ANNEX 8

Our replies to the authors of certain erroneous works, who tried to refute our astronomical datings

To Academician N. A. Plate, Editor-In-Chief of the "Vestnik Rossiyskoi Akademii Nauk" journal, a periodical edition published by the Russian Academy of Sciences

Dear Nikolai Alfredovich,

You have given a negative answer to my request to publish my open letter to Academician Y. S. Osipov with a response to his criticisms in the "Vestnik Rossiyskoi Akademii Nauk" journal. Instead, you suggested that an article be written in re the publication of Y. N. Yefremov and Y. A. Zavenyagin, with a foreword by V. L. Ginzburg. Said article was published in the "Vestnik's" 12th issue for 1999 (pages 1081-1082). I am enclosing a reply to this publication with a request to publish it in your journal.

Very truly yours,
Academician A. T. Fomenko,
21 March 2000.

A reply to the publication of Y. N. Yefremov and Y. A. Zavenyagin with a foreword by Academician V. L. Ginzburg, which was published in the "Vestnik Rossiyskoi Akademii Nauk" in 1999, issue 12

A. T. Fomenko and G. V. Nosovskiy

The article of Y. N. Yefremov and Y. A. Zavenyagin objects to our dating of the Almagest star catalogue ([m1] and [m2]) for the following reasons:

1. The authors disagree with our observation that the initial longitudinal reference point of the Almagest catalogue is prone to a certain ambiguity. Half of their article's section entitled "The Almagest and its Dating" is concerned with a discussion of this issue. This is also the subject of the second accusation in the list that one finds on page 1088 (article [m13]).

Our reply. Our method of dating the Almagest star catalogue does not refer to the position of the longitudinal reference point anywhere. Our observation in re the point in question, which was cited in our book ([m1] and [m2]), serving Y. N. Yefremov and Y. A. Zavenyagin as the impetus for their verbose commentary, happens to be of no significance whatsoever inasmuch as our method of dating is concerned.

We have actually performed the dating of the Almagest by longitudes and proper motion rates in [m2], pages 176-178. However, its precision turned out to be substantially lower than that of the latitudinal dating for the simple reason that the Almagest longitudes are less precise than latitudes, which must be known perfectly well to Y. N. Yefremov and Y. A. Zavenyagin. Their claims about us rejecting the longitudinal dating are therefore completely misleading (see [m13], page 1083).

As for the precession-based dating of the catalogue, we consider it in section 2 below.

This is the only actual direct "objection" against our dating of the Almagest catalogue that one finds in the article ([m13). All the other objections are of an indirect nature and come up to the following: "your dating cannot be correct since we believe that other calculations, not based on the Almagest cata-

logue in any way, contradict it". See Section 2 for more on this subject.

2. Y. N. Yefremov and Y. A. Zavenyagin point out the discrepancies between our work and the works of different researchers who have tried to date the Almagest and other old astronomical data, as well as the respective dating results. The following examples are given.

2a. Precession-based longitudinal dating of the Almagest catalogue yields the I century A.D. as a result.

2b. Star declination dating yields the beginning of the new era as a result (see accusation #5 on page 1088 in [m13]).

2c. Babylonian astronomical documents "doubtlessly confirm the antiquity of the ancient history" ([m13], page 1088 – see accusation #1 on page 1088 of [m13]).

Our reply. We have deliberately sought such methods of dating the Almagest that would be based on astronomical characteristics and principles unknown until the XVIII century. The justification of this methodology is a separate issue that we cannot discuss presently. At any rate, we have voiced this principle with enough precision and in perfectly explicit terms in our book ([m1] and [m2]) and implemented it consecutively. This is why we didn't use either star declinations or positions of the Sun for dating purposes, let alone longitudinal precession. All such characteristics and resulting dates may well have been employed by XVII century astronomers in their calculations (and their rather remote predecessors were already capable of using longitudinal precession for the same end). We know that data of this kind yield dates that concur with the Scaligerian version. Our discovery is that the use of other data, the kind that cannot be a product of XVII century calculations by default, gives altogether different dates. Therefore, the "objections" of Y. N. Yefremov and Y. A. Zavenyagin are merely a demonstration of their incapacity (or reluctance) to understand the general principles of our approach.

As for the "Babylonian astronomical records" – we deliberately refrain from discussing them in our book about the dating of the Almagest. It is an altogether different issue that requires an in-depth analysis – a mere passing reference very clearly will not do ([m13], page 1088). It has to be said that the researchers involved in the dating and the interpretation of such old documents are as a rule convinced that the traditional chronology cannot possibly be incorrect, and often rely on its implications – examples of such an approach exist in great abundance. The Babylonian tablets are no exception, either. We must once again note that the issue in question is of no relevance to our book about the dating of the Almagest catalogue.

3. Y. N. Yefremov and Y. A. Zavenyagin express their outrage about the fact that we do not use the calendar indications of months and days provided in the Almagest as they discuss our dating of planetary coverings of stars (accusation #6 on page 1088 in [m13]).

Our reply. The reason is the same as we specify in Section 2. The month and the day are de facto defined by the position of the Sun – a characteristic that might be a result of XVII century calculations. Also, the traditional interpretation of the month names inherent in the Almagest and their conversion into the modern calendar system is anything but obvious, and requires a separate discussion.

4. Y. N. Yefremov and Y. A. Zavenyagin appear to have comprehended nothing about our research that concerned calculating uniform systematic error areas in the Almagest catalogue. This is what they write: "The assumption that different catalogue copies were compiled by different observers happens to be one of Fomenko's main arguments in favour of choosing celestial areas that were allegedly observed better … contradicting all known information" ([m13], page 1086). This miscomprehension appears to have served as the basis of the third and rather vague accusation on page 1088 of [m13].

Our reply. Y. N. Yefremov and Y. A. Zavenyagin are making false claims by ascribing such assumptions to us – we never "assumed" anything of this sort. What Yefremov and Zavenyagin present as our "assumptions" are merely our explanations of possible (but by no means obligatory) reasons behind the statistical fact that the systematic error of the Almagest catalogue is non-uniform, as we have discovered. There may be different reasons behind this – different observers being just one of them. This may or may not have been the case; however, our method and our results are wholly independent from this circumstance. This "counter-argumentation" of Yefremov and Za-

venyagin looks rather odd and makes one wonder whether they actually understand the matter at hand.

5. We are particularly surprised by accusation 4 on page 1088 in [m13]. Y. N. Yefremov and Y. A. Zavenyagin write the following – we cannot help quoting this passage in its entirety: "Why do all the ancient catalogues, including the Arabic works, which have survived until our day and age, whose stellar coordinates were the very same Almagest coordinates converted to fit certain historical epochs, happen to hail from one and the same ancient epoch of the Almagest catalogue?" ([m13], page 1088). One wonders how Yefremov and Zavenyagin managed to access the drafts and intermediate calculations of mediaeval authors. It is perfectly obvious that their claims are based on their absolute trust in Scaligerian chronology, which spawns such corollaries.

6. Finally, let us consider the epilogue of Y. N. Yefremov, wherein he offers the reader his own version of dating the Almagest catalogue (co-authored by A. K. Dambis). Y. N. Yefremov refers to the two graphs one finds on page 1090, claiming them to represent his results. The first one corresponds to the dependency of the Almagest catalogue epoch on the number of stars used in calculation ordered by their proper motion rate values listed in descending order. The second is similar – it represents the dependency on the number of fast stars excluded from analysis ordered by their proper motion rate values listed in descending order. Intervals drawn around the "precise datings" are referred to as "square average discrepancy intervals" by Y. N. Yefremov. He is of the opinion that the intervals in question correspond to the error margin estimate for his method. This is directly implied by the text on page 1090. Even a cursory glance at the graphs reveals that the precision margin of "Yefremov's method" doesn't change in case of the first graph and changes very marginally in case of the second, once the fastest stars are excluded from calculations. How Yefremov and Dambis manage to date the Almagest catalogue with the precision of ± 400 years, having rejected 20 fastest stars, or all of the visibly mobile Almagest stars, remains a mystery. This is tantamount to dating the Almagest catalogue by the proper motion rates of immobile stars, or stars with virtually nonexistent proper motion rates. In the case of Y. N. Yefremov and A. V. Dambis considering all the Almagest stars, including the fastest ones, the precision of their dating is completely unrealistic – allegedly ± 100 years. Elementary estimates resulting from a division of the Almagest systematic error rate by the velocities of the fastest stars that can be reliably identified in the Almagest reveals that no smaller error margin than ± 300-350 years is possible in this case. Also, there are very few "fast" stars – a mere handful. The overwhelming majority of stars are all but immobile. Therefore, having excluded 20 fastest stars from their calculations, the precision estimate of the catalogue dating suggested by Y. N. Yefremov and A. V. Dambis shall equal ± several millennia. Y. N. Yefremov has already made a serious error in the estimate of his "method's" precision in [m12]. We have considered the error of Y. N. Yefremov in detail in our books [m1] and [m2] as well as the article [m5]. Nevertheless, Y. N. Yefremov manages to make the very same error. We must once again cite this very simple arithmetical calculation for Y. N. Yefremov and A. V. Dambis in order to demonstrate the absurdity of their precision claims for their attempt to date the Almagest catalogue by proper motion rates.

It is obvious that the precision of any dating method that refers to the proper motion rate of a fast star shall have its lower margin by the individual error in the estimation of said star's position in the Almagest divided by its proper motion velocity. Had there been an abundance of such stars (N items, for instance), we could raise the precision of our method employing division by roughly the square root of N. However, as we have already mentioned, there are very few fast stars in the Almagest catalogue, and the proper motion velocity rate falls very quickly. Therefore, the a priori known upper margin of the method's precision estimate shall be the calculation employing Arcturus, the fastest of the reliably identifiable stars. In general, one cannot use more than 20 Almagest stars for a proper motion rate dating, since the rest of them happen to be virtually immobile. Y. N. Yefremov de facto acknowledges this fact in the following passage: "All 1022 stars were used, the slow stars defining the coordinate system" ([m13], page 1089). In other words, slow stars are only useful for defining the coordinate system, but not the purposes of actual dating.

All the stars in the Almagest were measured with errata of some sort. This is doubtlessly true about the

slow stars that define the coordinate system of Y. N. Yefremov and A. K. Dambis. However, let us assume for a moment that the positions of slow stars are measured with ideal precision in the Almagest. Even in the ideal case we cannot assume the error in the Almagest estimate of the position of Arcturus to be smaller than 10' by either coordinate, since that is the value of a single step of the Almagest star catalogue coordinate scale. The real value of this margin is actually higher due to the imprecise coordinates of the neighbouring stars.

The arc distance error equals circa 14 arc minutes. If the possible error by each of the coordinates equals 10 arc minutes, it shall equal 14 arc minutes for the hypotenuse, according to the Theorem of Pythagoras. The annual proper motion rate of Arcturus equals circa 2 arc seconds. Thus, it takes Arcturus about 420 years to cover the distance of 14 arc minutes. The lower margin of ± is merely a rough estimate of the Arcturus dating precision with arc distances used in calculations, or latitudes together with the longitudes. The use of nothing but latitudes makes it possible to raise the method's precision somewhat and come up with the dating whose precision margin will equal ± 300 years. Dating the Almagest catalogue by proper motion rates of the stars it contains with any higher precision is impossible. The use of fast stars that cannot be identified reliably in the Almagest leads us to a vicious circle – such is the case with Omicron 2 of Eridanus, for instance.

The above makes the words of Academician V. L. Ginzburg that we find in his preface where he claims having finally encountered a "clear and precise analysis of A. T. Fomenko's errors" ([m13], page 1081) in the work of Y. N. Yefremov and Y. A. Zavenyagin. One cannot help wondering about the exact passage of the blatantly demagogical oeuvre concocted by Yefremov and Zavenyagin that struck Academician V. L. Ginzburg as "clear and precise", as well as whether or not he actually gave the problem in question any thought at all.

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[m12] Y. D. Pavlovskaya and Y. N. Yefremov. *Dating the "Almagest" by Proper Star Motions.* – "Doklady AN SSSR" (Reports of the USSR Academy of Sciences), 1987, Volume 294, #2, pages 310-313.

[m13] Y. N. Yefremov and Y. A. Zavenyagin (with a foreword

by V. L. Ginzburg). *On the so-called "New Chronology" of A. T. Fomenko.* – "Vestnik RAN", 1999, Volume 69, #12, pages 1081-1092.

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Our reply as given above was published in the "Vestik RAN", #9, 2000.

Our analysis of Y. N. Yefremov's article entitled "A New but False Chronology"

([p19], pages 142-146)

About one half of Y. N. Yefremov's article consists of biased emotional statements that reflect Y. N. Yefremov's absolute trust in the chronology of Scaliger and Petavius as well as the school course of history. For example, Y. N. Yefremov is of the opinion that "consensual chronology does not require any new proof or tests" ([p19], page 142). Furthermore, Y. N. Yefremov is convinced that historians "carry on publishing irrefutable proof with infinite politeness ... but politeness is of little aid here" ([p19], page 142). This is the very reason why Y. N. Yefremov decided to abandon the etiquette common for scientific discussions and resolved to "call a spade a spade", as he claims on page 142 of [p19]. However, most articles contained in [p19] and [p20] are characterised by extreme coarseness, so Yefremov's article is by no means exceptional in the context of [p19] and [p20].

Let us point out the following "important evidence" that works in favour of the consensual chronology according to the opinion of Y. N. Yefremov – as an amusing oddity. We quote verbatim: "The spirit of an epoch has a unique taste. Virgil doesn't resemble Dante, Julius Caesar has got nothing in common with Charlemagne, and the Gothic cathedrals are quite unlike Parthenon. No discussion is needed [sic!] to realise they are separated by many centuries of humankind's evolution" ([p19], page 142). The logic of Y. N. Yefremov strikes one as twisted. For instance, the Cathedral of St. Basil the Divine on the Red Square and the Blagoveshchenskiy Cathedral of the Muscovite Kremlin look nothing like each other, but were nevertheless built in the same epoch. What is the source of Y. N. Yefremov's unswerving trust (he "needs no discussion", after all) in chronological heterogeneity (as in "separated by centuries of evolution") of buildings that fail to resemble each other? There is a great abundance of examples to prove the contrary.

Now let us discuss actual chronological results of Y. N. Yefremov, who has attempted to perform a dating of the Almagest star catalogue by proper star motions. He came up with a result that he believes to prove Scaligerian chronology ([p21] and [p22]). Unfortunately, Y. N. Yefremov's works on the dating of the Almagest catalogue contain an error of roughly 1000 years in the precision estimate of the dates that he comes up with. This is what invalidates Yefremov's dating of the Almagest catalogue completely. We have studied Yefremov's errors in the dating of the Almagest star catalogue and written about them at sufficient length – see [p6], [p7] and [p8]. We shall refrain from yet another reiteration.

However, in the article published in [p19] and considered presently Y. N. Yefremov claims that his new work co-authored by A. K. Dambis ([p23]) contains an error-free (as he would like to believe) proof of the Scaligerian dating of the Almagest star catalogue, and, consequently, Scaligerian chronology in general. Moreover, Y. N. Yefremov claims that his old method of dating the Almagest, which we have discussed attentively in a number of publications, "has been rendered meaningless by the results of research related in the article" ([p23]; see [p19], page 145). In other words, according to Y. N. Yefremov, all his former errors in the dating of the Almagest have been corrected, and the result remains the same – one that proves Scaligerian chronology. Y. N. Yefremov reports no details concerning his new method of dating in [p19], referring the reader to the English publication in the Journal for History of Astronomy ([p23]).

Let us therefore consider the article in question (written by Y. N. Yefremov together with A. K. Dambis ([p23]}}. According to the authors, the article in question describes two radically new methods of dating Ptolemy's star catalogue. It goes without saying that both methods "prove the correctness of Scaligerian chronology", according to the authors of [p23]. However, our analysis of the publication in question ([p23]) demonstrates that, alack and alas, Y. N. Yefremov and his co-author A. K. Dambis stubbornly repeat the very same old error of Y. N. Yefremov – incorrect estimation of the precision of the approximate dates yielded by their research.

The first of the two novel Almagest catalogue dating methods as offered by Y. N. Yefremov and A. K. Dambis is described in the "Results of Mutual Distances Method" section of [p23]. The method itself was simply taken from our book ([p8]), as Y. N. Yefremov and A. K. Dambis tell the reader explicitly ([p23], page 121). They are of the opinion that we haven't noticed just how "good" the results of this method's application can be "in reality" ([p23], oage 121). However, in our book about the dating of the Almagest star catalogue ([p6], [p7] and [p8]) we explain it with sufficient clarity why the method in question, as well as several other simple approaches to the dating of the Almagest catalogue, cannot yield any non-trivial result. The main reason is the low precision of the dates that we arrive at when we use these methods; as a result, the scatter range for the actual dates turns out too great. Consequently, any dating of the Almagest catalogue that employs methods this simple turns out non-informative, or trivial. As for the method that Y. N. Yefremov and A. K. Dambis borrowed from our book, we refer the readers to Section 3 of Chapter 3 of [p6], or, alternatively, to section 3.3 of our book on the dating of the Almagest (edition [p7]). See also Section 7.4, "Dating the Almagest Catalogue by an Expanded Informative Kernel" in the last edition of said work ([p7]).

We are once again confronted by a strange reluctance of Y. N. Yefremov to treat the problem of precision estimation in the dating of the Almagest catalogue with due respect. Y. N. Yefremov's precision estimates of the resulting catalogue's datings are either altogether absent, as in the case we have just considered, or just erroneous. The above example of Yefremov and Dambis borrowing a dating method from our book – a method we rejected due to the insufficient precision of its results, no less, demonstrates Y. N. Yefremov's attitude towards the issue of precision estimates in general. Nevertheless, precision estimates are an issue of paramount importance insofar as this problem is concerned. See [p6] and [p7] for more details.

Let us consider the next section of the article ([p23]). It is called "The Case of *o* Eri. The authors tell us directly: "The fastest of the Almagest stars, *o* Eri is important for catalogue dating by means of proper motions". This is indeed the case. However, in order to use *o* Eri for the dating of the Almagest, one needs to be certain that the star in question was actually included in the Almagest catalogue at the very least. In order to prove this, Y. N. Yefremov and A. K. Dambis refer to the works of several astronomers who sought the identification of Almagest star 779 (in Bailey's numeration), called "the star in the middle" by Ptolemy in the Almagest. Indeed, most researchers identify this rather unremarkable Almagest star as *o* Eri, a modern star that is just as unremarkable. However, it has to be emphasised that the only basis for this identification is that the star in question corresponded to the coordinates of star #779 as given in the Almagest best in the epoch of the II century A.D., which is where Scaligerian chronology places Ptolemy. No other proof of the above identification was given except for coordinate correspondence – this star is neither characterised by luminosity, nor by anything in the way of a proper name or a detailed description in the Almagest.

However, let us recollect the fact that the star *o* Eri possesses a very high proper motion rate. Its visible position on the celestial sphere changes notably over the course of time. So if *o* Eri was indeed the best identification candidate for the Almagest star #779 in the beginning of the Anno Domini epoch, this is by no means the case for other historical epoch. The fact that the astronomers chose *o* Eri as their best identification candidate for the Almagest star #779 is a trivial consequence of the fact that the astronomers had already referred to information concerning proper star motions as well as the inevitable Scaligerian dating of the Almagest. In other words, the identification in question, which is of great importance for Y. N. Yefremov, is merely a consequence of the Scaligerian dating of the Almagest. To use it for the dating of the Almagest would be tantamount to solving the reverse problem of restoring the Scaligerian dating of the Almagest used by the astronomers of the XVIII-XX century for the identification of Ptolemy's stars. However, the dating in question is known to us perfectly well; it is a Scaligerian dating. It is obvious enough that Y. N. Yefremov's approach cannot lead him to any other dating of the Almagest but the Scaligerian. This is the vicious circle in Yefremov's conclusions that keeps on mistaking the effect for the cause.

We have explained it to Y. N. Yefremov several

times that the use of *o* Eri for the dating of the Almagest catalogue is useless, since it leads one to a vicious circle. Our book ([p6], [p7] and [p8]) discusses this at great length, citing the respective drawn copies of real stars and their Ptolemaic equivalents in the constellation of Eridanus. Nevertheless, Y. N. Yefremov keeps on dating the Almagest by *o* Eri, never quite free from the vicious circle in question. These explanations become rather taxing at the end of the day.

The next section of [p23] is entitled "The Bulk Method"; it concludes the actual content part of [p23]. The remaining sections of the article deal with conclusions and acknowledgements.

According to the authors of [p23], in this section they offer a method of dating the Almagest catalogue by proper motions that is substantially different from the old method of Y. N. Yefremov ([p21] and [p22]). According to Y. N. Yefremov and A. K. Dambis ([p23]), the crucial difference between the old and the new method is that this time all the fast stars of the Almagest at once were used for the dating of the Ptolemaic catalogue, whereas previously each of the fast stars was used for the dating calculations separately ([p23], page 125).

However, one instantly becomes somewhat astonished by the fact that the use of the new evolved dating method did not raise the precision of Y. N. Yefremov's end dating – on the contrary, the precision was impaired. In his previous work ([p21]) Y. N. Yefremov dates the Almagest to 13 A.D. with the precision margin of ± 100 years. In [p23], using a more evolved dating method, Y. N. Yefremov only managed to attain the precision of ± 122 years. The result of Yefremov's new dating of the Almagest is as follows: 90 B.C. ± 122 years ([p23], page 128). Thus, the method has evolved, yet the precision of results has deteriorated. How is one supposed to interpret this?

The answer is that similarly to the errata made in [p21], in [p23] Y. N. Yefremov gives a false estimate of the resulting datings' precision.

We already considered the figmental nature of the precision margin claimed by Y. N. Yefremov for his datings of the Almagest catalogue in our analysis of Y. N. Yefremov's prior works. See also an in-depth discussion of this issue in [p6] (pages 99-102) and [p7] (pages 200-212). A simple calculation demonstrates that the real precision margin of Y. N. Yefremov's method roughly equals a thousand years and not 100-120 years, as he believes for some reason.

Incidentally, in his very first work dedicated to the dating of the Almagest ([p21]) Y. N. Yefremov describes how he arrived at his precision estimate in sufficient detail. This gave us the opportunity of discovering the error in his postulations, which was duly pointed out to him ([p6], pages 99-102 and [p7], pages 200-212). In the last work of Y. N. Yefremov ([p23]) concerned with the dating of the Almagest by proper star motion rates he makes just as far-fetched claims of his precision estimates without any validation whatsoever. [p23] doesn't contain any formulae or algorithms that would lead Y. N. Yefremov to his estimates. He appears to have written no other works with any more details, either. At the very least, neither [p19], nor [p23] contain any references to any such works. Therefore, it is difficult to point out the actual errors made in the precision estimation by Y. N. Yefremov as per [p23]. However, there is no need to do any such thing. The fact that the dating precision estimates as given in [p23] contain an error is implied by our analysis of the Almagest catalogue precision characteristics as related in [p6] and [p7]. These characteristics imply that the precision of dating the Almagest catalogue by proper star motion rates with the method of Y. N. Yefremov cannot be any higher than ± 400-500 years if arc discrepancies are used, or, at the very least, ± 300 years (with the use of latitudinal discrepancies – see [p7], page 206, and [p7] in general).

Furthermore, it is possible that in [p23] Y. N. Yefremov conducted a deliberate preliminary selection of fast star neighbourhoods, hence the "desired" result. At least, the text of the article ([p23]) is rather vague about the rules adhered to in the choice of a given fast star's neighbourhoods for the final dating. Since the method of Y. N. Yefremov demonstrates no stability in face of neighbourhood star choice, a careful selection of neighbourhood stars will yield the very date of the Almagest catalogue that was intended a priori. See more details in our analysis of Y. N. Yefremov's method ([p6], pages 99-102; also [p7], pages 200-212).

In general, the new method of dating the Almagest by proper star motions as suggested in [p23] is little different from the initial method as related in [p21] and [p22]. The primary difference is that previously

Y. N. Yefremov would calculate the datings by each of the fast stars separately (after a certain choice of its neighbouring stars). It has to be explained that in Y. N. Yefremov's method the position of a fast star is defined in relation to its neighbourhood. We have discovered that a change in the choice of neighbouring stars can greatly affect the resulting dating yielded by this method ([p6], pages 99-102; also [p7], pages 200-212). Now, in [p23], Y. N. Yefremov suggests to calculate a single date with the aid of all the fast stars at once. He uses a certain neighbourhood selection rule that remains unclear from the text of [p23]. Y. N. Yefremov and A. K. Dambis define the desired single date as follows ([p23], page 125).

Yefremov and Dambis consider the ecliptic coordinates on the celestial sphere for the epoch of the beginning of the new era. One of the coordinates is fixed as a result – either the latitude or the longitude. After that, each of the datings is presented as a point on a plane. The proper motion rate component of a given fast star along the coordinate in question is represented on the horizontal axis (with a certain compensation of neighbourhood star velocities, which is of no substantial meaning here). Points on the vertical axis represent the discrepancy by a given coordinate for the averaged distance between the fast star in question and the stars of its neighbourhood. Chosen discrepancy represents the difference with the averaged distance calculated by the Almagest, and a similar distance on the calculated celestial sphere for the beginning of the Anno Domini era. The result is a point on a certain plane. After that, the dating of the fast star in question and its neighbourhood by the method of Y. N. Yefremov is represented by a declination of the straight line that crosses the beginning of the coordinate system and the point in question.

This procedure is performed by both ecliptic coordinates (latitude and longitude) for all the fast stars and their varying neighbourhoods. This results in a field of dots on a plane. Obviously enough, if the Almagest catalogue contained ideally precise star coordinates, all such dots would pertain to a single line, whose declination would represent the dating of the catalogue. However, given the erroneous coordinates of the Almagest stars, this is not the case. Y. N. Yefremov and A. K. Dambis got the idea of using the linear regression method in order to estimate the dating of the catalogue by the declination of the regressive line that crosses the resulting field of dots.

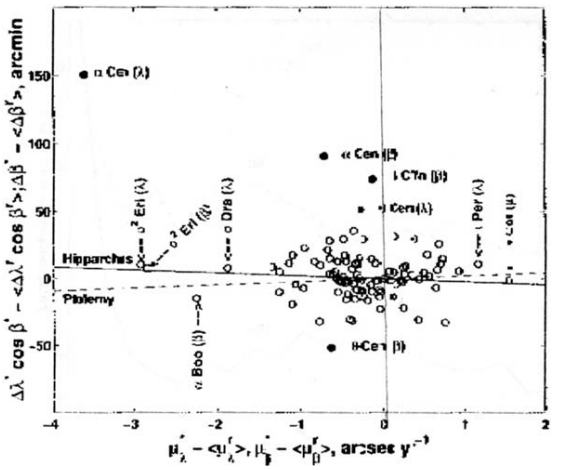

Fig. P8.1. An illustration from the work of Y. N. Yefremov and A. A. Dambis that depicts a field of dots that represent various Almagest datings made by separate configurations. Yefremov and Dambis drew a regression curve across the field of dots, whose declination is supposed to stand for the Almagest dating according to their method. There are two such curves in the illustration – one of them corresponds to the Scaligerian epoch of Ptolemy, and the other – to the Scaligerian epoch of Hipparchus. According to Y. N. Yefremov and A. A. Dambis, this star field defines the regressive curve with such precision that it renders the Ptolemaic version obsolete, whereas the Hipparchian version remains valid – in spite of the great proximity of both versions insofar as their drawing is concerned. This opinion of Y. N. Yefremov and A. A. Dambis is more than dubious. A field of dots such as we see on their drawing obviously does not permit so much as to define the declination of the regressive curve with precision sufficient for distinguishing between the beginning of the New Era and the XVI century, for example.

The idea makes perfect sense per se. However, the field of dots that Y. N. Yefremov and A. K. Dambis came up with for the Almagest ([p23], page 124, ill. 5) does not permit an estimation of the regressive line's declination with the precision margin they declare – little wonder, considering the principal lack of precision in their method.

The field of dots that we see in ill. 5 of [p23] more or less chaotically fills the area that resembles an ellipsis whose center coincides with the beginning of the coordinates. See fig. P8.1, which reproduces ill. 5 from

the work of Yefremov and Dambis. We have added the missing vertical axis that crosses zero. The ellipsis formed by the field of stars in fig. P8.1 is somewhat stretched horizontally (the relation between the half-axes roughly equalling 2:1). Y. N. Yefremov and A. K. Dambis claim that the declination level of the regressive line defined by such "ellipsoidal" field of dots is close to zero. Moreover, they are de facto making the claim that this level can be defined with the mind-boggling precision of several degrees ([p23], page 125, ill. 5). This is more than doubtful. Obviously, Y. N. Yefremov has once again made an error in the precision estimate of the resulting dating.

Let us make a conclusion. The new work of Y. N. Yefremov concerned with the dating of the Almagest that he refers to in [p19] is de facto but another version of his old Almagest dating method. It repeats the same error that Y. N. Yefremov made previously – a wrong precision estimate of the dating he comes up with. Moreover, in this work Y. N. Yefremov once again uses the star *o* Eri for dating purposes, whose very presence in the Almagest catalogue can only be justified by the assumption that the catalogue was compiled at the very beginning of the A.D. era, which is its Scaligerian dating. It is clear that the use of such a star for the purpose of dating the catalogue leads one to a vicious circle.

Our analysis of the article entitled "Dating Ptolemy's Almagest by Planetary Configurations" *([p19], pages 111-123)* by A. A. Venkstern and A. I. Zakharov and the article of Y. D. Krasilnikov, "On the Planetary Coverings of Stars in Ptolemy's Almagest" *([p19], pages 160-165).*

The first part of the article by A. A. Venkstern and A. I. Zakharov deals with the attempt to date the Almagest by the 23 planetary observations that Ptolemy claims as his own ([p19], page 111). A. I. Zakharov is an astronomer and a staff member of the Sternberg State Institute of Astronomy. A. A. Venkstern is a mathematician; A. T. Fomenko was her Academic Advisor at the MSU Department of Mathematics and Mechanics. The article contains a number of calculations that we did not verify ourselves – however, we have no reason to doubt their veracity. Let us cite the authors' result – it doesn't contradict our research of the Almagest in the least.

A. A. Venkstern and A. I. Zakharov make the following corollary: "We believe that one of the following two postulations is true: *a)* the planetary observations that served Ptolemy as a basis for his theory were indeed conducted in the II century A.D.; *b)* these observations have been calculated in accordance with a certain theory for the date in question" ([p19], page 111).

A. A. Venkstern and A. I. Zakharov tell us the following in re option b, or the falsification of the Almagest: In order to verify the possibility that the data were supplanted by mediaeval hoaxers (before Kepler's theory) we decided to check the growth rate of the discrepancy in Ptolemaic theory. This can also be formulated as follows: how far away was the hoaxer's (Ptolemy's) epoch from the traditional Almagest dating in reality to give him the opportunity of fabricating and introducing false observations using the theory related in the Almagest? ... Our conclusion is as follows: said observations could not have been falsified with the aid of a theory of the Ptolemaic sort – the "life expectancy" of such theories does not exceed 200 or 300 years" ([p19], page 114).

All of this happens to be in perfect correspondence with our calculations and our reconstruction (see "The Astronomical Analysis of Chronology" ([p7]) for more details). We are of the opinion that the Almagest (in the form that we know today – see [p7]) is a XVII century edition. In other words, it is a re-edition of some famed old astronomical work that was made in the epoch of Kepler. All the activities associated with the editing of the Almagest, which date from the XVII century, can be regarded as falsification. Its aim was to make the Almagest resemble a work of the alleged II century A.D. The epoch in question was taken from the Scaligerian chronological tables. The Scaligerite hoaxers have rendered all the astronomical data in the Almagest that they could calculate retroactively to the II century A.D. – Ptolemy's planetary theory, for instance. This is what A. A. Venkstern and A. I. Zakharov have now discovered in their work as published in [p19]. One must give them credit for being explicit about what exactly they have proved.

The astronomical data that could not be calculated reliably in the XVII century (such as solar

eclipses) were simply excluded from the Almagest. As a result, the modern version of the Almagest rather strangely fails to mention so much as a single solar eclipse. We are lucky that the XVII century hoaxers haven't removed the old Ptolemaic star catalogue from the Almagest altogether – most likely, they simply didn't suspect that such catalogues might give on the opportunity of dating the Almagest with the aid of such a subtle effect as proper star motions ([p7]). Rougher effects (such as longitudinal precession) were naturally taken into account.

As for longitudinal precession – calculating it in reverse for the I century A.D. was an easy enough task already in the XV-XVI century, let alone the XVII. We have to remark that another critic of ours, the astronomer Y. N. Yefremov, keeps making claims in numerous publications about the possibility of dating the Almagest by longitudinal precession, or de fact restoring the data introduced by the XVII century Scaligerite editors, thus "effectively confirming" the veracity of Scaligerian chronology. These rather amusing ruminations of Y. N. Yefremov can also be found in [p19], page 143.

And so, going back to the work of A. A. Venkstern and A. I. Zakharov, we can conclude with the statement that the result they came up with neither contradicts New Chronology, nor our reconstruction of history. It does, however, contradict the Scaligerian version of chronology and history, and very much so, although for some reason this circumstance isn't mentioned anywhere by Venkstern or Zakharov.

The matter is as follows. In the section of their article entitled "The Possibility of Falsifying the Almagest Planetary Observations with the Aid of Other Theories" ([p19], pages 113-114), A. A. Venkstern and A. I. Zakharov study the "life expectancy" of the planetary theory as related in the Almagest. It has to be said that the characteristics of planetary orbits change slowly with the course of time. Therefore, a given planetary theory that could function satisfactorily in the epoch of its creation could become utterly useless in several hundred years, and would naturally have to be replaced by a new theory, or at least improved by means of parameter correction. So what was the life expectancy of the Ptolemaic theory?

A. A. Venkstern and A. I. Zakharov provide the following answer: 300 years maximum. According to their calculations, "the error inherent in the Ptolemaic theory accumulates very quickly; therefore, the theory works very poorly with such parameters beyond the temporal vicinity of 300 years … The 'life expectancy' of such a theory equals a mere 200-300 years" ([p19], page 114).

Let us now assume the correctness of the Scaligerian historical and chronological system, and also that the Almagest as it is known to us today was indeed compiled by Ptolemy around the beginning of the new era – in the I-II century B.C. or the I-II century A.D. The implication is that the planetary theory as related in the Almagest ceased to be functional in the VI-VII century. If we add 300 years (the maximal life expectancy of this theory calculated by A. A. Venkstern and A. I. Zakharov) to the Scaligerian date of the Almagest's completion (circa 150 A.D. as per [p24], page 430), we shall come up with 450 A.D. (500 or 600 A.D. if we stretch the period to the maximum) – not any later. The Ptolemaic planetary theory should have become obsolete or modified after that.

What do we learn from Scaligerian history textbooks? The Scaligerian version is of the opinion that the Almagest remained the primary source of astronomical knowledge in general and the planetary theory in particular up until the Copernican epoch, or the XVI century A.D. ([p24], pages 445-458; also [p25], pages 2-3). See also our review of the Almagest's history in its Scaligerian version ([p7], pages 19-21).

It turns out that astronomers and mathematicians had kept a functional planetary theory for 200 or 300 years, and then used an utterly imprecise planetary theory for over a millennium – one that lost the last vestiges of precision by the V-VI century A.D. and became completely unsatisfactory, deciding to abandon it as late as the XVI century A.D. Up until that very moment they had no qualms about using it, translating it into other languages, studying it, admiring it etc. Nobody thought of so much as correcting the theory's planetary orbit parameters; had this been done, the calculations of A. A. Venkstern and A. I. Zakharov would yield the date of the last correction and not the I century A.D.

The picture we come up with is unrealistic. The only explanation of the results obtained by A. A. Venkstern and A. I. Zakharov that we deem reasonable is that the Almagest planetary theory as we know it

today was introduced into said work in the XVII century, the epoch of Kepler, with the goal of falsifying its dating – it was a matter of paramount importance for the nascent Scaligerian version of history and chronology (see [p7] for more details). Quite naturally, the hoaxers made the planetary orbit parameters fit the desired date – the beginning of the new era. This is precisely what A. A. Venkstern and A. I. Zakharov have discovered in their work.

In the next and final section of their article published in [p19] A. A. Venkstern and A. I. Zakharov criticise the astronomical solution of the four planetary coverings of stars as described in the Almagest that we came up with as a result of our research. Let us remind the reader that our solution was as follows: morning of 14 February 959 A.D. for Mars, morning of 18 October 960 A.D. for Venus, dawn of 25 July 994 A.D. for Jupiter and the evening of 16 August 1009 A.D. for Saturn. It is in excellent correspondence with the dating of the Almagest star catalogue by proper star motions. The possible interval for the Almagest star catalogue dating by proper motions is 600 A.D. – 1300 A.D. ([p7], page 392). Our solution falls right over the centre of the interval.

Apart from that, we have discovered that our solution for the planetary star coverings ideally satisfies to the time of day conditions of said coverings as per Ptolemy's own words ([p7], pages 454-467). For example, in case of Mars Ptolemy reports the covering to have been visible in the morning. Indeed, in our solution Mars could only be visible after midnight and until the morning. In case of Jupiter Ptolemy tells us that the covering could be observed at dawn; in our solution, Jupiter rose exactly an hour before sunrise, remaining in the dawn region of the sky all the time. As for the "traditional" (or Scaligerian) solution, it claims that Jupiter was visible all night, remaining right next to the star, which makes Ptolemy's words about the covering observed at dawn extraneous and rather odd, as a matter of fact. In other words, the traditional solution is rather farfetched – here as well as elsewhere. Further on, Ptolemy reports that Saturn approached the star in the evening. Quite so – in our solution Saturn set one hour after the Sun and was therefore only visible in the evening, at dusk. This is not the case with the Scaligerian solution, which claims that Saturn was visible all night long, once again rendering Ptolemy's report of evening observation inappropriate. In case of Venus, the concurrence between our solution and the Ptolemaic description is also excellent ([p7], pages 454-467).

On the other hand, we did not need our coverings solution to be the only one possible, given that there are no ideal solutions for the problem in question, seeing as how in case of Mars, for example, the term "covering" stands for the proximity of 15 arc minutes between Mars and the star in question. Such proximity is not actually a covering, strictly speaking. Moreover, Mars did not cover the star under consideration at any point on the historical interval. Therefore, the issue of a unique solution becomes rather vague. The ideal solution remains nonexistent; as for near-ideal ones, they multiply as the Ptolemaic stipulations are made less rigid. We pointed out this fact in [p7]; it is also confirmed in the article of A. A. Venkstern and A. I. Zakharov.

However, the comparison of our planetary coverings solution to the Scaligerian solution, which A. A. Venkstern and A. I. Zakharov perform in their article, compiling their results into a brief table ([p19], page 117), is perfectly unjustified and even erroneous. The table claims that our solution "doesn't satisfactorily correspond to the circumstances of the coverings", whereas the Scaligerian solution "describes said circumstances in a more or less satisfactory way" ([p19], page 117). This is incorrect, and we have just cited a number of examples to prove the opposite. This issue can be studied in greater depth in our previous books and also in the present book, see Chapter 10.

Also, the claim made by A. A. Venkstern and A. I. Zakharov about their discovery of five further series of datings for the coverings that conform to Ptolemaic descriptions just as well as the one discovered by the authors of the present work strikes us as rather doubtful. Of course, considering the lack of an ideal solution, one might well debate whether or not one of the possible solutions is "better" or "worse" than another. Nevertheless, we feel obliged to point out that none of the solutions cited by A. A. Venkstern and A. I. Zakharov in their table on page 119 of [p19] correspond to the visibility conditions as mentioned above ("in the morning", "in the evening" and "at dawn", as per Ptolemy's indication). This is already obvious if

we consider the "solar elongation" column of said table ([p19], page 119).

As for our solution, which was also included in the table of A. A. Venkstern and A. I. Zakharov, one cannot help noticing the strange misprint in the Jupiter line. In the second column it is indicated that on the day when Jupiter covered the star the end of the night (the dawn) came at 4:36 local time, whereas the fifth column of the same line states that the Sun rose at 4:58 local time. However, the Sun rises about an hour after dawn, or the end of the night. This fact is known to A. A. Venkstern and A. I. Zakharov perfectly well, and they say so clearly on page 117 ([p19]). This is also obvious from all the other lines of their table ([p19], page 119). Why, then, would the Sun rise a mere 20 minutes after dawn that day?

This might be a random misprint. However, A. A. Venkstern and A. I. Zakharov comment the line in question as follows: "The time of Jupiter rising is indicated up to 6 degrees over the horizon. The dim star delta Cnc cannot be seen due to its proximity to the Sun" ([p19], page 118). In other words, A. A. Venkstern and A. I. Zakharov point out what they believe to be a defect of our solution, according to which the covering as described by Ptolemy "could not have been observed anywhere in the world" ([p19], page 118). They make a similar claim concerning Saturn on the very same page. Both claims of A. A. Venkstern and A. I. Zakharov do not correspond to reality. However, the misprint in their table as mentioned above leaves one with the impression that the situation is exactly as they describe it, since it is presumed that the covering of the star by Jupiter could only be seen 20 minutes before sunrise (when the star could not be made out on the brightened sky by the observer, naturally enough – no covering could take place under such circumstances). In reality, calculations (such as one can make with the aid of the simple computer program "Turbo-Sky", quite convenient for approximated calculations, for example) demonstrate that in our solution the maximal propinquity between Jupiter, Saturn and the respective stars took place one hour before sunrise in case of Jupiter and one hour after sunset in case of Saturn. Therefore, the coverings could be observed perfectly well, albeit for a short time. This is precisely why Ptolemy refers to observations carried out "at dawn" and "in the evening".

However, the possibility of real star covering observation in our solution is an issue that has no principal significance for either the New Chronology in general or the dating of the Almagest. The matter is as follows: seeing as how the solution in question isn't strict (no ideal coverings), there is a theoretical possibility that the coverings were calculated and not actually observed – in other words, we are not dealing with de facto observation reports, but rather the results of mediaeval calculations, which, obviously enough, lacked sufficient precision.

Let us now consider the article of Y. D. Krasilnikov under the following title: "On the Planetary Coverings of the Stars in Ptolemy's Almagest" ([p19], pages 160-165). In this article Y. D. Krasilnikov considers the Scaligerian solution of the problem of dating the coverings. In particular, he is forced to acknowledge the fact that the covering of a star by Venus that Ptolemy claims to be "exact" is a mere case of 12 arc minute propinquity in the Scaligerian solution ([p19], page 161). This hardly classifies as an "exact covering", which makes the solution defended by Y. D. Krasilnikov obviously far-fetched. There are quite a few other such instances. For instance, Ptolemy emphasises the fact that the star's covering by Jupiter was observable at dawn, whereas in the solution of 241 B.C. (defended by Y. D. Krasilnikov) Jupiter and the star it approached was visible almost all night long – for circa five hours ([p19], page 163). This also makes the Scaligerian solution rather farfetched. Ptolemy's indication concerning the evening time of the observed proximity between Saturn and said star becomes suspended in midair, in a way, insofar as the solution favoured by Y. D. Krasilnikov is concerned. In this solution Saturn was visible all night long. The perplexed comment that Y. D. Krasilnikov made in this respect, with rather irrelevant complaints about the deficiencies of the software that he had used for the calculations of the covering results, can be read on page 163 of [p19].

Incidentally, Y. D. Krasilnikov, likewise A. A. Venkstern and A. I. Zakharov, is for some reason convinced that it is important for the New Chronology and for our dating of the Almagest that the covering solution that we suggest be unique. This is not so – the very existence of a coverings solution that is in good correspondence with our dating of the Almagest cata-

logue suffices, even if said solution is not the only one possible. See [p7] for more details.

At the end of his article Y. D. Krasilnikov makes a comparison of our solution and the Scaligerian solution that he favours, trying to prove ours to be "much worse". Y. D. Krasilnikov makes his primary emphasis on the fact that we did not account for the solar longitude as indicated in the Almagest by Ptolemy in his discussion of planetary star coverings. Our reply is as follow. Firstly, solar longitude isn't part of the observations used by Ptolemy. In the Almagest, longitudes are calculated for each covering. Secondly, it is easy enough to realise that the solar longitude is the very same thing as a date, albeit transcribed in a different manner.

Since the only version of the Almagest that we have at our disposal today is the one that was fabricated in the XVII century, it would make no sense to expect that Scaligerian editors of the Almagest would fail to render such simple things as solar longitudes to a desired Scaligerian date. There is no doubt about the fact that all such data were meticulously brought into correspondence with the Scaligerian version. This is exactly what Y. D. Krasilnikov discovers today, studying solar longitudes as indicated in the Almagest and believing to be "reconstructing" the true dating of the Almagest. In reality, he merely reconstructs the opinion of the XVII century hoaxer editors of the date in question. This opinion is known to us perfectly well, at any rate – every textbook contains Scaligerian datings. It is most peculiar that Y. D. Krasilnikov should fail to comprehend this – apparently, he has never bothered to read our book ([p6] and [p7]), which contains a detailed explanation of all the phenomena mentioned above.

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