

5

Concluding remarks

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- O.Gingerich (p.276)



Mary Helen Kennedy and O.Gingerich (first row from left),
A.Moitra and Minati Ray Chaudhuri (second row from left).

Concluding remarks:
ORIENTAL ASTRONOMY AND THE NATURE OF SCIENCE

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I have come to this colloquium not as a specialist in Oriental Astronomy, but as a historian of science passionately interested in the fundamental nature of science and its attempts to describe physical reality. A basic question in the philosophy of science concerns the nature of scientific laws: are scientific laws intrinsic to the universe, simply waiting to be discovered? Or are they in part creative inventions of the human mind, our particular way of looking at nature?

Perhaps the best approach to this open problem lies in comparative science. Do independent scientific systems inevitably lead to the same description of nature? As a first step in such an investigation, we must decide if a concept is an independent invention or a transmission from another culture or an earlier epoch. Only if the concepts are truly independent can we begin to compare genuine alternative descriptions, and in fact we cannot be sure that differing systems are even possible until we establish such independence. Olaf Pedersen addressed this question with respect to Greek astronomy, pointing out that if we have identical numerical parameters in two different situations, we can be fairly sure that transmission is involved, whereas it is difficult to be sure if an idea is independent or dependent on a transmission.

Consider the case of the epicyclet device used by Copernicus to eliminate the Ptolemaic equant. An identical construction was adopted by Ibn ash-Shatir nearly two centuries earlier, but no avenue of transmission has been traced. Did Copernicus invent this device independently? I think an independent invention is entirely possible, but transmission is by no means excluded. Even if Copernicus discovered the construction independently, he certainly inherited the same philosophical framework that motivated such an invention, namely, the reconciliation of the mathematical models of Ptolemy with the physical form of the celestial spheres. The example clearly demonstrates the ambiguity of deciding the independence or dependence of scientific ideas.

In any event, there is probably no better place to study comparative science than in Oriental Astronomy. Nevertheless, the field poses formidable problems, foremost being the variety of languages

involved. It does no good simply to know Chinese and no other language. We need Chinese scholars who know Sanskrit, Arabic scholars who read Chinese, Indian scholars who command Greek, and so on. I was impressed and envious to watch Dr. Teboul translating questions into Chinese and the answers into English, or Prof. Kennedy likewise speaking in Persian. Despite the significant progress demonstrated in the colloquium, we must recognize that these studies are only in their infancy, primarily because of the language barriers.

In particular, we need a far deeper examination of the transmission problems. E.S. Kennedy graphically described some of the paths between Greek, Indian and Islamic astronomy, but there is a scholarly *terra incognita* with respect to China. Just before this colloquium I had an opportunity to visit a thousand-year-old mosque in the ancient Chinese capital, Xian. At the end of the Silk Road, Xian clearly provided a gateway for the infusion of Islamic astronomy into China long before the Jesuit astronomer-missionaries of the sixteenth and seventeenth centuries arrived. Yet we know almost nothing of what happened then. Even earlier transmissions occurred, as indicated by the materials now being examined in the Buddhist temples of Japan.

One of the high points of this colloquium came in the exchange following Prof. Nakayama's paper. He had demonstrated the existence, in one of these Buddhist texts, of a method of parabolic interpolation unknown in Indian or Islamic texts, and he pointed to central Asia as a likely source. In the discussion, Dr. Mercier noted that a similar interpolation technique had already been reported from Tibet. It is such connections, individually perhaps minor, that will eventually build up our fabric of understanding concerning the dependence or independence of these scientific cultures, and give us a better possibility to answer whether scientific pictures can be independently formulated and directed in convergent paths.

A recurrent theme of the colloquium has involved the question of observations. One of the distinctive characteristics of Chinese astronomy, pointed out in Prof. Xi's contribution, is the long series of records of unusual celestial phenomena. This stands in striking contrast to India, the Islamic world, and the Latin West. In fact, it is a source of embarrassment that the West can scarcely point to any indigenous observations of the splendid supernova of 1054, the progenitor of the Crab Nebula. However, I think we may well be asking something of the Western chronicles that may be too much to expect. China had a long tradition of writing, printing, and the preparation of official histories with a high regard for omens. The West had neither the advantage of relative stability nor the disadvantage of a stultifying traditional bureaucracy, so the absence of supernova records in the West is an ambiguous deficiency.

There is, furthermore, a qualitative difference between the phenomenological omen observations so abundantly recorded in the Oriental annals, and the positional observations required for the

advance of planetary theories. What appears puzzling and even embarrassing to modern astronomers is the great paucity of such measurements in Asia as well as in Europe. This very fact must be telling us something about the different perceptions of our astronomical forebears and their notions of how to extract the secrets of nature, and this may also be revealing something about the uniqueness of modern science.

Several papers have dealt with instruments, which are related to the aforementioned problem of the existence or precision of positional observations. Again, we must be exceedingly careful not to be anachronistic when we discuss instruments of past ages. There were, for example, in Mughal India four generations of Lahore astrolabists, whose instrument designs stayed virtually constant for a century and a half. Now if these astrolabes were actually used for observations, we would expect the less-than-optimal style of the alidade to have been markedly improved over that period. The fact that this did not happen suggests that these instruments were designed for a different purpose than actually measuring celestial altitudes. Quite likely they were in part symbolic, providing, as the late Derek DeSolla Price argued, the opportunity to hold the machinery of the universe in the palm of one's hand. (In a sense today's wrist watches furnish the same connections with the universe at large.)

Not only astrolabes, but even the Jai Singh observatories must be at least partially, if not primarily, seen as cultural and symbolic monuments, and not truly working instruments. One could even remark today on how few telescopes on our college campuses are really observational research tools, and what a large fraction are there for the inspirational opportunity to get in closer touch with the universe. Nevertheless, it seems that at least one of the stone instruments in Delhi was successfully used to determine the latitude and the obliquity of the ecliptic, the use of these parameters in tables giving evidence of observing in the absence of specific extant observations.

In this regard the research reported by Dr. Mercier strikes me as particularly challenging. Using techniques pioneered by R. Billard, he has shown for a variety of Indian tables that the coordinate errors for the planetary positions come to a well-defined minimum at a specific time characteristic of each table. The sheer volume of material forces us to agree that the parameters were generally optimized at the time the tables were made. But is it necessarily true that the parameters were controlled by specific but no longer existing observations? David Pingree has argued that the parameters could have been established from existing Greek tables, and the Indian attempts to force the planets into arbitrary conjunctions at considerably earlier dates would have thrown out the agreement with the actual planetary positions except around the time of the tables. In the absence of any observations, or even records of how they might have been achieved, we must be very cautious in accepting the speculative conclusion that the parameters of the Indian tables were observationally established by the table makers.

Such dating techniques have to be used with great care. For example, Samuel Goldstein has attempted to date the Ptolemaic lunar parameters in a similar fashion, and his interpretation, that the parameters predate Ptolemy by several centuries and were therefore already established from observations of unknown earlier astronomers, are difficult to reconcile with reasonable assumptions about the precision of observational techniques or (equally importantly) the accuracy of whatever coordinate reference frame was used at such an epoch. In a more extreme case, two papers presented here dated the nakshatra (the Indian lunar mansions) to 2400 B.C. on the grounds that only then did the Pleiades rise directly in the east. Can we really demand such precision from the Vedic texts? Can we rationally accept a set of Indian lunar mansions dating that far back in the absence of any corroborative evidence from archaeology? If so, then what about the work of Bradley Schaefer (not reported here) on the Chinese hsiu, where a computer analysis based on various assumptions of symmetry and positions leads to a date before 3000 B.C. for the Chinese system of lunar mansions? I believe it is still an open question about where the lunar mansions originated — surely a fascinating question regarding transmission or possible independent invention — but I am sceptical that such dating procedures can by themselves lead us to firm ground. In contrast, I find the results of the Billard-Mercier method entirely reasonable, although I would like to see their findings placed into a convincing context with respect to observational realities.

By probing the character of Oriental Astronomy, the questions of transmission, and the roles of instruments and observations, our colloquium has become a long-overdue pioneering venture in a field that promises high returns for scholarly investment. We must look now to the future prospects. What we do not need is an intensive competition for who did something first, or for who had it right; what we need is to learn about the intellectual framework for these inventions and to learn why discoveries took place when they did. We need to examine closely the roles of innovation versus tradition in the ongoing development of astronomical theory and techniques. To do this we must cultivate skills in languages, we must analyze the texts, and we must search diligently for the evidences of cross cultural transmissions. On such a solid basis we can perhaps begin to illuminate some of the fascinating and fundamental problems about the nature of science itself.