

have an assurance of every datum proving compatible with every other one, as is shown by the rejection of one Ptolemaic and two or three Arabian eclipses. But this proportion of rejected material seems small, considering the necessary uncertainty of the channels through which the observations have reached us.

Now, returning to the material which, though rejected, may not be valueless, the objection to the use of the Chinese and Assyrian eclipses was substantially the same as that to the classical eclipses: the accounts inspired no reasonable certainty that the eclipses were severally total at definite points of the earth's surface. The occultations cited by PROLEMY seemed worthy of more confidence, and I went so far as to make a summary of PROLEMY'S statements, but they were so far from being precise that some hesitation was felt in deciding whether they were worth employing. I finally concluded to omit them from the fact that they are cited by PROLEMY, not for the purpose of presenting a complete series of observations, but to prove that the erroneous value of the constant of precession deduced by HERRARIUS, namely, one degree in a century, was correct. Being in all probability selected for the purpose of proving an erroneous hypothesis, it was hardly possible to employ them without the reservation above alluded to, that they should be accepted or rejected according as their results did or did not agree with those derived from other data.

The eclipse of TUGOZ was observed in the year 394, less than five centuries before the commencement of the series of eclipses given by EBU JOUZIS. It seems to me scarcely worthy of confidence. He begins by stating that he accurately calculated the time of beginning, and found it to be two hours and fifty minutes, and then, that having carefully observed the time of beginning, he found it exactly the same. The time of ending is given as observed, and as it was not stated to have been predicted, less suspicion may attach to it. In the absence of any indication how his times were determined, and in the suspicious coincidence of observed and computed time of beginning, to say nothing of a certain vagueness which runs through his narrative, it seems to me that we have reason to place this eclipse in the same category with other rejected material.

Granting that the question whether HAXSEN'S tabular mean longitude of the moon does or does not require a large negative correction during the centuries which preceded the Christian era (which question is the fundamental one) remains undecided, the question may arise how a decisive answer can be reached. This depends upon whether a correct theory of the apparent inequalities of long period in the moon's mean motion can be constructed. If such a theory cannot be formed, and if the mean motion of the moon is subject to such changes from age to age that no constant and well-defined value can be assigned to the secular acceleration, then it is not certain that the question can ever be conclusively settled, because, in this case, no conclusion can be drawn from observations made at any other time than during or near the period in question. The only course will be to make a complete re-examination of all ancient eclipses and other data which may throw light on the question, and to compare them with the results of the two hypotheses: first, that HAXSEN'S Tables are correct during the period in question; and, second, that they require a correction of sixteen minutes, subtractive. Should the evidence prove to be mainly in favor of one hypothesis, that