

Again on the Subject of the *Mingtang* of the Empress Wu

Over the past few years a remarkable number of reviews of my *Mingtang and Buddhist Utopias in the History of the Astronomical Clock*¹ have been published. This comes somewhat as a surprise, since I thought that the fact that the book covers various fields of research would discourage reviewers, who are usually specialized in one field or another. It is then with pleasure that I would like to discuss a few questions raised by the reviewers, hoping that they and other readers will allow me to profit from their criticism.²

It is very rare nowadays for an author to learn something from the reviewers of his book. Reviewers have developed the bad habit of simply praising or condemning, without making any real contribution to the advancement of knowledge. What is worse, praise or condemnation is often determined by criteria which have nothing to do with scholarship. To take just one example, there is a person in Italy who for years has been publishing amazing numbers of so-called reviews without ever contributing a grain of real scholarship. It is not the case that he is one of those who has largely contributed over the last thirty years to lowering the level of research in our universities. In sharp contrast to this, Paul Pelliot usually brought to bear in his

¹ Serie Orientale Roma, Vol. LIX/Publications de l'École Française d'Extrême Orient, Vol. CXLV, Rome and Paris: Istituto Italiano per il Medio ed Estremo Oriente/École Française d'Extrême Orient, 1988.

² An answer to Jacques Gernet's review, which was published in *T'oung Pao*, Vol. LXXVI, Nos. 4–6 (1990), pp. 337–40, appeared in 1992; see "On the Subject of the *Mingtang*", *Monumenta Serica* 40 (1992), pp. 387–96.

reviews all the richness of his objectivity and criticism. His book reviews fortunately did not suffer the limitations of space so frequently imposed today by the editors of journals, and would cover a considerable number of pages. A still unpublished review he wrote on Johannes Nobel's "Kumārājīva" (Berlin 1927, pp. 206–33) is probably longer than Nobel's work, as it covers thirty-two pages in Pelliot's handwritten original.³ This may be too extreme an example; still, it would not be so bad if one tried to attain to Pelliot's model, in terms at least of objectivity and honest criticism. I am fortunate that I did learn from the reviewers of my book. Although their reviews are rather short, objectivity and honest criticism are there—with one exception, however, which will be shown below.

Since they are by no means secondary, let us begin with some questions of readability, form, and style. Wendell L. Minnick observes, "This volume is for the specialist and cannot be recommended as undergraduate reading. I strongly recommend this book for graduate libraries as a primer for continued research into this phenomenon."⁴ The same idea is reiterated by Henrik H. Sørensen, when he writes, "Although Forte's study in my opinion is not suited for undergraduate courses, it certainly is essential for the serious researcher working with science, the legitimization of government, and imperial and Buddhist architecture during late seventh century Tang."⁵ For one as strongly convinced as I am that a research book should and can be written in a clear and attractive style in order to draw the attention of the wider public of non-specialists, to learn that the book is not suitable even for undergraduate students is indeed sobering, as it means that I completely failed in my attempts to write it in a clear and simple way. I must say that it is not easy to satisfy both the scholar's and the general reader's requirements, but the effort must be made, and I will try my best on the next occasion. For the time being I can only draw some consolation from the contrasting opinions expressed by Hubert Durt, who observes that my approach is reminiscent of both Sciascia and Eco;⁶ by Gernet, who thinks that my research is conducted "à la façon d'une enquête policière" (p. 347); and by the same Sørensen when he says that the book is written in a way "which gives the reader the impression of being on an expedition with the author" and praises my "general discursive style" (p. 108). If this is so, unless we think that undergraduate means under-developed, I do not see any reason why the book should not be suitable for them.

While one critic points out that "most readers would have been content with a much shorter discussion and an appendix for those interested, spelling

³ Pelliot's review will be published as soon as possible by the Italian School of East Asian Studies in Kyoto.

⁴ *Journal of Chinese Religions*, No. 18 (Fall 1990), p. 199.

⁵ *Studies in Central & East Asian Religions*, Vol. 4 (Autumn 1991), p. 110.

⁶ *Cahiers d'Extrême Asie*, No. 5 (1989–90), pp. 433–4.

out the details and alternate [*sic*] possibilities”,⁷ another suggests that the “highly elaborate annotation” in some cases should have been integrated “into the main text” (Sørensen, p. 109). The real problem is that research should be done in a way that all interested readers, present and future, can verify the relevant sources and judge at any given moment the correctness of the author’s interpretations and commentary. This must be done not only in order to be honest and clear, but also in order to save other researchers’ precious time, so that they may trace back the quoted sources and judge the correctness of the author’s conclusions. To give just the conclusions without explaining why and how the author reached them may appear time-saving both for the author and the reader. In reality this is quite often misleading. Readers usually tend to accept the conclusions, especially if they trust the author, and may eventually base their own research on such conclusions without checking. This can be disastrous, as any scholar, even the most scrupulous and brilliant, can make mistakes. Given that certain cumbersome parts cannot be eliminated, where should they be placed to be convenient to the reader? Personally I prefer footnotes and appendixes rather than endnotes, so that the interested reader can easily skip them and the interested one can have them at hand. There is, however, no doubt that all efforts should be made to present both the maximum level of the necessary information at a maximum of readability.

The majority of my reviewers have stressed the importance of the historical, social, and religious context of the appearance in the late seventh century of what could have been the first astronomical mechanical clock in the world. Company (p. 92) underlines that in China “the clock’s invention was ideologically linked to Buddhist eschatological hopes for the coming of Maitreya, the Future Buddha”. Gernet (p. 338) makes a very interesting observation when he points out, “Il est remarquable que ce soient les doctrines eschalologiques qui aient fixé une origine aux temps et fourni un comput à partir d’une date définie. La chose vaut aussi bien pour les religions monothéistes nées au Proche-Orient que pour le bouddhisme indien.”⁸ The reviewers, in substance, have no objections to my suggestion that the *dayi* (Great Regulator) built in the *mingtang* complex in the late seventh century may have incorporated a clockwork-driven armillary sphere and a clock. A cautious attitude that I perfectly understand and share (as I hope I have shown in the book itself) is expressed by Cullen, who after paying attention to Daoxuan’s seventh century description of “what sounds very much like a clock with hour-announcing jackwork” and to the connection with Maitreya’s descent, concludes, “. . . one feels compelled to leave the window open just a little. After all, there is the well attested case of Yixing’s astronomical clock

⁷ Robert F. Company, *Journal of Asian History*, 25.1 (1991), p. 93.

⁸ Both Christopher Cullen (*Bulletin of the School of Oriental and African Studies*, Vol. LIV, Part 3 (1991), p. 611a) and Sørensen (p. 108) also pay attention to the connection with Maitreya.

in 725, not long after the time of Empress Wu” (p. 611a).

One exception is constituted by John S. Major, who clearly is not interested in the historical and ideological questions discussed in the book. While accepting my main thesis that the *mingtang* was transformed from a Confucian to a Buddhist symbol of sovereignty, he prefers to concentrate his attention on the clock question. Basing himself on the relevant evidence I produced, he agrees that “a cast bronze *dayi* (‘big mechanism’) was associated with the *mingtang-tiantang* complex along with a bell, a drum, and statues of the twelve double-hours”. He thinks, however, that there is “no basis for supposing, as Forte does, that it incorporated a clockwork-driven armillary sphere, or that it was ‘clock’ in any sense at all”. Noticing that I defended Joseph Needham’s position that the clock was invented in China rather than in Europe centuries later, he puts forward serious reservations, remarking that “this was so in a manner of speaking”.⁹ Other parts of his review insist on these points, showing a lack of intellectual honesty that would not deserve any answer. I think, in fact, that the job of a responsible reviewer is, above all, to be honest. When this fundamental condition is fulfilled, the job becomes much easier. However, I decided all the same to send a communication to the Editor of the *Journal of Asian Studies*, in order possibly to stimulate some constructive response and also to try to determine the real reason why he was so upset by my suggestion that Empress Wu’s *mingtang* complex included an astronomical clock. In my communication I remarked, “It is always the same story. Every time the clock question arises, there are always some people who condemn without appeal the one who has dared to furnish them with some hitherto unknown historical evidence for reflection, and with some reasons ‘to suspend absolute disbelief’ ... concerning the hypotheses put forward. One has not yet forgotten the attacks to which Joseph Needham was subjected precisely on this question of the Chinese clock, and that the last (to my knowledge) of his detractors, David S. Landes, some years ago even charged Needham with the accusation that in his researches ‘his wishes became affirmation’.”

The communication was published in the February 1993 issue of the *Journal of Asian Studies* (Vol. 52.1, pp. 107–8), and was followed by Mr. Major’s reply (pp. 108–9). I observe with pleasure that, contrary to his review, in which no constructive criticism was noted, he has now made some feeble efforts in the right direction, although they are based as yet on a biased reading both of my book and of my communication to the Editor. I note that Mr. Major now believes that my book is not “pernicious”, that my suggestion that Empress Wu’s *mingtang* complex included an astronomical clock “is not unreasonable”, that he agrees that the clock made its first appearance in China, and that he does not doubt my sincerity in defending Needham’s positions (although I do not understand why Mr. Major is so concerned with the

⁹ *Journal of Asian Studies* 51.3 (August 1992), p. 644.

advancing of my agenda; it seems to me normal to base myself on sound and solid scholarship: why should I follow, in matters of Chinese clocks, Cipolla, Landes, or one of their unknown epigones?).

Curiously, Mr. Major also declares that he is not an enemy of Joseph Needham. I did not say in my communication that he was. Surprised by the tone of his review, I only asked myself whatever the reason could be for Mr. Major's attacking in such a way my suggestion that Empress Wu's *mingtang* complex included an astronomical clock. I thought that it might be due to previous disputes centred on the clock question and that Mr. Major's opinions might have been different from those expressed by Needham (by the way, is it not possible to have different opinions without being enemies?). His reservations on the nature of the Chinese clocks indicated, anyway, that he was not so near to Needham's positions. Then, it is not a question of "demonology", but only to know clearly what one's opinions are. Now, Mr. Major implicitly seems to distance himself from David S. Landes, and seems to wish to get near to Needham when he agrees that the clock made its first appearance in China (I congratulate him for his rapid abandonment of reserve; in his review, in fact, he had written something different). He even contrasts Needham's scrupulous regard for evidence with my "willingness to rely on supposition and wishful thinking". In other words, Mr. Major prefers to apply to me—in a more lenient version, I must say—the charge by Landes against Needham that his wishes become affirmation" (I suppose that I have to thank Mr. Major for his relatively lenient treatment). It will be noticed, however, that he carefully avoids pronouncing an opinion on the thorny question of the escapement and, although he agrees that the clock made its first appearance in China, he does not specify whether he also agrees with Needham that the first clock was the one made by the Buddhist monk Yixing in 725. Considering that Landes practically ignored Yixing's clock (see p. 12, note 25, of my book), it would be interesting to know—if this is not asking too much of him—Mr. Major's opinion on the question.

He contends, moreover, that "the several clock designs independently developed in China and Korea were technological dead ends. They are fascinating in their own right, but they had little or no influence on the development of clocks in Europe". In other words, not only does he attack Needham's idea of "stimulus diffusion",¹⁰ but he also affirms that Chinese clocks were dead

¹⁰ The "stimulus diffusion" hypothesis is defined in many of Needham's works. "The firm conviction of the prior successful solution of the problem elsewhere" may have "led European scholar-artisans to solve it themselves in a different manner" (see some references in my book, p. 11, note 22). Needham has always been conscious of the speculative nature of this idea. On the theoretical level it has exactly the same validity as the contrasting hypothesis that Chinese clocks had no influence on the development of clocks in Europe. In other words, both hypotheses have still to be proven. The difference is that Needham is conscious that his is a mere hypothesis, his detractors not only have the arrogance to present their own hypotheses as demonstrated facts, they also deceitfully try to invalidate his research method. Besides the instances I mentioned in the book, see Landes, 1983, p. 23: "Perhaps we can imagine ...".

ends. This, of course, also in order to show that they could not influence European clocks in any way whatsoever. This time he is clearly on the side of Landes, as he merely echoes the title of the first chapter (“A Magnificent Dead End”) devoted by Landes to the Chinese clocks. This shows at least that I was not very wrong with my guess that his opinions might have been similar to those of Cipolla and Landes. This is confirmed by his comment, “What then is one to make of Mr. [*sic* —*Ed.*] Forte’s passionate defense of Needham against all criticism?” This question may be intended in the sense that I defended Needham’s hypothesis of the “stimulus diffusion”, but may also imply that I read Needham’s work uncritically, always defending him passionately from any criticism.

Should I be said to be defending the hypothesis of “stimulus diffusion”, I must state clearly that I never defended it in my book, as it was beyond the scope of my study. I was simply not concerned with it. I only defended *Needham’s right to make hypotheses*—among others the one on “stimulus diffusion”—against the opprobrious attempt, just because of a difference of opinion, to show that he was writing “optative history” and that he confounded reality with fantasy. Now, little by little, Mr. Major’s position on the question is emerging and I have the impression that he has not told the whole story. If Mr. Major agrees with Landes, he should say so freely. I do not understand his reticence, and I do not think that Needham himself would be upset by Mr. Major’s position. Nor would Landes—I suppose—be shocked were he to realize that Mr. Major has a different opinion from his own. Besides, nobody would object if he formulated personal opinions different both from Landes’ and from Needham’s.¹¹

Should, instead, Mr. Major imply that I read Needham’s work uncritically, always defending him passionately from any criticism, this would be unfair or would mean that he has not read my book with attention. I see no reason why I should not express my appreciation for a man whose work I deeply respect and whose intellectual honesty should be a model for any scholar. Needham has been in many ways one of my ideal teachers and I think that I would never have been able to write the parts of my book concerning the astronomical clock without studying his *œuvre*. This does not mean that I defend him from all criticism, but simply from bad or malevolent criticism. Curiously, Mr. Major seems not to have realized the extent of my criticism towards Needham when new evidence authorized me to do so.¹² Had I to

The historian may imagine what he will. Needham himself was persuaded.” The way adopted by Mr. Major to invalidate my method is the same already adopted by Cipolla and Landes to invalidate Needham’s (it should not be too difficult to learn who Mr. Major’s models were).

¹¹ As to the question of the “dead end”, Prof. Jacques Gemet held a position similar to that of Landes on p. 339 of his review of my book. I do not think it necessary here to repeat my opinion, stated on p. 390 of my reply in *Monumenta Serica*, q.v.

¹² On the astronomical clocks being related to the more general question of the *mingtang*, on Needham’s not giving any importance to the fact that the clock entered history as a Buddhist clock [pp. 24–5], on the Buddhist conception of time [pp. 26ff], on the “bells that ring by

attack him rudely in order to attract the attention of certain readers?

Another reviewer is more objective when, while remarking that I was reverent to Needham, shows awareness of the criticism also contained in the book. He only regrets that I did not use “more space on a general reappraisal of Buddhism’s contribution to the development of science in China. This would have provided a good opportunity to meet Needham’s overly negative attitude to Buddhist science. Especially *in lieu* of the material brought forth and discussed by Forte, it would have been a relatively easy task for him to correct Needham’s misconceptions.”¹³ To Sørensen I have only to say that mine was not a book devoted specifically to the subject of Buddhism and science.

It is also curious that Mr. Major cites Needham’s rejection of one of his initial hypotheses as a example of the contrast he wishes to set up between Needham and me. It seems to me natural that one should renounce one’s own hypothesis if new proofs invalidate it. Mr. Major may rest assured that I will gladly welcome any proof he can produce that my hypothesis about what the *dayi*, bell, drum, and statues of the twelve double-hours may have been, although he vaguely admits that “possibly this amounted to some sort of automated time-announcing device”. The purpose of my book was to provide some hitherto unknown evidence for reflection. As to the hypothesis, I had the duty (as well as the right) to put it forward simply because it happens that nowhere—unfortunately—is it clearly written what the Great Regulator actually was. Possibly it is a bad hypothesis, but I eagerly await (as I clearly wrote in my preface, p. xi) any evidence from other scholars that might correct it. The Great Regulator, regardless of all my efforts, may very well still be a mysterious instrument, but we have to do all we can in order to try to understand what it was. I did what I could to reveal the mystery, and I have shown that the reasons which made me think of it as an astronomical clock were not few, and that all concomitant evidence allowed me to put forward such a hypothesis. I have not, of course, exhausted all possible sources, and am certain that the best criticism may come from the result of further inquiries rather than from prejudices. Even limiting ourselves to the subject of the *mingtang* in the late seventh century, there is still much material that awaits careful study. So, if Mr. Major expects that I shall change my opinion just because he has a different, unsupported opinion, he is completely wrong.

Some supplementary information may be useful to evaluate the issue. I have corresponded since October 1983 with Joseph Needham on the question of the Great Regulator. In his letter of 9th January 1984, he wrote to me, “I see no reason why the *ta i* or ‘Great Regulator’ ... built by the Empress Wu Tsê-Thien around +691, could not have been a mechanical clock.” Needham

themselves” [pp. 44–6], etc.

¹³ Sørensen, *Studies in Central & East Asian Religions*, p. 109.

then informed John M. Combridge of our correspondence. Combridge then wrote to me in May 1984, and we corresponded frequently thereafter. For the International Exposition, Tsukuba, 1985, Nagai Masaru built from Combridge's drawings a working, timekeeping waterwheel which was presented at the Exposition as having been of the time of Empress Wu. Although I was somewhat surprised to see so soon before my eyes (if I remember correctly, at the History Pavilion, Japanese Government Exhibit) a working reproduction of what I was suggesting in my as yet unpublished book, and thought that Combridge was probably more optimistic than myself, I took it as a sign that our correspondence had convinced Combridge that my suggestion was reasonable.

Needham's above assertion was based on my correspondence, not on the manuscript, which I had not sent him because it was not yet finished. Although I have no information on his present opinion, I would like to quote here the relevant parts of a letter Needham wrote to me on 25th February 1986, after reading the completed manuscript I had just sent him:

Since receiving your packet, dated 9 January last, I have had time, I am glad to say, to read your most interesting monograph entitled *Ming-tang and Buddhist Utopias in the History of the Astronomical Clock*. I should like to congratulate you very much on this achievement. ...

As regards the criticisms of Cipolla and Landes, I am sure you did well to go into them. I feel that their attitudes rather belong to what we like to call the "Department of Face-Saving Re-definitions". In other words, if you find something which answers to the definition of a device which "cuts up time into very short, equal intervals", and thus slows a wheel so that it keeps time with the diurnal revolution of the heavens; and this is much earlier than the verge-and-foliot escapement of Europe—the people will say, "Ah, yes, but the only true, the only veritable, escapement is the 'verge-and-foliot escapement'—thereby saving the prestige of Europe. It is exactly the same with the origin of the axial rudder. If you show (as I did) that this began in China in the +1st century, then of course the Europeans say, "Ah, yes, but the true axial rudder is the stern-post rudder attached by pintle and gudgeon." So again Europe's prestige is saved. ...

In any case, I would continue to believe that the "water-wheel, link-work escapement" was first developed either by I-hsing in +735 [error for +725], or by the engineers of Wu Tsê-Thien some thirty or forty years earlier....

I found what you had to say about Buddhism and time very interesting ... , and that the connection between Buddhism and clocks was perhaps no coincidence. ... I also found most interesting what you had to say ... about the self-sounding bells; I can well believe that this might have had some influence on the terminology introduced when the Jesuits brought their clocks in the +17th century. Finally, what you had to say about Tao-Hsüan (d. +667) and his description of the Jetavana cloister, with all its instruments for knowing the time ... , was really fascinating.

If I may be allowed to make any comment upon the monograph, I would like to say that it would greatly gain by a series of enumerated conclusions right at the end. It is after all rather hard for a reader to bear in mind all that has gone before, and I think he would be greatly helped by some series of conclusions along the lines I suggest.

With best wishes, and renewed congratulations,

Ever yours,

Joseph Needham

Probably Mr. Major will never go so far, but he can rest assured that, as far as I am concerned, this has absolutely no importance. I will not be offended if he is of a different opinion. His arrogance both in the review and in the replies is, however, regrettable.

Let me deal with another point which has aroused the interest of the reviewers, the question of whether a 300-metre tall wooden pagoda could have been built in Tang China. Cullen writes, "A pagoda-like structure of oak or similar wood is unlikely to fail by simple crushing under its own weight until it has reached over 2,000 feet in height. Failure as a result of wind-induced swaying would surely have come much earlier. But then, as Forte points out, the tower in question was indeed destroyed by strong winds in 690 or 691." Mr. Major is to be acknowledged for having contributed in his replies, contrary to what he had done in his review, some arguments about the question. "Cullen is correct," he writes, "in stating that a building of the dimensions specified would not have been crushed by its own weight. However, wood is highly vulnerable to shearing. ... An uneven settling of the structure's foundation ... of only a few degrees, under the weight of tens of thousands of tons of wood, would bring the whole crashing to the ground; if the foundation miraculously remained true, shearing forces created by even a moderate wind would doom the building long before it attained its planned height." In other words, contrary to Cullen, who concluded that perhaps "we must be prepared to 'leave open a window on the impossible'" (although he could not "easily believe" in such a height), Mr. Major has no doubt that the window must be closed.

I think that the above question in itself deserves all our attention, and I hope that other specialists will also express their opinions. However, clarification may be needed in order to avoid being misunderstood—the reader may think that in the book I reached the conclusion that the *tiantang* pagoda was 300 metres tall. I never wrote such a thing and was careful throughout the book to avoid stating the height of the *tiantang*, for the simple reason that I had no precise idea of how tall it could have been. What I knew for certain was only that it was extraordinarily high. That is why I had recourse to expressions like "incredible height" (p. 15) and preferred to say that the builders "raised it to the greatest height that building techniques and the material chosen (wood) permitted, the might of the universal reign de-

manded, and immense wealth afforded. The result was the *tiantang*” (p. 23). In fact, a special section is devoted to the “Height of the *tiantang*” (pp. 75–9), wherein I wrote, “It is true that the ‘thousand palms’ stated by Zhang Zhuo could simply be a way of expressing an extraordinary altitude, but we have not the means to be certain of this” (p. 79). I added that I was “fully aware of the exceptional size or rather the improbability of these measures (p. 79, note 88). As simple clarity is never enough, in the conclusion I expressed again my incapacity to solve the question, saying that “although there are good reasons for supposing that it could have been a thousand palms (three hundred metres) in height, one cannot be certain of this. Let us assume that it was nearer the thousand palms recorded by Zhang Zhuo (660–733) than the hundred palms of other sources” (p. 253). When I suggested, dealing with the similar case of the pagoda of the Yongning Monastery, to “leave open a window on the impossible” (p.134), I wished to express my incapability both of rejecting Yang Xuanzhi’s assertion and of solving the question. In other words, I left the question open for inquiry: one cannot demand from an author that in a single book he should resolve all the questions of the world. I must add that, five years after the publication of the book, I still have no idea of how high the *tiantang* could have been, also for the simple reason that I have made no further inquiries on the subject, involved as I have been in other projects.

Cullen’s and Major’s answers, even allowing for their provisional nature, do not completely agree with each other. It must be clear, however, that even in the case that the final response would be that, yes, sixth or seventh century China could raise in her capital Luoyang a 300-metre tall wooden pagoda, this would not automatically mean that the *tiantang* was that high. Let us remember that the only source to state such a height is Zhang Zhuo, and that we have no certain proof that he was not using the expression “thousand palms” just as a way of conveying the idea of extraordinary altitude. It is true, however, that we are not completely deprived of clues for figuring out the height of the *tiantang*. Some sources I have produced (for example, p. 89, the one specifying that the head of the statue originally destined for the *tiantang* pagoda was 83 palms, that is, *c.*25 metres, from the sinciput to the chin), if better exploited than I was able to do, will certainly give good results. Some useful suggestions are given by Sørensen (p. 109), but my feeling is that we are still far from a careful inquiry into the great architectures and the great statuary of East Asia. In discussing the issue of the great pagodas of the Yongning Monastery or of the *tiantang*, both in Luoyang, it may be helpful not to forget that during the eighth century (the *tiantang* was built in the late seventh century) the isolated and infinitely less rich and powerful Japan, which had only recently come into the limelight of history, could raise, not one, but two pagodas simultaneously, both around 100 metres tall, and that these pagodas stood for a long time.

I would like to conclude the present note with a comment on Hubert Durt’s

review in the *Cahiers d'Extrême Asie*, No. 5 (1989–90), pp. 433–4. Due to the great esteem in which I hold him for his extraordinary generosity and wide cultural interests, due, above all, to our long-standing friendship, I was particularly touched by his lucid, objective, and synthetic review. There is a minor point, however, that is curiously—and purposefully, I think—incorrect and needs to be amended for the sake of truth, although I must say that I was much amused by the funny way in which it was expressed. Durt writes that I recognize that I was better able to elucidate certain passages of the *Commentary on the Great Cloud Sutra* (presented by Faming and others in 690) thanks to the much wider information I had at my disposal during my apprenticeship at the Kyoto Hōbōgirin Institute. In reality I wrote only that it would not have been possible to finish my book in a short time “if I had not had the rare privilege of staying in Kyoto as a member of the E.F.E.O. for most of the period of my research” (p. xiii of the Preface). At the time, in fact, I was a member of the École Française d'Extrême Orient sent to Kyoto in order to work on the Hōbōgirin project, exactly as Durt was (and still is). Hubert Durt has passionately devoted most of his life to the Hōbōgirin and he must have been vexed at the fact that I did not mention it. In apologizing for not mentioning it, I also wish to assure him that I am not offended by his goliardic “revenge”, especially if it can be of some help to the Hōbōgirin cause.

Antonino Forte
School of East Asian Studies (Kyoto)