vasco da Gama's barbaric attack on the unarmed people and ships in Calicut port (because the Samudiri refused to concede Vasco's extraordinary demand [mixing state, church, and trade] that trade in spices be made a Christian monopoly) and the gruesome consignment of mutilated human corpses he sent to the Samudiri, the Portuguese dared not come anywhere near Calicut, and Cochin became their centre. However, the missionaries spread into the interior of Kerala setting up establishments in places like Cannanore and Quilon within a couple of years (1502). Syrian Christians were well versed in the local customs and also in a variety of languages ranging from Arabic to Aramaic in which their Bible was written. Two Syrian Christians, a certain Mathias, and his brother Joseph left for Portugal with Cabral.[49] A Syrian Christian bishop gave Vasco da Gama a sceptre and promised all assistance. Two Syrian Christian priests accompanied Vasco da Gama to Rome, and Vasco da Gama was initially buried in Cochin at the Franciscan church.

The initial cooperation of the Portuguese with the local Syrian Christians was according to their original plan of forging a religious-military alliance with "Prester John", in their religious war. While the Syrian Christians were hardly in a position to provide any sort of military support, this tie-up did initially create a sort of fifth column that gave the Portuguese full access to a variety of local knowledge, thus enhancing their opportunity to acquire it.

The contacts with the Syrian Christians were soon institutionalized. By 1510, the Portuguese had started a school in Cochin, with Afonso Alvares as the teacher. This school was later rebuilt by Franciscans in 1520 along with various churches and seminaries. Another Roman Catholic college in India was opened in Kotinallur, around 1536, specifically for the Syrian Christians, and a residence of the Jesuit society was established in Cochin by 1550, a few years after the arrival of Francis Xavier in Cochin in 1542. In 1550, the Jesuits built a large three-storied college in Cochin, attached to the church. By 1558 Cochin had been converted to a diocese, and the church into a cathedral. By 1590 this college had a couple of hundred students, which is a very large number compared to the then population of Cochin.

The Jesuits were rapidly acquiring and translating local texts; this further enhanced their opportunity to acquire the calendrical texts containing trigonometric values. Starting from ca. 1575, the Jesuits also had adequate knowledge of mathematics. Therefore, they would have understood the content in these texts. All the above shows that opportunity, too, is established beyond all reasonable doubt.

It should be emphasized that the Jesuits were not the sole channel for information transfer—they were not the only ones to have the opportunity to transfer books from India to Europe. Many others were also involved in collecting information about the new places that the Europeans had "discovered".[50] Travellers and sailors often acquired books as souvenirs, and these books found their way into the libraries of collectors. Naturally, books related to astronomy and navigation would have been high on the agenda of such travellers and sailors. Mersenne, for example, writes of the knowledge of Brahmins and "Indicos" and mentions the orientalist Erpen and his "les livres manuscrits Arabics, Syriaques, Persiens, Turcs, Indiens en langue Malaye".[51]

Circumstantial Evidence

There is also strong circumstantial evidence that transmission of Indian mathematics and astronomy texts did take place to Europe in the 16th and 17th c. CE.

In the first place there is Mercator's mysterious source of trigonometric values. Had Mercator obtained his values from some source like Regiomontanus there would have been no need for him to hide his sources, nor any possibility of doing so. On the other hand, since he had been arrested by the Inquisition, he had strong reason to keep any "pagan" sources a secret. Therefore, the very fact that he kept his sources a secret, combined with the fact that his map was similar to a Chinese map, is strong circumstantial evidence that his sources were non-Christian sources. As pointed out above, calculation of loxodromes is equivalent to the fundamental theorem of calculus, and Indian texts were the best possible source for this information.

The trigonometric values published by Clavius, who was at the centre of the Jesuit web, provide further circumstantial evidence that the Jesuits had obtained the latest Indian texts on mathematics and astronomy, and had studied them. Thus, Clavius' trigonometric values use exactly the Indian definition of the sine and also the same value of the radius[52] used by Indian sources in stating Mādhava's sine values. Further, Clavius was unable to give any explanation for the way those trigonometric values were derived, and, obviously enough, the derivation of such precise values required essentially calculus techniques. Had Clavius himself discovered a striking new procedure, by which to obtain more precise trigonometric values, would he not have announced it, to establish his priority, especially since this was towards the end of his life? In fact, Clavius, though he published sophisticated trigonometric tables in his name, lacked a proper understanding of even elementary trigonometry, since he was unable to use trigonometry to determine a key navigational parameter—the size of the globe.

Similarly, the "Julian" day-number system (not to be confused with the Julian calendar) supposedly invented by Scaliger, a contemporary of Clavius, is, except for its zero point, exactly the *ahargaṇa* numbering system used by Indian astronomers and mathematicians from the time of the *Sūrya Siddhānta*, if not earlier.

Then there is the evidence of Clavius' student Ricci's interest in searching for Indian methods of timekeeping, and his visit to Cochin, just before the Gregorian calendar reform authored by Clavius.

The decimal system of representing numbers (using powers of ten) has been in continuous use in India since Vedic times to the present. (While the names remain much the same, the current usage assigns different values to them, corresponding to different powers of ten, as we saw in Chapter 3.) Europe in the 16th c., however, used the Roman system of numeration, and Stevin in 1585 first propagated the use of the decimal system in Europe.

Then there is the polemic written ca. 1610 CE against the *Vedāṅga Jyotiśa*, by Roberto de Nobili,[53] which he could hardly have written without consulting that source. The Jesuits had read a variety of Indian literature to which they sought to adapt their own gospels, as is clear from the very titles of these publications. (The cases of Ricci, de Nobili, and the adapted gospels should properly be counted as documentary evidence rather than circumstantial evidence, but we mention it here to enable a clear view of the chain of transmission.)

Tycho Brahe was another contemporary of Clavius, and the "Tychonic" system of planetary orbits is remarkably similar to the model of Nīlakaṇṭha, author of the *Tantrasaṅgraha*. Couldn't Tycho have independently rediscovered this model? Since noted historians like Owen Gingerich have advanced just such a thesis about Copernicus, contending that he might have independently rediscovered the model of Ibn-as-Shātir of Damascus, this point seems in need of an explanation.

As already noted, the vast number of such claims of "independent rediscovery" by Europeans suggests that a *general* historiographical explanation is required for this peculiar phenomenon. Apart from the racist historian's double standards of evidence (the origins of which we have traced to religious intolerance), the curious thing here is how Westerners developed a culture of hiding their sources. Like Mercator, Scaliger, and Clavius, Tycho too was very secretive about his sources, hiding them even from his assistant Kepler. Doubtless, priority was a Western a cultural value, unlike the case with Indians, Arabs, or Chinese—none of whom valued historical priority. However, it is not clear that the value of priority can, by itself, explain such extraordinary secrecy.

On the other hand, in Tycho's time, the thousand-year old practice of religious intolerance was scaling new heights, and had long made the rejection of non-Christian knowledge into a Christian *value*. This was an important value: the Inquisition propagated the belief that those who still held on to any remnants of "pagan" customs (e.g. in dress) deserved to be physically eliminated. As Mathematician/Astronomer of the Holy Roman Empire, it would have endangered Tycho's position if knowledge of his non-Christian sources were to have leaked out. Furthermore, this was a time of intense religious turmoil, and the slightest evidence of religious impropriety in the upper echelons of the church hierarchy would surely have been made into a big issue by the opponents. This readily explains the extraordinary level of secrecy Tycho maintained.

More recently, it has been claimed that, infuriated by this secrecy, Kepler murdered Tycho to get at his secrets.[54] While the recent forensic analysis of Tycho's remnants seems to have established that Tycho died of poisoning due to a sudden overdose of mercury, it is obviously not going to be easy to establish after all these years who did the poisoning, especially since Kepler is already the hero of a certain story. The broad argument is that the forensic analysis shows *two* peaks of mercury concentration, so that the poison was administered twice—hence that it was administered by someone whom Tycho trusted. Since other members of Tycho's household all stood to lose by his death, while Kepler stood to gain (Tycho's papers and his job), Kepler could well have done it.

For our purposes, it is irrelevant whether, in fact, Tycho was murdered, or whether, as the earlier story went, he died of excessive drinking. The point is only the curious secrecy he maintained about his papers, even from those he otherwise trusted. Similarly, it is irrelevant whether or not Kepler was a murderer. More to the point he was an astrologer by profession, who calls astrology the natural means of subsistence for an astronomer. If he did not believe in the astrology he practised, he was obviously a charlatan, and a professional liar. If he did, he could hardly be reckoned to be a scientist. (If he only partly believed in astrology, he must have been both a charlatan and a confused person.) In any case, there is every reason to be sceptical about the stories given out by Kepler.

In the particular case of Tycho, the additional factor to be considered is that the model came first, and the observations followed. (Recall that in 1582, Tycho could not accurately determine a simple astronomical parameter like the length of the year.) Theory preceded observations also in Kepler's *New Astronomy*, stating the first two of Kepler's "laws".[55] The simple fact is that Tycho's observations were inadequate[56] for the accuracy with which Kepler obtains the orbit of Mars. To cover up this discrepancy, Kepler fudged his data.[57] It has been argued that Kepler did not intend fraud,[58] and also (by Owen Gingerich) that he somehow made up for the inadequacy of his data through "brilliant insights". As opposed to this sort of explanation by magic, there is a much simpler explanation. Nīlakaṇṭha's model was based on a very long baseline of observations, going back some 3000 years. His predecessor Parameśvara, for example, had himself carried out fifty years of painstaking observation. Nīlakaṇṭha also had the calculus techniques at his disposal, and a planetary model which used variable epicycles (equivalent to elliptic orbits in a heliocentric frame). Therefore, he was able to arrive at a very accurate orbit of Mars. When Indian astronomy works, translated by Jesuits in Cochin, started arriving in Europe, Tycho, as one of the most famous astronomers of his day, and the Mathematician of the Holy Roman Empire, would naturally have been chosen as the person to whom they were referred. Nīlakaṇṭha's model was what later came to be called the "Tychonic" model, which Tycho was trying to check against observations. Why, after all, was Tycho so secretive about his papers, not even allowing his trusted assistant Kepler to see them? In any case, on Tycho's sudden death, Kepler obtained not just Tycho's observations, but also the rest of his papers which contained the underlying theory. Being inclined towards heliocentrism, Kepler transformed Nīlakaṇṭha's "Tychonic" orbits to a heliocentric frame (a simple transformation). This made Nīlakaṇṭha's variable epicycles come out as ellipses. Being a professional astrologer, Kepler was good at making up stories, and he made up the story about how he had arrived at his results using Tycho's data. Realizing that someone might want to check the data, he fudged it. This is a much simpler explanation than having to believe first in the magic about Tycho, then in the magic about Kepler, then in prolix explanations about why the fudging of data by Kepler did not constitute fraud.

Unlike the case of trigonometric values and planetary models, both of which had earlier precedents, the infinite series of the calculus had no previous precedents in Europe. However, the calculus *suddenly* starts appearing prominently in European mathematical texts and discourse from the 1630's, less than half a century after the calendar reform (discounting the case of Kepler himself, who toyed with the calculus in 1615).

Cavalieri, himself a Jesuati, had produced a book on "indivisibles". Cavalieri apparently waited for five years for Galileo, whom he regarded as his teacher, to publish first on the matter. Why did Cavalieri wait five years for Galileo to publish, before publishing his results on the calculus? This would have been a rather strange arrangement if Cavalieri had invented the calculus himself. It would have been even stranger if Galileo had invented it, for Galileo himself published nothing on the calculus. Galileo's access to Jesuit sources at the Collegio Romano is well documented.[59] Galileo did not himself take up the calculus because he did not quite understand it, as is clear from the difficulties and the various paradoxes of the infinite that he raised in his letters to Cavalieri.[60] Thus, this state of affairs is better explained by supposing that there was a common body of Indian work related to the calculus, known to both Galileo and Cavalieri, and that Galileo was not satisfied with Cavalieri's interpretation of it, and not willing to risk his reputation, while Cavalieri was. Nevertheless, out of deference for his teacher, he waited five years before staking his claim.

The in uence of Cavalieri's work on Torricelli and Roberval is well known. Roberval was a member of Mersenne's discussion group, and was involved, along with Fermat and Pascal, in debating with Descartes, the validity of these new methods. There is a clear chain of in uence from Cavalieri to Torricelli, to Wallis to Gregory and Newton. As is well known, while Newton acknowledged the in uence of Wallis, Leibniz acknowledged the in uence of Pascal on their respective works relating to the calculus. A diffusionist model for the calculus in Europe is, therefore, rather more appropriate than the simplistic Eurocentric model which gives all credit to Newton and/or Leibniz just because the two had a nasty priority dispute!

There is further circumstantial evidence of transmission. The calculus methods of Cavalieri, Roberval, Fermat and Pascal are very similar to those of the *Yuktibhāsā*, *TantrasaṅgrahaVyākhyā*, *Kriyākramakarī*. As seen earlier, the key step in the derivation of the arctan series is the calculation

$$\frac{1}{n^{k+1}} \sum_{i=1}^{n} i^k \approx \frac{1}{k+1}, \quad \text{for large } n. \tag{7.5}$$

This is exactly the formula used by Fermat and Pascal to evaluate the area under the "parabola" $y = x^k$. Moreover, as Pascal remarks about this formula, it immediately makes manifest how to carry out the quadrature of curves of all types.

There is circumstantial evidence that other Indian mathematical texts were available to Fermat. Fermat's challenge problem to European mathematicians, and particularly Wallis, involving the so-called Pell equation, is a solved example in the text of Bhāskara II. (The name Pell's equation was given by Euler; Pell is innocent.)

> In a letter of February 1657 (*Oeuvres*, II, 333–335, III, 312–313) Fermat challenged all mathematicians (thinking in the first place of John Wallis in England) to find an infinity of integer solutions of the equation $x^2 - Ay^2 = 1$, where A is any non-square integer.[61]

Fermat also wrote a letter to Frenicle, at about the same time, elaborating upon this problem: "What is for example the smallest square which, multiplied by 61 with unity added, makes a square?" It is well known that Indian mathematicians had a solution to this problem.[62] In fact, singularly enough, exactly the case $A = 61$ is given as a solved example in the *BījaGaṇita* text of Bhāskara II. (A similar problem had earlier been suggested by the 7th c. CE Brahmagupta, and Bhāskara II provides the general solution with his *cakravāla* method.[63]) This "coincidence" is not trivial when we consider that the solution, $x = 1766319049$, $y = 226153980$, involves rather large numbers, so that there is little possibility of independent rediscovery, especially in a Europe which, barely fifty years earlier, did not even know how to write such large numbers. This small possibility of independent rediscovery is also emphasized in the fact that Fermat chose this for a challenge problem.

This is further corroborated by the fact that it took until Euler for European mathematicians to find a solution to the problem.

Moreover, like many others in that age, Fermat was deeply interested in reconstructing ancient manuscripts. Fermat's interest in Diophantus is particularly well known, and we have already seen how Bombelli remarks on Diophantus' citations of Indian sources. Hence, it is rather more likely that Fermat had obtained a translation of (at least some portions) of Bhāskara's text through his close Jesuit friend Jacques de Billy. Perhaps also, along with Bhāskara's texts, Fermat also obtained some commentaries on Bhāskara's work, such as the *Kriyākramakari*, which contains all relevant details relating to the calculus. Accordingly, Fermat's work on calculus also originates in Indian sources, though these sources perhaps were, at least partly, independent of Cavalieri's.

Pascal's triangle, as is well known, is found in Chinese sources, and in much earlier Indian sources such as Piṅgala's *Chandahsūtra*, about 1800 years before Pascal. Since "Pascal's triangle", relating to the "binomial theorem", was also known to various European mathematicians before him, it establishes wide access to information owing in from India and China.

Gregory, who worked in Padova, and does not claim originality, writes about exactly the series that are found in the Indian texts. Why did he make no claim to originality considering how bitterly Newton and Leibniz fought over the issue?

Singularly enough, not only are the infinite series the same, but even the term "indivisible", used by Cavalieri, exactly re ects the terminology in the *Yuktibhāsā*! Naiyāyika-s, although Cavalieri obfuscates matters.

Finally, there is circumstantial evidence that other material in Indian astronomy texts was, in fact, continuing to be transmitted to European mathematicians like Euler wrote about the Indian sidereal year in an article on "Hindu astronomy", and the "Hindu year".[64] Thus, Euler, as is well known, drew much inspiration from Fermat, and even solved the challenge problem of "Pell's equation" naming it as such. Euler was, of course, interested in the navigation problem, and was one of the recipients of the prize instituted by the British Board of Longitude. This suggests that Euler was familiar with the Indian sources to which Fermat had access.

Numerous mathematical algorithms suddenly appeared in that period in Europe, which, till a little while earlier, was suspicious even of algorithms for addition and multiplication! We have already pointed out "Stirling's" method (Stirling was a contemporary of Newton) as an example.

This list can go on, but (unless one is a Western historian) it is very hard to believe that all these discoveries were, by a fortuitous coincidence, made independently at just the time when there was such splendid opportunity for their transmission, and such overwhelming motivation for Europeans to learn from Indian sources, along with such an active European effort to acquire this knowledge.

The trail left by the circumstantial evidence is summarized in Table 7.1.

Table 7.1: The circumstantial trail.

Person	Work	Circumstantial evidence
Mercator ca. 1560	Chart giving loxodromes as straight lines	1. Worked with Gemma Frisius, at Catholic University of Louvain. 2. Obtained projection from China, table of secants from India. 3. Sources of secant values not known to this day—was arrested by the Inquisition. Had reason to hide "pagan" sources. 4. Chart required more precise secant values than were then available in Europe. 5. Precision attributed to magical skill at draftsmanship.
Christoph Clavius 1582	Gregorian calendar reform	1. Pope stated length of year obtained from Alphonsine tables—but these were known from centuries earlier, from Toledo. 2. Tried to determine the length of the year observationally, but failed. 3. Protestants did not accept the new calendar, since length of the year could not be accurately determined then by Europeans.
Clavius 1610	Accurate trigonometric values	1. Sudden increase of accuracy to 8 decimal places. 2. Method not explained. 3. Same process took a thousand years in India. 4. Was well aware of practical importance of these values but did *not* know how to use them to fix the size of the globe. 5. As top Jesuit, had full access to translations coming in from India.
Julius Scaliger ca. 1570	Julian day number system	1. Similar to Indian *ahargaṇa* system, with adjusted zero point. 2. No earlier background of astronomical calculations using this system in Europe.

Person	Work
Simon Stevin 1585	Decimal system
Roberto de Nobili ca. 1610	Polemic against *Vedāṅga Jyotiṣa* (of ca. −1350 CE, rejected by Indian astronomers as obsolete since at least the 6th c. CE)
Tycho Brahe ca. 1585	Tychonic system
Johannes Kepler ca. 1615	Kepler's laws, and super-accurate orbit of Mars

Table 7.1: continued

Circumstantial evidence
Proposed a revolutionary change from the existing Roman system of numeration.
Learnt Sanskrit and also the Veda-s falsely claiming to be a Brahmin (truth-seeker)! This was a pre-planned strategy of getting in uential higher-caste converts to Christianity. Shows how tenaciously Jesuits sought Indian sources like the Veda-s.
1. Introduced naturally by Nīlakanṭha a century earlier. 2. Tycho had the system first, and looked for observations later. 3. His fame as an astronomer, and position in church hierarchy made him the natural person to whom new astronomical books from India would have been referred.
1. Obtained access to Tycho's papers on his death but fudged his observations, since his theory (very similar to Nīlakanṭha's) was more accurate than Tycho's observations which he claimed to have used. 2. Was a professional astrologer, used to telling stories. 3. Experimented with the calculus in *Stereometria Doliorum* (1615).

Person	Work
Galileo ca. 1627	Refused to write on the calculus (presumably since he did not understand it).
Cavalieri 1632	Method of Indivisibles
Pierre Fermat 1635	(Did not publish.)
Pascal 1635	Quadrature of higher order parabolas
⋮	⋮

Table 7.1: continued

Circumstantial evidence
1. Had full access to the Jesuit Collegio Romano. 2. Cavalieri waited five years for him to write. 3. Criticised Cavalieri's approach to infinities, suggesting various paradoxes.
1. "Discovery" comes barely 50 years after first use of decimal notation. 2. Uses Indian techniques, but tries to provide a geometric explanation for them.
Challenge problem to British mathematicians was a solved exercise in Bhāskara II.
1. "Pascal's" triangle known to Indian tradition as Pingala's Meru Prastāra from some 1800 years earlier. Also known to Chinese. 2. Uses the leading order expressions for $\frac{1}{n^{k+1}} {}_{i=1}^{n} i^k$.
⋮

Table 7.1: continued

Person	Work	Circumstantial evidence
James Gregory 1667	"Gregory" Series	Did not claim originality.
Gottfried Leibniz ca. 1672	"Leibniz" series	1. Newton argued that he had not understood the nature of the series, and had asked Gregory for details. Lack of understanding showed, according to Newton, that he was the second inventor. 2. Claimed he had looked at the work of Pascal. 3. Tried to put indivisibles on "sound" footing.
Isaac Newton ca. 1685	Sine series	1. Claimed credit for himself, mainly for Mādhava's sine series. 2. Also claimed credit for rigour, but his uxions had to be discarded because of their conceptual obscurity.
Leonhard Euler ca. 1740	Euler solver, "Pell's" equation, etc.	1. Wrote an article on Indian sidereal year. 2. Published first European solution of Fermat's challenge problem from Bhāskara, and called it "Pell's equation"; must have had access to Fermat's Indian sources. 3. His ODE solver similar to Indian interpolation techniques. 4. Received prize for work on longitude. 5. His continued fraction expansion for π similar to Indian continued fraction expansion.

Documentary Evidence

The documentary evidence from Indian sources has been fully covered in Chapter 3.

Matteo Ricci's search for Indian calendrical sources, de Nobili's polemics against the *Vedāṅga Jyotiṣa*, Jesuit publications displaying their knowledge of the local language and local books, Euler's article on the Indian sidereal year all provide direct documentary evidence of transmission.

Of course, someone might ask why Mercator, Clavius, Scaliger, Cavalieri, Galileo, Fermat, Pascal, Gregory, etc. did not acknowledge their pagan sources of knowledge in signed confessions recorded for posterity. (Newton acknowledged the whole stream of earlier European sources.)

There are several reasons why it is unreasonable to expect documents from European sources explicitly acknowledging transmission.

First, let us recall that because claims of transmissions have been used to further racist history, we needed a higher standard of evidence than is common in historiography. The higher standard of evidence we are using here corresponds to the current legal standard of evidence for proof beyond reasonable doubt, adequate to convict a person of murder. Singularly, documentary evidence does *not* play such a significant role here, compared to the evidence of opportunity, motivation, and circumstantial evidence. The reason is simple: given that the punishment for such a crime may involve a person losing part or all of his life, a confession is hardly to be expected. Recognizing this natural tendency towards self-preservation, under the Indian legal system, signed confessions are *inadmissible* as legal evidence, for it is naturally supposed that such confessions have been extracted under significant duress.

Likewise, one must recognize the natural tendency of European mathematicians towards self-preservation, under the circumstances of extreme religious intolerance in Europe, especially during the . As is now beginning to be pointed out even by Western scholars like Hobson, Europeans had so much to learn from India and China, so that if even a small part of it were acknowledged, there would have been a tremendous amount of documentary evidence—and the great paucity of documentary acknowledgements by Europeans is inexplicable, except under the above hypothesis.

Although documentary evidence is highly valued in a scriptural tradition, as in a bureaucracy, it is also very easy to manipulate. False documents can easily be created, and true documents can be suppressed. We have seen this in the case of the remark about "Euclid" or the suppression of Newton's history of the church. Further, given the differential costs of obtaining documentary evidence (I cannot, for instance, obtain a microfilm of Clavius' trigonometric tables), many unreasonable consequences ow from this emphasis on documentary evidence. On the basis of the absence of documents, those who did not produce or maintain documents have no history. This inequitable feature of the rules of evidence is painfully evident in contemporary society where absence of documents is often used as a pretext to deprive people (like forest dwellers) of their lands, on the grounds that there are no documents to establish their claim to have been staying where they may have been staying for over a century. Once again the double standards are evident: if direct documentary evidence is indeed such an essential thing, it should also be made impossible to convict anyone of theft or murder, until there is a signed confession to that effect. On the other hand, Indian law has rightly made such confessions legally inadmissible—recognizing also how easy it is to manipulate documentary evidence.

Next, the social value of priority in the West is manifest, and this value certainly existed also at that time—Newton, for instance, threatened to stop publication of his *Principia* to establish his priority over Hooke, whereas Cavalieri waited five years for Galileo to publish on the calculus. Further, we have seen how the Doctrine of Christian Discovery mandated that this priority should be assigned to a Christian—like the "discovery" of America by Columbus. Under these circumstances, Europeans saw it as personally advantageous, consonant with prevailing social practices—and even morally correct—to not acknowledge their "pagan" sources.

Next, to expose the duplicity of standards underlying the demand for documentary evidence, in such a situation of copying without acknowledgement, it is interesting to speculate what would happen if every recent publication (by prominent Westerners) were to be copied (non-verbatim) by others claiming to have independently rediscovered it, and shifting the onus of providing documentary evidence on those who claim that it was copied. To refute the claim of independent rediscovery, one would need to produce a document to establish that the person concerned had actually seen the work in question. There seems little reason why prominent Westerners should enjoy a monopoly on this strange rule of evidence, the privilege of which should be extended to all and sundry—and especially to thousands of researchers struggling in India and China without adequate libraries. There is not the slightest doubt that, if the work of prominent Westerners were to be systematically "independently rediscovered" in this way, there would be an outcry, and the "independent rediscoverers" would be branded as plagiarists without further ado, brushing aside as unreasonable the demand for the above kind of documentary evidence! The demand for documentary evidence varies with the direction of transmission.

Finally, this process of appropriating "pagan" knowledge to the West was assisted by dishonest European historians who rushed to give credit for any discovery to the first European or Christian name they could attach to it. For example, current histories associate trigonometry not with Āryabhaṭa but with Regiomontanus who comes some thousand years later, and obviously got his information from Arab sources. Such a piece of false history, once articulated, can quickly be made persistent—and this process of falsifying history continues to this day. (For an ironic contemporary example, see Appendix 7.A; for the theory of this racist history, see the previous chapter.) Over centuries, documents repeating the falsehood accumulate. Note that, in this same racist history, a document from the 13th c. CE is treated as valid documentary evidence of Archimedes' work from the −3rd c. CE. Going by this established historical norm of documentary evidence, there is still a lot of time left to build up the necessary documentary evidence!

In general, it is possible to defend untruth by seeking to sharpen the standards of evidence in an unreasonable way. For example, someone caught copying in an examination might insist on the evidence of video footage (knowing that no cameras were installed in the examination hall). We have, however, seen that it is possible to sharpen the standards of evidence in another way, which bypasses the demand for this particular (documentary) type of evidence. So let us move on to other means of eliciting the truth.

VI
THE EPISTEMOLOGICAL DISCONTINUITY

The epistemological evidence consists of two parts. If two students come up with identical (or very similar) answer sheets, then the way to determine whether one has copied is to test understanding, by means of an oral viva voce, or refer to the past background. The principles here are the following. First, an oral test is superior to a document as a means to test understanding. Second, one who writes without understanding is one who is articulating another's thoughts. The third principle is that if someone has a background of poor performance (in the immediate rather than remote past), and suddenly starts performing well by saying roughly the same thing as another with a long background of good performance, then an explanation is required.

While the calculus had a long and continuous past background in India, this background is missing in Europe, where the calculus appears suddenly. (Even if we grant the fairy tales about Archimedes, there is no development worthy of note between Archimedes and the 16th c.)

Secondly, compared to the clear and comprehensive understanding of the calculus in India, Europeans had difficulty in understanding it. These difficulties about the calculus persisted for nearly three centuries after its first appearance in Europe. It is understandable that new ideas are often not immediately accepted, but it is another matter that those proposing the new ideas are themselves not clear about what they are saying. Moreover, the persistence of this state of affairs for so long a period as three centuries requires a separate explanation.

Why was the new knowledge not immediately accepted? That the calculus was *transmitted* like the algorismus enables an immediate answer to this question. The difficulties about "indivisibles", " uxions", and "infinitesimals", that plagued the understanding of the calculus in Europe for centuries, can be understood as analogous to the difficulties with zero.

The calculus, being imported like the algorismus, involved a different *kind* of mathematics. Hence, it involved epistemological differences, right at the outset, as evinced by Descartes' difficulty. Indian geometry naturally considered curved lines since it used a exible rope (rather than a rigid ruler) for measurement. Hence, there were was complete conceptual clarity about the meaning of the length or circumference of a circle. Simply lay the rope along the line, and measure it, as one would measure a straight line. European geometry, however, was based on the straight line and the straight edge. As we saw in Chapter 1, many Europeans doubted whether measuring the length of even a straight line was part of geometry: and these beliefs decisively in uenced Hilbert's synthetic interpretation of the *Elements* which disallows measurement (and which interpretation became part of the school curriculum). While Descartes championed metric geometry, he believed that only straight lines could be measured, and hence stated in his *La Geometrie* that calculating the length of a curved arc was "beyond the capacity of the human mind". Descartes incorrectly presupposed that measurement necessarily involved a rigid rod. With this presupposition measuring the length of a straight line made sense; but measuring the length of a curved line was visualized as a process whereby the curved line was broken into an infinity of infinitesimal parts. Because Descartes' understood mathematics as perfect each part *had* to be infinitesimally small for it to be measurable by means of a straight rod. Consequently there had to be an infinite number of such parts to be measured and the result summed. Hence, Descartes thought, like Galileo, that this process of measuring the length of curved lines involved infinity which was beyond the grasp of the human mind. Presumably Descartes' statement expressed also his opinion about the techniques in Indian texts then being used by his contemporaries Fermat and Pascal, without naming them.

Clearly, the epistemologically new features of the calculus especially disturbed minds accustomed to anti-empirical ways of doing mathematics. The present-day classification of "pre-calculus" and "calculus" by historians of mathematics[65] is just another indication of the persistent nature of those epistemological difficulties. There is no way to comprehend this classification, since there was no clear epistemological advance until Dedekind, and all that happened was that Newton and Leibniz conferred a certain social respectability on the calculus, which had been denied to it under the in uence of Galileo and Descartes.

An analogous epistemological discontinuity had occurred earlier in Europe in relation to the algorismus. The suspicion then centred around zero, and the technique of zeroing the non-representable. Because of these suspicions, it took Europe some five centuries to assimilate the algorismus. Therefore, it is not hard to understand that the techniques of the infinitesimal calculus were viewed with great suspicion, and it took over three centuries for these techniques to be accepted as valid mathematics, after the formalisation of the real number system and mathematical analysis in the 19th/20th c. CE. This issue is taken up in the subsequent chapters.

A key point to note is that people do not simply grab any foreign piece of knowledge, even when it is of tremendous practical importance, especially when it conflicts with an established tradition. Hence, Europeans, accustomed to geometric techniques of navigation, did not shift immediately to a technique of navigation based on mental calculation, but waited for a couple of centuries, for the development of the chronometer, an appliance that could be mechanically used without application of the mind. Calculations are, likewise, used today, now that calculations can be performed mechanically, without application of the mind!

APPENDIX 7.A
THE TRANSMISSION OF THE TRANSMISSION THESIS

The manner in which history has been written, and still continues to be written, was brought home to me in a rather personal way in the course of writing this book. It would not be appropriate to discuss all aspects of the matter at this stage and in this book—some details have already appeared prominently in newspapers, which can be pursued by those interested. However, given the striking parallels between the transmission of the calculus and the transmission of the transmission thesis, a few facts can certainly be very brie y recapitulated here to illustrate in a striking way the point of the preceding principles of evidence in historiography.

> In 2001 a paper appeared in a little known journal on the subject of the transmission of the calculus from India to Europe.
>
> The trio of authors cited several of my papers; however, they did not cite some key papers, important ideas from which were used in their publication in a significant way, and to which papers of mine the authors undoubtedly and undeniably had access. (See below.)
>
> Sometime around 2003 an essay by a student appeared on a well-known website (McAndrews) on the history of mathematics. The essay gave the entire credit to the trio for various ideas related to the alternative epistemology and transmission of the calculus, without once mentioning my name. (The misleading nature of the article was, subsequently, brought to the attention of the student, as also those responsible for maintaining the website, but they refused to withdraw it. A physically or digitally signed statement of such refusal was also refused. Nor did they subsequently make it historically more accurate.)
>
> In 2003 the above paper and student essay were brought to my notice, through a column and letter written by Subhash Kak, which sought to publicize this student essay and what he mistakenly called the work of "three British mathematicians". (None of the three was either a British national or a mathematician, though they were Christians of various denominations—Roman Catholic, of Portuguese descent, Syrian Christian, etc. None of the trio has a doctorate degree in mathematics—one has no doctorate degree, while the other two have doctorates in physics and Greek classics respectively.)

In the case of the calculus, the principle of epistemological continuity was used above, by pointing out the thousand year old background of the calculus in India compared to its sudden appearance in Europe. Analogously, by 1998, I had already done enough preparatory work in connection with the transmission thesis to obtain a project from the Indian National

Science Academy (INSA), on "Madhava and the Origin of the Differential Calculus". A position for a research associate in this project was advertised on 18 July 1998, and appeared in the Historia Matematica website (http://sunsite.utk.edu/math_archives/.http/hypermail/historia/jul98/0067.html) as follows.

> Wanted: Research Associate in the History of Mathematics
>
> This is a unique opportunity to work in a high-profile area for the project
>
> Madhava and the Origin of the Differential Calculus,
>
> sponsored by the Indian National Science Academy, coordinated by C. K. Raju. The project seeks to revise the current history of the calculus. It will focus on early developments in the calculus 1400-1720, and will cover all aspects of the Madhava/Gregory/Taylor series expansions, and its transmission from the Malabar coast to Europe, especially through manuscripts of Jyeshthadeva's Yuktibhasha... the work will involve a close comparison of the contents of these manuscripts with some of the work of Kepler, Cavalieri, Fermat, Pascal, Wallis, Gregory, Newton, Leibniz and Taylor.

Next let us look at the question of opportunity. Following the above advertisement, one of the trio of authors was selected for this INSA project as a Research Associate. To cut a long story short, in 2000 he was asked to resign on ethical grounds, and did so retrospectively, but failed to return a variety of source materials. This failure to return source materials was acknowledged. Shortly before the trio's paper was submitted, in Feb 2001, in a signed handwritten statement, setting out an unconditional apology, on 8 November 2000, one of the trio of authors, J. K. John, promised to return all the source materials of the INSA project still in his possession, and thereafter did return some of them, though not all.

After the appearance of the above advertisement, another member of the trio, D. F. Almeida, visited me, and a collaboration was set up. In 1999, shortly after the process of writing this book was formally initiated, I visited the School of Education, University of Exeter, where Almeida was based, and gave a talk there, on epistemological issues, based on a paper on "Mathematics and Culture" that was subsequently reproduced in Philosophy of Mathematics Education (http://www.people.ex.ac.uk/PErnest/pome11/art18.htm. The University of Exeter subsequently funded my brief visit to Rome, along with a translator, for collection of source materials. The result of this collaboration was to be jointly reported in the paper on transmission of the calculus[66] to be presented at a conference in Trivandrum in Jan 2000. (This is one of the papers not cited by the trio; at the suggestion of one of the organizers of the conference, G. G. Joseph, the paper was split into two parts, to provide more time for presentation.)

However, I was suddenly invited to the 8th East West conference in Hawai'i, with overlapping dates, in Jan 2000. It was agreed that I would go to Hawai'i, while Almeida would present the paper in Trivandrum. Almeida, being in the School of Education, felt that this invitation for a plenary talk in an international conference, that too in a session related to technology and education, would greatly add to the credibility of these ideas with his colleagues, and was very particular that I should send him a copy of the paper,[67] when I last met him in Goa in Dec 1999. (This Hawai'i paper is the other key paper not cited by the trio.) At that time, Almeida also formally agreed to be co-author of a chapter in this book, originally conceived as a series of essays by different authors. Later he asked for the revised copy of the paper in connection with a bid for funds from the Leverhulme trust, a bid in which G. G. Joseph, then a Reader in Economics at the University of Manchester, was invited to join, on the grounds that a British citizen was required to obtain this funding, and he also had some popular writings on the history of mathematics. At this stage the collaboration was terminated, due to disagreements, and I pointed out that it would be unethical for others to continue pursuing these ideas without my participation.

Finally, my Bangalore talk on the transmission of the calculus,[68] in Dec 2000, happened to be in a session chaired by G. G. Joseph, who naturally had a copy of the detailed abstract, and was at that time giving the School of Education of the University of Exeter as one of his affiliations, and was obviously associated with at least one member of the trio, though he, himself, was not a signatory to the trio's paper submitted subsequently on 22 February 2001—it is not necessary to go here into what transpired in Bangalore.

It would not be appropriate to discuss motivation etc. in the context of this book, although I have discussed it elsewhere, for instance in my formal complaint to the University of Exeter.

Finally, there is the principle of epistemological discontinuity which can be very well illustrated in the context. The principle is very simple. Those who copy without acknowledgement, also very often copy without adequate understanding. Therefore, lack of understanding is a good indication of lack of originality.

This lack of understanding is barely illustrated here using a couple of the more obvious howlers in the trio's paper.

The authors state[69] (p. 87)

> latitude was determined in the northern hemisphere by **measuring** the polar star **declination** (the angle of the pole star)—latitude was approximately equal to the altitude of the pole star. [Emphasis added]

As the deliciously vague phrase "angle of the pole star" suggests, there is a confusion here between the two angles: DECLINATION and ALTITUDE. The meaning of the sentence is quite unambiguous: the authors intend that the declination of the pole star is to be measured, and the altitude is presumably to be calculated!

This, of course, defeats a key aspect of the novel[70] thesis that was advanced above: namely that Jesuits searched for calendrical manuals in India because Europe then needed a good calendar for navigation. Why was a good calendar needed for navigation? According to my novel thesis, a good calendar was needed just because there was no easy way to measure declination at sea, but the (solar) declination could be easily estimated using a calendar, provided the calendar correctly fixed the day of the equinox. So if declination could have been *measured* so easily and directly at sea in the 16th c. CE, there would hardly have been any European need for a good calendar!

That this is no typo, but involves a conceptual confusion, is clear in the next howler, when the trio subsequently speak of

> **measuring** the solar **declination** at noon and then looking up tables correlated with the calendar. [Emphasis added]

Since, according to the repeated claim made by the authors, the solar declination could be directly measured at sea, and since it is the case that altitude could easily be observed with a simple instrument like a cross-staff (or *kamāl*), latitude could be readily calculated, using the *Laghu Bhāskarīya* formula. So what on earth was a "table correlated with the calendar" needed for? To help the navigator determine the date, perhaps!

That this is no typo, but a conceptual confusion, is proved beyond all reasonable doubt, when the authors repeat the same thing a third time, on the next page:

> **observations** of solar **declination** or pole star.... [Emphasis added]

It was, I believe, an established principle in Europe since the 17th c. CE to "booby trap" a mathematical table by deliberately injecting errors in it, just as some computer programmers (like me) have been known to booby trap source code (when compelled to disclose it against their wishes to persons whose credentials are not established) by deliberately injecting bugs in it. The source of these errors can be found in the first part of the Trivandrum paper NOT cited by the trio, which makes the same mistake, on p. 6:

> The widely distributed *Laghu Bhaskariya* (abridged works of Bhāskara) and *Maha Bhaskariya* (extensive works of Bhaskara) of the first Bhaskara (629 CE) explicitly detailed methods of determining the local latitude and longitude, using **observations** of solar **declination** or pole star, and simple instruments like the gnomon, and the clepsydra. Since local latitude could easily be determined from solar **declination** by day and e.g. pole star altitude at night (using an instrument like the kamal) an accurate sine table was just what was required.... [Emphasis added]

Since the objective here is only to illustrate the principles of evidence used to establish transmission, we take up just one more example to demonstrate the consequences of conceptual confusion regarding key aspects of the transmission thesis closely related to my other key (Hawai'i) paper that is also not cited by the trio. This involves a somewhat subtler point.

In the abstract of that paper, "Computers, mathematics education, and the alternative epistemology of the calculus in the Yuktibhāṣa", presented before a large number of scholars at Hawai'i, I had argued as follows:

> Current (formal) mathematics, being socially constructed, may change with technology.... Computers also use a different notion of number': unlike Turing machines, computers necessarily use oating point numbers, fundamentally different from real numbers on which mathematical analysis is currently based. An alternative pedagogy and epistemology of the calculus, bypassing real numbers is thus needed. A suitable alternative epistemology is found in the c. 1530 CE YuktiBhasa of Jyeshthadeva... Given the practical uses of computer simulation and the consequent social pressure to teach a changed notion of number' can the incompatible epistemologies of mathematics be reconciled?

Or even more succinctly, as stated in the four-line abstract of the paper for the table of contents of *Philosophy East and West*:

> Current formal mathematics, being divorced from the empirical, is entirely a social construct... Computer technology, by enhancing the ability to calculate, has put pressure on this social construct....

The paper pointed out the representation of real numbers involves a supertask not necessary for practical purposes.

> For practical purposes, no supertask is necessary; the representation of numbers on a computer is satisfactory for mathematics-as-calculation, but it is unsatisfactory or "approximate" or "erroneous" from the standpoint of mathematics-as-proof. Indian mathematics, which dealt with "real numbers" from the very beginning ($\sqrt{2}$ finds a place in the *śulba sūtras*), does not represent numbers by assuming that such supertasks can be performed, any more than it represents a line as lacking any breadth, for the goals of mathematics in the Indian tradition were practical not spiritual. The **Indian** tradition of mathematics worked with a finite set of numbers, **similar** to the numbers available on a computer, and similarly adequate for practical purposes. Excessively large numbers, like an excessively large number of decimal places after the decimal point, were of little practical interest. Exactly what constitutes "excessively large" is naturally to be decided by the practical problem at hand so that no universal or uniform rule is appropriate for it. [p. 340, emphasis added][71]

The trio seize without acknowledgement this thesis that I had presented a year earlier in Hawai'i:

> we believe that mathematics is a social construct that alters with changing technology and that the current revolution in information technology will induce changes in mathematics.... (p. 96)

How is this to be linked to Indian mathematics? They take off:

> we re-iterate that oating point numbers were used by the Kerala mathematicians.... (p. 96)

Note that the thesis has been slightly changed: the term *similar* has been dropped, changing the thesis from analogy to identity, and the term *Indian mathematics* has been replaced by "Kerala mathematicians". Note also how these slight changes have oversimplified the thesis, laying it open to all sorts of doubts. (Where did Kerala mathematicians use the concepts of non-normal numbers and gradual under ow that one associates with oating point numbers?[72] Why only mathematicians confined to Kerala? Did they use numbers in a way different from other Indian mathematicians? What are the sources for this belief about use of numbers? etc.)

Through this oversimplification, the trio betray their lack of acquaintance with the philosophy of number underlying Indian mathematics. The problem with this is, as Nāgārjuna remarks, a half-understood concept of *śūnya* can be as fatal as a snake grasped wrongly—even slightly wrongly. This lack of understanding proves fatal to the trio's thesis as follows. Not quite understanding the Indian philosophy underlying the use of number, the trio of authors revert to a seemingly safe and conventional Western position (p. 96):

> We accept that mathematical analysis is based on the complete real number system needed for the existence of limits and that limiting processes can never be accomplished [sic] by a computer which uses a oating point number system.

However, this sudden reversion introduces a clash of epistemologies which stalls the original thesis in mid air, resulting in the inevitable crash. For, what after all is the use of Indian mathematics in the context? The trio continues

> we believe that a study of Keralese calculus will provide insights into computer-assisted teaching strategies for introducing concepts in *mathematical analysis*.... [p. 96, emphasis mine]

But how on earth can oating point numbers be used to motivate or teach formal real numbers? That amounts to putting the cart before the horse! And even supposing that oating point numbers (and concepts like non-normal numbers) can somehow be used to motivate formal real numbers, why not simply use computers for this purpose? Thus, it seems quite obvious to me that the task of computer-aided mathematics teaching can be performed perfectly well by software like my CALCODE (Calculator for Ordinary Differential Equations, which was purchased by the University of Exeter), especially since the ultimate object is to teach *mathematical analysis*! So, why bring in "Kerala mathematics" at all? Of course, the easiest way to understand the origin of these insoluble problems is to suppose that these problems have arisen from the oversimplification of a complex thesis, used without acknowledgement.[73]

The more important point here is to observe how the attempt to bring a novel thesis into a conventional epistemic fold so quickly makes it meaningless. This is exactly what happened also in the case of the calculus when it came to Europe with an epistemology of mathematics and number, that was incompatible with the European perspective into which it was forced to fit.

NOTES AND REFERENCES

1. A revolution this might have been in European thought, but the theory was that of Ibn as Shātir, from Damascus, and heliocentrism was a part of the Arabic and Indian tradition for at least a few centuries before that. Otto Neugebauer, "On the Planetary Theory of Copernicus," *Vistas in Astronomy*, **10** (1968) pp. 89–103. "The question therefore is not whether, but when, where, and in what form he [Copernicus] learned of Marāgha theory". N. M. Swerdlow and O. Neugebauer, *Mathematical Astronomy in Copernicus's De Revolutionibus*, Part 1, Springer, New York, 1984, p. 47. (Studies in the History of Mathematics and Physical Sciences 10.) George Saliba, *A History of Arabic Astronomy*, New York University Press, New York, 1994, Chapter 15, "Arabic Astronomy and Copernicus", p. 291. The heliocentric theory was one of the competing theories in Indo-Arabic astronomy, from before al Bīrūnī, and a reference to it may be found even in the poetry of Amir Khusrau, for which last reference I am grateful to Professor Javed Ashraf.
2. Charles Whish [1832], "On the Hindu quadrature of the circle and the infinite series of the proportion of the circumference to the diameter exhibited in the four Shastras, the Tantrasamgraham, Yukti-Bhasa, Carana Padhati, and Sadratnamala," *Trans. Royal Asiatic Society of Gr. Britain and Ireland* **3** (1835) pp. 509–523. G. Heyne had remarked earlier that these developments were probably not confined to Kerala but were available also in Tamil Nadu, Telangana, and Karnataka, though this possibility has not yet been properly investigated. J. Warren, *Kāla Sankalitā*, Madras 1825, pp. 93, 309–310.
3. *Karaṇa-paddhati of Putumuna Somayāji*, ed. P. K. Koru, Astro Printing and Publishing Co., Cherp (Kerala), 1953, p. 203; *Karaṇa Paddhati of Putumuna Somayāji*, ed. S. K. Nayar, Govt. Oriental Manuscript Library, Madras, 1956, pp. 189–193. For an exposition of the numerically efficient formula, see C. K. Raju, "Kamāl or rāpalagāi," *Ninth Indo-Portuguese Seminar on History*, INSA, New Delhi, Dec. 1998. In: *Indo-Portuguese Encounters: Journeys in Science, Technology and Culture*, ed. Lotika Varadarajan, Indian National Science Academy, New Delhi, and Universidade Nova de Lisboa, Lisbon, 2006, vol. 2, pp. 483–504.
4. For the text and translations, see, e.g. C. K. Raju, "Approximation and proof in the Yuktibhāṣā derivation of the Mādhava sine series". Paper presented at the National Seminar on Applied Sciences in Sanskrit Literature, Various Aspects of Utility, Agra, 20–22 Feb 1999. (In Proc.)
5. C. K. Raju, "The mathematical epistemology of śūnya", *The Concept of Śūnya*, ed. A. K. Bag and S. R. Sarma, IGNCA, INSA, and Aryan Books International, 2002, pp. 168–181.
6. C. K. Raju, "Computers, mathematics education, and the alternative epistemology of the calculus in the Yuktibhāṣā", *Philosophy East and West* **51** (3) pp. 325–362. C. K. Raju, "How should Euclidean' geometry be taught?", *History of Science: Implications for Science Education*, ed. G. Nagarjuna, Homi Bhabha Centre, TIFR, Bombay, 2002, pp. 241–260.
7. Written in abbreviated form as "Dedd. reckoning".
8. G. H. Tibbets, *Arab Navigation in the Indian Ocean Before the Coming of the Portuguese* (Tr. of Ibn Majid's *Fawā id*), Royal Asiatic Society, London, 1971.
9. The Arabic *zām*, derived from the Indian *yāma* or *prahara*, which corresponds to 8 ghatis or approximately 3 hours. (The Indian system divided the day not into 24 hours or 60 minutes, but, in accord with the sexagesimal system, into 60 ghatis of 24 minutes.) Later on, the *zām* itself became a unit of distance, as "the distance from here to the horizon". See, C. K. Raju, "Kamāl or rāpalagai", cited above.
10. J. W. Norie, *Norie s Nautical Tables*, London, 1864, pp. 59–60.
11. Douglas Peck, "Columbus used dead reckoning navigation in his 1492 voyage of discovery to the New World", *Encounters: a Quincentenary Review* (Summer, 1990) pp. 18–21.
12. According to some accounts, Vasco da Gama did carry an astrolabe, which, however, could be used only on land. Furthermore, he also possibly had a table of solar declinations, compiled by Zacuto, which he could not use for obvious reasons.
13. E.g. Dava Sobel, *Longitude: The True Story of a Lone Genius who Solved the Greatest Scientific Problem of his Time*, Penguin, 1996, and a condensed one-paragraph account of the same in G. J. Whitrow, *Time in History*, Oxford University Press, 1989, p. 141.
14. J. W. Norie, 1864, cited earlier, p. 262.
15. *Laghu-Bhāskarīya*, and also *Mahā-Bhāskarīya, Bhāskara I and His Works*, Part I and Part II respectively ed. and trans. K. S. Shukla, Department of Mathematics and Astronomy, Lucknow University, 1963. Prior to Varāhamihīra, the term *jyotiṣa* referred simply to timekeeping and had no connotation of astrology. Moreover, even Varāhamihīra, in his *Pañcasiddhāntikā*, does not mention any astrology.
16. *The Travels of Pietro Della Valle in India*, trans. Edward Grey, reprint, Asian Educational Services, New Delhi, 1991, vol. 1, pp. 11–12.

17. *Laghu Bhāskarīya*, III.22–23.
18. *Gītikā* 3-4. *Āryabhaṭīya of Āryabhaṭa* ed. and trans. K. S. Shukla, and K. V. Sarma), INSA, New Delhi, 1976, pp. 6–7. This incidentally also shows the mindlessness with which Western scholars keep parroting the claim that Āryabhaṭa's work was copied from that of Ptolemy! However, as a consequence of this claim, even today many people misread Āryabhaṭa's accurate statement of the length of the sidereal year as an inaccurate statement of the length of the tropical year.
19. These included the publication of Hyginus's *Poeticon Astronomicon*, by Erhardt Ratdolt, Venice, 1482, the *Phaenomenon* of Aratus published by Firmicus Maternus Julius, as *Mathesos Liber*, Venice 1499. Hyginus's *De Mundi et Sphere*, Venice, 1512, the planisphere and star charts of Albrecht Durer (1515), and the star charts of Johannes Hunter, *Imagines Constellationum Borealium*. . . , Basel, 1541, Alessandro Piccolomini, *De le stelle fisse*, Venice 1540, and its ten subsequent editions, Giovanni Paolo Galucci, *Theatrum Mundi*, Venice 1588.
20. *The Principal Works of Simon Stevin, Vol. III. Astronomy and Navigation*, ed. A Pannenkoek and Ernst Crone, Swets and Seitlinger, Amsterdam, 1961.
21. Christoph Clavius, c. 1575? "A method of promoting mathematical studies in the schools of the society", Document No. 34 in E. C. Phillips, "The proposals of Father Christopher Clavius, SJ, for improving the teaching of mathematics", *Bull. Amer. Assoc. Jesuit Scientists* (Eastern Section), **18** (1941) (No. 4) pp. 203–206.
22. Christophori Clavii Bambergensis e Societate Iesv, *Epitome Arithmeticae Practacae*, Dominici Basae, Rome, 1583, Tr. into Chinese by Matteo Ricci.
23. D. Ferroli, *The Jesuits in Malabar*, 1939, Vol. 2, p. 402.
24. P. S. S. Pissurlencar, "Concerning the first Marathi books printed in Goa" (translated title), *Bol. Inst. Vasco da Gama, Panaji*, **73** (1956) pp. 55–79.
25. Letter by Matteo Ricci to Petri Maffei on 1 Dec 1581. *Goa* **38 I**, ff 129r–30v, corrected and reproduced in *Documenta Indica*, **XII**, 472-477 (p. 474). Also reproduced in Tacchi Venturi, *Matteo Ricci S.I., Le Lettre Dalla Cina 1580–1610*, vol 2, Macareta, 1613. Ricci states, "Com tudo no me parece que sera impossivel saberse, mas has de ser por via d algum mouro honorado ou brahmane muito intelligente que saiba as cronicas dos tiempos, dos quais eu procurarei saber tudo."
26. V. Cronin, *A Pearl to India—the Life of Roberto de Nobili*, Darton, Longman and Todd, 1966, pp. 178–80. Acquiring access to the Veda was a great achievement for de Nobili who falsely gave out that he was a Brahmin and refrained from overt contact with the Mission for several years.
27. Varāhamihīra, *Pañcasiddhantikā*, III.21, trans. G. Thibaut and Sudhakara Dwivedi, [1888], Reprint, Chowkhamba, Varanasi, 1968, p. 18. At the time of the *Vedāṅga Jyotiṣa* the winter solstice was at the beginning of the constellation Sravistha (Delphini), and the summer solstice was in the middle of Āslesā. Varāhamihīr stated that in his time the summer solstice was at the end of three quarters of Punarvāsu and the winter solstice at the end of the first quarter of Uttarāsādha, so that there had been a precession of the *nakṣatra* of about 23 degree 20 minutes in roughly 1700 years, between the *Vedāṅga Jyotiṣa* and Varāhamihīra, taking the time period of precession as about 26000 years, or roughly about 1 degree in 72 years.
28. G. V. Coiner, SJ, M. A. Hoskin, and O. Pedersen (eds) *Gregorian Reform of the Calendar, Proceedings of the Vatican Conference to Commemorate its 400th Anniversary 1582–1982*. Specola Vaticana, 1983, Part VI.
29. Shakespeare, *A Winters Tale*, iv, 2, Let me see. Every 'leven wether tods; every tod yields pound and odd shilling; fifteen hundred shorn, what comes the wool to? . . . I cannot do't without counters'. [11 wether [sheep] give one tod [28 lbs] of wool, which sells for a guinea [21 shillings]. How much is the wool from 1500 sheep?]
30. *Āryabhaṭīya*, cited earlier. See Chapter 3.
31. E.g. W. G. L. Randles, "Pedro Nunes and the discovery of the loxodromic curve, or how, in the sixteenth century, navigating with a globe had failed to solve the difficulties encountered with the plane chart". *Revista da Universidade de Coimbra*, **35** (1989) pp. 119–30.
32. *Alguns Documentos da Torre do Tombo*, ed. J. Ramos Coelho, Lisbon, 1892, pp. 138–139.
33. Pedro Nunes, *Defensao do Tratodao da Rumacao do Globo para a Arte de Navegar*, Coimbra, 1952 [pre 1566].
34. D. J. Struik, *A Source Book on Mathematics, 1200–1800*, Harvard University Press, Cambridge, Mass., 1969, p. 253.
35. Edward Wright, *Certaine Errors in Navigation, Detected and Corrected*, London, 1610. And E. Wright, ca. 1599, "A chart of the world on Mercator's projection", in: Richard Hakluyt, *The Principall Navigations, Voiages, Traffiques and Discoveries of the English Nation*, London, 1598-1600.

36. Thus, the traveller's description of measurement of solar altitude continued: "...With this instrument, and several others, many of the English perform'd their operations every day; such as knew not how to do them well were instructed; and if any one err'd, in computation or otherwise, his error was shew't him, and the reason told him, that so he might be train'd to work exactly.... In the Portugal ship ... the contrary came to pass; because the Pilots, being extremely jealous of their affairs... will be alone to make their observations, and for the most perform them in secret without an Associate to see them. Should any other person in the ship offer to take the altitude of the Sun, or look upon the Map or Compass, or do anything that relates to the well-guiding of the Vesser and knowing its course, they would quarrel with him. *The Travels of Pietro Della Valle in India*, trans. Edward Grey, reprint, Asian Educational Services, New Delhi, 1991, vol. 1, pp. 11–12. Thus, in the present instance, Mercator kept his method a secret, while Stevin and Wright published it, and it was used only after that.
37. Joseph Needham, *The Shorter Science and Civilization in China*, vol. 2, abridged by Colin A. Ronan, Cambridge University Press, 1981, p. 123. Alexander Hellemans and Bryan Bunch, *The Timetables of Science*, Simon & Schuster, New York, 1988, p. 70.
38. Stevin, cited above.
39. Christophori Clavii Bambergensis, *Tabulae Sinuum, Tangentium et Secantium ad partes radij 10,000,000...*, Ioannis Albini, 1607. As the title suggests, this table concerns not trigonometric values, as today understood, but values of functions such as the Rsine, which are what are given in Indian manuscripts. Stevin follows the same practice for his secant tables, *The Haven Finding Art*, cited above, p. 483.
40. J. Fauvel and J. Gray, *The History of Mathematics*, Macmillan, 1987, p. 264.
41. In particular, it is possible that the Malayalam *Yuktibhāsā* was the first text obtained by the Jesuits. As noted earlier in Chapter 3, and also by Srinivasiengar, the *Yuktibhāsā* does *not* fully explain the derivation of the infinite series, and this would further explain why the early attempts to understand these infinite series in Europe went awry.
42. Al Bīrūnī's method was, first, to measure the height of a hill by measuring the angles subtended by the hill at two points a known distance apart, and applying the usual trigonometric formula. He, then, climbed the hill and measured the angle of the dip of the horizon. Assuming the earth to be a perfect sphere, the line of sight is tangential to the sphere, hence orthogonal to the radius, and a simple calculation gives the radius, hence the circumference of the earth. Al-Bīrūnī's value of the radius of the earth was equal to 3928.77 English miles (using 1 Arabic mile = 1.225947 English miles). This compares favourably with the mean radius of curvature of the reference ellipsoid at his latitude which is 3947.80 miles. it has been surmised that al Bīrūnī's self-constructed instrument could measure angles only up to 1 minute of the arc; however, as we showed in Chapter 5, it is easy to construct a hand-made instrument which is accurate to 10′ of the arc. So the key to the precision is precise sine values, which he seems to have obtained from various Indian sources including *Karaṇa Tilak* of Vijay Nandi of Benares. See, S. S. H. Rizvi, "A newly discovered book of Al-Bīrūnī: *Ghurrat-uz-Zijjāt*, and al Bīrūnīs measurements of earth's dimension", *Al Bīrūnī Commemorative Volume*, ed. H. M. Said, Hamdard Academy, Karachi, 1979, pp. 605–680, and H. M Said and A. Z. Khan, *Al Bīrūnī: His Times, Life and Works,* Hamdard Academy, Karachi, 1981, pp. 164–173. The latter book also points out various methods of determining longitude mentioned by al Bīrūnī.
43. C. K. Raju, "Kamāl..." cited earlier.
44. *Laghu Bhāskarīya*, II.8; tr. adapted from (ed. and trans.) K. S. Shukla, cited earlier, p. 53.
45. The Colledge will the whole world measure,/ Which most impossible conclude, /And Navigators make a pleasure/ By finding out the longitude./ Every Tarpalling shall then with ease/ Sayle any ships to th'Antipodes.
46. *Catholic Encyclopaedia*, article on Cochin.
47. K. S. Mathew, *Portuguese Trade with Indian in the Sixteenth Century*, Manohar, New Delhi, 1983, p. 35.
48. *Diario di Viagem de Vasco da Gama*, vol. I, p. 59; reproduction of the 16th c. manuscript in the Municipal Library, Porto, No. 804; E. G. Ravenstein, *A Journal of the First Voyage of Vasco da Gama 1497–99*, London, 1898; cited in K. S. Mathew, *Portuguese Trade with India in the Sixteenth Century*, Manohar, New Delhi, 1983, p. 13.
49. K. S. Mathew, *Portuguese Trade with India*, cited earlier, p. 57.
50. E.g., Francois Pyrard de Laval, *The Voyage of Francois Pyrard de Laval*, trans. Albert Gray, London, 1610.
51. *Correspondence du P. Marin Mersenne*, 18 volumes, Presses Universitaires de France, Paris, 1945–. Letter from the astronomer Ismael Boulliaud to Mersenne in Rome, Vol. XIII, pp. 267–73. C. K. Raju and Dennis Almeida, "Transmission of the calculus from Kerala to Europe, part 2", Paper presented at the Aryabhata Conference, Trivandrum, Jan 2000.

52. Christophori Clavii Bambergensis, *Tabulae Sinuum, Tangentium et Secantium ad partes radij 10,000,000...*, Ioannis Albini, 1607.
53. V. Cronin, *A Pearl to India—the Life of Roberto de Nobili*, Longman and Todd, Darton, 1966, pp. 178–80.
54. J. Gilder and A.-L. Gilder. *Heavenly Intrigue: Johannes Kepler, Tycho Brahe and the Murder Behind One of Historys Greatest Scientific Discoveries*. Doubleday, 2004.
55. J. Kepler. *New* Astronomy, trans. W. H. Donahue, Cambridge University Press, 1992.
56. Due to engineering problems, Tycho's massive instruments, like those of Jai Singh, did not come up to the theoretical expectations.
57. "Planet fakery exposed. Falsified data: Johannes Kepler", *The Times* (London) 25 January 1990, 31a. William J. Broad, "After 400 years, a challenge to Kepler: he fabricated data, scholars say", *New York Times*, 23 January 1990, C1, 6. William Donahue, "Kepler's fabricated figures: covering up the mess in the *New Astronomy*", *Journal for the History of Astronomy*, **19** (1988) pp. 217–37.
58. W. Vanderburgh, "Empirical equivalence and approximative methods in the *New Astronomy*: A defense of Kepler against the charge of fraud", *Journal for the History of Astronomy*, **27** (1997) pp. 317–336.
59. William Wallace, *Galileo and his Sources: The Heritage of the Collegio Romano in Galileo s Science*, Princeton University Press, 1984.
60. For a quick review, see Paolo Mancosu, *Philosophy of Mathematics and Mathematical Practice in the Seventeenth Century*, Oxford University Press, Oxford, 1996, pp. 118–122. It was for similar reasons that Descartes maintained that the length of curved lines was beyond the human mind.
61. D. Struik, *A Source Book in Mathematics*, cited earlier, p. 29.
62. D. Struik, *A Source Book in Mathematics*, cited above, p. 30.
63. *Bījagaṇita of Śrī Bhāskarācārya*, ed. Sudhakara Dvivedi, Benares, 1927 (Benares Sanskrit Series, No. 159), chapter on *cakravāla*, p. 40. For a formalised account of Bhāskara's *cakravāla* method, see I. S. Bhanu Murthy, *A Modern Introduction to Ancient Indian Mathematics*, Wiley Eastern, New Delhi, 1992, pp. 114–16. Note that Bhanu Murthy's book has a typo here.
64. Euler's article was an appendix to T. S. Bayer, *Historia Regni Greacorum Bactriani*; see G. R. Kaye, *Hindu Astronomy*, 1924, reprinted Cosmo Publications, New Delhi, 1981, p. 1.
65. Carl B. Boyer, *A History of Mathematics*, Wiley, New York, 1968; C. H. Edwards, *The Historical Development of the Calculus*, Springer, Berlin, 1979.
66. C. K. Raju and Dennis Almeida, "Transmission of the calculus from India to Europe, Part I: motivation and opportunity". "Part II: circumstantial and documentary evidence", papers presented at the International *Aryabhata Conference*, Trivandrum, Jan 2000.
67. "Computers, mathematics education, and the alternative epistemology of the calculus in the *YuktiBhāṣā*", invited plenary talk at the *8th East-West Conference*, University of Hawai'i, Jan 2000. In *Philosophy East and West*, **51**:3, July 2001, pp. 325–362. The issue of transmission of the calculus from India to Europe is taken up in the section on the History of the Calculus, especially from paras 3, 4 et seq., p. 351. A draft copy of the paper is available from http://www.IndianCalculus.info/Hawaii.pdf
68. "How and why the calculus was imported into Europe". Talk delivered at the International Conference on *Knowledge and East-West Transitions*, National Institute of Advanced Studies, Indian Institute of Science Campus, Bangalore, Dec 2000. Available at http://www.IndianCalculus.info/Bangalore.pdf
69. D. F. Almeida, J. K. John and A. Zadorozhnyy, "Keralese mathematics: its possible transmission to Europe and the consequential educational implications", *Journal of Natural Geometry* **20** (2001) pp. 77–104.
70. The relation of the calendar to navigation seems never to have been discussed in the Western history of science.
71. To demonstrate these aspects of the notion of number, the illustrative C programs in Chapter 2 were actually compiled and run "live" before the assembly in Hawai'i.
72. For details regarding the concept of non-normal numbers, see e.g. my *Lecture Notes in C* (to appear), based on the corresponding televised lectures.
73. Apart from this, there is, of course, circumstantial evidence, and documentary evidence etc., which it is not necessary to go into here. Basically, this is not a personal issue, especially after one of the authors (Almeida) has offered an unconditional apology, if only by email. The reason to take up this issue here was that an author cannot avoid certain seemingly personal matters. For example, failing to spell out one's political stand in a social science book would be seen as being evasive. Likewise, if a theory of transmission cannot be applied re exively, that could well be seen as a lacuna in the theory, for contemporary events are obviously an excellent test of the theory. These contemporary events also serve as an excellent illustration of the epistemic test. The epistemic difficulties of Newton et al. are hardly as obvious as the mistakes

made in the contemporary case. Again contemporary events provide a wealth of detail not available about the past. For example, in the context of the earlier assertion of how European historians compounded the systematic denial of credit to "pagan" sources, it is worth observing how well this is illustrated by the work of the student historian Ian Pierce where even the slight acknowledgement to me in his sources disappears! Finally, it is also worth observing that the episode also illustrates how historical authority has been systematically misutilized to hang on to an historical account known to be incorrect, by refusing to withdraw or amend it—thus deliberately propagating a false account of history. There was a time when this was not the laughing matter it is today!

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CHAPTER 8

Number Representations in Calculus, Algorismus, and Computers

Śūnyavāda vs formalism

> A wrongly perceived notion of *śūnya* ruins a person of meagre intelligence. It is like a snake that is wrongly grasped or knowledge that is wrongly cultivated.
>
> Nāgārjuna
> *Mūlamādhyamakakārikā* 24.11[1]

OVERVIEW

THE issue of transmission does not end with the receipt of the calculus in Europe. Because of the epistemological differences between Indian and European mathematics, actual assimilation of the calculus took a long time. It is worthwhile trying to understand this assimilation process, since this sheds light on the historical as well as the contemporary mathematical situation, and since such a task seems never before to have been attempted by historians of mathematics, who have not acknowledged or understood the historical existence of epistemological differences within mathematics.

There were great difficulties in understanding the calculus, within the frame of European mathematics, because Europe accepted the practical value of the Indian method of calculation, without accepting the accompanying method of proof, or even the accompanying notion of number, involving the idea of zeroing non-representables—an idea used also in present-day computation, and still regarded as an "error". Hence, the eventual assimilation of the calculus within formal mathematics required the formalisation of ideal "real" numbers using set theory (as in Dedekind's theory of cuts) and the formalisation of set theory in the

1930's. In viewing retrospectively the Indian infinite series through the filters of present-day mathematical analysis, we need to recognize that though a concept of real number (particularly irrationals like $\sqrt{2}$) was in use in India since the days of the *śulba sūtra*, there was a difference—because of the difference in mathematical orientation, the absence of supertasks, and the explicit acceptance of the non-representable (which we call *śūnya*, not to be confounded with zero).

A similar problem arose in the algorismus, where non-representables (*śūnya*) could be zeroed, as in Indian tradition. This was a source of great confusion for several centuries after the algorismus first began to be imported into Europe, starting with Gerbert, later Pope Sylvester II (d. 1003 CE). The tradition was pushed into Europe by Florentine merchants who could see the clear practical value of the algorismus (over abacus) for commerce. These merchants were not much concerned about finer epistemological issues, and treated the algorismus as something of a trade secret. As in the case of the calculus, practical value forced an epistemological transformation in the notion of number—unlike the numbers used in the abacus, the numbers used in the algorismus could no longer be literally held in the hand. Also, with numbers on the abacus, difficulties in representation were confined to large numbers (which difficulties could hence be ignored); however, the arithmetical operations of the algorismus gave rise to numbers, such as $\sqrt{2}$ which could not be accurately represented. Though algorismus triumphed over abacus, the abacus tradition itself lingered in Europe, for uses of the "exchequer", until it ended dramatically with the burning down of tally sticks leading to the burning down of the British Parliament. The confusion regarding non-representables also lingered; it persists to this date and can be traced even in the way zero is handled in recent computer languages such as Java.

Formal real numbers (or even integers) cannot be used with present-day calculus-related computations on computers, which use oating point numbers (and ints) instead, since no computer will ever be able to perform the supertasks that Platonic mathematicians take as the basis of reality. Floating point numbers do not obey the algebraic rules ("laws") that real numbers do (e.g. the "associative law" does not apply). The difference between ideal real numbers, and oating point numbers becomes apparent exactly in the matter of non-representables. The manner in which non-representables are handled with oating point numbers on a computer is however distinct from the manner in which they were handled in Indian tradition or algorismus—a machine necessarily requires a mechanical rule, which was not the case in Indian tradition, which supposed the calculations to be done by an intelligent human being, who could handle exceptions intelligently.

Finally, because of the European understanding of mathematics as necessarily idealistic, the use of non-representable, as made evident in computer-based calculation, is seen as erroneous, so that the epistemological validity of a computation always is suspect. Thus, as in the earlier cases of algorismus and calculus, the same problem of practical value versus epistemological difficulty has arisen also in the case of computers, suggesting that a further transformation of the notion of number is required. This has serious implications for the future understanding of science, given the increasing role of computer-based calculation in scientific research.

As already argued earlier, there is no great virtue to the current notion of mathematical proof—except that it is tied to a particular brand of theology. Under these circumstances it is more appropriate to shift to the view of mathematics-as-calculation (as distinct from mathematics-as-proof). To enable such a shift it is necessary to review the use of (a) oating point numbers (in place of formal reals), and (b) non-representables (in place of infinitesimals) as providing an alternative basis to the calculus, a basis distinct from the basis provided by formal real numbers (or non-standard analysis).

To this end, I suggest that Western philosophy is impoverished by having solely an idealist approach to mathematics. Nāgārjuna's *śūnyavāda* rejects idealism as erroneous, and acknowledges the reality of non-representability. I propose this as an alternative to the formalist philosophy of mathematics.

I
INTRODUCTION

From all lines of evidence presented so far, it is clear that the calculus was transmitted from India to Europe in connection with the European navigational problem, and specifically the problem of the determination of the three "ells": latitude, longitude, and loxodromes.

On the other hand, it is also quite clear that mathematics is not universal, and that there were fundamental differences in the Indian and European understanding of mathematics. Therefore, it is quite natural that the calculus was initially received in Europe with great suspicion, and not regarded as mathematics at all.

The tremendous practical value of the calculus was, however, manifest. Regardless of its theoretical acceptability among European mathematicians, the undeniable fact was that the series expansion method gave very precise trigonometric values, and the undeniable fact was that these trigonometric values could be used to great advantage in the very practical and important problem of navigation. This was especially true of the European method of navigating by dead reckoning, which required loxodromes, calculating which was a problem equivalent to the fundamental theorem of calculus.

The tension between the obvious practical value of the calculus to Europe, and its philosophical unacceptability in Europe, was closely analogous to the present-day tension between the practical utility of numerical simulation on a computer, and the theoretical belief among present-day mathematicians that such numerical simulation is epistemologically inferior, hence less reliable than mathematically proven theorems. Numerical simulation of the stock market using, say, stochastic differential equations driven by Lévy motion,[2] may help one to make a lot of money, but no formal mathematician will accept the simulated solutions as reliable in the absence of a formal mathematical proof that such equations admit solutions.

Historically, the tension between the obvious practical value of the calculus to Europe and its philosophical unacceptability in Europe led to a transformation of the basis of the calculus. This process went through various stages.

In the first stage, within about a century of its import in Europe, Newton's way of using the calculus in his *Principia* had made it socially acceptable. Social acceptability comes from agreement with the prevailing social prejudices. The *Yuktibhāṣā* treatment related the mathematics of series expansion to the physical belief in atoms, but it would have aroused horror in Europe for basing mathematics on physics—that too the physics of atoms championed by that political unworthy Democritus! Cavalieri was criticised by Guldin[3] on exactly this ground that his indivisibles, like Kepler's, were really atoms of some sort, and he was called a "land surveyor rather than a geometer". On the other hand, Newton's use of uxions also related the mathematics of series expansions to physics, but it aroused widespread social approval for it sought to base physics on mathematics—a procedure which is, till today, regarded as entirely appropriate in the West. The presentation in Newton's *Principia Mathematica* is modelled on the presentation in "Euclid", and as the word " uxion" suggests, Newton did not deviate from the "Aristotelian orthodoxy" of the belief in the continuum.

Despite the social acceptability, the epistemological unacceptability of uxions and infinitesimals persisted among mathematicians and philosophers. A detailed historical study of the epistemological reception of the calculus in Europe, from the angle of contrasting epistemologies, would be a matter of great interest.[4] Such a detailed study, however, would be beyond the scope of the present book. We brie y review some of the highlights in the section below.

It is, however, entirely appropriate to take a look at the contemporary consequences of this clash of epistemologies. As is well known, the eventual epistemological acceptance of the calculus in Europe required a transformation of the number system.

II
THE RECEPTION OF THE CALCULUS IN EUROPE

Berkeley s Criticism

The attempt to absorb the Indian series using Kepler's or Cavalieri's notion of indivisible, then Newton's notion of uxion or Leibniz's notion of "difference", or infinitesimal, led to great conceptual confusion in Europe. A quick overview of the con ict regarding the reception of the calculus in Europe provides some useful insights.

The first thing to notice is that infinite series were perceived differently in India and Europe. Where Indian mathematics aimed to use the series for practical computation, European mathematicians (who also had the same aim) also sought to relate the infinite series to the imagined perfection of mathematics in Platonic idealism. Newton's own account of the difference between his uxions and Cavalieri's indivisibles or Leibniz's "difference" claims exactly this "perfection".[5] This attempt to claim "perfection" for something intended to be practical and useful was the genesis of all the confusion.

In India, infinite series were used to calculate reliable and accurate numerical values. As we have seen, there was valid *pramāṇa* for the Indian infinite series. The way the sum of an infinite series was defined, in practice it required only a finite task, analogous to summing an indefinite series, since it required the sum of only a *finite* number of terms together with the calculation of a correction or exceptional term. Hence, in Indian tradition infinite series were handled smoothly, and there were no paradoxes of the infinite.

However, the Europeans (in the time of Newton and Leibniz) had a rigid rule-based approach to infinite series; hence they neglected the exceptional term and its significance, and regarded the series as extending to an infinite number of terms. Summing the series was seen to require a supertask of summing an infinite number of terms, since the notion of mathematics as being "perfect" required that even the smallest quantity could not be neglected. This naturally led to the question of what exactly the sum of an infinite number of terms was, and how the sum was to be carried out. This entailed all the associated paradoxes of the infinite, from Galileo, Descartes and onwards. It should be observed that 17th and 18th c. European mathematicians did NOT think that this infinite sum had no intrinsic meaning; they did not think that the meaning assigned to the sum was just a matter of definition, and they did *not* proceed to define the sum in an arbitrary way. Rather, they thought an infinite sum had an intrinsic meaning; while some thought this meaning to be beyond the grasp of the human mind, others claimed to have found that intrinsic meaning, and to have given a rigorous formulation to it.

By hindsight, the definition of the sum of an infinite series, from this idealist viewpoint, required some clear notion of "limit", and a satisfactory notion of "limit" had to await the idealization of the real number system, which took around two centuries after Newton's alleged discovery of the calculus.

In the meanwhile, Cavalieri's indivisibles, Newton's notion of uxions, and Leibniz's notion of differences caused enormous confusion. For our purposes it suffices to point out that this confusion persisted long after these worthies, and is manifest in Berkeley's criticism of Newton and Leibniz. Berkeley's criticism was perhaps motivated by an awareness of Newton's real religious views which were so vehemently against the church. (These little known views surfaced brie y, shortly after Newton's death.) Irrespective of his motivation, Berkeley's criticism was devastating. Berkeley[6] had a variety of objections. First, he objected to the method of derivation, in which one first supposed an infinitesimal increment, and then supposed the increment to vanish. His argument was that if one supposed it to vanish, this contradicted the earlier supposition, and so one ought to begin *de novo*, in which case the required result could not possibly be obtained, for there would be no increment. On the other hand, he pointed out that from a pair of contradictory assumptions, any conclusion whatsoever could be drawn.

He pointed out that the infinitesimals could neither be finite quantities (for that would destroy the alleged perfection of the theory), nor could they be infinitely small quantities (since they could then be neglected without fear of error), nor could they even be zero (for all the derivations would then fail). Finally, he pointed out that mathematicians of his time were unable to pin down the nature of infinitesimals which always disappeared from the final result. This led to his famous polemic:

> And what are these same evanescent Increments? They are neither finite Quantities nor Quantities infinitely small, nor yet nothing. May we not call them the Ghosts of departed Quantities?[7]

It is evident that Berkeley had accurately spotted the difficulties that arise in the transition from numbers according to a realistic philosophy such as *śūnyavāda* (which explicitly recognizes the existence of non-representables) to numbers according to an idealist philosophy (which denies the existence of non-representables, in assuming that everything has an ideal representation).

Berkeley pointed to the difficulty in conceiving of infinitesimals, and how this difficulty was exacerbated by trying to conceive of infinitesimal parts of infinitesimals—i.e. of infinitesimals of the second order, and of infinitesimals of various higher orders—"so that according to them an inch does not barely contain an infinite number of parts, but an infinity of an infinity of an infinity *ad infinitum* of parts." These difficulties originated with the assumed perfection of mathematics:

> It is said, that the minutest Errors are not to be neglected in Mathematics: that the Fluxions are... not proportional to the finite Increments though ever so small; but only to... nascent Increments... And... there be other Fluxions, which Fluxions of Fluxions are called second Fluxions. And the Fluxions of these second Fluxions are called third Fluxions: and so on, fourth, fifth, sixth, &c. ad infinitum. Now as our Sense is strained and puzzled with the perception of Objects extremely minute, even so the Imagination, which Faculty derives from Sense, is very much strained and puzzled to frame clear Ideas of the least Particles of time, or the least Increments generated therein... And it seems... to... exceed, if I mistake not, all Humane Understanding. The further the Mind analyseth and pursueth these fugitive Ideas, the more it is lost and bewildered; the Objects, at first eeting and minute, soon vanishing out of sight. Certainly in any Sense a second or third Fluxion seems an obscure Mystery. The incipient Celerity of an incipient Celerity, the nascent Augment of a nascent Augment, i.e. of a thing

which hath no Magnitude: Take it in which light you please, the clear Conception of it will, if I mistake not, be found impossible...And if a second Fluxion be inconceivable, what are we to think of third, fourth, fifth Fluxions, and so onward without end?

V. ...They suppose finite Quantities to consist of Parts infinitely little...Now to conceive a Quantity infinitely small, that is, infinitely less than any sensible or imaginable Quantity, or any the least finite Magnitude, is, I confess, above my Capacity. But to conceive a Part of such infinitely small Quantity, that shall be still infinitely less than it, and consequently though multiply'd infinitely shall never equal the minutest finite Quantity, is, I suspect, an infinite Difficulty to any Man whatsoever; and will be allowed such by those who candidly say what they think; provided they really think and re ect, and do not take things upon trust.[8]

On the other hand, if one conceives of only the first uxions, as certain mathematicians then favoured, why not the second, the third, and fourth, and so on?

Curiously, a key point of Berkeley's criticism seems to have been overlooked by historians of mathematics so far: *Newton s uxions and Leibniz s infinitesimals were anti-atomic*. In the Indian tradition, the subdivision of an inch stopped when one reached atomic dimensions; we have seen how this was used in the course of the derivation of the sine series; but this belief in atomicity did not fit well with the then-prevalent Western theology—and atomistic implications were rejected by the "Aristotelian orthodoxy".[9] Guldin contemptuously called Cavalieri a "land-surveyor" rather than a geometer just because of his own smug belief in the superiority of metaphysics over physics. While social desirability compelled the use of a continuum, the West then lacked a clear account of magnitudes or numbers as a continuum, which could be forever subdivided into "an infinity of an infinity of an infinity *ad infinitum* of parts". Nevertheless, this was what Newton himself took credit for—for having replaced Cavalieri's atomic indivisibles by continuous uxions—of which latter he could provide no clear idea. Note also how Berkeley echoes Descartes belief that these infinite procedures are beyond the human mind.

We reiterate that these difficulties with the calculus were as peculiarly European as the European difficulties with navigation. These difficulties were not intrinsic to the subject; they arose only because European mathematicians mistakenly took as universal their own idealistic philosophy of mathematics. There is no intrinsic difficulty in wearing a sari, unless one insists on wearing it like a skirt, imagining that it is a sacred and universal law of nature that women should wear only skirts.

Berkeley emphasized that he was not objecting to the *conclusions* reached by Newton and Leibniz: the conclusions might well be true, but he pointed out that the *method* of deriving them was not clearly explained, and hence not science. (Note that the conclusions had already been derived earlier by another easier-to-comprehend method—according to present-day historians of mathematics, the contribution of Newton and Leibniz was mainly the method *per se*.)

> For Science it cannot be called, when you proceed blindfold, and arrive at the Truth not knowing how or by what means.[10]

In particular, he gave examples of how one could arrive at valid conclusions by a wrong method which involved multiple errors:

> the two errors being equal and contrary destroy each other; the first error of defect being corrected by a second error of excess.[11]

Responses to Berkeley

The cogency of Berkeley's arguments is evident from the fact that his contemporaries were completely unable to respond to his arguments, and were left frothing at the mouth. Jurin[12] argues with lengthy polemics and little substance, defending Newton against the charge of being an infidel. Jurin was obviously ill-informed about Newton's religious beliefs, and his belief in the British system was misplaced: it was the British system which censored and held on to the secret of Newton's religious beliefs for quarter of a millennium, and these are still not particularly well known. As for Jurin's mathematical discourse, its level can be judged by the following quote:

> The foundation of the Method of Fluxions I take to be contained in the following
>
> POSTULATUM.
>
> Mathematical quantities may be described, and in describing may be generated or destroyed, may increase or decrease, by a continued motion.[13]

In the same spirit of wonderful clarity, Jurin goes on to define such mathematical quantities as " owing quantities" and their velocities as " uxions". And then adds,

> A nascent increment is an increment just beginning to exist from nothing, or just beginning to be generated, but not yet arrived at any assignable magnitude how small soever.

There seems little doubt that Bhāskara II, some 600 years earlier, did a considerably better job of clearly defining the instantaneous velocity of a moving point (planet, as observed in the sky), when its velocity was continuously changing. Perhaps, since he believed in clarity from the beginning, and did not first create needless philosophical confusion and then struggle with it, his work is not valued!

Similarly, Robins[14] responds by restating Newton's mathematics—which does not help to meet Berkeley's arguments.

Relation to Present-Day Notions of Limit and In nitesimal

In particular, the attempt to justify Newton s argument retrospectively through the work of future mathematicians does NOT meet the objections raised by Berkeley. As I have argued in another context, vague statements followed by retrospective disambiguation is a favourite technique of astrologers and oracles.

A well-known example of retrospective disambiguation is the Oracle of Delphi, who was asked what would happen if Croesus attacked the Persians, and the Pythoness responded that Croesus would destroy a mighty empire.[15] A bit wary of oracular ambiguity, Croesus sought a clarification about how long his rule would last, and the Oracle informed him that it would last until a mule ruled the Medes. Convinced that a mule could hardly become king, Croesus attacked Persia and he lost. After the magnanimous Cyrus had granted him a reprieve, Croesus complained to the Oracle, sending his fetters. The Oracle responded that Croesus had neither understood what was said, nor took the trouble to seek enlightenment, so he had only himself to blame.[16] The empire that would fall was his own, and the Mule in question was Cyrus, who had mixed ancestry. The meaning of the Oracle's statement was crystal clear in retrospect; it was absolutely muddy and unclear in prospect. Therefore, the fact that a vague statement admits a valid retrospective disambiguation cannot validly be used to give credit to the source of the original vagueness, unless one simultaneously wants to give credit also to other vague predictions made by astrologers and oracles, for they will admit *several* retrospective disambiguations.

When we had no formal real numbers, Newton's statements were disambiguated in one way as a case of uxions vs indivisibles (as in James Jurin's "understanding" of uxions). When we had limits and formal real numbers with no infinitesimals, Newton's uxions were retrospectively disambiguated in another way, as limits, which had banished for ever the confusion about infinitesimals. When we have non-standard analysis, and internal set theory, Newton's uxions are retrospectively disambiguated in a third way. It is clear that Newton's vague statements can be and have been disambiguated in more than one way. To an unbiased observer, this sort of thing establishes nothing except the great anxiety of the persons concerned (astrologers/oracles/historians) to allocate social credit in a particular way or to a particular person.

It may be perfectly possible, today, to set up a formal theory which retrospectively disambiguates in a formally acceptable way what Kepler did. In fact, computers do something similar: an apparently smooth curve drawn on a computer screen, or printed on a piece of paper, consists of a large number of straight lines that are indivisible at the level of pixels on the screen, or dots on the paper. If one believes that retrospective disambiguation is a valid argument for conferring credits, then every such fresh retrospective disambiguation should lead to a change of credits. Suppose a suitable retrospective disambiguation of Kepler were found. (This should be possible, since Kepler was an astrologer by profession!) Would one then give credit to Kepler for the calculus? (And, if so, why not to earlier Indians!? Especially considering that Kepler probably obtained translations of Indian texts from Tycho Brahe's papers.) The simple fact is that the logic of Newton's uxions or of Leibniz's differences or infinitesimals was not clear to them, and could not *then* be explained in a satisfactory way to their contemporaries even until after Newton's death. Nothing that happens subsequently can alter this fact. Both of them are credited with the creation of the calculus, although they did not fully understand it.

The argument also works in the other direction. Notice that the infinite series made sense in India, *at that point of time*, but according to the Indian norms of proof (*pramāṇa*), and the Indian notion of real real numbers (distinct from formal real numbers). In contrast, Newton's uxions did *not* make sense according to the then-prevalent European norms of proof, as is clear from Berkeley's argument which has never been clearly refuted. Therefore, the fact that the current political dominance of the West has made the Western notion of proof socially dominant today, cannot be validly used to retrospectively confer credit on Newton and Leibniz: we have to recognize that, unlike the Indians, both these worthies were groping in the dark in a way that was unacceptably confused, so far as their contemporaries were concerned. In any case, the only clear thing was the practical application of the calculus, which, to this day, does not require formal real numbers, or limits, does not use them, and manages remarkably well with finite differences—similar to those used by Āryabhaṭa.

Newton on His and Leibniz s Contributions to the Calculus

It is also curious to see how strikingly at variance are present-day historians' exaggerated accounts of the achievements of Newton and Leibniz regarding the calculus compared with Newton's own account of his and Leibniz's achievements! In summing up his priority dispute with Leibniz, Newton[17] expresses familiarity with the earlier work of Cavalieri, Descartes, Fermat, Pascal, Barrow, Wallis, Gregory, Brouncker, and N. Mercator,[18] on the calculus. Unlike present-day Western historians, Newton himself could not, at that point of time, pretend that his ideas of the calculus had been immaculately conceived.

Accordingly, Newton takes for himself only the credit for discovering the sine series! Newton repeatedly emphasizes that the series for the arctangent ("Gregory–Leibniz" series) was obtained by Leibniz from other sources. As Newton further says (about Leibniz), the "second inventor" of the same thing deserves no particular credit, and Newton was, at best, the fifth or sixth inventor of the sine series which is the thing for which he himself claims credit in the course of his priority dispute with Leibniz.

Clearly also, in the process of incorporating this sine series into European mathematics, Newton, like other European mathematicians before him, misunderstood the Indian way of handling infinite series; for he was unwilling to discard the smallest quantities, had no correction term, and futilely attempted to assign a clear meaning to the supertask of summing an infinite number of terms. As we have just seen, though Newton received far more social approval than Cavalieri, Newton could not offer an adequate account of his uxions, which were required just because Newton claimed "perfection" for his method (of uxions). Had Newton's true religious beliefs surfaced in 1735, the present-day historians might well have viewed Newton in a different light! Further, as Newton repeatedly remarks about Leibniz,[19] how could someone claim to be the inventor of something he did not quite comprehend? Newton was applying exactly what we have called the epistemological test: lack of understanding tends to indicate lack of originality. The same applies to Newton, if we disallow retrospective disambiguation (as we ought to).

III
FORMAL REAL NUMBERS

Images and Dedekind Cuts

In the traditional account taught to mathematicians, acceptance of the calculus (mathematical analysis) required the formalisation of real numbers by Dedekind. As a prelude to this account, it is necessary to observe that, as pointed out in Chapter 3, real numbers were known to the Indian *śulba sūtra* from some fifteen hundred years before even the use of the abacus was introduced in England, for example, and that words like "surd" deriving from those Indian ways of handling square roots and real numbers give away the real history of the subject. Thus, it is not as if Dedekind invented some new kind of numbers—what he did was to give an idealized or formalised or metaphysical account of them. Thus, we need to differentiate between real real numbers long known to tradition, and formal (or unreal) real numbers—and the kind of real number being talked about, whether real or unreal, should usually be clear from the context.

To return to Dedekind, recall Proposition I.1 of the *Elements*. The proposition is to build an equilateral triangle on a given base. The earlier doubt about the proof related to the question of picking and carrying lengths. But there was a further doubt. The proof of that theorem in the *Elements* must be regarded either as an empirical matter ("go ahead, carry out the construction and see that they intersect") or as making use of an *image*, similar to Fig. 2.2. The image is visually so compelling that no mathematician sought to challenge the proof for well over a millennium after Proclus.

So far as the *Elements* were concerned, the image was essential to the proof: the two arcs were visually continuous, and their intersection was an intuitive necessity. In Proclus' philosophy of mathematics, the use of images was even more permissible than the use of the empirical at the beginning of mathematics: for Proclus noted that Plato agreed that images served to stir the soul, and remind it of its innate knowledge. This was entirely in accord with Proclus' understanding of the *Elements* as espousing "Neoplatonic" religious beliefs against the changed Christianity of the 4th c.

However, the use of images had created political difficulties. This idea of the Alexandrian school was even more explicitly articulated in Porphyry's *On Images*, which went on to explain in a beautiful way the detailed symbolism of the images of gods and goddesses maintained in "pagan" temples. However, Porphyry's *On Images* was burnt along with the temple by the Christians in Alexandria who violently objected to the use of images.

Of course, Christians did later come around to use of images, after the second council of Niacaea—they now objected only to the images of "strange gods". A few centuries after the fall of Alexandria, al Bīrūnī observed:

> Many of the leaders of religious communities have so far deviated from the right path as to give such imagery in their books and houses of worship, like the Jews and Christians...[20]

At the time of the reformation, many people were critical of the prevailing Christian orthodoxy. Thus, Newton observed in his suppressed *History of the Church*, and its drafts, that Christianity in pre-Modern Europe made numerous appeals to images—such as its dominant image of Christ on the cross—this was to him a gross corruption of the religion. The ideal quite clearly visible to Newton, and one to which he subscribed, implicitly at any rate, was to avoid the use of images altogether, for fear that one might confuse the image with the reality.

Therefore, regardless of actual practice in Europe, the ideal still was to avoid images in mathematical proof, and to this day, the epistemological subjugation of visual imagery creates problems for children learning mathematics. In suspecting images, Dedekind was being theologically correct.

Arithmetization of Geometry

Further, by Dedekind's time, the arithmetic imported into Europe had been partly naturalized by marrying it to geometry, and the arithmetization of geometry ("Cartesian geometry") was well established. Therefore, it was natural for Dedekind to try to translate the visual and geometric proposition about arcs into a proposition about numbers. This raised a new doubt. The image suggests that the two lines intersect; but did they intersect in reality? If the intersection of the arcs was translated into a proposition about numbers, then the two arcs could intersect only if the point of intersection in the plane could be represented by a pair of numbers. Did such a pair of numbers exist? Thus, by Dedekind's time, the assertion about the intersection of the arcs translated naturally into an assertion about the existence of numbers—"existence" being understood in a Platonic rather than practical sense.

If these numbers did exist, what *sort* of numbers were these? If we take the base of the equilateral triangle as one unit, and represent it as the line segment joining the point (0, 0) to the point (0, 1), then the arcs would intersect at the point (x, y), where $x = \frac{1}{2}$, and

$y = \sqrt{(1^2 - (\frac{1}{2})^2)} = \frac{\sqrt{3}}{2}$. This last is not a whole number, either positive or negative, nor is it a fraction. Now, as we have seen in Chapter 3, from the time of the *śulba sūtra*-s, Indian tradition had no difficulty in handling such numbers, referring to the representation of $\sqrt{2}$ as 1.4142156 *sāviśeṣa* (this number and something left out), or later to π as 3.1416 *āsanna* (near value), etc. However, such a representation of numbers, which left out something, was not acceptable in Platonic idealism, which demanded nothing short of "perfection". While whole numbers and fractions could be satisfactorily represented from the idealist point of view, no such "perfect" representation was available for numbers like $\frac{\sqrt{3}}{2}$.

If one now shifts to a set-theoretic view, using the relation of belonging, where the points in the plane are regarded as pairs (x, y), where x and y are natural numbers, or integers, or rational numbers, then the point $(\frac{1}{2}, \frac{\sqrt{3}}{2})$ can *not* be regarded as belonging to the plane. The required point of intersection of the two arcs would, therefore, not "exist" (in a Platonic sense) in the plane, and there would be "gaps" in the two arcs.

Therefore, by reinterpreting the *Elements*, rejecting images and using the arithmetization of geometry, and set theory, Dedekind was led to suppose that the validity of the *Elements*, and, in particular, proposition I.1 of the *Elements*, required that the arcs and line segments in the *Elements* were without any "gaps". Translated into a statement about the existence of numbers (in a Platonic sense), this would happen only if, no matter where one "cut" the arc, there would be a number at that point, and never a gap.

It is well known how the formalisation of this idea of "Dedekind cuts" leads to the formal real numbers. Since this is something that is taught in elementary courses on mathematical analysis, and can be found in basic texts,[21] it will not be covered here. It is also well known, and equally elementary, that it is through this theory of formal real numbers that the calculus today finds a "rigorous" basis in mathematical analysis, which is why most texts on mathematical analysis begin with an account of the formal real numbers.

The current history of mathematics practically attributes the real numbers to Dedekind. This overlooks, of course, the use of real numbers from two-thousand year earlier, but it also overlooks another key point. The formal construction of real numbers used set theory—a subject which itself aroused various serious mathematical doubts. Thus, what Dedekind achieved was to replace one set of idealistic doubts (about numbers) by another set of doubts (about sets), at least until these new doubts were supposedly settled by the formalisation of set theory in the 1930's.

Completeness of Reals

From the point of view of contemporary mathematics, the key mathematical property of formal real numbers, R, that is needed is the topological property of completeness: R is complete.

From the perspective of contemporary mathematics, this property of completeness of reals ensures that any sequence of real numbers which is "trying to converge" will actually find a real number to converge to. Formally, such an intrinsically convergent sequence is called a Cauchy sequence. More formally, a sequence is called a Cauchy sequence if for any given number   one can find a natural number M such that the distance between the nth and mth term of the sequence is less than   for all n and m greater than M. That is, for a Cauchy sequence, for large enough n and m, the distance between the nth and mth terms of the sequence can be made as small as we please. The completeness property of the reals is that every Cauchy sequence of real numbers actually converges to a real number.

A similar criterion applies to the sum of an infinite series of numbers, $_{i=1} a_i$, by considering the sequence consisting of the partial sums up to n terms, $S_n = {}^{n}_{i=1} a_i$. If this sequence of partial sums, S_n, forms a Cauchy sequence then the infinite series $_{i=1} a_i$ is regarded as summable, and the limit of S_n is the sum. We note in passing that, modulo a few quibbles, there is no fundamental difference between this current notion of the sum of an infinite series, and the way the sum of an infinite series was defined in India—viz., to the given fixed but arbitrary precision to which one is working, the successive partial sums of the series should become constant, in which case that constant value is the sum. Thus, ultimately, after nearly three centuries of groping in the dark, trying to comprehend the calculus from the days of Clavius, European mathematicians finally saw the light when they arrived at the Indian point of view, with a few legalistic caveats!

The way this criterion works may be illustrated with an example. The sequence of numbers 1.4, 1.41, 1.414, 1.4142, 1.41421, 1.414213, 1.4142135, 1.41421356, 1.414213562, ...(which intuitively corresponds to the decimal expansion of the real number $\sqrt{2}$) is a Cauchy sequence, since the terms of the sequence cluster together, and the nth term of the sequence differs from the mth term only in the $(n+1)$th decimal place (assuming m to be larger than n), and this can evidently be made as small as we please.

How exactly does this differ from the *śulba sūtra* statement that $\sqrt{2}$ is "1.4142 and something more" (where the "something more" can be calculated to any desired degree of precision using the algorithm for square-root extraction, stated by Āryabhaṭa)? Well, asserting the completeness of real numbers does not help one to *calculate* the limit except to a desired degree of precision, so one would still have to say that $\sqrt{2}$ is "1.4142 and something more", where the "something more" can be calculated to any desired degree of precision. However, since there is a mathematically proven theorem which asserts the existence of $\sqrt{2}$ one now has the assurance that the limit in question "exists", with "existence" understood in a suitable sense (as in the statement "God exists") which has nothing to do with real physical existence.

Although the above sequence is a sequence of rational numbers, the number to which it is "trying to converge", viz. $\sqrt{2}$, is not a rational number (i.e. the square of the ratio of two whole numbers can never be 2). Rational numbers are incomplete, so this sequence is not convergent if we limit ourselves to the rational number system, for there is no rational number to which it can converge, even though the terms of the sequence are coming closer to each other. In this situation, Dedekind visualized that one would have to add to the rational numbers the various such limits of all Cauchy sequences. (Indeed, another way to construct real numbers is to regard a real number as an equivalence class of Cauchy sequences of rational numbers, two Cauchy sequences being deemed equivalent, if their difference converges to zero.) This gives the real numbers, which are thus complete by construction. The irrational numbers are then the real numbers that are not rational, i.e., they correspond to numbers whose decimal expansion neither terminates nor recurs.

The completeness of the real number system means that we can answer questions like "what is the sum of $1 - \frac{1}{3} + \frac{1}{5} - \frac{1}{7} + \cdots$?", not in the sense that we can state the value of this sum precisely, but in the sense that we can assert with assurance that this sum "exists", in a certain sense. Similarly, it also means that we can define derivatives and integrals as limits,

$$\frac{dy}{dx} = \lim_{\Delta x \to 0} \frac{y(x + \Delta x) - y(x)}{\Delta x} \tag{8.1}$$

$$\int_a^b y(x)\, dx = \lim_{n \to \infty} \sum_{k=1}^{n} y(x_i) \Delta x_i, \tag{8.2}$$

provided the limits in question "exist". The completeness of the real number system does not guarantee that all such limits required to differentiate and integrate functions exist. However, if the limit fails to exist, this failure is now believed to be intrinsic to the function.

IV
THE CENTRAL PROBLEM OF REPRESENTATION

The key non-elementary point here of course is this: the formalisation of real numbers did *not* resolve the central problem of representation—for real numbers cannot be represented as concretely as whole numbers or fractions. Thus, the formalisation did not really resolve the central dispute between Nāgārjuna and Plato, between *śūnyavāda* and idealism, which was identified above as being at the heart of the European difficulties with the calculus. Instead, it shifted the battlefield: the problem of representability was just pushed into more obscure corners, where it did not need daily attention.

Thus, an engineer does not normally bother about the finer points of physics, feeling, in fact, a bit superior for not wasting his time bothering about these impractical details which are the concern of the physicist. The physicist tackles these finer points with gusto, confident that his approach is superior to the crude approach adopted by the engineer. But the physicist, in turn, feels that subtler mathematical problems are hardly his concern, and, in a superior sort of way, leaves such impractical matters to mathematicians—who are happy to spend a lot of time establishing to their satisfaction the existence of something that is obvious to others. The mathematician tackles these finer points with gusto.... Dedekind's formalisation of real numbers, by representing real numbers using sets, enabled mathematicians to pass on the problems with infinitesimals to set theory, which was the domain of the logician or the metamathematician, who can, in turn, sidestep various subtle issues as the impractical concerns of the philosopher proper!

This way of sidestepping problems is credible just because within the existing social structure specialization is encouraged by industrial capitalism, so this amounts to not encroaching on the other's territory—a "proof by territory limitation"[22]—and enables the persons concerned (engineer, physicist, mathematician, ...) to carry on with their daily jobs. To dispel such false credibility arising from the social acceptability of the argument, in the appendix to this book we demonstrate how the philosophical differences, though subtle, connect to physics and engineering, and have important practical implications also for high-speed aerodynamics, the geology of earthquakes, and quantum field theory.

Are Formal Real Numbers Appropriate for Calculus?

To bring out the problems involved in a preliminary way, let us ask: are formal real numbers at all necessary for the calculus, and are they appropriate?

Thus, it is a historical fact that the development of the calculus preceded the formalisation of real numbers. (This is true, even by those accounts which maintain that Newton invented it.) It is also true that all present-day calculations involving the calculus are usually done on a computer which simply cannot use real numbers. Thus, it is apparent that calculus can get along fine without formal real numbers. The calculus certainly uses informal real numbers—but then those date back to at least 2500 years, to the time of the *śulba sūtra*.

Even the claim that real numbers are appropriate for the formalist epistemology of the calculus does not seem to be valid. For example the limit of the difference quotient in (8.1) would fail to exist if we take $x = 0$, and $y = \text{sgn}(x)$, where the signum function

$$\text{sgn}(x) = \begin{array}{rcl} 1 & : & x > 0 \\ 0 & : & x = 0 \\ -1 & : & x < 0. \end{array} \tag{8.3}$$

According to the standard mathematical narrative, this difficulty is intrinsic to the signum function, and is not a difficulty with numbers, for the standard mathematical narrative asserts that "every differentiable function must be continuous". However, that belief, often called a theorem, is not really acceptable even to formal mathematicians. It has been known for the good part of the previous century (ever since Sobolev and Schwartz, and, in fact, since the days of Heaviside, in the 19th c. CE, who comes only shortly after Dedekind) that it is perfectly possible to interpret the notions of "function" and "derivative" in such a way that "every integrable function is differentiable", which is quite contrary to the above narrative, since non-continuous functions *can* be integrated.

Therefore, the standard mathematical narrative is far from being the complete story, and it is perfectly possible to have a theory of differentiation which permits us to differentiate the signum function. Such a theory is required and used in present-day physical theory. As shown in Appendix A, the real number system is inadequate for an appropriately modified theory of this notion of differentiation.

Thus formal real numbers are not necessary for the calculus (since calculus may be done with informal real numbers), nor are formal real number the most appropriate for the calculus (since that does not permit unrestricted differentiation of integrable functions, say).

Real Numbers vs In nities and In nitesimals

On the other hand, *from within the formalist perspective*, what happens if we use some other number system, say a formal number system *larger* than the real number system?

Apart from the notion of completeness, the real numbers can be alternatively characterized by what has been called the "Archimedean property". (I do not know the origin of this terminology; though I find it a bit jarring, I will continue to use it in the following—assuming, of course, that Archimedes was a short black man!) That is, given a positive real number x, one can always find a natural number n such that

$$x < \underbrace{1 + 1 + \cdots + 1}_{n \text{ times}}. \tag{8.4}$$

Intuitively speaking, the Archimedean property says that no real number is infinitely large. Consequently, no positive real number can be infinitesimally small, and for any real number y such that $0 < y$ we can always find a natural number n such that $0 < \frac{1}{n} < y$. (This can be seen by applying the Archimedean property to $x = \frac{1}{y}$.)

The Archimedean property can be used to characterize the real numbers as follows: R is the largest Archimedean ordered field.

The formal definition of the algebraic structure called an ordered field would be tedious and would take us too far afield. This definition can be found in any elementary text on (formal) algebra.[23] Basically an ordered field is a set of numbers that can be added, subtracted, multiplied, and divided, and also compared in a way that is compatible with addition and multiplication. Rational numbers also constitute an ordered field—that is the smallest such field. On the other hand, R, as stated above, constitutes the largest Archimedean ordered field.

Thus, if one considers an ordered field which is larger than R, then the Archimedean property must fail in such an ordered field. So, in such an ordered field, call it S, we can find a number x such that

$$x > n$$

for every natural number n. Such a number corresponds to the intuitive notion of an infinitely large number. Since non-zero numbers can always be inverted in a field, the existence of infinitely large number in S means also that its inverse is infinitesimally small:

$$\frac{1}{x} < n$$

for every natural number n. This means that any system of numbers larger than R must admit both infinities and infinitesimals.

In present-day formal mathematical analysis, suppose we take as the basic set of numbers a set S of numbers constituting a non-Archimedean ordered field. S could, for example, be any ordered field which is a proper extension of R. Calculus would look somewhat different in S. It would be possible to define idealized "limits", but limits would no longer be unique, since any two "limits" might differ by an infinitesimal. (What we are saying here about calculus on a non-Archimedean field should not be confused with non-standard analysis, which is considered separately in Appendix A.) I have remarked elsewhere that the present-day mathematicians' obsession with proving the existence and uniqueness of things is remarkably similar to the Christian theologians' obsession with proving the existence and uniqueness of God; on this analogy, calculus in a non-Archimedean field would be a "pagan calculus", where one would celebrate an infinity of (non-unique) God-s!

Discarding infinitesimal differences would not be a process very different from the process of rounding. If we do decide not to be bothered about infinitesimal differences, then with the availability of infinities and infinitesimals in S, it would be possible to make "infinitesimal changes" to the signum function in an infinitesimal region around zero, so that it becomes smooth, and can be differentiated in the classical sense. The derivative would be a function which would be infinite in an infinitesimal region of zero, and would be infinitesimal elsewhere—a function today known as the Dirac delta function, and widely used in physics, though for nearly half a century mathematicians declared it impossible for such a function to exist.

The point here is only this: the non-existence of the limits of the difference quotient, for a discontinuous function, is not a "natural" property—it is not a universal truth. It all depends upon how one understands the calculus. Even within a formal understanding of the calculus, it all depends upon the choice of the underlying number system, and the definition of limit that is adopted. There is nothing sacred about the real numbers R, or the definition of limit in mathematical analysis—this just happens to be the first formalisation which succeeded in gaining wide social acceptance. The use of real numbers represents only a temporary consensus among socially authoritative Western mathematicians—a consensus which seems compelling only because the present-day mathematician grew up with this narrative.

V
ALGORISMUS

A similar problem related to the representation of real numbers had also arisen earlier in Europe with the import of the algorismus.

Roman Numerals and the Roman Calculus

To understand this, it helps to go back to the Roman system of representing numerals. Just as the Roman calendar was more systematic than the haphazard Greek calendar, the Roman numerals were more systematic than the earlier Greek (Attic) numerals. Where the Greeks would have written the number 47 as ΔΔΔΔ , the Roman still wrote XXXXVII; however, the Roman system scored over the Greek system of numeration in representing the numbers from 5 to 10 in a systematic way.[24] However, the Roman system is not well adapted to express numbers much larger than a thousand or so. Thus, even a small number such as 1786 is expressed as MDCCLXXXVI, using 10 symbols! As can be seen, this system quickly runs into difficulties if one wants to represent large numbers. This is not to say that the system could not be modified to enable the expression of large numbers—it surely could—but the need to express large numbers did not historically arise for the Romans, so that they did not, in fact, modify the system. (When the need for more sophisticated calculations did arise in Europe, the Roman system of numerals was not modified, but was abandoned in favour of the Indian system of numerals, usually called Arabic numerals.) This suggests that the Roman system was used mostly for counting and addition, and rarely was there a need for more complex arithmetical operations like multiplication which would have thrown up large numbers.

Several aspects of this system have been pointed out earlier. Square-root extraction was far too formidable a matter with this clumsy system. There was no good way in the system even to write down or represent the square roots. In fact, this system of numeration created great difficulties even with the representation of rational numbers (fractions). Obviously a fraction like $\frac{1}{4}$ cannot be expressed in Roman notation as $\frac{\mathrm{I}}{\mathrm{IV}}$. The basic Roman system of fractions was to use the *uncia*, which was the 12th part, related to the 12 ounces in a pound. Therefore, Romans could readily express fractions like $\frac{1}{4}$ which corresponded to three *uncia*-s. Romans had special words like *tres octavae* for fractions like $\frac{3}{8}$ which did not fit into this uncial system. Hence, Romans found it easier to represent the length of the year by the figure of $365\frac{1}{4}$ days, using an uncial fraction which was a standard part of their system of numeration. They would have had a difficulty even in *stating* the correct length of the year as given by Āryabhaṭa.[25] Expressing precise fractions as a ratio of two very large numbers, as was done by Āryabhaṭa, was quite out of question with Roman numerals, first because of the difficulty in representing large numbers, and secondly because there was no standard way of representing fraction nor any standard method of division available to the Romans, a point which becomes obvious if one tries dividing LCX by XXIII. (The terminology of "Euclid's" division algorithm is obviously bogus.)

Setting aside questions of division and multiplication, in fact, the Roman system of representing numerals did not easily permit even the elementary arithmetic of addition—the difficulties with the system of numeration become obvious if for example one tries adding XVII + LCXI. In fact, such additions could only be done by recourse to the Roman calculus, or rather *calculi*. The word *calculus* is the diminutive of the term *calx* or stone, and refers to pebbles used in a manner similar to the abacus. Examples of how the Romans and Europeans up to the 18th c. CE used the abacus for addition and subtraction and multiplication can be readily found in the literature[26] or online.[27] In fact, the Roman system of numeration was closely tied to this technology of the abacus or the counting board. Multiplication was done by repeated addition, and there was no standard technique for division.[28] Notwithstanding vastly exaggerated historical claims about astronomy and mathematics in the Roman empire, this was the canonical way of doing arithmetical calculations that Europe inherited and started off with at the beginning of the second millennium CE.

The House of Wisdom

With the rise of Arabs, and even before the formation of the House of Wisdom in Baghdad, in the early 9th c. CE the Indian way of doing mathematical calculations and astronomy travelled to Baghdad. The story is fairly well known. When the Arabs first turned their attention from military conquests to intellectual conquests, their interest in astronomy was aroused by the *Zij-i-Shahryar* which was an Arabic translation from Pahlavi of an Indian text on astronomy earlier translated from Sanskrit to Pahlavi at Jundishapur. Ibn al-Ādamī, in the preface of his astronomical tables *Nazm al-iqd*, records that during the reign of Caliph al-Mansūr, an Indian "astronomer" ("Gaṇaka" = calculator/accountant) visited Baghdad and brought with him various astronomical tables and texts for computations. Under the orders of Caliph al-Mansūr, Brahmagupta's *Brāhmasphuṭasiddhānta* and *Khaṇḍkhādyaka* were translated into Arabic with the assistance of Indian pandits by Ibrahim al-Fazārī (d. 806) and Ya'qub ibn Tariq (d. 796), as the *Sindhind* and the *Arkand*. Kennedy[29] provides a long list of Arabic *Zijes* which incorporate characteristically Indian features like the use of the meridian of Ujjayinī (under the name Arin), the Kaliyuga era (beginning −3102 CE), called the "Era of Flood", the tables of sines ($R = 150$ used by Brahmagupta), tables of solar declination, and methods of spherical trigonometry. It is interesting that there are found also a large number of multiplication tables, typically giving 3600 entries, for multiplying sexagesimal numbers to the second sexagesimal minute.[30] It is worth observing that this transmission of Indian astronomy to Arabs *predates* the Arabic manuscripts (hence, also, the Byzantine Greek manuscripts) from which information about Ptolemy is conjectured.[31]

Al Khwarizmi who worked in the House of Wisdom (*Bayt al Hikma*), and had learnt Sanskrit, wrote a text on arithmetic, compiling and putting together various Indian texts. The original text (perhaps called *Kitab al-hisab al-hindi*) is now lost, and survives only in a 12th c. anonymous Latin translation: *Algoritmi de numero Indorum* (Al Khwarizmi on the Indian numbers). He also prepared a Zij, based on Indian parameters, and wrote a famous text *Kitab al-jabr wa-l muqabala*.[32] The present-day word "algebra" derives from this text, just as the present-day word "algorithm" derives from successive Latin corruptions of al-Khwarizmi's name as Algorismus, Algoritmus, and Algorithmus. Various other Arabic mathematicians, such as Ibn Labban,[33] wrote treatises on the Indian system of arithmetic. Ibn Labban's treatise (ca. 1000) also incorporates the earliest known Arabic multiplication table.

Algorismus in Europe

Though Indian numerals were already known in some pockets of West Asia by the seventh c. CE, where they were mentioned appreciatively by a Christian monk Severus Sebokht, it was largely through the Arabs that this Indian technique of calculation systematically travelled to Europe,[34] where the numerals came to be known as Arabic numerals, and the arithmetical technique itself came to be known as the Algorismus—one of the Latinized corruptions of the name of al Khwarizmi. Among the first to try this algorismus technique, which had become famous by his time, was the 10th c. CE Gerbert (later Pope Sylvester III). Gerbert did not understand the technique. In fact, he did not understand the representation of numbers on the new technique. Con ating it with his own way of representing numbers, he merely used Indo-Arabic symbols on counters for abaci!

> Yet it would be wrong to see in the *apices* nothing more than a trivial innovation introduced by Gerbert. The truth is that he did adumbrate the use of the new numerals; he had heard marvellous things about the new computation which they made possible but which he, and perhaps also his informants, did not essentially understand.[35]

However, Europeans did eventually understand the algorismus. Various Europeans are known to have translated al Khwarizmi's text, while probably drawing on various other Arabic sources.[36] These include Adelard of Bath (ca. 1142), John of Seville (ca. 1135, *Liber Algorismi*), Robert of Chester (ca. 1141), Alexander de Villedieu (d. 1240, *Carmen de Algorismo*), his contemporary John Sacrobosco (*Algorismus Vulgaris*), and Leonardo of Pisa (ca. 1202, *Liber Abaci*). This process of popularizing practical arithmetic in Europe continued actively until the the 16th c. which saw the publication of Cardano's, *Practica Arithmeticae*

(1501), Stifel's *Arthmetica Integra* (1514), Tartaglia's *Trattato di Numeri* (1556), and Clavius's *Arithmetica Practicae* (1583).[37]

While Florentine merchants were quick to realize that the reliance on counters put them to a competitive disadvantage, which they sought to eliminate by learning the algorismus, the rest of the society did not immediately follow suit, for the algorismus, of course, entailed difficulties. Unlike the abacus, where one could hold the numbers in one's hand, the numbers in the algorismus were abstract. The abstraction involved non-representables in an essential way. The use of non-representables to zero the numbers in a calculation was confounded with the use of the numeral zero.

The difficulty with the *numeral* zero was simply this: zero, by itself, stood for nothing; but when appended at the end of another number it enhanced the value of the preceding number. To understand this difficulty of representation, we need to understand that the system of Roman numeration, then in use in Europe, was primarily an *additive* system. That is, XIII represented X and III, or "ten and three", as on the abacus. The representation of numbers using the place value system was impossible to understand on this logic. Thus, zero was understood to mean nothing, and the above additive logic suggested that 20 should then be read as "two and zero", or "two and nothing", which ought to have amounted to 2.

These difficulties with the numeral zero led to the abuse of the numeral zero in contracts and to financial frauds. In 1299 the city of Florence came out with an edict prohibiting the use of the new figures for banking. (A similar thing survives to this day; a cheque must be filled with both words and numerals.) The figures themselves came to be known as ciphers (from *as sifr* = zephyr = zero), a term which even today means a hard to understand code.

These difficulties of representing numbers were compounded with the problem of representation and non-representability. First, there was no way to represent fractions with Roman numerals. However, Brahmagupta's fraction series expansion, which we have already encountered in Chapter 3, was a common sort of manipulation that enabled a fraction with an inconvenient denominator to be replaced by a fraction with a more convenient denominator—to the required level of precision. Secondly, this process of manipulating fractions could eventually lead to discarding or zeroing something as non-representable. This zeroing was very hard to understand, for here something which had a definite value, in another context, was treated as if it were zero. The two sorts of difficulties, notational and epistemological, were con ated and attributed to the mystique of "zero".

The difficulties persisted for centuries, but the practical value of the algorismus dominated in the end. According to standard histories of mathematics, the final victory of the algorismus is usually taken to coincide with the publication of Gregor Reisch's *Margarita Philosophica* (Basel, ca. 1517), which shows a smiling Boethius and a glum Pythagoras, the former representing the algorismus, and the latter the abacus. Of course, Boethius was not the originator of the algorismus, as contemporary European myths made out, but current-day depictions of the victory tend to be equally misleading.

> One might say, in a nutshell, that zero overcame the abacus. But its victory, which started in the Middle Ages, took a long time.[38]

As emphasized above, the real issue was not zero, but one of the practical value of the algorismus, versus its epistemology (which involved the non-representable). This was exactly the problem also in the case of the calculus. In both cases, the practical value forced acceptance: the practical technique remained unchanged, but epistemology was changed to accommodate it.

The End of Abaci

Also, abaci did not actually go out of circulation in the 16th c. Indeed, in the 17th c. we find the situation on the ground depicted by Shakespeare

> Let me see: every 'eleven wether tods; every tod yields pound and odd shilling; fifteen hundred shorn, what comes the wool to? ... I cannot do't without counters.[39]

Here "wether" refers to sheep, and tod to 28 lbs. Thus, the problem that the clown has to solve is the following: 11 sheep together give wool amounting to 28 lbs, which sells for 21 shillings. Given 1500 sheep, how many shillings will the clown get? In fact, as late as 1673, in Moliere's play *Malade Imaginaire* the opening scene has the hero checking his doctor's bills with counters. It is only in the 18th c. CE that there was a serious decline in the use of counters in Europe.

The suspicions about the algorismus had meant that counters continued to be prescribed for use for purposes of the exchequer—a word which derives from the chequered (or chessboard like) form of the table cloth on which counters were used, in the manner of an abacus, to keep an account of revenue. The exchequer of the Norman kings was also the court in which the whole financial business of the country was transacted, so the supreme court in Normandy was also called the exchequer, a term which was later superseded by the term *parlement*. These counters actually went out of history in quite a dramatic way, taking with them the British Parliament, as is well described in an 1855 speech by Charles Dickens.[40]

> Ages ago a savage mode of keeping accounts on notched sticks was introduced into the Court of the Exchequer and the accounts were kept much as Robinson Crusoe kept his calendar on the desert island... it took until 1826 to get these sticks abolished. In 1834 it was found that there was a considerable accumulation of them; and the question arose, what was to be done with such worn-out, worm-eaten, rotten old bits of wood? The sticks were housed in Westminster, and it would naturally occur to any intelligent person that nothing could be easier than to allow them to be carried away for firewood by the miserable people who lived

> in that neighbourhood. However ...the order went out that they were to be privately and confidentially burned. It came to pass that they were burned in the stove in the House of Lords. The stove, over gorged with these preposterous sticks, set fire to the panelling; the panelling set fire to the House of Commons; the two houses were reduced to ashes; the architects were called in to build others; and we are now in the second million of the cost thereof.

Thus, the burning down of the British Parliament in the 19th c. CE marked the real end of the clash between algorismus and abaci in Europe!

Irrational Numbers

Accustomed as they were to the abacus, the difficulties that the European encountered with the algorismus are also built into the very names for numbers. Thus, the solution of quadratic equations was very much a part of the Indian tradition. For example, we find Mahāvīra posing the following problem to a child:

> O! tender girl, out of the swans in a certain lake, ten times the square root of their number went away to Mānasarovara when the rainy season arrived, $\frac{1}{8}$th of that number went away to the Sthala Padminī forest. Three pairs of swans remained in the tank, sporting in the water. What is the total number of swans?

In present-day terminology, if the number of swans is x, the above problem corresponds to solving the quadratic equation $10\sqrt{x} + \frac{1}{8}x + 6 = x$. In this case, the problem has an integer solution ($x = 144$), but the attempt to generalize this procedure to other situations leads to non-integer, and irrational solutions. Indian tradition had no problem with irrational numbers like $\sqrt{2}$ or the so-called transcendental numbers like π for which it long accepted the impossibility of stating an exact value.

However, such irrational numbers arising from the solution of quadratic equations, or interest calculations, were viewed with suspicion in European tradition, since the abacus could not very well be used to solve quadratic equations, or to represent irrational numbers. They were called "surds" in European tradition. The term "surd", from the Latin *surdus*, means "deaf" in an active sense and silent, dumb, in a passive sense. By extension it refers to something not endowed with sense or reason (as in "dumb animals"), hence stupid and insensitive. The term is a Latin translation of the Arabic *acamm*, as in *jabr acamm* (=surd root), arising in the theory of the forcible (*jabrdasti*) or algebraic (*al-jabr* = algebra) consequences of putting two quantities on opposite sides of an equation (i.e., setting up a *muqabala*, which results in the resolution of an issue by force).[41] In the days when rational theology was being vigorously advocated, these numbers were also called irrational numbers, and this was understood not only in the sense that they were non-ratio numbers, but also in the sense that they were stupid and unreasonable numbers—numbers not quite endowed with the divine reason that rational theology championed after Proclus.

Dedekind Cuts and Supertasks vs Practical Tasks

Against this background of the experience of the algorismus in Europe, we can again ask: what exactly did the formalisation of real numbers achieve? Dedekind cuts helped to soothe fears of the irrational and socially deviant behaviour of these numbers, for they helped to assert confidently the existence of real numbers. But according to what standards is this existence today asserted? As already seen in Chapter 2, the formal mathematical existence of real numbers has nothing to do with any real or physical existence. This assertion of existence in a metaphysical or Platonic sense has not brought one any closer to the *specification* of an irrational real number like $\sqrt{2}$, for the full specification of any such number requires a supertask—an infinite series of tasks—which will take an infinite amount of time to be performed. Indian tradition does not admit the possibility of such supertasks, which are, from a practical point of view, at any rate, impossible.

As a matter of fact, Western mathematicians have been quite hypocritical about this point, and have adopted a double standard with regard to supertasks. While mathematics permits supertasks, metamathematics does *not* permit supertasks. For example, the "decidability" in Gödel's theorem relates to recursive decidability; if one were to allow supertasks (especially transfinite induction) in *meta*mathematics, every theory would become trivially decidable, simply by using transfinite induction to select a proof of a given statement from all sequences of statements that are proofs. Thus, Western mathematicians are, of course, well aware of not only the practical impossibility of performing supertasks, but also of the inadvisability of founding mathematics on such beliefs. Nevertheless, they permit supertasks *within* mathematics. This is sheer hypocrisy: a principle not good enough for metamathematics is regarded as good enough for mathematics. Traditional Indian thought did not accept such hypocrisy in matters concerning truth and knowledge: the same general principles of proof were applied to all situations. Western thought, however, seems to have double standards everywhere!

As we have already seen in Chapter 2, social and cultural authority, rather than logic or reason, is the key force behind this Platonic myth that such supertasks lead to something more real than ordinary reality. Ultimately, the only "argument" available to the mathematician is to disregard such scepticism about real numbers as socially unacceptable, just as the intuitionist skepticism about non-constructive proofs was deemed to be socially unacceptable. Professional mathematicians won't accept a change, since that might affect their jobs. Mathematicians are supposedly sceptical, but they can be sceptical only in a socially acceptable way! This way of determining social acceptability presupposes that professional mathematicians are the only ones who need to be consulted. How "social acceptability" can come to be decided in a way that excludes the majority of the human population is, of course, a separate question which we do not examine here. This sort of thing tends to reduce the seriousness of mathematics to that of a social event like a ball—possibly a very serious matter for the participants, but a mere play of vanities and conventions from the viewpoint of an outside observer.

VI
ŚŪNYA

Most physicists and engineers, even today, do manage to get along without knowing or caring what a formal real number is. That is because, for any actual application of the calculus, to physics or engineering, one still needs to calculate, and any such calculation is always done (and can be done) only to a required finite precision.

Today, such calculations are typically done on a computer. Howsoever good a computer one might use, the computation can never go beyond a certain precision. The same conclusion applies to a calculation done by hand, or to what is called indefinite precision arithmetic. Indefinite precision simply means that one can use as many decimal places as one is likely to require for all practical purposes.

Practically speaking, for any calculation one never needs more than a certain amount of precision, though the exact amount of precision one needs may go on changing from time to time, and from application to application. One never ever needs or *can* go to the limit. That means that there will always be an awkward part in any calculation that needs to be discarded. This part perforce has to be left non-represented.

Indian tradition has acknowledged the existence of non-representables, and has adopted a similar (though not identical) practical attitude to non-representables. The acknowledgement of non-representability is the focus of the Buddhist philosophy of *śūnyavāda* advocated by Nāgārjuna.

However, Western tradition has been very uncomfortable with this non-representable which it saw as impinging on the imagined perfection with which it had endowed mathematics. A key problem in the European assimilation of both the algorismus and the calculus was the cultural inability of the West to come to terms with this idea of non-representable, or *śūnya*, nowadays often facilely interpreted as zero.

The non-representable does indeed drop out of a calculation, like zero, but the process of zeroing a non-representable is not the same thing as the process of operating with the algebraic entity zero—the non-representable need not follow any of the simple algebraic rules followed by zero.

Secondly, in contrast to the abacus which gives a concrete representation to each number, the place value system provides a systematic nomenclature for numbers (including integers) which already encounters a first difficulty with the non-representable, a difficulty which exists also for integers, "because numbers are limitless, while signs are limited".[42] As opposed to the Roman system of numeration, where the nomenclature for numbers was somewhat haphazard and related to the word names and the abacus, the place value system involved a nomenclature for numbers that was not only systematic, but was closely linked to the basic arithmetic operations, as incorporated in the algorismus. This made the problem of non-representability manifest.

We saw above that there is nothing sacred about R. Even from within a formalist perspective, one may be required to work with a larger number system, where it would be necessary to accommodate infinities and infinitesimals, and disregard differences between numbers that are infinitesimally different. On the other hand, if we shift our philosophy of mathematics from an idealist to a realist position, one can perfectly well work, in a similar way, with a *smaller* number system.

The Non-Representable and Integers on a Computer

Present-day calculations done on a computer necessarily involve the use of such a smaller, finite number system, for a computer can only deal with entities that admit a concrete representation. Hence, integers on a computer are different from the idealized integers of Peano's arithmetic, for a computer can never do integer arithmetic of the sort formalised by Peano. We saw, in the earlier program, how addition of two numbers in a C-program would lead to the sort of arithmetic in which

$$20000 + 20000 = -25596$$

. This happens because the computer reserved only 16 bits to represent an integer; so it could only represent integers between −32768 and 32767. The same C-program compiled on a 32 or 64 bit Windows platform would be able to represent a wider range of integers. To obtain a similar "failure" of integer arithmetic on a computer, one would need a larger number of zeros in the numbers being added on the left. Indeed, the exact point at which computer integer-arithmetic fails can be pushed very far off, in a region where we don't at all care what happens. The point, however, is that, unlike in Peano's formal arithmetic, there always will be such a "don't care" or non-representable region for any calculation done with integers or any other sorts of numbers on a computer. The limit is specified by the total storage available to the computer, which may be very large, and more than adequate for all practical purposes.

The Non-Representable and Floating Point Numbers

Of course, one is not obliged to use one bit to represent one place in the binary expansion of an integer; one can use instead the oating point (mantissa-exponent) representation. The range of numbers that can be expressed using 32-bit oats is now greatly increased, and typically extends from around 1.18×10^{-38} (or 1.415×10^{-45} for non-normal numbers) to around 3.37×10^{38}. But now one encounters another problem. As a trivial example, if we extract the square root of 2 and square it, this will not give us back 2. Any practical calculation using floats on a computer involves explicit zeroing of terms regarded as insignificant.

Indian tradition was comfortable with this fact of life, for it did not see the world or mathematics as something that reflected the ideal rational laws of a transcendent God. However, *if* we regard the operations of extracting a square root and squaring as formal inverses of each other, then we are bound to say that the calculation of squares and square roots is approximate and involves an error. In this way of looking at things, all calculations, hence all practical mathematics, must forever remain erroneous.

The difficulty that the idealist philosophy has in grappling with non-representables has not entirely disappeared as of today, because it has not been correctly understood as such, and computational calculations are still seen with the idealist gaze.

Computational floating point numbers are formally described by the IEEE standard 754. An even more idealized system of computational floating point numbers is sometimes used for theoretical purposes. These correspond to rounding or chopping arithmetic.[43] As we saw in Chapter 3, the calculations involving what would today be called irrational numbers were done perfectly well in Indian traditions using rational numbers. However, in traditional Indian mathematical calculations rounding, for example, was done on a rule-and-exception basis. This is different from the current treatment of floating point numbers on a computer, which is rule bound in a mechanical way that characterizes both present-day computers and the Western understanding of mathematics. Thus, traditional Indian numbers are not identical with floating point numbers used on a computer. However, for our immediate purposes we can regard them as similar, for the key point here is that whichever of these representations of numbers we choose, it must involve non-representables.

Failure of Algebraic Laws for Floating Point Numbers

Our key concern here is with the concrete mathematical consequence of the existence of non-representables. Consider the practical version of the floating point number system used on computers (IEEE 754). Setting aside the more technical case of underflow, there are various types of non-representables. One type of non-representable, called NaN (Not a Number), is that which cannot at all be represented as a floating point number on computers using the above standard. From the point of view of formal arithmetic, most real numbers fall in this category, as also most rational numbers and most integers! Changing the standard to what is euphemistically called "infinite precision" arithmetic will only change the "don't care" threshold, but will not change any of the above statements.

The existence of non-representables has various curious arithmetic effects. For example, most numbers on a computer will behave like zero when added to a much larger number. (Thus, there *is* a relation between non-representable and zero; the non-representables are zeroed in a calculation.) For example, if we are working with the IEEE oating point standard, then we have

$$1 + \quad = 1,$$

where is any number less than about 10^{-7} (or less than the "machine epsilon" if double precision is used). The technical reason for this is that the computer must bit-shift the mantissa to equalize the exponent to add two numbers in the oating point representation. The above might not seem much of a catastrophe, but we also have, by the same logic,

$$10^7 + 1 = 10^7$$

if we use oating point representation on a computer.

That is, *there is no absolute or mechanically representable notion of a non-representable*. A number which is representable in one context may become non-representable in another. Thus, for example, the number $= 10^{-8}$ is easily representable as a oating point number; however, in the arithmetic operation of adding it to 1, becomes non-representable, so that $1 + \quad = 1$. Non-representability may be relative, and may vary with the context.

Various "laws" that formal integers and real numbers "ought" to obey are today taught to children. By these standards, the integers and oating point numbers on a computer are outrageous criminals who don't respect any of these laws! As a consequence, most of the usual "laws" of arithmetic, including the associative "law" for addition and multiplication, fail. For example,

$$1 + ((-1) + \quad) = 0 = \quad = (1 + (-1)) + \quad .$$

Numbers on a computer can hence never form a field or any of the more common algebraic structures to which idealized numbers are subject.

Another type of non-representable, called INF and $-$INF, arises when the exponent is larger than permitted. To understand how this case is handled, we need to understand the extended real number system.

Formally, the extended real number system $R \quad = R \quad -\infty, \infty$. Here, the two symbols $-\infty, \infty$ satisfy the following kind of algebraic identities: $\frac{a}{\ } = 0$, $a \cdot \infty = \infty$ (if $a > 0$), etc. Division by zero is not defined, hence also $\frac{0}{0}$ is not defined; but this is more a matter of empty fastidiousness, for something very similar is defined: viz. the product $0 \cdot \infty$ is usually[44] defined as 0 (since such a convention is especially needed in probability theory and the theory of Lebesgue integration).

Accordingly, the IEEE standard has three additional kinds of not-quite-numbers, INF, $-$INF, and NaN. The last is an abbreviation for Not a Number. It has been put in to take care of the kind of situations where an operation with oating point numbers is undefined, such as division by zero.

Like all standards, this standard too is undergoing a subtle practical transformation. For example, the Java language claims platform indifference. That is, a program built on one system will run identically on all other systems. (This is different from C or C++ programs that are portable, but may run differently on two different platforms, such as 32-bit Windows and DOS.) To this end, the Java language defines primitive data types like oats and ints in terms of bits rather than bytes. It claims to respect the IEEE oating point standard. However, the following problem arises. In Java (Version 2) an integer divided by zero leads to a run time error. But if the same int is cast as a oat and then divided by zero, the result is INF! That is to say, if 2 is regarded as a real number, then $\frac{2}{0} = \infty$, while if 2 is regarded as an integer, then $\frac{2}{0}$ is an illegitimate arithmetic operation. Java is a language of very recent origin. Thus, confusion about non-representables is still widespread to the present day.

Classifying and representing a few types of non-representables does not, of course, solve the problem of non-representables. The non-representable, *per se*, can no longer be ignored; its existence is today undeniable because computers cannot deal with non-representables—and, unlike human beings, present-day computers simply cannot pretend to be able to deal with a thing (an ideal point, for example) if they can't!

Calculus on a Computer

Finally, let us notice that most practical applications of calculus to science and engineering typically require calculation of the solution of some sort of differential equation—that can be done very well on a computer. However, to do a calculus-related calculation on a computer, it is necessary first to translate calculus into the language and numbers available on a computer. This is usually done by translating derivatives to finite differences, and real numbers to oating point numbers.

So we see that, to arrive at something of practical value, we are compelled to throw away formal real numbers, and return back to the starting point.

The only question that remains is one of epistemological security. Calculations done on a computer, though adequate for practical purposes, are regarded as intrinsically "erroneous".

VII
ŚŪNYAVĀDA VS FORMALISM

It is therefore worthwhile to brie y examine things from the perspective of *śūnyavāda* philosophy, according to which it is idealist Platonic philosophy that is intrinsically erroneous.

We see here two clearly differing philosophies of number. According to one—the Platonic philosophy and its derivatives—only the ideal can be real. It can never be practically attained. The practical must remain forever erroneous and inferior. According to the other—the Buddhist philosophy of *śūnyavāda*—comprehension of reality requires us to come to terms with non-representability, and not to posit the existence of ideal entities that are neither manifest nor can be inferred from the manifest: idealizations are intrinsically erroneous and empty.

It is worth expounding this idea: since it is so contrary to the theology that has motivated Western mathematical thought since Plato, many people may have difficulty in understanding it.

Thus, the motivation for idealistic mathematics has been mostly theological: Plato related mathematics to the soul as did Proclus nearly a thousand years after him, and both were quite explicit about the relation of mathematics to religious beliefs. The Christianization of mathematics at Toledo was also motivated by the key concern of making mathematics theologically correct, by transforming Procluvian philosophy and Islamic rational theology to something acceptable to the revised Christian doctrine of the 4th c. Though formalism secularized this, idealizations always tend to be coloured by religious beliefs, and only the practical can be truly secular. For example, it is well known how Hilbert's notion of proof was initially found to be "theology, not mathematics" by Paul Gordon.[45] One understands that believers will persist in their beliefs, but there seems no reason why anyone else is bound to accept this idealist theology or the valuation of the metaphysical over the physical, especially since this is of no particular practical value. On the contrary, idealistic mathematics is of some anti-practical value, since the theologification of mathematics is what makes it hard for students to understand.

Therefore, as an alternative to formalism and Platonism, we articulate the consequences here of the *śūnyavāda* philosophy of Nāgārjuna. This philosophy is antithetical to the Platonic philosophy of idealism.

We have seen that the Buddhist notion of *pramāṇa* accepts only two principles: the empirically manifest and inference. Therefore, while *śūnyavāda* would readily concede the existence of a physical dot on a piece of paper, it would deny the existence of an idealized mathematical point, or a notion of "pointness" the existence of which is neither manifest nor can be inferred from other things that are manifest. Therefore, instead of saying that the dot on a piece of paper is an erroneous representation of a geometrical point, *śūnyavāda* would say that the idealized geometrical point is an erroneous representation or empty conceptualization of the real dot on the piece of paper. Although this point of view is really very simple and natural, it may seem very hard to understand for those who have been conditioned from childhood into unnatural ways of looking at natural things.

This point of view is called *Mādhyamika* (more popularly known as zen, derived from *śūnya*) or the "middle way" because it neither accepts the extreme of *śaṣvatavāda*—the doctrine of the eternal existence of idealized entities—nor does it accept the nihilistic (*ucchedavāda*) position of denying all existence altogether. This is expressed by Nāgārjuna in the succinct formula which opens his *Mūlamādhyamakakārikā*: *anucchedam, aśāṣvatam* ("Neither non-existence nor permanence").

For the same reason, *śūnyavāda* denies the existence of an immortal soul or idealized notion of identity—for the existence of the soul is neither manifest, nor can it be inferred. *śūnyavāda* would point out the manifest fact that the empirical world changes every instant, and so do individuals.[46] There is no evidence to suggest that something "essential" in individuals remains the same across all these changes. Thus, the seed in the granary is not the cause of a plant, because it is different from the seed in the ground (which is bloated up etc.). Although the seed has changed, we continue to use the same name "seed" since there are so many seeds in the granary and they keep changing every moment, so that it is practically impossible to give all of them distinct names. Similarly, it is due to this *paucity of names*, that one gives only a single name to an individual from birth to death, neglecting the variety of actually observed changes as non-representable. Since the very existence of the soul (or any kind of God) is denied, therefore, there is no question of mathematics being good for the soul, as asserted by Proclus. The idealized notion of a geometrical point or "point-ness" is empty. Similarly the notion of an idealized real number, or "pi-ness" is empty—devoid of any reality.

We recall from Chapter 3 that since −500 CE the *śulba sūtra*-s had exactly this practical attitude towards real numbers, when they described the value of $\sqrt{2}$ and π as *sa-viśeṣa* meaning "this with something remaining". A similar point of view was adopted a thousand years later by Āryabhaṭa in the *Āryabhaṭīya* where the value of π is described as *āsanna* meaning "near". We have also seen in Chapter 3 how yet another thousand years later Nīlakaṇṭha unambiguously accepted this state of affairs. Thus, there is nothing specifically "Buddhist" in the acceptance of non-representability, it is just that *śūnyavāda* philosophy provides a complete and explicit ontology and epistemology for this practical attitude, which has otherwise been dismissed as "erroneous" on grounds of high idealistic philosophy which may itself be erroneous as we have pointed out.

If social acceptability among professional mathematicians is the ultimate test of mathematics, then it is possible that mathematicians with religious leanings or cultural predispositions may be inclined to choose one sort of mathematics over another. However, in that case, a better solution might be to clearly separate Platonic mathematics, Christian mathematics, etc. from secular, practical mathematics. The one sort of mathematics could be pursued, like music, or theology, for cultural and religious reasons, while there would be a much wider agreement on the other sort of mathematics. This would *not* be the same as the division between pure and applied mathematics, for the latter would no longer be epistemologically dependent upon the former—while "pure" mathematics would vary with the cultural milieu, practical mathematics would not.

NOTES AND REFERENCES

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2. For my own work in this direction, initially targeted for C-DAC supercomputer, in 1994, see, C. K. Raju, "Supercomputing in finance", *Pranjanā*, **3** (2000) pp. 11–36.
3. Paulo Mancosu, *Philosophy of Mathematics and Mathematical Practice in the Seventeenth Century*, Oxford University Press, 1996. For our immediate purpose, it is irrelevant whether this accusation was valid; the point is only that the thought of atoms aroused horror, which the thought of the continuum did not.
4. Many of the epistemological problems attendant on the arrival of the calculus in Europe have been reviewed in the book by Mancosu, which however adheres to the rigid frame of racist history pretending that the calculus and mathematics had no existence outside of "Greeks" ("Archimedes") and Europe. What is being suggested here, to the contrary, is a review of the epistemological problems in the light of contrasting Indian and European epistemologies. Paolo Mancosu, *Philosophy of Mathematics in the Seventeenth Century*, cited above.
5. Newton provided this account anonymously, in his summary of his priority dispute with Leibniz. Isaac Newton, "An account of the book entituled *Commercium epistolicum collinii Et aliorum, de analysi promota*", *Philosophical Transaction of the Royal Society of London*, No. 342. January and February 1714/15, pp. 173–224.
6. George Berkeley, *The Analyst or a Discourse Addressed to an Infidel Mathematician*, ed. D. R. Wilkins, 1734, available online at http://www.maths.tcd.ie/pub/HistMath/People/Berkeley/Analyst/Analyst.html
7. George Berkeley, *The Analyst*, cited earlier, XXXV.
8. Berkeley, *The Analyst*, cited earlier, IV, V.
9. Mancosu, *Philosophy of Mathematics in the 17th c.*, cited earlier, p. 54/
10. Berkeley, *The Analyst*, cited earlier, XXII.
11. Berkeley, *The Analyst*, cited earlier, XXI.
12. James Jurin, *Geometry no Friend to Infidelity*, London, 1734, and *The Minute Mathematician*, London, 1735.
13. James Jurin, *The Minute Mathematician*, cited earlier.
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15. Herodotus, *The History*, 1.53, p. 11.
16. Herodotus, *The History*, 1.91, p. 22.
17. Isaac Newton, "An account of the book entituled *Commercium epistolicum collinii Et aliorum, de analysi promota*", *Philosophical Transaction of the Royal Society of London*, No. 342. January and February 1714/15, pp. 173–224
18. Not to be confused with Gerhard Mercator the chart-maker.
19. My own sense is that, despite their unfortunate ethical context, there is some weight in Newton's arguments against Leibniz. In particular, I am unaware of any detailed point-by-point response to Newton's charges ever having been given.
20. Al Bīrūnī, *Kitab al Hind*, trans. E. C. Sachau, *Alberuni s India*, reprint, Munshiram Manoharlal, New Delhi, 1992, vol. 1, p. 111.
21. E.g. Walter Rudin, *Principles of Mathematical Analysis*, McGraw Hill, New York, 1969, , chp. 1. Cohen, *Structure of the Real Number System*, Kreiger publishing, 1977.
22. For an explanation of why a "proof by territory limitation" is credible, see C. K. Raju, "Patterns of irrationality", Appendix to *The Eleven Pictures of Time*, Sage, New Delhi, 2003.
23. For example, I. N. Herstein, *Abstract Algebra*, Wiley, New York, 1972.
24. The Alexandrian Greeks seem to have shifted to a Hebrew way of representing numerals, which is corroborated by Ptolemy I's interest in the Jewish customs.
25. The alleged use by Ptolemy of the sexagesimal system, to represent fractions, does not correspond to any Greek or Roman numerals in actual practice—Ptolemy's alleged system of numeration like his astronomical parameters were unknown to the rest of the Roman empire. The sole evidence for its use comes to us only from late Arabic manuscripts. There is no evidence for the claim that this system of numeration was perhaps obtained by Ptolemy from Babylon and transmitted from Ptolemy to Arabs. On the contrary, the simplest explanation for the alleged use of the sexagesimal system and Arabic notation by Ptolemy is that this was the system used by the Arabic astronomers who penned the manuscript incorrectly attributed in its entirety to Ptolemy—quite naturally the Arabian scribe would have used the notation for numerals

prevalent in his time, which was a couple of centuries after Arab astronomers obtained this sexagesimal system of representing fractions from India.

26. J. M. Pullan, *The History of the Abacus*, Praeger Publishers, New York, 1968.
27. E.g. http://mathforum.org/dr.math/faq/faq.roman.html
28. This sort of thing also makes it very hard to understand the meaning of the statement attributed to Claudius Ptolemy, in the context of the table of chords "we shall follow through the multiplications and divisions, aiming always at such an approximation as will leave no error worth considering as far as the accuracy of the senses is concerned". R. Catesby Taliaferro (trans.), *Ptolemy: The Almagest*, Great Books of the Western World, 2nd edn., vol. 15, Encyclopaedia Britannica, Chicago, p. 14. This statement is hard to understand for it seems to presuppose that the multiplication and division are done using an algorithm rather than an abacus. The statement, however, would make perfect sense in the context of an early Arabic astronomer, at a time when the algorismus had just been imported into the Arab empire.
29. E. S. Kennedy, "A survey of Islamic astronomical tables", *Transactions of the American Philosophical Society*, N. S., **46** (2) (1956) pp. 123–177.
30. David A. King, "On medieval Islamic multiplication tables", *Islamic Mathematical Astronomy*, 2nd edn., Variaroum, Hampshire, 1993, chp. 15, pp. 317–23.
31. In the context of the doubt expressed above, about Western historical claims about Ptolemy, it is noticeable that very few of these "Islamic" multiplication tables involve the third minute. "Ptolemy's" table of chords, however, involves multiplication and division using the third sexagesimal minute, and "Ptolemy" gives some values to the eighth minute!
32. F. Rosen (trans.), *The Algebra of Mohammed Bin Musa*, Royal Asiatic Society, London, 1831.
33. Ibn Labbān, *Kitab fi usual hisab al-hind*, trans. M. Levey and M. Petruck, *Kushyār Ibn Labbān: Principles of Hindu Reckoning*, Univ. of Wisconsin, Madison, 1965.
34. E.g. George Sarton, *Introduction to the History of Science*, vol. 1, Carnegie Institute, Washington, 1927, p. 530.
35. Karl Menninger, *Number Words and Number Symbols: A Cultural History of Numbers*, trans. Paul Broneer, MIT Press, Cambridge, Mass., p. 325.
36. Some of these details may be found in Suzan Rose Benedict, *A Comparative Study of the Early Treatises Introducing into Europe the Hindu Art of Reckoning*, Ph.D. Thesis, University of Michigan, 29 April 1914, Rumford Press, Concord (no date given).
37. S. N. Sen, "Indian elements in European Renaissance", *Organon*, **4** (1967) pp. 55–59.
38. Menninger, cited above, p. 331.
39. *The Winters Tale*, IV.III, in *William Shakespeare, The Plays and Sonnets*, ed. W. G. Clarke and W. A. Wright, vol. II, vol. 25 in Great Books of the Western World, Encyclopaedia Britannica, Chicago, 1996, p. 506.
40. Charles Dickens, *Speech to the Administrative Reform Association*, 18 June 1855, *Speeches of Charles Dickens*, ed. K. F. Fielding, Clarendon, Oxford, 1960, p. 206. More details in Katherine Solender, *Dreadful Fire! The Burning of the Houses of Parliament*, Indiana University Press, 1984. This "Robinson Crusoe technology" of accounting was first *introduced* in Britain by Normans in the 12th c., over six hundred years after Āryabhaṭa; see, e.g., J. M. Pullan, *The History of the Abacus*, Praeger Publishers, New York, 1968, p. 51.
41. We recall that *Al jabr w al-muqabala* was the title of a book written by al Khwarizmi, or Algorismus. A book with the same title on the same topic was also written by Abu Kamil.
42. Nīlakaṇṭha, *Āryabhaṭīya Bhāṣya*.
43. E.g. Pat A. Sterbenz, *Floating-Point Computation*, Prentice-Hall, EngleWood Cliffs, NJ, 1974.
44. W. Rudin, *Real and Complex Analysis*, McGraw Hill, New York, 1973.
45. Max Noether's obituary of Gordan in *Math. Ann.* **75** (1914) pp. 1–41, p. 18. Felix Klein, *Vorlesungen ueber die Entwicklung der Mathematik im 19. Jahrhundert*, vol. I, Berlin 1926, p. 330.
46. This is sometimes called the Buddhist doctrine of ux. However, Nāgārjuna denies the entityness of time as later also argued by the Advaita Vedantin ŚrīHarṣa. These arguments are known to Western philosophy under the name of "McTaggart's paradox".

Part IV

The Contemporary Relevance of the Revised History

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CHAPTER 9

Math Wars and the Epistemic Divide in Mathematics

European historical difficulties with Indian mathematics and the present-day learning difficulties in mathematics

OVERVIEW

WHY do school (K-12) students find mathematics especially difficult? What is a good way to ameliorate these difficulties? Would the new technology of computation *fundamentally* change the *content* of mathematics?

Learning difficulties peculiar to mathematics are here traced to an epistemic schism in mathematics. Using "phylogeny is ontogeny" these difficulties are seen as reflections of actual historical difficulties. Much mathematics taught at the K-12 level is of Indo-Arabic origin: (1) arithmetic, (2) algebra, (3) trigonometry, (4) calculus. This mathematics arose in a different epistemic context, and Europe experienced difficulties in assimilating it because it recognized only a single "universal" European mathematics. This led to the real math wars, lasting for a thousand years, first over algorismus and zero and then over calculus and infinitesimals. During this period the imported mathematics was slowly "theologified" to make it compatible with Western metaphysics. This also complexified mathematics: the formalistic understanding even of integers is far too complex to be taught at an elementary level. The concerns underlying formalism being metaphysical, formalisation did not add any practical or secular value to mathematics—but practical value is the main reason to teach mathematics at the elementary level.

Computers have precipitated a third math war by again greatly enhancing the ability to calculate in a way regarded as epistemically insecure—according to Western metaphysics. The suggested correction is to recognize the distinct epistemic setting of mathematics-as-calculation and teach it accordingly.

I
INTRODUCTION

The Math Wars in the United States

In recent times, mathematics education in the United States has been ravaged by the so-called Math Wars. Worry over the poor performance of US students in mathematics tests[1] again focused attention on mathematics education in the 1980's. This led to the formulation of a set of standards by NCTM[2] in 1989 (contested by e.g. California,[3] and updated[4] in 2000). The US Education Department brought out a White Paper on mathematics education,[5] and, in October 1999, endorsed as "promising" certain texts promoting "constructivist"[6] or discovery-learning methods of teaching mathematics. This "constructivist" curriculum has since been labelled "new new math", "fuzzy math",[7] and "no correct-answer math" by opponents, who include Field Medalists and Nobel Prize winners.[8] Worries about poor performance in mathematics persist,[9] and the TIMSS-R[10] sought to relate this poor performance to a variety of factors (apart from the curriculum), such as university degrees of maths teachers, home education resources, etc.

The Epistemic Divide

None of this addresses the root cause of learning difficulties specific to mathematics. The controversy surrounding the "new new math" of the 1990's, like that surrounding the "new math" of the 1960's, is situated by this chapter as only a symptom of a deeper and more persistent malaise, an epistemic schism within mathematics. The quarrel about *what* and *how* mathematics should be taught simply re ects fundamentally divergent perceptions of what mathematics *is*.

This divide in mathematics is rooted in history. Much of what is today taught in K-12 mathematics—arithmetic, algebra, trigonometry, calculus—is a product of a complex historical process of cultural assimilation as some of the very names "algebra", "sine", "surd", and "algorithm" indicate.[11] Elementary arithmetic algorithms, for example, competed with abaci for over six hundred years in Europe because of the difficulties encountered in this process of assimilation.

Phylogeny is Ontogeny

This chapter proposes that we learn from these historical difficulties by applying in a novel way the principle that "phylogeny is ontogeny"—that the learning process re ects the historical evolution of the subject, telescoped into a much shorter period of time. Thus, the attempt is to understand the difficulties that students today have in assimilating elementary mathematics by studying the difficulties that arose historically in the process of culturally assimilating that mathematics. Correction naturally follows a better understanding.

II
DETOXIFYING THE HISTORY OF MATHEMATICS

This, of course, requires a fresh approach to history not as an instrument of glorification, but as a means of understanding. This new approach makes epistemology the key to understanding the history of mathematics.

The Two Streams of Mathematics

Brie y, Europe inherited not one but two mathematical traditions: (i) from Greece and Egypt[12] a mathematics that was spiritual, anti-empirical, proof-oriented, and explicitly religious, and (ii) from India via Arabs a mathematics that was pro-empirical, and calculation-oriented, with practical objectives.[13] Much mathematics taught at the K-12 level is of Indo-Arabic origin: (1) arithmetic, (2) algebra, (3) trigonometry, and (4) calculus.

Despite the obviously different philosophical orientations of these two streams of mathematics Europe recognized only a single possible philosophy of a "universal" European mathematics, into which it forcibly sought to fit both mathematical streams. One can understand how this happened under the in uence of religious politics as follows.

The Role of Religious Politics

In Europe ever since state and church came together some 1700 years ago, history became a malleable instrument of religious politics. Through Constantine, Charlemagne, crusades, and colonization, the church thrived on the most extreme agenda of hate and violence ever known to humanity. Papal *fatwa*-s, like the bull Romanus Pontifex, promulgated a doctrine known as the "Doctrine of Christian Discovery",[14] which required, *inter alia*, that no "theologically incorrect" part of the world could, in principle, make any significant contribution to knowledge or discovery. Though these Bulls have been widely regarded[15] as setting the agenda for religiously motivated genocide in the Americas,[16] they also set the agenda for intellectual genocide, by seeking to eliminate the contributions of the Persians, the Egyptians, Indians, and the Arabs, up to the 11th c. CE, by the crude device of attributing all of it to the "Greeks". Furthermore, the extreme violence of the church was also directed inwards: in the days of the Inquisition, the slightest acknowledgment of "pagan" in uence could easily have led to one being denounced by some rival, with grave and excessively painful consequences. Even in England, a Newton kept his theological deviance secret throughout his life, and the final version of his 8-volume *History of the Church* still remains a secret.[17] All this resulted in the amusing historical fantasy that mathematics originated in "Greece" (located in Africa!)

This distorted history inevitably impacted also the philosophy of mathematics, so that mathematics came to be defined in Europe as something that imitated the "Greek" method of proof—as sanitized by Christian rational theology.[18] A key element of this sanitization was the complete elimination of the empirical from mathematics, as in the current notion of mathematical proof due to Hilbert and Russell. The complete elimination of the empirical conveniently reduced mathematics to a branch of metaphysics.

III
THE REAL MATH WARS

Because of this agenda of forcing all knowledge to fit a convenient theological mould, Europe attempted to force the imported practical mathematics into a metaphysical mould of mathematics-as-certitude. This led to a protracted struggle lasting a thousand years: the resulting tensions were re ected not only in Clavius' advocacy of practical mathematics and his in uential reform of the mathematics syllabus,[19] but also in popular satire[20] on Platonic mathematics. The difficulties with the infinitesimal calculus, and, more recently, computational mathematics, are some of the other high points of this struggle.

More systematically, in this thousand-year old and continuing clash of mathematical epistemologies, one can identify three phases, concerning algorismus, calculus, and computers, respectively.

(1) **Algorithms and the First Math War.** Today's elementary arithmetic algorithms were accepted in Europe after some six hundred years of battle (from the 10th to the 16th c. CE) between earlier abacus methods and algorismus methods. Gerbert (Pope Sylvester II, d. 1003 CE) first used Indo-Arabic symbols on counters (*apices*) without understanding that method of computation.[21] Algorismus texts were based on (al Khwarizmi's) translations of Indian mathematical texts of the 7th c. CE, and these methods of arithmetical computation, studied for their practical value by Florentine merchants, were viewed with great epistemological suspicion in Europe. The turning point of this war is usually placed in the 16th c. CE,[22] but the war truly ended only in 1834 with the burning of tally sticks which also burnt down the British Parliament.[23] The difficulties have usually been regarded as relating to the symbolic representation of numbers versus the concrete representation of numbers in the abacus. (The usual algorithms for addition, subtraction, multiplication, and division, are impossible with the Roman numerals used in Europe, and explicitly require a place-value system.) Thus, zero was problematic since it had "no value in itself, but added any amount of value on being placed after a number". But there were various other differences. The "Greek" notion attached a mystical significance to numbers, so that a typical challenge problem to a mathematician in 16th c. Europe was this: "Is unity a number?" (The expected answer being that unity is not a number.) The Indian notion, on the other hand, did not have such hang-ups. A more subtle problem related to the question of non-representable (*śūnya*, both infinitely large and infinitesimally small, later zero[24]). Thus a key problem was that, unlike Buddhist philosophy (particularly *Śūnyavāda*), idealist philosophy failed to seriously address the problem of non-representables. These difficulties, by the way, are not entirely

over: look at the peculiar conventions relating to zero in the Java computing language: zero as integer behaves differently from zero as a oating point number!

(2) **Calculus and the Second Math War.** The infinitesimal calculus is another key aspect of mathematics-as-calculation, and the struggle to assimilate the calculus may be seen as exactly analogous to the case of the algorismus. As my earlier papers have sought to show, from the 16th c. onwards, Indian mathematics/astronomy texts of Āryabhaṭa, Bhāskara, Nīlkanṭha, Śaṅkara Vāriyar, and Jyeṣṭhadeva, containing key results of the calculus, were transmitted from Cochin[25] to Europe by Jesuits like Matteo Ricci[26] in connection with the European navigational problem (of determining latitude and longitude at sea), the related problem of computing precise trigonometric values,[27] and the related[28] calendar reform of 1582. Despite the obvious practical merits of the calculus, its inherently foreign epistemology was mathematically unacceptable to many in Europe, so that there followed another three centuries of warfare about the exact mathematical status and worth of "infinitesimals". Basically, the Indian infinitesimal techniques involved two features that were unacceptable in Europe. The first was that the Indian notion of *pramāṇa*, since it permitted the use of the empirical, was different from the European notion of mathematical proof. The second was that Indian techniques of calculation routinely used rounding, while the European notion of mathematics as certitude required that the smallest quantity should not be neglected. (This difference can still be seen in everyday commercial transactions today; in India, a vegetable vendor will routinely try to round off Rs 18 to Rs 20, by adding a small purchase, while Rs 20.50 will equally be rounded down to Rs 20. This is not the case in the West, and this cultural difference is not really to do with the non-availability of small change.) Thus, while valid *pramāṇa* was available for the infinite and indefinite series in Indian tradition, Cavalieri, Wallis, Gregory, Newton, Leibniz, etc. struggled in vain to convert it into mathematical proof that was acceptable to Europeans. Despite the historical glorification with which we have been inundated, it is clear from Berkeley's objections[29] an actual epistemic advance had to await Dedekind's semi-formalisation of real numbers in the late 19th c., and the formalisation in the 20th c. of the set theory that it used. Thus it took a long time to assimilate the calculus within formalistic mathematics.[30]

(3) **Computers and the Third Math War.** Computers, today, are rapidly widening this divide in mathematics. Numbers represented on a computer necessarily disobey key theoretical "laws", such as the associative law, required of numbers in formal number systems, and taught to K-12 students. However, using this oating point representation of numbers,[31] computers enable numerical calculations that stretch far beyond what can be mathematically proved; such calculations may have great practical value, as in solutions of stochastic differential equations driven by Lévy motion, used to estimate financial risk, or study perturbation related to controlled fusion, or in solutions of functional differential equations used in my proposal for a new physics.[32] Nevertheless, such numerical solutions continue to be regarded as mathematically valueless in the absence of a proof that the solution exists.

IV
RESOLVING THE MATH WARS

The root cause of this thousand-year old math war may now be identified: each case of algorismus, calculus, and computers, enhanced the ability to calculate, but with techniques regarded as epistemologically insecure from the Platonic viewpoint. Being not indifferent to the practical value of the mathematics, Europeans sought to force this mathematics to be "theologically correct" by reinterpreting it. The difficulty of this task is what made the assimilation of mathematics in Europe so difficult that it took nearly a thousand years. Using "phylogeny is ontogeny", it is this superimposition of theology that makes mathematics difficult to learn today. To resolve the quarrel about the teaching of mathematics, we must first address this epistemic schism in mathematics. We must first decide in a culturally neutral way: does mathematics relate to calculation or to proof? And, what are valid methods of proof?

On the one hand, from a formalist perspective, proof[33] has a higher epistemological value than calculation: it is today mathematically acceptable for a mathematical theorem to prove the existence of something without providing any accompanying method of calculation (or even construction), but no Field's medal was ever given for making a complex calculation, unsupported by a proof; for something that lacks proof would not today be regarded as mathematics, and would not, therefore, qualify for a Field's medal.

On the other hand, there is the undeniable fact that for all practical applications of mathematics, such as sending a man to the moon, it is not the existence theorem *per se* but the calculation that is important; and that calculation usually involves many layers of approximation, and potential sources of error, in obtaining a numerical approximation to an approximate solution of a physical model which is itself "approximate". Thus, the result of a typical calculation, though useful like the physical model, cannot but be "approximate", empirically based, and fallible—quite unlike the result of a mathematical proof, which is believed to be an exact, formal, perfect, and certain theorem.

That belief is questionable.[34] Brie y, Plato regarded mathematics as universal for he believed it concerned necessary truths. Formalists, while maintaining the Platonic divorce from the empirical, have shifted the locus of this necessary truth from theorem to proof, which is believed to connect arbitrary axioms to their necessary consequences. However, this belief too is incorrect, for proof uses logic, which is neither culturally universal (e.g. Buddhist or Jain logic[35]) nor empirically certain (e.g. quantum logic[36]). Furthermore, the notion of valid proof has varied across cultures: so formal mathematics contains no necessary or universal truths, but is purely a system of aesthetics like music.

This aesthetic does not suit practical mathematics-as-calculation which needs an alternative epistemological basis, a basis which acknowledges inexactitude, fallibility, differences from formal notions of "number", and accepts a role for the empirical ("contingent") within mathematics. Practical and useful mathematics, as decried by Plato, but as used in algorithms, calculus and numerical computation, needs a separate, non-Platonic, non-Neoplatonic ("non-Euclidean") epistemology, and it needs to be taught in a different way.

V
CORRECTING MATH TEACHING

So what does the revised history and "non-Euclidean" epistemology of mathematics mean for classroom teaching?

Brie y, since formal mathematics is no more than a culturally-dependent system of aesthetics, while it may continue to be taught like Western music, there is no need to impose its consequences on K-12 children. What we need to teach children is practical mathematics. And this can be taught much more easily in the epistemic setting in which it originated.

As a concrete example, consider the case of "Euclidean" geometry, which has been part of the traditional European mathematics curriculum almost since the inception of Oxford University, and part of the Arabic and Neoplatonic mathematical syllabus for centuries before that. Allowing unrestricted recourse to the empirical in mathematical proof trivializes the book.[37] On the other hand, Hilbert's[38] synthetic reinterpretation of the *Elements*, leading to the 1956 recommendations of the US School Mathematics Study Group,[39] still used in Indian schools, has serious problems that have already been discussed.[40] However, the fact that a certain book would get de-valued is hardly a valid reason for imposing a non-intuitive, non-metric geometry on K-12 students. Synthetic geometry should be set aside as an unsuccessful attempt to make "Euclid" theologically correct. Though teaching geometry in the traditional Indian way with a rope would involve a serious epistemic shift away from present-day formal mathematics, it is practical, free from artificial theological encumbrances, and is very easy for children to understand. Thus, the "Pythagorean" "theorem" can be established in one step instead of 47 steps. Any philosophical or theological problems with this could well be discussed at the appropriate advanced level, instead of forcing children to grapple with the consequences of obscure theological concerns.

There would be similar radical changes also in the way one teaches numbers, algorithms, and calculus. For example, although the computer is ubiquitous, the way students are taught about calculations on a computers is roughly as follows. First, students are taught that numbers obey certain "laws" (note the theological overtones). Then, at an advanced level (provided they specialize in mathematics), they are taught the basis of those laws along with number systems such as the real number system. Only then are they positioned to understand the rounding conventions used in oating point arithmetic, and the resulting "errors" as studied in numerical analysis. (Thus, most students, including many who specialize in mathematics, never learn about the actual way in which calculations are performed on a computer.[41]) Instead of this long-drawn route, one could simply explain the technique of calculation with rounding, as done by Brahmagupta, for example, so that it would be very easy for even a K-12 student to grasp the process. The point here is not that one should copy what Brahmagupta did, but that one should proceed on practical rather than theological concerns.

In particular, it may be worth re-examining whether one might want to teach as entirely separate subjects, from the outset, the two mathematical streams: practical mathematics and formal mathematics, with their distinct notions of number and proof. At the same time, one may want to re-examine the feasibility of teaching the consequences of formal mathematics at an elementary level where formalist philosophy itself cannot be taught. Such a re-examination would be particularly timely since the sudden growth of computer technology has again upset the earlier balance (in the West) between mathematics as proof and mathematics as calculation, and this calls for a fundamental review of what mathematics should be taught and how.

It is not being proposed that one should rush into the classroom right away with the suggestions that arise from this work. These suggestions are to be seen as constituting a future research program, which is a clear consequence of the revised historical understanding. A more precise set of recommendations would need to be evolved and documented in consultation with a variety of people including students, historians and philosophers of science, math educators, computer scientists, etc. The classroom trials of these new teaching recommendations should be taken up only after allowing a reasonable gap of at least a few years, to allow the documentation to circulate, to elicit reactions and suggestions from a wider circle of educators.

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2. National Council of Teachers of Mathematics, *Curriculum and Evaluation Standards for School Mathematics*, NCTM, Reston, VA, 1989.
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4. National Council of Teachers of Mathematics, *Principles and Standards for School Mathematics*, NCTM, Reston, VA, 2000.
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6. The constructivist approach, based on the theories of Jean Piaget, holds that all knowledge is constructed by individuals by assimilating new experiences into an existing base, or constructing new schemas. Radical constructivists believe that each person discovers truth anew. Hence, constructivists believe that instead of being handed down the rules of mathematics authoritatively by the teacher, students should be exposed to mathematical situations, and should discover the rules inductively from experience. For an empirical study in this direction, see e.g., Geoffrey B. Saxe, "The mathematics of child street vendors", *Child Development* **59** (1988) pp. 1415–25. Opponents of constructivism hold the view that students may fail to construct the right rules, and the wrong rules they construct may be difficult to deconstruct later on. R. Davis, C. Maher, and N. Noddings, *Constructivist Views on Teaching and Learning of Mathematics*, NCTM, Reston, VA, 1991. Constructivism must be distinguished from social constructivism, which raises even more fundamental questions of whether in mathematics, as in music, there is at all any "right" rule, independent of culture; see, e.g. Ubi D'Ambrosio, *Ethnomathematics*, and *Socio-Cultural Bases for Mathematics Education*, Unicamp, Campinas, 1985; Paul Ernest, *Social Constructivism as a Philosophy of Mathematics*, SUNY Series, Reform in Mathematics Education, 1998; and C. K. Raju, *Journal of Indian Council of Philosophical Research* **18** (2001) pp. 267–270.
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11. Elementary arithmetic algorithms from "Algorismus" being a Latinization of Al Khwarizmi, and his translation of the texts of Indian mathematicians like Brahmagupta, Bhāskara, and Mahavira; "algebra" from the Arabic *al jabr*;"sine" from the Latin *sinus*, being a translation of the Arabic *jaib*, being a misreading of *jībā* (from the Sanskrit *jīvā*, both being written without vowels as *jb*); "surd" from the Latin *surdus* = "deaf" from "bad ear", with "ear" = *karṇa* being a misreading of the Sanskrit "*karaṇī*" = diagonal (used for square root extraction). Some details of transmission may be found in Suzan Rose Benedict, *A Comparative Study of the Early Treatises Introducing into Europe the Hindu Art of Reckoning*, Ph.D. Thesis, University of Michigan, 29 April 1914, Rumford Press, Concord (no date given).
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13. C. K. Raju, "Computers, mathematics education, and the alternative epistemology of the calculus in the *Yuktibhāṣā*", *Philosophy East and West*, **51**(3) (2001) pp. 325–62.
14. Pope Nicholas V, bull Romanus Pontifex, 1453: "[W]e bestow suitable favors and special graces on those Catholic kings and princes... intrepid champions of the Christian faith... to invade, search out, capture, vanquish, and subdue all Saracens and pagans whatsoever, and other enemies of Christ wheresoever placed, and... to reduce their persons to perpetual slavery, and to apply and appropriate... possessions, and goods, and to convert them to... their use and profit." This was later followed by the bull Inter Cetera of Pope Alexander of 3 May 1493, giving the rights to conquest and subjugation of one part of the globe to Spain, and the other part to Portugal. F. G. Davenport, *European Treatises bearing on the History of the*

United States and its Dependencies to 1648, vol. 1, Carnegie Institute of Washington, Washington, DC, 1917, pp. 20–26, and pp. 61–68. *Hence,* according to US law, Indians lost their right to their ancestral land upon being "discovered" by the Christian Columbus. *Johnson and Graham s Lessee V. McIntosh* 21 U.S. (8 Wheat) 543, 5 L.Ed. 681 (1823)

15. E.g. Steven T. Newcomb, *Pagans in the Promised Land: Religion, Law, and the American Indian,* Indigenous Law Institute, 1995.
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17. C. K. Raju, "Newton's secret", *The Eleven Pictures of Time,* Sage, New Delhi, 2003, chp. 4.
18. It is generally overlooked that Proclus justified the appeal to the empirical at the beginning of mathematics, as in the first few theorems of the *Elements,* since this was quite acceptable for his understanding of mathematics, as also for the understanding of mathematics in Islamic rational theology. However, in Christian rational theology, because mathematics was regarded as certain, or as incorporating necessary truth, and the empirical *had* to be regarded as contingent to permit God to create a world of his choice, therefore, the empirical had no place in mathematics.
19. Christoph Clavius, ca. 1575? "A Method of Promoting Mathematical Studies in the Schools of the Society", Document No. 34 in: E. C. Phillips, "The proposals of Father Christopher Clavius, SJ, for improving the teaching of mathematics", *Bull. Amer. Assoc. Jesuit Scientists* (Eastern Section), Vol. XVIII, May 1941, No. 4, 203–206. Also, Christophori Clavii Bambergensis e Societate Iesv, *Epitome ArithmeticaePractacae,* Rome, Dominici Basae, 1583, Tr. into Chinese by Matteo Ricci.
20. Jonathan Swift, *Gulliver s Travels, Part III, A Voyage to Laputa...,* Wordsworth Editions, 1992, p. 125, "His Majesty discovered not the least curiosity to enquire into the laws, government, history, religion, or manners of the countries where I had been; but confined his questions to the state of mathematicks, and received the account I gave him, with great contempt and indifference... ".
21. Karl Menninger, *Number Words and Number Symbols: A Cultural History of Numbers,* trans. Paul Broneer, MIT Press, Cambridge, Mass., p. 325, "Yet it would be wrong to see in the *apices* nothing more than a trivial innovation introduced by Gerbert. The truth is that he did adumbrate the use of the new numerals; he had heard marvelous things about the new computation which they made possible but which he, and perhaps also his informants, did not essentially understand."
22. To coincide with the publication of Gregor Reisch's *Margarita Philosophica* (Basel, 1517) which shows a smiling Boethius and a glum Pythagoras, the former representing the algorismus, and the latter the abacus.
23. Charles Dickens, *Speech to the Administrative Reform Association,* 18 June 1855, *Speeches of Charles Dickens,* ed. K. F. Fielding,Clarendon, Oxford, 1960, p. 206: "Ages ago a savage mode of keeping accounts on notched sticks was introduced into the Court of the Exchequer and the accounts were kept much as Robinson Crusoe kept his calendar on the desert island... it took until 1826 to get these sticks abolished. In 1834 it was found that there was a considerable accumulation of them; and the question arose, what was to be done with such worn-out, worm-eaten, rotten old bits of wood? The sticks were housed in Westminster, and it would naturally occur to any intelligent person that nothing could be easier than to allow them to be carried away for firewood by the miserable people who lived in that neighbourhood. However ...the order went out that they were to be privately and confidentially burned. It came to pass that they were burned in the stove in the House of Lords. The stove, over gorged with these preposterous sticks, set fire to the panelling; the panelling set fire to the House of Commons; the two houses were reduced to ashes; the architects were called in to build others; and we are now in the second million of the cost thereof." More details in Katherine Solender, *Dreadful Fire! The Burning of the Houses of Parliament,* Indiana University Press, 1984. This "Robinson Crusoe technology" of accounting was first introduced in Britain by Normans in the twelfth century, over six hundred years after Āryabhaṭa; see, e.g., J. M. Pullan, *The History of the Abacus,* Praeger Publishers, New York, 1968, p. 51.
24. Zero played a key role in the transition from abacus to algorismus: "One might say, in a nutshell, that zero overcame the abacus. But its victory, which started in the Middle Ages, took a long time", Menninger, cited above, p. 331. However, the role of śūnya in Brahmagupta etc. is far more sophisticated than a mere symbol used in algorithms to replace counters. Specifically, śūnya was used in a way similar to non-representables in modern-day oating-point computation. C. K. Raju, "The mathematical epistemology of śūnya", summary of interventions in the *Seminar on the Concept of Śūnya,* IGNCA, and INSA, New Delhi, 1997, in: *The Concept of Śūnya,* ed. A. K. Bag and S. R. Sarma, IGNCA, INSA, and Aryan Books International, New Delhi 2002, pp. 168–181.

25. These books were readily available in Cochin where the first Catholic mission was established in 1500 CE and worked together with the local Syrian Christians.
26. Matteo Ricci, *Goa*, **38** (I) ff 129r–130v; corrected and reproduced in *Documenta Indica*, XII, 472–477 (p 474). Also reproduced in Tacchi Venturi, *Matteo Ricci SI, Le Lettre Dalla Cina 1580–1610*, vol. 2, Macareta, 1613.
27. Many navigational theorists were concerned with precise trigonometric values. See C. K. Raju, *Philosophy East and West* cited above, for references to the works of Pedro Nunes, Simon Stevin, and Christoph Clavius on sine (secant) tables used to calculate Mercator's loxodromes. For the use of these trigonometric values in traditional navigation, see C. K. Raju, "Kamāl or rāpalagai", Paper presented at the *Xth Indo-Portuguese Conference on History*, INSA, New Delhi, 1998, in Proc.
28. C. K. Raju, "How and why the calculus was imported into Europe", paper presented at the *International Seminar on East-West Transitions*, Bangalore, Dec 2000. At http://www.IndianCalculus.info/Bangalore.pdf.
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30. The question remains partially open, for even the Schwartz theory of distributions is inadequate to settle the way the calculus is practically used in quantum field theory (renormalization problem), and Non-Standard analysis showed incidentally that infinities and infinitesimals have a formal existence in non-Archimedean fields larger than the reals. C. K. Raju, "Products and compositions with the Dirac delta function." *J. Phys. A: Math. Gen.* **15** (1982) pp. 381–96; "On the square of x^{-n}" *J. Phys. A: Math. Gen.* **16** (1983) pp. 3739–53; "Renormalisation, extended particles and non-locality." *Hadronic J. Suppl.* **1** (1985) pp. 352–70; "Distributional matter tensors in relativity", *Proc. MG5*, ed. D. Blair and M. J. Buckingham, (series ed. R. Ruffini), World Scientific, Singapore, 1989, pp. 421–23.
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32. C. K. Raju, *Time: Towards a Consistent Theory*, Kluwer Academic, Dordrecht, 1994; Fundamental Theories in Physics, Vol. 65. While the book covers the relevant aspects of the existence theory for retarded functional differential equations, it advocates the use of mixed-type functional differential equations, for which no such mathematical results of any consequence are available. For the difficulties with assessing the reliability of risk estimation, using numerical solutions of stochastic differential equations driven Lévy motion, see C. K. Raju, "Supercomputing in finance", *Pranjana*, **3** (2000) pp. 11–36. Since large sums of money (USD 30 billion in the above case) are involved, the precise epistemological value of a numerical solution sans proof becomes critical.
33. As defined in the currently dominant notion of mathematics.
34. C. K. Raju, "Computers, mathematics education, and the alternative epistemology of the calculus in the *Yuktibhāṣā*", *Philosophy East and West*, **51**(3), cited above.
35. Buddhist or Jain logic is not two-valued or even truth-functional. C. K. Raju, "Mathematics and culture", *History, Culture and Truth*, ed. Daya Krishna and K. S. Murthy, Kalki Prakash, New Delhi 1999, pp. 179–193. Reprinted in *Philosophy of Mathematics Education* **11** (1999), available at http://www.people.ex.ac.uk/PErnest/pome11/art18.htm C. K. Raju, "Some remarks on ontology and logic in Buddhism, Jainism and quantum mechanics". Invited talk at the conference on *Science et engagement ontologique*, Barbizon, October, 1999.
36. C. K. Raju, "Quantum mechanical time", *Time: Towards a Consistent Theory*, Kluwer Academic, Dordrecht, 1994, chp. 6b.
37. C. K. Raju, "Euclid" cited earlier; for this reason, although Euclid's book was long known in India, it remained confined to religious instruction, and was not translated into Sanskrit (from Persian) until the 18th c. See, C. K. Raju, "Interaction between India, China, and Central and West Asia in mathematics and astronomy", *Interaction between India, China, and Central and West Asia*, ed. A. Rahman, PHISPC, New Delhi, 2002.
38. D. Hilbert, *The Foundations of Geometry*, Open Court, La Salle, 1902.
39. School Mathematics Study Group, *Geometry*, Yale University Press, 1961.
40. "Equality" in the *Elements* was related to political equity by Neoplatonists and Arab rationalists. While Hilbert reinterpreted this equality as congruence, this reinterpretation does not hold good for "equality",

from *Elements* Prop. 1.35 onwards, which refers to equal areas. (Synthetic geometry does not define length, hence it is bit pointless to define area synthetically.) Consequently, the *Elements*, though known in India, remained part of sectarian education for centuries, until the book was finally translated from Persian to Sanskrit in the 18th c. See C. K. Raju, "Euclid", and "India, China and Central and West Asia" cited above, and C. K. Raju, *Philosophy East and West*, cited earlier.

41. And even a well-established formal mathematician slipped in stating that there was only one possible "accurate" way to do rounding.

Appendix

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APPENDIX A

Distributions, Renormalization, and Shocks

Difficulties with the continuum approach to the calculus and an example of how advanced formal mathematics needs empirical inputs

OVERVIEW

THE previous chapters put forward the view that mathematics is not universal, that the epistemology of mathematics has varied across cultures, and that these epistemological differences have had a key role to play in the historical development of mathematics. A key difference related to the role of the empirical in mathematics: the mathematics that enters into a physical theory, is it a tautology or is it an auxiliary physical theory? Separating mathematics from the claims of necessary truth practically sounds the death-knell of formal mathematics, although it can well continue as an aesthetic form like music, which varies across cultures.

Present-day formal mathematics is so massive a structure that a doubt might well arise: exactly how would empirical considerations play a role at its frontiers? To address this doubt we temporarily adopt formalism solely to demonstrate its self-limiting nature to formalists, in the manner of Śrīharṣa who used the tools of Nyāya to demonstrate the inadequacy of Nyāya in his *KhaṇḍanaKhaṇḍaKhādya* (= breaking the opponent's arguments into bits and devouring them). Thus, the aim is to use the techniques of formal mathematics to show to the formal mathematician the unsustainability of his own uncritical beliefs about formal mathematics.

To this end, we start by reconsidering the question about the foundation of the calculus *within* formal mathematics. This also involves an issue of substantial historical importance: since the days of Newton and Leibniz it has been uncritically taken for granted in the West that the continuum approach, and present-day formalisation, is somehow the "right" approach to the calculus. We have seen that this is not correct: applications usually require actual numerical calculations that are better done in other ways, usually using finite differences. We now bring out the difficulties created by the uncritical acceptance of the continuum approach even at the level of formal theory.

To start with, at the level of present-day elementary mathematical analysis, the continuum approach requires that the functions to be differentiated must themselves be continuous. Thus, there is the difficulty that a discontinuous function cannot be differentiated. This inability to differentiate discontinuous functions is put down to the intrinsic "nature of things", rather than to the limitations of the method of formalisation. However, because the equations of physics are formulated as differential equations, practical applications of the calculus have needed to differentiate discontinuous functions from the time of Riemann, whose little known work on discontinuities, related to shock waves, is almost exactly contemporaneous with Dedekind's work on the continuum. Proceeding on cultural presuppositions, Riemann fell into a mathematical error that was later corrected on practical grounds by Rankine and Hugoniot. This "practical" trend in mathematics was continued by Heaviside and Dirac. The pressure of practice eventually prevailed on mathematical authority, as it usually does, and the "disreputable" practice of differentiating discontinuous functions was later formalised and accorded sanction in various ways by various formal mathematicians such as Sobolev, Mikusinski, and Schwartz, although it is the Schwartz theory of distributions that is today regarded by authoritative mathematicians as being the most satisfactory formalisation. These later theories permit unrestricted differentiation of a discontinuous (in fact integrable) function. However, in the process, something else is lost: namely the ability to multiply two Schwartz distributions pointwise.

Numerous definitions of the product of distributions have been suggested by now. Thus, the problem is no longer one of supplying a definition—the problem today is one of surfeit rather than a paucity of definitions. That is, the more serious problem now is that of selecting one from among the large number of definitions that have been supplied. One would like that the selection is based on considerations more serious than an appeal to the social authority of this or that mathematician. This frontier area of contemporary formal mathematics very well brings out how formal mathematics quickly reaches a dead end. We demonstrate how further progress in this area is impossible without a reference to the empirical, and, in particular, to the areas of mathematical applications to physics where these products of Schwartz distributions especially arise. Two such key areas are the renormalization problem of quantum field theory and the problem of shocks in real uids. In particular, for shock waves, because of the failure of the associative law for the product of distributions, different (otherwise equivalent) forms of the same differential equation may lead to different conclusions, so that the *form* of the differential equations at a discontinuity, must be empirically determined—a failure to do this was Riemann's original error.

Curiously, the definition of the product of distributions used in the stock renormalization procedure of quantum field theory, leads to a bad physical theory if the same definition is applied to shock waves—giving an example of how present-day formal mathematics has actually varied with the physical theory to which it has been applied. This example should be contrasted with the pretentious claims of ideal truth attached to formal mathematics.

The other interesting consequence that seems to follow is that the appropriate product of Schwartz distributions necessarily involves, either implicitly or explicitly, an extension of the concept of number, accompanied by a failure of the associative law, at some level, as happens with oating point numbers. Thus, it would seem that, within formalism, an appropriate foundation for the calculus can only be provided in a setting which uses a number system larger than the real numbers. Present-day formal mathematics asserts that the "Archimedean property" *must* fail in any proper field extension of the reals. That is, this larger number system must admit infinities and infinitesimals, that are used in a way much like non-representables are used with a finite set of numbers in computing. Thus, the idealistic understanding of the calculus, using Dedekind's semi-formalisation of the real numbers, and its subsequent formalisation within set theory, did not resolve the key epistemological problem of *śūnya* or non-representability, but merely hid it by burying it under a massive epistemological superstructure.

Renormalization in Quantum Field Theory

Today, the Schwartz theory of distributions is considered to be the most satisfactory extension of the calculus. Although Schwartz thought it impossible to multiply distributions pointwise, without losing some key aspect of the theory, many definitions exist today. Instead of relying on mathematical authority, we propose to probe the empirical context in which the definitions are applied.

One such area of empirical application is quantum field theory. The propagators of quantum field theory (fundamental solutions of the field equations) are generalized functions or distributions. Pointwise products of these propagators enter into the S-matrix expansion of quantum field theory, which is a formal infinite series expansion, analogous to the "Taylor" series. All the verifiable consequences of quantum field theory rest on calculations which use this S-matrix expansion. By means of a formal Fourier transform which is ritualistically assumed by physicists to map (undefined) pointwise products to (undefined) convolutions, these propagator products in configuration space are presented as divergent convolution integrals in momentum space. (This way of using the Fourier transform is an excellent example of how physicists use mathematics as a ritual to advance truth claims.) Even prior to the development of the Schwartz theory, physicists had developed ways to extract a finite part from these divergent integrals. The much acclaimed agreement of quantum field theory with experiments (to the seventh decimal place) depends critically on the method of extracting finite parts—and obviously there could be several possible methods. The philosophical model of falsifiability of a theory supposes that the mathematics that enters into physics represents necessary truth or tautologous connections between hypothesis and conclusions. If, however, that is not the case, and this mathematics represents only a social agreement, or a complex social ritual, then the mathematics that enters into the theory is itself an auxiliary physical theory that should also be open to physical test. From the viewpoint of physics itself, the acceptance of mathematics as auxiliary physics may also help to overcome the limitation of present-day renormalization theory to a very restricted class of Lagrangians, which tends to exclude quantum gravity for example.

Classical Shock Waves and Relativistic Singularities

The classical understanding of the calculus required every differentiable function to be continuous. Accordingly, the classical (post-Newton) formulation of physics using differential equations has generated the myth that nature has divinely ordained physical quantities to vary continuously, except at "real" discontinuities such as Hawking–Penrose singularities, often interpreted as events of cosmic significance, on the grounds that physics fails there! Likewise, it has also long been believed that "real" discontinuities or shock waves cannot exist in the presence of dissipative phenomena like viscosity and thermal conduction, although every observed shock falsifies this belief. Accordingly, the classical Rankine–Hugoniot conditions apply only to the case of Euler equations corresponding to a "perfect uid". Of course, a Rankine–Hugoniot shock is only a *model* of a physical phenomenon, and considering that the real phenomenon of shock and blast waves occurs in real uids like air and water, there seems no *a priori* reason to exclude models of discontinuities or shocks in real uids. Mathematically, the real difficulty is that discontinuities in the presence of viscosity leads to the same problem of "products of distributions". Trying to use here the products used in quantum field theory may well result in complete nonsense. The questions thus are: (a) whether it would be appropriate to have separate definitions of the product for each separate application to physics? and, if not, then (b) on what principle should one proceed to select a single product of distributions for several applications?

If the empirical is essential to mathematics, then mathematics must be regarded as an auxiliary physical theory, and in that case, one must apply to it the criteria such as simplicity of hypothesis (usually called Occam's razor, in Western philosophical literature), and refutability, usually applied to physical theories. If one does that, then one would naturally prefer that definition of the product which allows the same product to be used for both quantum field theory and for shock waves in real uids, and leads to empirically acceptable results in both cases. At present there is only one such product—proposed by this author. This approach incidentally also opens a completely independent empirical way to probe the validity of the renormalization procedure used in quantum field theory.

As regards shock waves, the Rankine–Hugoniot conditions do not provide information on possible jumps $[\frac{v}{n}]$, $[\frac{T}{n}]$ in temperature and velocity *gradients* across the shock, and this information is needed to (provide full Cauchy data to) enable solution of the full Navier–Stokes equations behind the shock. The new junction conditions here obtained provide the requisite Cauchy data. For normal shocks, the new conditions indicate ("predict") departures from the Rankine–Hugoniot conditions proportional to the coefficients of viscosity and thermal conductivity , λ, ; but small departures from those conditions are consistent with large jumps $[\ v\ \ n]$, $[\ T\ \ n]$. Shock curvature has an effect, in addition to gradient effects, due to terms like $\mathrm{Tr}(K)\ [T]$ (where K is the extrinsic curvature tensor of the shock *hyper*surface, and $[T]$ is the jump in temperature across it).

However, one more condition is needed, and because of the failure of the associative law, one has to distinguish between different forms of the same differential equation: two forms that have equivalent smooth solutions may have inequivalent non-smooth solutions. Unlike the case of Lax's "conservation form", a post-facto rationalization, the only way here is to proceed empirically.

I
INTRODUCTION

One of the aims of this appendix is to examine whether the viewpoint developed in the previous chapters has any relevance to contemporary formal mathematics at an advanced level. As stated in the abstract, this appendix will adopt the techniques of formal mathematics to bring out the limitations of formal mathematics from within formal mathematics—i.e., to show that without reference to the empirical, formal mathematics is a self-limiting dead-end, even if we forget about all "external" considerations of history and philosophy that have been covered earlier.

Another key aim of the appendix is to examine the reality of the claim that there is only one natural way to formalise the calculus—using the continuum. That is, we aim to examine whether that claim merely represents a long-standing and uncritical social consensus in the West, which is ultimately unjustifiable like so many socially accepted things.

A third key aim of the appendix is to expose the reality of how formal mathematics has, *de facto*, varied with the physical theory under consideration, showing that it is, at best, an auxiliary physical theory, rather than something necessarily true.

To this end, this appendix addresses three questions from different fields that nevertheless need to be addressed in one place. A book dealing with the historical development of the calculus in relation to the philosophy of mathematics perhaps provides the most appropriate setting, though these considerations are substantially more technical than the preceding, and may be skipped by those who lack the background or the interest.

The first question concerns a foundational issue in mathematics, from a viewpoint purely internal to formal mathematics. The classical semi-formalisation of the calculus in the works of Dedekind, Cauchy et al., soon proved to be unsatisfactory because of its limited applicability. From the 1930's mathematicians like Sobolev started searching for alternative ways of doing the calculus. The Schwartz theory of distributions,[1] or the equivalent theory of generalized functions,[2] is today regarded as the most satisfactory extension of the calculus, though other theories like Mikusinski's continue to linger. In the Schwartz theory, the extension of the calculus is achieved at a certain cost: namely the classical function concept is reinterpreted, or rather surrendered, so that pointwise values and pointwise products of Schwartz distributions do not make sense. It is, of course, possible to extend the Schwartz theory and define a product of Schwartz distributions, and numerous such products have been proposed. *Q. Given several competing and inequivalent products of Schwartz distributions, on what basis should one select the "correct" one?*

This issue is closely related to the second question of propagator products in quantum field theory. The propagators of quantum field theory (fundamental solutions of the basic equations like the wave equation) are generalized functions or distributions. The verifiable consequences of this theory are derived using products of these propagators. By a *ritualistic* application of the classical calculus (Fourier transform) such propagator products are converted to divergent (undefined) integrals. That is, we suppose that

$$(f \cdot g) = f \quad g.$$

Here, f and g are possibly generalized functions, denotes the Fourier transform, and denotes convolution. For an appropriate class of functions f and g it is a theorem[3] that the Fourier transform carries pointwise products to convolutions. The ritual consists in applying this theorem in a situation where neither the pointwise product on the left nor the convolution on the right is meaningfully defined. For example,

$$(\ \cdot \ ) = \qquad = 1 \quad 1 = \qquad dx.$$

In quantum field theory, a finite part is then extracted from such divergent integrals through an elaborate process called renormalization. As we shall see, these ritualistic beliefs can be made "rigorous", i.e. they can be formalised or put in the framework of formal mathematics. However, the deeper question that we still need to consider is this. If the mathematics underlying physical theory corresponds not to necessary truth, but only to the choices of mathematical authority, or to a mere social convention, or a sanitization of a complex ritual, or introduces auxiliary physical hypothesis into the theory, what consequences does that have upon the refutability of the physical theory? Does the refutation of a physical theory refute only the physical hypothesis underlying the theory, or might it not also refute the underlying mathematics? If the latter, as seems to be the case, then would it not be more appropriate to base mathematics on empirical considerations rather than on social custom and mathematical authority? This question about the nature of mathematics underlying physics can also be considered from a more acute angle internal to the theory, when the theory itself is in an unfinished state (like most physical theories) and one needs to determine whether the non-renormalizability of quantum gravity, for example, represents a failure of the physical hypothesis of that theory or whether it represents the unsatisfactory mathematics used in the theory. *Q. Does the non-renormalizability of a theory represent a failure of the underlying physical hypotheses or only of mathematical technique?*

The third question relates to shock waves. In classical physics, uid ow is described by means of differential equations. This presupposes that the functions entering into the equations are differentiable. According to the classical calculus, prior to the Schwartz theory, a differentiable function is required to be continuous. This limitation of the mathematical model and technique has been elevated almost to the status of a divinely ordained natural law: the assumption is that physical quantities must vary continuously or smoothly. (Indeed, Stephen Hawking has gone so far as to characterize "singularities" at which this "natural law" of continuity breaks down as the "beginning" of the cosmos—a situation at which all "natural laws" themselves break down—a claim of great importance for religious politics.[4])

In practice, of course, an explosion or blast gives rise to a shock wave which is better modelled as a surface of discontinuity across which physical quantities like pressure, temperature, etc. do change abruptly or discontinuously. According to present-day physical theory, quantities such as the pressure, etc. of a uid are local statistical averages, and these local averages cannot be meaningfully determined when large changes take place across the thickness of a shock wave which is typically of the order of a few molecular mean free paths. The quantities, however, are meaningful on either side of a shock wave. The quantities are, therefore, regarded as being discontinuous *at* the shock. That is, though the shock has a measurable thickness, this thickness is neglected, treated as non-representable, and the shock is treated *as if* it were infinitesimally thin or had zero thickness.

Since it was thought that discontinuous functions could not be differentiated, in place of the usual differential equations, these discontinuous changes are governed by a set of junction conditions (finite difference conditions) called the Rankine–Hugoniot equations, which enable one to calculate the conditions behind the shock, when conditions are known in front of the shock. (One can similarly work out junction conditions at Hawking–Penrose singularities[5]—this is too technical a topic to take up here, and we will stick to ordinary shock waves in non-relativistic uids.) According to classical wisdom, the surface of discontinuity represented by a shock can actually arise only in an idealized model of a perfect uid which obeys the Euler equations (a simplified form of the full Navier–Stokes equations). In real uids, it is believed, dissipative effects due to viscosity and thermal conductivity would smoothen the shock into a thin layer across which there are large though continuous changes.

This piece of classical wisdom, based on the limitations of the continuum approach to the calculus, overlooks the manifest: shock waves are observed in air, for example, which is a real uid. Further, from a practical viewpoint, it is not feasible to compute a solution of the full Navier–Stokes equations across a shock regarded as a thin layer across which there are large but smooth changes. It is not even clear whether it is theoretically meaningful to speak of the Navier–Stokes equations within a shock, since the observed thickness of a shock may be of the order of only a few mean-free paths, at which level one can expect a breakdown of the continuum approximation used to derive the differential equations of uid ow. Under the circumstances, the only possible way out seems to be to revert to the statistical mechanics underlying the continuum approximation. This possibility is blocked in the case of relativistic shocks, etc. where there *is* no statistical mechanics underlying the continuum approach (because general relativity lacks an appropriate description of "particles" of matter). *Q. In real uids like air and water if one is interested in studying heat ow or viscous effects behind the shock, can this be done directly from the equations of uid ow?*

(Something more than the Rankine–Hugoniot equations is obviously needed, since the Rankine–Hugoniot equations do not provide adequate (Cauchy) data to be able to solve the full Navier–Stokes equations behind the shock. The relation of this question to products of distributions is as follows: this author pointed out long ago[6] that, with suitable conventions about products of distributions, the Rankine–Hugoniot equations can be regarded as identical to the Euler equations *at* the shock. The question then is: what are the conditions corresponding to the Navier–Stokes equations?)

The Calculus and Generalized Functions

The three questions above are all posed in a way that seems to be "internal" to the respective fields: functional analysis, quantum field theory, uid mechanics. However, each of the above questions relates, in one way or another, to the desirable nature of the calculus. The calculus is believed to have acquired a "canonical form" after the semi-formalisation of real numbers by Dedekind. This "standard" form of the calculus has gained widespread acceptance, and today this is the form on which the mathematician is brought up, for this is the "rigorous form" of the calculus that is taught in standard courses on real analysis. This form is satisfying in many ways from within the formalist viewpoint. However, there is the difficulty that a discontinuous function cannot be differentiated, although the need to differentiate discontinuous functions arose in many applications. Riemann first encountered this around 1870, and, at the turn of the 20th century, Oliver Heaviside was bold enough to use such discontinuous "jump functions", in engineering applications, contrary to the prevailing opinions of socially important mathematicians.

As usually happens, social opinion eventually bent before practical advantage. Another great innovator, P. A. M. Dirac, saw the worth of the idea as an engineering student. He applied this engineering technique to physics, using especially the derivative of the Heaviside function, nowadays known as the Dirac delta function. This is a "function" which is infinite in an infinitesimal neighbourhood of zero, and is infinitesimal elsewhere. Since (formal) real numbers have the Archimedean property which does not permit infinities and infinitesimals, the delta function led to many a raised eyebrow, for (if one chooses not to abandon real numbers) the delta function challenged the very concept of function which some historians have claimed as central to the calculus.

The mathematician Sobolev started to put together a theory of such generalized functions. Eventually, the theory was developed further and came to be accepted under the name of the Schwartz theory of distributions, though other theories of generalized functions, such as that of Mikusinski have many elegant characteristics.

The Schwartz theory reinterprets a function as a linear functional, to permit unrestricted differentiation under the integral sign, using the formula for integration by parts

$$\int_a^b f' g = f(b)g(b) - f(a)g(a) - \int_a^b f g' .$$

To tidy up this formula, we assume that the "test function" g vanishes at the limits of integration, for example, by allowing the limits of integration to be $-\infty$, and ∞, and letting the function g vanish outside a compact (bounded) set:

$$\int_a^b f' g = - \int_a^b f g' .$$

Thus, within the integral sign the derivative can always be transferred to the test function g. When f is not differentiable, the right-hand side can be regarded as the definition of the left-hand side. To ensure that this formula always makes sense, we assume that g is infinitely differentiable. The vector space of all infinitely differentiable functions which vanish outside a compact set constitutes one class of test functions, denoted by D (when equipped with an appropriate topology). Its dual space, i.e., the space of all continuous linear functionals on D, is called the space of distributions, denoted by D'. If $f \in D'$, and $g \in D$, we write $\langle f, g \rangle = \int fg$. With this notation, every $f \in D'$ has a derivative $f' \in D'$, defined by $\langle f', g \rangle = -\langle f, g' \rangle$, for all $g \in D$. Every ordinary function corresponds to a distribution; when it has a *continuous* derivative, the two notions of derivative coincide.

With this understanding we arrive at a situation where every integrable function is infinitely differentiable—the integral in question being the Lebesgue integral, which generalizes the Riemann integral. The class of Lebesgue integrable functions obviously includes functions that are discontinuous in a variety of ways, including those discontinuous functions that are Riemann integrable.

However, this ease of unrestricted differentiation is achieved at a certain cost. The cost is that we can no longer speak of the value of a function *at* a point. This is convenient in a way, for we can speak of the delta functional, without being obliged to say what the value of the delta function is *at* zero. The inconvenience is that since we cannot speak of the value of the function at a point, we cannot also speak of the pointwise product of two functions.

The Product of Distributions

According to Taub,[7] "Fortunately, the product of such distributions [as arise] is quite tractable". Thus, for example, consider the Heaviside function θ, which is defined by

$$\theta(x) = \begin{array}{ccc} 1 & : & x > 0 \\ 0 & : & x < 0. \end{array}$$

(The exact value at 0 is unimportant, because a single point has Lebesgue measure zero, and even if the value here is infinity, the earlier-mentioned convention $0 \cdot \infty = 0$ ensures that it will contribute nothing to the integral.) Taub's argument is this: from

$$\theta^2 \;=\; \theta,$$

we can easily apply the "Leibniz" rule (for the derivative of a product of two functions) to conclude that

$$2\theta \cdot \theta \;=\; \theta \;,$$

with primes denoting differentiation. Since $\theta \;=\;$, this can be rewritten as

$$2\theta \cdot \quad = \quad ,$$

which immediately tells us that

$$\theta \cdot \quad = \frac{1}{2} \cdot \quad . \tag{A.1}$$

This is simple enough except that we also have

$$\theta^3 \;=\; \theta,$$

from which, by the same logic, it would follow that

$$3\theta^2\theta \;=\; \theta \;.$$

Since

$$\theta^2 \;=\; \theta,$$

this corresponds to

$$\theta \cdot \quad = \frac{1}{3} \cdot \quad . \tag{A.2}$$

Comparing (A.1) and (A.2) leads to the interesting conclusion that $\frac{1}{2} = \frac{1}{3}$! Something is obviously wrong here. Similarly,

$$x^{-1}(x \quad) \;=\; 0 \;=\; 1 \cdot \quad \;=\; (x^{-1}x) \quad .$$

Schwartz Impossibility Theorem

In fact, Schwartz[8] generalized this to a theorem, nowadays called the Schwartz impossibility theorem, which suggested the impossibility of defining products of distributions, under certain natural-looking conditions. The product of a smooth function h C and f D is easily defined in the natural way by $hf, g = f, hg$, for all g D. The right-hand side makes sense, since if h C and g D, then hg D. This product has been called the Schwartz product. The Schwartz impossibility theorem asserts that there does not exist an *associative* differential algebra A D in which the product agrees with the Schwartz product. That is, it is impossible to define the product of distributions so that the associative law holds and the product agrees with the Schwartz product defined for a smooth function and a distribution. More generally, the above examples show that either the associative law or the "Leibniz" rule (for the derivative of a product) must fail for any product of distributions.

Earlier De nitions of the Product

Nevertheless, by now dozens of definitions have been proposed.[9] So, the problem now is this: which amongst these many definitions is the "correct" definition? This raises a fundamental question. Each such definition extends the Schwartz theory (which itself extends the calculus from the viewpoint of mathematical analysis). But, *between competing mathematical theories, which theory should one choose?*

One possibility is to argue that definitions are arbitrary. This possibility suits the formal mathematician, for in practice this means the value to be attached to a definition is proportionate to the social authority of the mathematician proposing the definition. So, in practice, accepting the arbitrariness of definitions translates into a bald reliance on social authority.

This may be fine from the point of view of the pure mathematician. But, just as the pure mathematician has been blind to the arbitrariness in the choice of logic underlying proof, so also those who *use* mathematics for practical applications (physicists, engineers, and so forth) have been blind to the arbitrariness underlying mathematical definitions. A definition selected merely on social authority amounts to an auxiliary hypothesis—a social belief introduced into physical theory. Thus, the refutation of a physical theory might well mean only a refutation of one of a number of arbitrary definitions in the mathematics that that theory used. This would be an extremely inconvenient situation for the refutation of a theory would not provide any serious guidance about the alternative physical theories to be explored.

The question of which definition to use can partly be settled by another approach which implicitly appeals to a principle of simplicity: if one definition of the product subsumes another, one would prefer the more general definition. To this end, let us consider various classes of definitions of the product. The implicit appeal to generality is just a disguised form of the appeal to brevity or simplicity of hypothesis, used to decide between competing physical theories.

Fourier transform method: For $f, g \quad D$, define $f \cdot g = (f \quad g)$, where the superscripts $\wedge$ and $\vee$ denote respectively the Fourier transform and its inverse, and denotes convolution, provided the convolution on the right-hand side is meaningful. This method of localisation and the Fourier transform has been used by Hörmander,[10] Reed and Simon,[11] Vladimirov,[12] and Ambrose.[13]

Products defined by this method have been shown[14][15] to be a particular case of products defined by the following method.

Sequential method: For $f, g \quad D$, define $f \cdot g = D - \lim_n \quad (f \quad _n) \cdot (g \quad _n)$, where $_n, \ _n$ are appropriate delta-convergent sequences.

This method has been used by Hirata and Ogata,[16] Mikusinski,[17] Fisher,[18] and Kaminski.[19]

In view of the suggestion by Parker[20] to use Hörmander's product in a context similar to ours, it is well to clarify that most sequential products do *not* include the product $\theta \cdot$ which is required for our purposes. Also, no sequential product can hope to define ^{2}. This entity may arise from the product $\theta \cdot$, if the "Leibniz rule" holds, and its need is demonstrated later on.

Colombeau s definition: Colombeau[21] defined an associative differential algebra $G \quad D$. The "Leibniz rule" also holds, but there is no contradiction with the Schwartz impossibility theorem because the product does *not* agree with the pointwise product of C functions. The Colombeau product of any $f, \ g \quad D$ always exists in G, but admits an "associated" distribution iff[22] the sequential model product exists. However, the product is not coherent with "association", so that $\theta \cdot$ need not have a unique associated distribution. The Colombeau product is, thus, closely analogous (Todorov[23]) to the simplistic pointwise product of *-smooth functions in the non-standard space $E = \quad C$ (Stroyan and Luxembourg[24]) with "association" being like the selection of a standard part. Because the above product does not cohere with selection of a standard part, exactly as the Colombeau product does not cohere with "association", Stroyan and Luxembourg leave it as an exercise to show why this simplistic definition is obviously unsuitable.

Hahn–Banach method: A similar ambiguity arises in the products defined by the Hahn–Banach methods ("subtraction of infinities") used in quantum field theory (Bogoliubov and Parasiuk,[25] Bremmermann and Durand,[26] de Jager,[27] and Manoukian[28]). Many physicists and philosophers of science are under the wrong impression that the procedure of "subtraction of infinities" used in quantum field theory is "not rigorous". As a matter of fact, the procedure can be made perfectly rigorous (i.e., formalised) using the Hahn–Banach theorem. The Hahn–Banach continuous extension theorem[29] asserts that, for a locally convex topological vector space V, a continuous linear functional defined on a subspace S of V

can be continuously extended to the whole space V. This enables us to "subtract infinities" as follows. First consider the product $\cdot$. By means of a formal Fourier transform,

$$(\ \cdot\ ) = \qquad = 1 \quad 1 = \qquad dx.$$

To make sense of the divergent integral on the right, we notice that differentiating the integrand makes the integral converge (to zero). That is, we are in a position to define 1 1 (= 0). We can now apply the Hahn–Banach theorem to extend this to the whole space.

That is, the Hahn–Banach product is, in the first instance, similar to the product defined by the Fourier transform method: for f, g D , we define $f \cdot g$ by the Fourier transform method, as the inverse Fourier transform of f g. But when the convolution on the right leads to a divergent integral, we "subtract infinities" as follows. Define f g as the Hahn–Banach extension of $(f \quad g)$ f g, where the multi-index α is so chosen that the convolution on the right-hand side is meaningful. Thus, f g is defined on the subspace of Fourier transforms of test functions $D(\alpha) =$ D, $=$. Alternatively, $f \cdot g$ is defined on the subspace of test functions $D(\alpha) =$ D, $= x$. We notice that this subspace is also the null space of and its derivatives to order α. Since the topology of D is locally convex, the Hahn–Banach theorem guarantees the existence of *some* extension.

The problem, of course, is that the Hahn–Banach extension is not unique. However, any two extensions to the whole space must agree on the above subspace; hence, their difference must vanish on the above subspace, which is also the null space of and its derivatives to order α. Hence,[30] any two extensions will differ by a linear combination of and its derivatives to order α. (Thus, this definition leads to $\theta \cdot \ = A$, $\cdot \ = B$, etc., where A, B are arbitrary constants.)

Thus, the real problem with the definitions used in quantum field theory is not the absence of rigour, but the presence of arbitrariness. This arbitrariness is present at two levels: first in the choice of the definition, and then in the choice of the arbitrary constants that arise in the definition. One layer of arbitrariness may be removed in quantum field theory by appealing to invariance under various gauge groups. But this method does not work and creates obvious problems when dealing with distribution solutions of the Navier–Stokes equations. Very similar difficulties in fixing an "associated distribution" arise in Colombeau's theory if one attempts to apply it to shocks in viscous and thermally conducting uids.

Nonstandard product: According to the definition advanced by this author (in the days when he still believed in formal mathematics),[31] the symmetric product of f, g D is defined by

$$f \cdot g = \frac{1}{2} \quad (f \qquad)g + (g \qquad)f \ , \tag{A.3}$$

where denotes the Nonstandard extension to D , and is a positive infinite integer. The product defined in this manner always exists in D , is unique, and coincides with the sequential product when the latter exists. The "Leibniz rule" holds, but the associative law fails. (The failure of the associative law is discussed below in more detail.) We have,

$$\theta \cdot \quad = \quad \frac{1}{2} \;, \qquad \text{(A.4)}$$

$$\theta \cdot \quad = \quad \frac{1}{2} \quad + \quad {}^{2}, \qquad \text{(A.5)}$$

$$ {}^{2} \quad = \quad (0) \;, \qquad \text{(A.6)}$$

where (0) is infinite. For applications to distribution solutions, a kind of "linear independence" (Raju[32] and section below) ensures that the final results are standard. In view of the Nonstandard transfer principle[33] it follows that the final results could well have been derived without resorting to Nonstandard techniques. Nevertheless, the use of Nonstandard techniques makes the final results far more transparent. Irrespective of the availability of a formal justification, this approach is remarkably similar to that of discarding non-representables.

There are various definitions in addition to those mentioned above.[34] Which definition should one choose? Comparison theorems can only partially settle the question. In fact, formal mathematics simply cannot answer this question unaided, except by the exercise of the social authority of the mathematician. This exercise of social authority may (and often does) assume some very peculiar forms as when a reviewer (N. Ortner) implicitly put forward the absurd *ad hoc* proposal to use the ease of proving some theorem as a criterion for selecting between different definitions of the product of distributions!

The problem, however, cannot be settled by such frivolous reasoning, because mathematics is routinely applied to practical and empirical problems of physics, and the choice of mathematics is re ected in the resulting physical theory. The social prejudices that creep into mathematics re ect also upon the physical theory which relies on that mathematics. (And if we don't believe in the myth that the present society is a utopia, then the method of deciding mathematical truth by social authority would also mean that various social evils can get re ected in both mathematics and physics.) If the "rigorous mathematical proof" of the existence of singularities merely means that singularities are socially acceptable, what does that say about the physics of singularities?[35] In particular, in mathematics as in physics it is always preferable to appeal to the empirical rather than to social authority. This is especially true if the mathematics in question is to be usable in physics, i.e., it is better to regard mathematics as an auxiliary physical theory which itself needs to be empirically verified/refuted.

To understand this better, let us consider two example areas of physical applications of products of distributions: quantum field theory and shocks. (We reiterate that, although some of the terminology and arguments used are those of formal mathematics, the underlying philosophy has changed radically.)

Quantum Field Theory

Quantum field theory usually begins with a set of field equations: the Klein–Gordon equation and the Dirac equation. The propagators of quantum field theory are fundamental solutions of these equations. These fundamental solutions are readily obtained by using the formal properties of the Fourier transform (namely that it carries differentiation with respect to x into multiplication by p and vice versa; in physics the Fourier transform also maps configuration space, whose variables are denoted by x, into momentum space whose variables are denoted by p). Using the conventions of Bogoliubov and Shirkov,[36] for the Klein–Gordon equation, a fundamental solution $D(x)$ satisfies

$$(\Box - m^2)D(x) \;=\; - \;\;(x), \tag{A.7}$$

where $\Box$ denotes the D'Alembertian ($\Box = - \;\;{}_k\, g^{kk} \frac{\;^2}{x^k \; x^k}$, and the metric tensor g^{mn} has signature -2). Using the above-mentioned formal property of the Fourier transform, we see that we should have

$$(p^2 - m^2)D(p) \;=\; -1, \tag{A.8}$$

where, as is the custom in physics, the Fourier transform of $D(x)$ is denoted by $D(p)$ by confusing a change of function with a change of the argument. Specifically,

$$D(p) \;=\; -\frac{1}{p^2 - m^2}, \tag{A.9}$$

where the function on the right is to be formally understood in the sense of the Cauchy principal value.

The retarded and advanced propagators are now obtained by specifying that the support of these propagators should be respectively the forward and backward null cone. Formally, one multiplies $D(x)$ by the Heaviside function $\theta(x^0)$ to obtain the retarded propagator $D^{\mathrm{ret}}(x)$. Here x^0 denotes the time coordinate. The causal propagator $D^c(x)$ is often used, and is described by its Fourier transform as $D^c(p) = \frac{1}{m^2 - p^2 - i0}$. The photon propagator[37] $D_0^c(x\;)$ (fundamental solution of the wave equation) and electron (spinor) propagator $S^c(x)$ (fundamental solution of the Dirac equation) can both be obtained from $D^c(x)$ as follows:

$$D_0^c(x) \;=\; D^c(x)\;{}_{m} = 0, \tag{A.10}$$

$$S^c(x) \;=\; (i \;\; + m)D^c(x), \tag{A.11}$$

$$\;=\; \sum_{=0}^{3} \gamma \qquad , \tag{A.12}$$

γ being the Dirac matrices, and $\frac{}{x}$ denoting differentiation with respect to the coordinate x . Though the expressions for these propagators are very simple in momentum space (p-space), we do not have the same simplicity in configuration space (x-space). Indeed, configuration-space representations of these propagators are hard to find in the physics literature.

The approximate expressions for the configuration space representation of these propagators near the null cone can be obtained[38] using the approximate expression for the singular part of $D^c(x)$ near the null cone:[39]

$$D^c(x) = \frac{1}{2\pi} \_(\lambda) - \frac{m^2}{8\pi}L, \quad (A.13)$$

where

$$\_(\lambda) = \frac{1}{2} \quad (\lambda) - \frac{i}{\pi}.\frac{1}{\lambda} \quad , \quad (A.14)$$

$$L = \frac{1}{2} \quad \theta(\lambda) - 2\frac{i}{\pi}\log\frac{1}{2}m \quad \lambda^{\frac{1}{2}} \quad , \quad (A.15)$$

$$\lambda = (x^0)^2 - (x^1)^2 - (x^2)^2 - (x^3)^2, \quad (A.16)$$

and the logarithm and its derivative $\frac{1}{\ }$ are both to be understood in the sense of principal value.

If we naively assume the chain rule

$$f(\lambda) = f\ (\lambda) \quad \lambda, \quad (A.17)$$

where f is a 1-dimensional distribution (i.e., a distribution on R) and f its derivative, then we have

$$f(\lambda) = 2f\ (\lambda)\, x, \quad (A.18)$$

where x is defined exactly like using the Dirac matrices.

Observing that $L = \ \_$, we obtain

$$D_0^c(x) = \frac{1}{2\pi} \_ \quad (A.19)$$

$$S^c(x) = \frac{i}{\pi} \_(\lambda)x - \frac{im^2}{4\pi} \_(\lambda)x + \frac{m}{2\pi} \_(\lambda) - \frac{m^3}{8\pi}L. \quad (A.20)$$

Propagators and Field Equations

Actually, we do not have to worry too much about the exact relation of the propagators to the field equations. The propagators are the substance of the theory; the equations are mere ritual. If we change the propagators we change the theory; we can derive all the empirical consequences of the theory from the propagators without once knowing what the equations of the theory are—though such a procedure might shock some physicists.

The derivation of the empirical consequences of the theory from the propagators requires perturbation theory or the S-matrix expansion.

S-Matrix Expansion

Consider the second-order terms of the S-matrix expansion corresponding to the electron self-energy. In physical terms this may be visualized as a process in which the electron gives out a photon and recaptures it. This process may be geometrically visualized using the Feynman diagram for electron self-energy in Fig. A.1. Likewise, the photon self-energy diagram may be visualized in physical terms as a process in which the photon creates an electron–positron pair; the two then annihilate to give back the photon. This process may be geometrically visualized using the other Feynman diagram shown in Fig. A.1.

Figure A.1: **The Feynman diagrams for electron and photon self-energy.** The diagram at left shows an electron which interacts with itself via a photon. The diagram at right shows a photon which spontaneously produces an electron–anti-electron pair which then recombines to give back that photon. These diagrams are primarily meant to be able to write down the terms in the S-matrix expansion, but some physicists tend to attribute a physical reality to them.

No reality is necessarily to be attached to either the physical process or the Feynman diagram. Both can well be regarded merely as convenient aids to the calculation of the S-matrix elements, involving the propagator products $S^c(x) \cdot D_0^c(x)$ and $S^c(x) \cdot S^c(x)$. The corresponding terms are ritualistically written as follows.

Electron self energy:

$$-i : \overline{\Psi}(x)\underline{\sum}(x-y)\Psi(y) :, \tag{A.21}$$

where the colons denote the normal product, and

$$\underline{\sum}(x) \;=\; -ie^2 \sum_n g^{nn}\gamma^n S^c(x)\gamma^n \, D_0^c(x). \tag{A.22}$$

Here, g^{mn}, as before, is the metric tensor (with signature -2) and e is the electron charge.

Photon self energy:

Similarly, the photon self-energy corresponds to the S-matrix term

$$-i\sum_{m,n} : A_m(x)\Pi^{mn}(x-y)A_n(y) : \tag{A.23}$$

where

$$\Pi^{mn}(x) \;=\; -ie^2 \mathrm{Tr}\, \gamma^m S^c(x)\gamma^n S^c(-x) \tag{A.24}$$

For our purposes, it is only necessary to consider the product $S^c(x)S^c(x)$, since the "singular" part of S^c is actually a function of λ, and λ is an even function of x.

If the product is defined in such a way as to permit the use of the "Leibniz rule" (as in the symmetric product defined by this author[40]), then it is easy to see that the key necessary condition for the propagator products to be finite is the finiteness of

$$\_^{2} = \frac{1}{4}\ ^{2} - \frac{1}{\pi^2}\ \frac{1}{x}\ ^{2} - \frac{i}{\pi}\ \frac{1}{x}\cdot\ + \ \cdot\frac{1}{x}\ . \qquad \text{(A.25)}$$

In the above expression, $\left(\frac{1}{x}\right)^2$ should not be confused with $\frac{1}{x^2}$, from which it may be infinitely different, for the former expression represents the product of the distribution $\frac{1}{x}$ with itself, while the latter is (up to sign) the derivative of $\frac{1}{x}$. Indeed, Mikusinski first attempted to prove the following identity involving the square of the delta function:

$$^{2} - \frac{1}{\pi^2}\ \frac{1}{x}\ ^{2} = -\frac{1}{\pi^2}\frac{1}{x^2}. \qquad \text{(A.26)}$$

With this author's definition of the product, both distributions on the left are infinite, but the difference is finite.[41]

The actual way in which the renormalization process is carried out in quantum field theory is a bit different. The conventional approach (subtraction procedure) corresponds to using the Hahn–Banach method, already explained. That is, by means of a (ritualistic) Fourier transform, one transfers the propagator products in configuration space to divergent convolution integrals in momentum space. One then differentiates under the integral sign (corresponding to differentiating one of the convolvants) until the integral is convergent. The Hahn–Banach theorem now provides an extension to the whole space. The arbitrary constants that arise in this process are fixed by an appeal to some sort of symmetry or invariance requirements. The renormalizable theories are exactly those for which it is possible to eliminate the arbitrary constants in this way. (More recently, in the context of quantum gravity, it has been argued that one can get by without the need for renormalization; we will not consider this argument.)

New Renormalization Prescription

The above result, however, enables us to arrive at finite results for every theory as follows. (Although finiteness has been formally proven only for theories with a polynomial Lagrangian, there is no reason why this should not hold also for non-polynomial Lagrangians, since the basic source of arbitrariness has been eliminated.) Referring back to the definition of λ ($\lambda = 0$ is the null cone), we see that although the one-dimensional products in (A.25) and (A.26) are finite, the propagators are actually functions of λ. If we define $f(\lambda) \cdot g(\lambda) = fg(\lambda)$, the divergences are recovered.

The key point is that the *product* fg is finite, and the divergences arise in defining the *composition* $fg(\lambda)$. To give a close analogy, the function is well defined, but the definition of $(x^2 - y^2)$ leads to a divergent integral (see Jones[42] or Gel'fand and Shilov[43]).

Geometrically, this is easy to understand. Suppose we have a distribution in one variable, say (x), and we wish to define it so that it is concentrated on a surface, say $(x, y) = 0$. The natural thing to do is to introduce local coordinates (Gaussian normal coordinates, say) so that the equation of the surface is, locally, $n = 0$. Having done this, we define $(\ ) = (n)$.

This procedure fails if the surface is not regular. In the case of the surface $x^2 - y^2 = 0$, the failure is at the point (0, 0). For the null cone, $\lambda = 0$, the failure is likewise at the vertex.

The proposed solution[44] accordingly is to replace the null cone by a regular surface. This can be easily done by introducing a single parameter l and replacing the null cone by a hyperboloid, with separation l. Unlike simple-minded techniques like a cutoff (which destroys Lorentz covariance) or a smooth regularization being left on (which destroys positivity of energy) this prescription is compatible with various requirements such as Lorentz covariance and the positive energy condition. All products of propagators are now finite, so there is no need to appeal to symmetry principles, etc.

Of course, changing the null cone to a hyperboloid changes the support of the propagators, hence the propagators themselves. Since the propagators are fundamental solutions of the field equations, this procedure changes the basic field equations of quantum field theory. That should not be a matter of much consequence, since the content of the theory, as it currently stands, is in its propagators. The value of l would have to be determined empirically.

Choosing a Product

The wider question, with which we started, is this. Can the calculus be formalised without any reference to the empirical world? This led us to the question of the product of distributions. Just what makes a particular definition of the propagator product appropriate? As argued earlier, this can only be decided on empirical grounds, and by using principles usually associated with physical theories, like the principle of simplicity (Occam's razor) or Poincaré's criterion of convenience. The "simplicity" or "convenience" in this case lies in being able to apply the same definition of the product also to the classical case of shocks. This is "simpler" or more convenient than the alternative which has multiple hypotheses, corresponding to having a separate definition of the product for each physical theory in which the mathematics is used.

The "subtraction of infinities" fundamental to quantum field theory never seemed quite satisfactory. One may now articulate this dissatisfaction more precisely as follows. As we have seen, the conventional renormalization process does *not* lack rigour (the procedure can be perfectly well formalised); rather the dissatisfaction relates to the *arbitrariness* inherent in the procedure. If we try to benchmark the procedure by applying it outside the limited context for which it was invented, for the study of shock waves, for example, the conventional procedure fails the test. From the point of view of particle physicists, who may well prefer to stick to their mathematical rituals, an underlying mathematical unity, like Occam's razor, cannot be compelled, so particle physicists may well disregard the proposed test. But it is well to realize that the mathematics they use is no necessary truth, but something that falls between an auxiliary physical theory and mere social convention. On the other hand, a new arena of application not only provides unexpected insight into the strengths and weaknesses of an old *ad hoc* procedure, but it opens the possibility of improving upon the existing renormalization procedure. Putting the mathematical procedure to empirical test, in a context other than the immediate context for which it was invented, will also help to allay the suspicion that the procedure was invented solely to enable back-calculation of known results!

Unlike the Hahn–Banach products of quantum field theory, the above definition of products applies to both quantum field theory and to shocks in an Eulerian ow, and can be used to derive the classical Rankine–Hugoniot equations in that case.[45] The question now is this: can this theory be used to say something new? In the case of quantum field theory, as already pointed out, the new renormalization prescription allows one to try out arbitrary Lagrangians. In the case of shock waves, we now show how the Rankine–Hugoniot conditions can be extended to shocks in real uids with viscosity and thermal conductivity.

II
SHOCKS IN REAL FLUIDS

Viscosity and thermal conductivity are *not* usually associated with the ow behind a shock, but there are many contexts where they are needed.

Viscosity of the Earth s Outer Core

The viscosity of the earth's metallic liquid outer core has been called one of the most important and least well-determined of all geophysical parameters—estimates of the viscosity span 13 orders of magnitude! A systematic increase in both shear and bulk viscosity with increasing compression is predicted by the standard Enskog model of a hard sphere uid. When applied to water at 15 GPa, this method leads to very large effective viscosities.

At present, the direct experimental measurement of the viscosity of liquid metals at outer core pressures is not possible in the laboratory. Shock-wave methods have been devised to overcome this problem.[46] To theoretically validate these methods, *ad hoc* inclusion of boundary-layer effects is clearly inadequate at such high viscosities, and a systematic theory of shocks with viscosity is needed—one needs to be able to solve the full Navier–Stokes equations behind a shock. Shock loading methods used in the laboratory study of the viscosity of liquid metals presuppose a proper theory to handle viscous effects behind a shock.

Aerodynamics

In high-speed aerodynamics, such as the ight of the NASA space shuttle, shocks are usually studied using the Rankine–Hugoniot conditions and the Euler equations. This excludes an accurate study of, say, heat conduction effects in the high-speed ow behind the shock. While existing computer architectures have insufficient computing power for a "grand challenge" problem like accurate simulation of high-speed Navier–Stokes ow, TFlop/s machines are now well above the horizon, and one may use them with appropriate modifications of existing parallel numerical codes as envisaged by Raju et al.[47]

Shock Re ections

Recent experiments on shock re ection suggest that ignoring viscosity and thermal conductivity behind the shock may result in larger inaccuracies than has previously been supposed.

Consider a compressive wedge mounted in a steady or unsteady supersonic ow. Since the ow is supersonic it cannot smoothly negotiate the obstacle that it suddenly encounters, and an attached or detached shock wave is formed depending upon the angle of the wedge. This shock wave may itself meet another obstacle further down (such as the wall of the wind tunnel or the wings of the aircraft or the strap-on boosters of a launch vehicle) and is re ected in different ways. Substantial increases in e.g. temperature may take place on account of multiple re ections.

The case of regular re ection is fairly well described by the inviscid two-shock theory with one incident and one re ected shock, using the oblique shock (Rankine–Hugoniot) conditions.[48] But there are photographs of regular re ection configurations in which the re ected shock wave is seen to be curved along its entire length, and right up to the point of re ection. There is a discrepancy between experimental observation and theoretical prediction of the two-shock theory for the angle between the incident and the re ected shock. Shirouzu and Glass[49] suggested that viscous effects are responsible for the discrepancy. Ben-Dor[50] compared experimental observations with the angles between the various discontinuities at the triple point predicted by the inviscid three-shock theory. The discrepancies (as large as 5°) were certainly larger than experimental uncertainties,

The Dif culty of Incomplete Cauchy Data

Though technologically feasible and needed for some important applications, as indicated above, certain theoretical and mathematical difficulties are encountered in trying to solve the full Navier–Stokes equations behind a shock.

To solve the full Navier–Stokes equations behind the shock, the Rankine–Hugoniot conditions clearly cannot be used. For this purpose, let us once again neglect the thickness of

the shock and model it as infinitesimally thin, so that as the shock evolves, its history is modelled by a hypersurface . Assume that the state ahead of the shock is known. To calculate the state behind the shock it is necessary to know the Cauchy data behind the hypersurface
.

The full Navier–Stokes equations involve the second derivatives of velocity and temperature. Consequently, the Cauchy data on must include the initial values of velocity and temperature on *and* the values of normal derivatives of velocity and temperature on . The usual Rankine–Hugoniot conditions, which may be used for the Euler equations, do not involve the derivatives of velocity and temperature at all, and consequently provide no information on the values of any derivatives behind the shock. Since the use of the Rankine–Hugoniot conditions results, in this case, in an improperly posed initial value problem, one needs a new set of junction conditions to be able to solve the full Navier–Stokes equations behind . Brie y, the Rankine–Hugoniot conditions do not provide information on possible discontinuous changes in velocity and temperature *gradients* across the shock; this information is needed to solve the full Navier–Stokes equations behind the shock.

It is possible, of course, to make up for insufficient Cauchy data through various *ad hoc* assumptions. For example, for very weak shocks one may suppose that is characteristic. Or one may suppose that the ow behind the shock is steady, and that changes in temperature are inconsequential, thereby arbitrarily equating to zero any initial velocity and temperature gradient behind the shock. One may suppose that the gradients of velocity and temperature are continuous across the shock, even though the velocity and temperature themselves have a discontinuity. Such *ad hoc* solutions are clearly unsatisfactory, and may lead to completely erroneous conclusions.

Irrelevance of Shock Structure

To get over the difficulty of inadequate Cauchy data, should one rather drop the simplifying assumption of an infinitesimally thin shock? In favour of this it could be pointed out that shocks are actually observed to have a small thickness (of the order of a few mean free paths). Viscosity also has a "smoothening effect"—viscous diffusion tends to smoothen out sharp velocity gradients—and it is generally believed that in the presence of viscosity the shock broadens from an infinitesimally thin surface of discontinuity to a finite region across which there are large but smooth changes. This belief is supported by the success of numerical schemes which use artificial viscosity.

Several issues need to be clarified here. Firstly, a viscous profile does not necessarily exist[51] for all shocks satisfying the Lax entropy conditions,[52] and the Riemann problem does not admit a unique solution in the class of shocks admitting viscous profiles. Moreover, the profile does not remain smooth if thermal conductivity[53] is taken into account.

Secondly, in the traditional continuum theory of the viscous shock profile, based on the works of Becker,[54] Gilbarg and Paolucci,[55] Gel'fand,[56] weak, steady solutions of the hyperbolic system of conservation laws

$$u_t + f(u)_x = 0 \tag{A.27}$$

are obtained as the limits (in D , as $\rightarrow 0$) of smooth solutions of the associated family of parabolic equations

$$u_t + f(u)_x = {}^2 u_x x. \tag{A.28}$$

However, the solution of the associated parabolic equation (A.28), for a given , only provides an *interpolation* between the boundary values assumed to be given by the usual Rankine–Hugoniot conditions—the viscous profile is derived by assuming that the inviscid approximation applies on either side of the shock. Since the "boundary conditions" (i.e., the junction conditions across the shock) must be prescribed *first* to obtain the viscous profile, it would be incorrect to use the viscous profile to draw any conclusions about the conditions behind the shock in the viscous case. The theory of the viscous profiles is, in fact, quite irrelevant to the question at hand. For similar reasons, existing kinetic theories of shock structure are not of much help, apart from being inconvenient to apply (impossible to apply in the case of relativistic shocks).

Thirdly, in numerical computations, precision suffers in the presence of large gradients in a thin region, so that non-numerical methods to get across the shock are desirable.[57]

Fourthly, the observed thickness of shock waves is not especially relevant—the continuum approximation is simply more *convenient*. The observed thickness *is* small enough to be consistently treated as infinitesimal in the continuum approximation. Moreover, the fact is that shocks *are* observed in real uids like air and water which *do* have some non-zero viscosity and thermal conductivity. Using the Euler equations and the Rankine–Hugoniot conditions to model shocks in air or water amounts to neglecting viscosity to set up a convenient model for calculations across shocks. Taking thermal conductivity and viscosity into account enables an alternative model with greater precision, without losing the convenience of the continuum approximation.

Using the above product of distributions it turns out that while solutions with a simple discontinuity are not possible unless viscosity and thermal conductivity are both zero, the Navier–Stokes equations *do* admit solutions with singular support on a regular hypersurface . The physical interpretation is that viscous diffusion and thermal conductivity would smoothen out any jump (simple discontinuity) in velocity and temperature on the two sides of a shock *except* in the presence of a (dynamically created) surface layer, which must therefore accompany shocks in real uids.

The Form of the Equations

Thus, it seems desirable to develop an appropriate modification of the Rankine–Hugoniot conditions, for use with the full Navier–Stokes equations. The new conditions should enable the study of solutions of the full Navier–Stokes equations behind a shock.

A new problem that arises is this: what form of the equations of uid ow should be used? To obtain the right conditions, in the Eulerian case, it is necessary to fix on a specific quasi-linear *form* of the equations (called the "conservation form" by P. D. Lax). While this approach works for the Euler equations, it fails for the full Navier–Stokes case.

The Schwartz impossibility conclusion explains the need to fix on a specific form of the differential equation (such as the conservation form): equivalent forms of an equation have the same smooth solution, but for a discontinuous solution, the implicit assumption of both the "Leibniz rule" and the associative law, used to establish the "equivalence", is no longer valid. Therefore, for smooth ows of a perfect uid, one may use equivalently the equations of conservation of mass, momentum, and energy or mass, momentum, and entropy, whereas in the case of a shock the two systems of equations are known[58] to be inequivalent after Riemann, and only the first system of PDEs is physically meaningful.

III
DERIVATION OF THE NEW JUNCTION CONDITIONS

The Form of the Equations

We use the Navier–Stokes equations in the following form:

$$\frac{\quad}{t} + \frac{u_i}{x^i} = 0, \tag{A.29}$$

$$\frac{u_i}{t} + \frac{(\ u_i u_j)}{x^j} = -\frac{p}{x^i} + \frac{\ _{ij}}{x^j}, \tag{A.30}$$

$$\frac{\quad}{t}\ \frac{1}{2}\ u_k u_k + e = -\frac{\quad}{x^j}\ u_j\ \frac{1}{2}\ u_k u_k + e + p - u_i\ _{ij} - \frac{T}{x_j}\ . \tag{A.31}$$

Here $e =$ is the energy density per unit volume, being the usual energy density per unit mass, $_{ij}$ is the viscous stress tensor, $_{ij} = (u_{i,j} + u_{j,i}) + \lambda u_{i,i}$, with $\lambda = -\frac{2}{3}$, , being the usual coefficients of shear and bulk viscosity respectively, a comma in the subscript denotes differentiation as usual, and the summation convention applies to repeated suffixes.

Because of the Schwartz impossibility theorem, the correct form of the equations, for distribution solutions, can only be decided by recourse to empirical considerations. Relativistic covariance is one of the reasons for choosing the above form of the equations: the above form is appropriate for generalization to the relativistic case. The above form is also quasi-linear, and reduces to the usual "conservation form" if viscosity and thermal conductivity are set to zero.

Notation and Coordinates

Let denote the shock hypersurface (assumed to be regular). divides spacetime into two half-spaces V^+ and V^-, where the superscript + denotes the undisturbed region ahead of the shock. As an aid to derive the junction conditions, we introduce Gaussian normal coordinates x based on , so that the equation of is $x^1 = n = 0$. Here n denotes the normal to and x^0 denotes the timelike coordinate.

Let $^+$, $^-$ denote the characteristic functions of V^+ and V^-. In terms of coordinates,

$$^+ = \theta(n) = \begin{matrix} 0 & n < 0, \\ 1 & n > 0, \end{matrix} \tag{A.32}$$

$$^- = 1 - \ ^+, \tag{A.33}$$

$$^+_{\ ,} = \ (n) \ ^1 \ , \tag{A.34}$$

where $_{,} = $ —, (n) denotes the Dirac delta concentrated on (for an invariant definition see, e.g., Gel'fand and Shilov, vol. 1), and ^{1} is the Kronecker delta. Here, (A.34) is a compact way of stating the result called Green's theorem (Gauss theorem, Stokes theorem, fundamental theorem of calculus).

From a formal perspective, what we are doing is to seek distributional solutions of the Navier–Stokes equations in the form

$$f = f^+ \ ^+ + f^- \ ^- + f \ , \tag{A.35}$$

where f^+, f^- are smooth functions, and $f = \lim_{\text{supp}} \ f, \ $, for a test function , with $= 1$ in a neighbourhood of . We use the usual notation

$$[f] = \lim_{p} \left(f^+(p) - f^-(p)\right), \tag{A.36}$$

$$f = \lim_{p} \frac{1}{2}\left(f^+(p) + f^-(p)\right), \tag{A.37}$$

to denote the jump and the mean values of f across .

Failure of the Associative Law

The failure of the associative law is manifested through

$$(fg) = f\ g - \frac{1}{4}[f][g], \tag{A.38}$$

for functions f, g of the form (A.35) with $f = g = 0$. Thus, $(fg) = f\ g$ unless one of f, g is further continuous across . The precise form of the association of factors must be decided semi-empirically. That is, if we forcibly impose the associative law upon the number system underlying the calculus, the associative law fails somewhere else, and the only remedy for it is to refer back to the empirical world!

Reduction to Standard Form

The junction conditions are derived by assuming that the given partial differential equation holds *on* in the sense of distributions, using the above product of distributions. The infinities disappear from the final result because an equation of the type

$$f^{+} \quad ^{+} + g^{-} \quad ^{-} + a \quad (n) + b \quad (n) + c \quad ^{2}(n) \; = \; 0, \tag{A.39}$$

can hold in the sense of (Nonstandard) distributions iff[59]

$$f^{+} \; = \; g^{-} \; = \; a \; = \; b \; = \; c \; = \; 0. \tag{A.40}$$

(Here, denotes the derivative of the Dirac which is well defined under the assumption that is regular.) Thus, the original partial differential equation splits into partial differential equations for V , while on it reduces to a set of algebraic or ordinary differential equations giving the junction conditions.

Junction conditions for shocks in arbitrary continua may be derived in an invariant manner using the above algorithm. For the general relativistic case of arbitrarily curved shocks, these were first reported in Raju,[60] and the following may be regarded as the non-relativistic counterpart of those conditions.

Junction Conditions for Plane Shocks

Assuming that , p, e in (A.29)–(A.31) are of the form (A.35), applying the above algorithm and assuming for simplicity that , λ, are constants, we obtain the following conditions for the case of a plane (straight) shock

$$\tilde{\mathrm{p}} \;=\; (2\eta + \lambda)\,[v], \tag{A.41}$$

$$\tilde{\mathrm{e}} \;=\; \frac{\kappa}{v|}\,[T], \tag{A.42}$$

$$[\quad v] \;=\; 0, \tag{A.43}$$

$$p + \quad v^{2} \;=\; (2\eta + \lambda)[\frac{\partial v}{\partial n}], \tag{A.44}$$

$$(\quad v)\;[w + \frac{1}{2}v^{2}] \;=\; (2\eta + \lambda)[v\frac{\partial v}{\partial n}] + \kappa[\frac{\partial T}{\partial n}] - \frac{\partial \tilde{e}}{\partial t}. \tag{A.45}$$

Here v is the velocity normal (and relative) to the shock, and $w \;=\; e + p$ denotes the enthalpy. The bold-faced terms emphasize the difference from the Rankine–Hugoniot conditions: all these terms vanish, and we obtain back those conditions if we put $= \lambda =$ $= 0$. If the shock is steady, the time-dependent term in the last equation vanishes and the conditions reduce to a set of algebraic equations. In the above, , , λ were assumed constant: to allow for their discontinuous variation, they should be moved inside the square brackets.

The terms p, e may be regarded as purely mathematical constructs to enable better modelling and computation with shocks. But a physical interpretation is possible. The term p signifies the presence of interfacial tension on the shock. Viscous diffusion is unable to equalise velocities on both sides of the shock because of the presence of this tension. The presence of tension also indicates that the evolution of a compressive shock dissipates energy. Similarly, the term e signifies the presence of an interfacial energy, which prevents thermal conduction from equalising temperatures on the two sides of the shock. Both terms are purely dynamic in origin. Visualized in terms of a smooth approximation, one would say that the pressure dips at some point inside the shock while energy density per unit volume peaks inside the shock. (Of course, as Poincaré stated long ago, an infinity of physical explanations may be possible for the same mathematics.)

Junction Conditions for Curved Shocks

The junction conditions may be evaluated quite similarly for the case when is curved, with extrinsic curvature tensor (second fundamental form) K. The conditions in full relativistic generality were derived by Raju.[61] The non-relativistic limit was worked out subsequently by Shukla.[62] The first three of the above conditions remain unchanged, while the last two conditions change to

$$
\begin{aligned}
[p + \ v^2] &= (2 \ + \lambda)[\frac{v}{n}] + \{ \ \eta \mathbf{Tr}\,(\boldsymbol{K}) \\
&\quad + (\mathbf{2}\boldsymbol{\eta} + \boldsymbol{\lambda})\frac{\boldsymbol{\partial} \ln \boldsymbol{g}^{1/2}}{\boldsymbol{\partial n}} \quad [v],
\end{aligned} \tag{A.46}
$$

$$
\begin{aligned}
(\ v)\,[w + 1 \ 2v^2] &= (2 \ + \lambda)[v\frac{v}{n}] + \ [\frac{T}{n}] - \frac{e}{t} + \{\boldsymbol{\kappa}[\boldsymbol{T}] \\
&\quad + \boldsymbol{\eta} \cdot \frac{\mathbf{1}}{\mathbf{2}}[\boldsymbol{v}^2] \quad \mathbf{Tr}\,(\boldsymbol{K}),
\end{aligned} \tag{A.47}
$$

where $g = \det(g_{ij})$, g_{ij} being the first fundamental form of . The changes from (A.44) and (A.45) are indicated by boldface, and , , λ are again assumed constant. Unlike the case of a normal shock, it is clear that curvature effects are present even if changes in temperature and pressure gradients are negligible across the shock, and the term $[T]\,\mathrm{Tr}(K)$ seems numerically the largest. However, a clearer understanding of the significance of the various terms must await detailed simulations and testing.

IV
THE FIRST LAW OF THERMODYNAMICS

The Two Additional Conditions

Apart from altering the Rankine–Hugoniot conditions, we have obtained *two* new conditions. However, there are now *four* new variables: e, p, $\frac{v}{n}$, $\frac{T}{n}$. Therefore, two more conditions are required to complete the specification of Cauchy data on , since an arbitrary equation of state may not be used on —one expects that the nature of the surface layer is determined by uid properties and shock kinematics. Both these conditions may be obtained directly from the first law of thermodynamics as follows.

Consider the first law in the form

$$de = wd + TdS. \qquad \text{(A.48)}$$

The form (A.48) of the first law does not change if the thermodynamic variables appearing in it are regarded not as functions of other "independent" thermodynamic variables but as functions of the coordinates. Therefore, following Misner et al.,[63] the "d" in (A.48) may be interpreted as an exterior derivative. Taking the inner product of both sides with the unit normal to , we obtain, in Gaussian normal coordinates,

$$\frac{e}{n} = w\frac{}{n} + T\frac{S}{n}. \qquad \text{(A.49)}$$

To interpret (A.49) for distribution solutions we need the product $\theta \cdot$ defined by

$$\begin{aligned} \theta \cdot \quad &= (\theta \cdot \ ) - \theta \cdot \\ &= \frac{1}{2} \ - \ ^{2}. \end{aligned} \qquad \text{(A.50)}$$

Applying the above procedure, we obtain three equations:

$$[e] = w[\ ] + T[S], \qquad \text{(A.51)}$$

$$e = (\ T)\ S, \qquad \text{(A.52)}$$

$$w[\ ] = [\ T]S. \qquad \text{(A.53)}$$

Eliminating S, and using $w = e + \frac{p}{}$, we obtain the required two conditions.

The Form of the First Law

The difficulty is that the associative law fails, and one does not know the "correct" form of the first law of thermodynamics to begin with. Thus, if we had started with the first law in the form

$$dw = Vdp + TdS, \qquad \text{(A.54)}$$

we would have obtained, in place of (A.51), the equation

$$[w] \;=\; V\,[p] \;+\; T\,[S]. \tag{A.55}$$

How does one decide between these two conditions? One way is to go back to the case of an ideal uid for which $= \lambda = \;= 0$, so that the Rankine–Hugoniot conditions together with the conditions (A.51) or (A.55) form an overdetermined system. The requirement of consistency may be used to choose between the two forms. Thus, the Hugoniot relation

$$[w] \;=\; V\,[p] \tag{A.56}$$

is a consequence of the conditions (A.43)–(A.45), for the special case of an ideal uid. It is clear that (A.56) is consistent with (A.55) only if $[S] = 0$, so that (A.55) and *a posteriori* (A.54) must be rejected.

Entropy Change and the Impossibility of Rarefaction Shocks

On the other hand, if (A.51) is interpreted as

$$[e] \;=\; w\;[\;\;] + \;\; T\;[S], \tag{A.57}$$

then it follows (using some algebraic manipulations) that consistency with the Hugoniot relation (A.56) holds if

$$[S] \;=\; \frac{1}{4T\;V\;\;j^4}\;[p]^3. \tag{A.58}$$

where $j \;=\; (\;v)$ is the mass ux across the shock. This is an exact expression for the entropy change across a shock, and shows the impossibility of rarefaction shocks. In the case of an *ideal gas*, in the weak shock limit, one may suppose $v_+ \to a,\; a^2 \;=\; \gamma p\;\;$, a being the sound speed. We may also suppose that $T\;\; \to\; T_+,\; V\;\; \to\; V_+$ to obtain

$$[S] \;=\; \frac{3}{\gamma+1}\cdot\frac{1}{12T_+}\frac{(\gamma+1)V_+}{\gamma^2 p_+^2}\;[p]^3. \tag{A.59}$$

This differs from the usual approximate expression[64] for entropy change only by the additional factor of $\frac{3}{\;+1}$, where γ is the ratio of specific heats. The interpretation (A.59) of (A.51), however, is definitely valid only in the weak shock limit.

Incidentally, to obtain $[S] > 0$, we can do away with the restriction to weak shocks, by writing (A.51) in the form

$$[w] \;=\; \frac{1}{\;}[p] \;+\; T\;h_1[S], \tag{A.60}$$

where h_1 is an "uncertainty" factor given by

$$h_1 \;=\; 1, \tag{A.61}$$

$$\text{or}\quad h_1 \;=\; 1 - \frac{1}{4}\frac{[\;\;][T]}{T}. \tag{A.62}$$

Since and T are positive quantities (except in anti-matter), it now follows that $\frac{[\,]}{2} = \frac{+ - -}{+ + -} < 1$, since $+ - - < \max(+, -) < + + -$. A similar inequality holds for T, so that $1 - h_1 < 1$, so that $h_1 > 0$. If we start with the interpretation (A.60) in place of (A.57) the only change is that the left-hand side of (A.59) gets multiplied by h_1, so that we obtain the impossibility of rarefaction shocks without any of the usual assumptions[65] such as the positivity of $\frac{2V}{p^2}$.

From the variety of forms of the equations that we have needed to use, it should, however, be clear that ultimately the decision as to the form of the equation, or the precise association of factors, can only be taken empirically.

V
CONCLUSIONS

The idea that the calculus had finally found a satisfactory formulation in the works of Dedekind, Cauchy, and Weierstrass is true in only a very limited sense, even within formal mathematics. This formulation of the calculus is not only no good for computing (as we have already seen) but it is also inadequate for a variety of applications, particularly quantum field theory and the classical theory of continua.

The further development of the calculus to include contemporary applications requires a fundamental shift in mathematical philosophy, with an explicit acknowledgment of the role of the empirical as better than relying upon the social authority of the mathematician.

The re-introduction of the empirical into mathematics makes mathematics an auxiliary physical theory, so one selects between different possible mathematical theories by applying criteria similar to criteria used for physical theory. In particular, one may validly apply criteria such as simplicity, or Occam's razor, or the less-problematic principle of convenience suggested by Poincaré. This means that one must choose as valid that formulation of the calculus with the widest possible physical applicability.

This also enables us to extend the formulation of the calculus by singling out a product of distributions. As a means to empirically test this mathematics, we explained how this leads to a new prescription for renormalization, which makes any quantum field theory finite (although a formal proof of finiteness is available only for theories with an arbitrary polynomial Lagrangian). The new renormalization prescription involves only a single new parameter that can be determined empirically.

This also leads to two new sets of junction conditions: (a) (A.41)–(A.45) and (A.51)–(A.53) for plane shocks, and (b) (A.41)–(A.43), (A.46)–(A.47) and (A.51)–(A.53) for curved shocks in *real* uids. These conditions provide Cauchy data for the full Navier–Stokes equations behind the shock, when conditions ahead of the shock are known.

For normal shocks, the new conditions indicate ("predict") departures from the Rankine–Hugoniot conditions proportional to , λ, . For small values of these coefficients, small departures from the Rankine–Hugoniot conditions are consistent with large jumps in velocity and temperature *gradients* across the shock.

The conditions for curved shocks explicitly involve the extrinsic curvature of the shock hypersurface, and the terms $[T]\mathrm{Tr}(K)$ and $[v]\mathrm{Tr}(K)$ introduce departures from the Rankine–Hugoniot conditions, even if gradient effects are ignored behind the shock.

The underlying mathematics is, therefore, empirically refutable.

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