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The Birth of Early-Medieval China

Studies in a Global and Interconnective Context

Sometime around spring of 2007 I started an informal discussion with our then coeditor John Kieschnick about creating a special issue on early-medieval China. With his input, I presented ideas to *Asia Major*'s editorial board and to the then Director of the Institute of History and Philology (Academia Sinica) Wang Fan-sen. Several board members responded with cogent and useful suggestions (Nicola Di Cosmo, Susan Naquin, Stephen F. Teiser, Chen Jo-shui), as well as non-board members Nancy Shatzman Steinhardt and David Graff (with apologies for anyone left out). The overall reaction from all parties was swift and positive. I thank Director Wang for his support and the brand new director of the Institute, Huang Ching-shing, who has carried the project forward, and Liu Shufen, as well, for timely and detailed consultation.

First of all, I defined the time-frame as the period from about 220 to 589; I proposed no particular theme – only the intent to showcase recent directions in research. The result, it was hoped, would be to clarify for readers the nature of those research directions and to assess what is needed for a still under-developed area and how it can utilize advances that have taken place especially in China. We asked twenty-five scholars to submit proposals, which we said we wuld winnow down later in order to create a broad distribution of types of research, regions of China, and time frames. Eighteen proposals were received, from which thirteen were chosen that formed the distribution. The thirteen were submitted to our blind-review process. We were prepared to publish all thirteen thanks mainly to the commitment of Wang Fan-sen and the Institute; but after the review process and after some withdrawals for lack of ability to deliver on time, we had our final group. The result is what you see here – nine articles that demonstrate precisely why writing on early-medieval China has reached a point at which it is poised to provide new and definitive shapes.

Moreover, it is with great pleasure that I announce that the special issue of *Asia Major* that you are now reading will have a second incarnation in a slightly different form. Later this year, the majority of

the present contents will come out as a book from the publisher E. J. Brill (Boston and Leiden). In the Brill version the bibliographical lists that we present, below, will be expanded and recategorized; they will provide a useful tool for scholarship in the coming years. In addition, the Brill book will contain approximately three new articles that offer research into government institutions and society. This hopefully addresses the well-recognized dearth in those areas and will take advantage of archeological developments in China.

What about the notion of a "birth" of an area of study? I believe that the word describes accurately the situation as it exists. Naturally, already in premodern China there was an influential gestation period involving thought and study devoted to China's own early-medieval times, even though "research fields" did not exist per se. Such fields were more often than not determined simply by the several famous writers who had become entrenched by early-Tang times as the peerless classical narrators of pre-Han and Han eras and societies. The voices of those great Han historians became canonical and to some degree drowned out later historians' voices, for example those of Chen Shou, Hua Qiao, Sima Biao, Gan Bao, Pei Songzhi, and Shen Yue. The early-Tang project to capture Jin and Nanbeichao history for immediate Tang purposes resulted in something of a flattening, especially with the lack of any thorough historical approach to institutions and the often clipped editing of *Jinshu* (in contrast to the inclusive pasting of Pei and Shen). Although the talents of Sima Qian and Ban Gu had provided a literary model for history writing, nonetheless by the end of Eastern Han skilled literary minds began to be consumed by other styles and genres of written expression and literary work; and concurrently archives suffered depletions. In Western Jin times there was a short resurgence of the Sima–Ban style of historiography but political disasters in the early 300s choked off those developments. In my opinion, of course simplifying the picture, it was the failure of the Western Jin to endure peacefully and thus support a renewed historiography that spelled disaster for the way early-medieval China was accounted and later perceived. Whereas from the 170s to about 300 court-oriented scholars maintained the mission to write treatise-like materials on scholarship and literature (including palace book-holdings), court rites, law, money and food production, and an interest to convey and edit documents on court policies (one can cite such scholars as Cai Yong, Qiao Zhou, Du Yu, Sima Biao, Xun Xu, Zhang Hua, Zhi Yu, Jia Mi, and Lu Ji), the more encompassing concerns of literati after about the 280s and continuing until the end of the Nanbeichao period were funneled

into literary products and editing, and into philosophical, social-ethical, and religious expressions.

With perhaps the exception of Shen Yue, from about 300 to 600 traditional historiography dissipated where it mattered – that is, in products that would create a full and forceful "standard history"; and early-medieval China would be increasingly treated as a time of poetic or philosophical revolutions (or failures) and a time during which there was a confusing cascade of rulers and would-be rulers. The tide turned with the high-level scholarly conferences and research sponsored by the Sui and early-Tang courts, for example the the work of Niu Hong and, later, Li Chunfeng, and the outstanding treatises found in *Wudai shizhi* and *Suishu*. Unless I am misreading the words of my colleage Hou Xudong's Introduction, above, I believe that my opinion finds support (and of course the problem was noticed by premodern Chinese writers like Gu Yanwu and modern ones like Pi Xirui). Hou stated:

> The literary sources of the Wei-Jin and Nanbeichao make it seem that metaphysics, Buddhism, and Taoism dominated the realm of thought, and that Confucianism found itself in a "slumbering" condition. Many in modern times believed this impression, and thus they paid scant attention to Confucianists and Confucianism. But studies of state ritual, as just mentioned, give us another picture. Although the study of the classics received a severe drubbing, still it occupied the core of political thought and was developing its strength with *Zhouli* as the central ritual teaching.

One can interpret the above as indicating, at another level, just how *buried* and greatly in need of attention extant primary sources are. They have indeed slumbered for a long time, as other aspects of early (not just early-medieval) China have developed. From about 1000 to 1200 and then from 1600–1930, there were resurgences of antiquarianism (ancient objects and commemorative inscriptions) and regionalism. In fact, Gu Yanwu, with his years invested in local dialectology, poetics, and ancient history, was one of the first to apply "genre" names to problems in early-medieval China like the critical changes during the "Zhengshi era 正始," "the consultations of the Pure Stream 清議," and so forth. Nonetheless, by the time Western-style archeology appeared in a large way in China in the 1910s, and items and tombs of the 200–600 period were in fact becoming known, leading writers were often otherwise engaged – filtering historiography through a series of lenses that spoke more about their contested positions in what one might call *Lebensphilosophie* debates than about rebuilding histories of early institutions. Through such May Fourth intellectual concerns about literature

and modernity came hugely talented revisions of early-medieval Chinese thought (I am thinking of Fung Yulan) and philosophy (here I cite Tang Yongtong for his captivating, local-history-imbued understanding of the relationship between third-century *qingyi* and *xuanxue* on the one hand, and Buddhism and so-called Neo-Daoism on the other). But ultimately, while archeology since the 1950s has compeletely rewritten the history of 400 BC to 50 AD China, the same cannot be said of early-medieval China, even though in fact its archeology has developed at a good clip since the early 1980s. The time is now ripe for an application of archeology equal to that surrounding pre-Han and Han history.

The story is different concerning literature and religion. These two areas are in fact what have utterly represented and defined early-medieval China for the past seventy-five years. Early-medieval literature was helped by the fact that its belles lettres were at the center of Chinese scholarly attention even as early as the sixth century and only intensified, all the way into Qing times. Ever since the spurt of interest in Cao Cao brought on by Mao's campaign to glorify this early-medieval general, one can say that historical studies of early-medieval China in modern times and using modern historicist terms began and ended with the Caos – their declamations, strategies, and poetry (although to be fair, literary scholarship in the past twelve or so years has moved on into later centuries). Literary and religious studies (the latter spurred by the work of the French school of sinology) have spawned subfields that integrate poetry, aesthetics, and philosophy in exciting ways.

In about 1984 or 1985 Denis Twitchett called together at Princeton a conclave of writers who could potentially form the missing early-medieval volume of the *Cambridge History of China*. He recognized the achievements of recent scholarship on literature and religion, and he wanted to see if institutional history could measure up. Although the written contributions that eventually arrived were predictably excellent in the areas of literature and institutional Buddhism, nevertheless because a Stanford conference of 1980 was still unpublished (see below) and there were no institutional networks set up for early-medieval scholarship, many of the hoped-for *Cambridge History* contributions either never came or else had various problems. Twitchett always warned that nothing would come of early-medieval China studies until archeology, institutional histories, and a political overview could mature. He was right.

Although sadly enough the volume of the *Cambridge History* may never be published, at least what we can celebrate now is the maturation for which Twitchett waited. In the U.S. before 1980, specialized

interest in the early-medieval period had already taken slight root from European stock: one thinks of the work of Peter Boodberg (especially in historiographical and linguistic analysis) and Wolfram Eberhard (his broad range of sociological, cultural, local, and even astronomical interests). Albert Dien followed along these lines generally, and has given us a leg-up in archeological interpretations. Ever since Boodberg and Eberhard, and under the impact of Dien's work as well as via the leadership of literary scholars who unite history and literature, for example, Jean-Pierre Diény, Donald Holzman, and David R. Knechtges, Western researchers on early-medieval China have improved their language capabilities and their exposure to scholars in China and Taiwan.

Since 1990 there have been successful conferences and symposium volumes, the most important ones being (in chronological order):

Albert Dien, *State and Society in Early Medieval China*, 1990 (stemming from the 1980 Stanford conference);

A special issue of *Acta Asiatica*, 1991 (in English, but presenting Japanese scholarship);

Patricia Ebrey, Scott Pearce, and Audrey Spiro, eds., *Culture and Power in the Reconstitution of the Chinese Realm, 200–600 AD*, 2001, stemming from a conference, Dialog with the Ancients, at Western Washington University (Bellingham, Wash.) in 1996;

Anette Juliano and Judith Lerner, eds., *Nomads, Traders and Holy Men along China's Silk Road*, 2002; this emerged from a conference at the Asia Society, New York City, in November of 2001;

Wu Hung, ed., *Between Han and Tang: Visual and Material Culture in a Transformative Period*, 2003 (published in China).

What the list demonstrates is the global aspect of our studies and the importance of making connections among the findings of literature, institutions, and archeology. Dien's volume presented the work of one Chinese and one Taiwan scholar, as well as the well-established Australian researcher Jennifer Holmgren. The Chinese articles required translations (which I believe were performed by Dien). The Ebrey/Pearce/Spiro volume contained a definitive and sensitive introduction, the gist of which in fact dovetails with my own thoughts – namely, that archeology, linguistic and ethnographic advances, and recent Chinese scholarship have created a critical impetus for early-medieval China studies and that the key to it all is the interconnections among these interests. The last two volumes mentioned, above, have continued in this vein, bringing together scholars from Europe, the U.S. and China, and, in the case of Wu Hung's volume, providing translations and abstracts in Chinese and English. The Juliano/Lerner work is especially good in

bringing out European research with a focus on the non-Han societies and polities at the Central Asian and northern edges of China.

Speaking of global aspects, as is commonly known, Japan has produced a deeply talented pool of researchers since the 1950s. Their first *Problematik* involved the sorting out and dating of the very idea "early-medieval" China, as well as raising a strong debate about the nature of local organization and control in those centuries, and the relationship of the new aristocracy to those developments. The *Acta Asiatica* volume, above, is still an excellent starting point, bringing all those debates and other research up to the late 1980s. Yet, in the past decade Japanese scholars have continued to excel, focusing on the intersection between social, political, and economic forces during early-medieval China. Increasingly they are writing about the era's cultural and local history. For example, on the links between political power and local society, one should look into the following: Yasuda Jirō's 安田二郎 *Rikuchō seijishi no kenkyū* 六朝政治史の研究 (Kyoto daigaku gakujutsu shuppankai, 2003), Kubozoe Yoshifumi's 窪添慶文 *Gishin Nanbokuchō kanryōsei kenkyū* 魏晋南北朝官僚制研究 (Kyūko shoin, 2004), and Nakamura Keiji's 中村圭爾 *Rikuchō Kōnan chiikishi kenkyū* 六朝江南地域史研究 (Kyūko shoin, 2006). Nakamura's work is noteworthy for its chapters on material culture; and for deep interactions between culture and politics, see Watanabe Yoshihiro's 渡邊義浩 *Sangoku seiken no kōzō to 'meishi'* 三國政権の構造と名士 (Kyūko shoin, 2004). Finally, just as Liang Mancang, Liu Jing, and others mentioned in Hou's Introduction have been researching early-medieval ritual systems, so has Kaneko Shūichi 金子修一, in *Kodai Chūgoku to kōtei saishi* 古代中國と皇帝祭祀 (Kyūko shoin, 2001), especially in relation to dynastic legitimacy. Finally, a word must be said about Russian scholarship, which has developed important work especially on the Russian coastal and border zones in the Pacific area, touching on Bohai and their precursors and utilizing archeological fieldwork; see especially the works of Alexander L. Ivliev, Evgeniya Gel'man, and Nikolai N. Kradin.

The upcoming Brill version of our *Asia Major* special issue will contain important Japanese and Russian works in its bibliographic lists, focusing on economics, politics, military, and rites, with special interest in recent archeological findings.

Thus far, I have not thanked my coeditor Hou Xudong, formerly of the Chinese Academy of Social Sciences and now Tsinghua University. The reason is that a special kind of thanks is due. First of all, I should humorously apologize to Hou for perhaps having saddled him with more work than he had hoped for, but he gave unstintingly and hap-

pily of his time, especially by making global connectivity come alive. Prof. Hou introduced several Chinese scholars' names into the mix of proposed authors. Then during meetings in Beijing in 2008 and 2009 I received his consultations about the translations I was making and the editing process concerning the three accepted articles from Beijing: his own and those of Li Yuqun and Zhang Zong. But even more important was the help to the overall field that Hou Xudong has supplied in the form of his own Introduction, just above.

Hou Xudong's Introduction, "Recent Research in China on Wei-Jin Nanbeichao History," is a trove of information, and is a beacon for the future fifty years of work. Given the powerful "global connectivity" that CNKI and other information sevices offer, I am sure that readers of this issue of *Asia Major* will start ordering the books and articles he has listed. Moreover, Hou's remark that I quoted, above, is a stepping-off point for discussing a particular goal both of this issue and the upcoming Brill book. It is this: although we are fortunate to have fifty years of breakthroughs and insights into early-medieval literary and religious ideas and practices, and our special issue presents three such articles, we must now attend to the weak twin that lags behind in the borning – namely, nuts-and-bolts philology and interpretations of documents both from the center and from non-Han ethnic groups, local economies and social patterns, and Confucian rites and state institutions. Hou shows us that in China the work is ongoing and significant in just these ways. It is telling that at the head come "Rites and Law." Hou perhaps sees the great value in taking up these institutions at this point in time. After decades in the West *and in China* of rather an unfortunate turn away from such studies as overly power-oriented or Confucian (occasionally demonstrating the benighted opinion that Confucianism is no longer relevant or is boring), we are seeing that for this period of time (as Howard Wechsler and David McMullen discovered about the Tang) the human struggles invested in rites and law, and as well in court scholarship, are utterly revealing. We not only learn about the rise and fall of careers and dynasties, but given that early China was so heavily centralized even when the center was wobbly, the intellectual and political ramifications of rites and laws and official scholarship can aid in our cultural studies.

From Hou Xudong's Introduction we learn about interconnections among areas of studies, as well. For example, in the summary of research into the bureaucracy, Hou discusses attempts to solve the way naming patterns of offices in North China stemmed from tribal naming; he touches on studies that attempt to compare or relate military offices

and jurisdictions to civilian bureaucratic forms. We learn of the failure of recent scholarship to treat comprehensively the economic history of early-medieval China, but at the same time he describes the current makeup of research that uses findings of archeology and will rebuild this area of study. Once again, the pointers reach out to other fields, for instance the way the study of Ming-Qing economic history helps to context early-medieval research, and the way Western-developed fields such as spatiality (Skinnerian systems) and environmental studies can hold great promise for early-medieval China studies that will involve technical work directly in Chinese locales.

All of this now leads me to comment briefly about the *Asia Major* articles. Taking up the cue in Hou's Introduction, I wish to emphasize global and intellectual connections in the nine articles that we have before us. Our issue by no means is a complete review of the major fields of study, but our strength is in the talents of the participants, their use of international scholarship, and the unspoken ways in which they go past their own fields to touch upon others. I have placed three articles under the rubric "social patterns, family, and social activity." Hou Xudong's article on kinship patterns and notions of kinship shows us how much this sociological topic is imbedded in the close reading of standard histories, and that it has roots going back to Qing scholars, experiencing a renaissance, so to speak, in Republican China with much influence from early sociologists in the West. The article will have profound effect on the way we see social history, but specifically on how Chinese saw their own social structures and the changes they tried to imagine or institute in the third and fourth centuries. Andrew Chittick's article is a perfect example of international cooperation, in that he uses appropriate links with historians of the ancient West and the "new" cultural studies, as well as with other China-study fields, such as Chinese religions. Keith N. Knapp easily demonstrates the value of philological work; he provides well-shaded insights about levels of status in fourth-century China in a deep and convincing reading "through" Shen Yue's late-fifth-century history. Kate Lingley's piece is another example of a philological reading, but the word should be changed to "philography," since her forte is to read everything there is to be learned from the graphic and pictorial layout of an intentionally designed space. I feel that Lingley's method will be a leading one in the future, and her use of Chinese colleagues' interpretations supports her contribution all the more.

I call our next three articles "visions and insights." Much work in the U.S. occurred in 2008-09 in the area of visions and envisioning,

and there is a curiously close contact between, on the one side, Wang Ping's subtle analysis of the quite Madhyamika-like philosophy of the void as found in Xiao Tong's appreciation of Tao Yuanming, and, on the other, Michael Puett's analysis of the interiority of Laozi-inspired visions. Granted, my claim here is an indirect one and one to which the authors may even object, but I believe that the link helps us understand a three-century development in writings about imagination and, to be blunt, about what went on inside people's heads. It circles back to a wider understanding of the changes in literary vehicles and genres, the increasing inclusion of notions of transformation, revelation, and a certain kind of "mind-only" autonomy claimed by certain thinkers and social actors and that defied everyday logic. If Puett's and Wang's articles are pointing to at least some aspects of the Daoist mind, then those aspects help explain how China went from the essay and the poem at the end of Han to the sastra and tetralemma negation of the fifth and sixth centuries. David Knechtges' piece is on the "essay/poem" (the earlier) end of the spectrum. His method points to global scholarship through a full and relevant articulation of the best of current Chinese and Japanese scholarship. For decades, his numerous readers have come to expect from him the utilization of every possible compendium and florilegium to analyze the ways that a genre or an oeuvre was shaped through time. Here, Knechtges makes a strong point about a relatively obscure poet's, Ying Qu's, impact upon the politics of his time. We get cogent arguments about Cao-Wei factionalism, facts about Wei-court architecture, and the history of early-medieval hunts.

The interconnectivity of our current research into early-medieval China is reflected in recent "discoveries." An obvious interconnection is that Li Yuqun's own published articles are referred to in the research deployed in Kate Lingley's and Zhang Zong's pieces. Yet at a more subtle level, we have interconnectivity of method and scope. In fact, Lingley's essay might have been placed here in the "discoveries" section: it demonstrates why border-crossing research is important. Zhang's article provides another such case; it could have gone into our "visions and insights," for after all, he shows us that engravers in Northern Qi were concerned about creating something special out of older genres of sutra and sastra translation; style of text and intellectual formulation were high on their minds. Further research may tell us more about the small links that Zhang raised that have to do with "visions" of the end-times (*mofa*) and reconfigurations of the tortuous filiations of Mind-only Schools in Buddhism.

As early-medieval China emerges, it must have deep roots in the academy but it also should relate to general readers. One envisions a review in a major New York intellectual sheet, in which configurations of *yi*-societies are linked with new translations of poetry, inscriptions, and Buddhist vinaya to make a brand new type of picture. The previously obscured world of China from 220–580, can provide a fascinating landscape for the kind of reader who is already interested in, for example, the Hellenistic Mediterranean, pre-colonial India, or the Eastern Roman world of cultural contacts.

Early-medieval China needs painstaking work in material culture, philological unlocking of standard-histories and of belles lettres, and the new discoveries from epigraphy and archeology. In the past, a convenient but weak caricature of early-medieval China has always come down to: 1. establishment of "new" religions, 2. influxes of non-Han, and 3. everyone's internecine battles. Those are not untrue, but the more we reveal patterns of hierarchy and production in small locales and real policy struggles at the capitals (and the reasons *why*), then the more we can demonstrate both the truthful and troublesome parts of the old caricature.

Right now in U.S. universities, except for literature, we have no established and ongoing programs in late-Han–Tang studies. (N.Y.U. has created the Institute for the Study of the Ancient World, see <www.nyu.edu/isaw/>, but there is a long way to go before the China side gels.) U.S. and European colleges have a superior record for the past twenty years in providing survey courses in Asian history and excellent courses in Chinese language training and literature. Early Chinese art, archeology, and religion, too, are in a heyday. Far from having "programs," even U.S. institutions that simply teach both history and literature of the post-Han through Tang period are a mere handful (to add historical linguistics and material culture would reduce the total to perhaps one or two). Moreover, those offering both history and literature seldom treat the early-medieval period per se. It is most unfortunate that U.S. academic institutions are entranced by the notion of a market-like operation that floats along under the care of CEO-like administrators. Instead, we need scholar-leaders who understand developments in international research, interconnections, border-crossing faculty, and courses for Ph.D.'s; visionary leaders of libraries and digital databases; and we need research journeys, visiting lectures, and conferences. China, of course, is a different story; and in fact, one easily envisions the bulk of publications, conferences, and ongoing programs as located there in the long-term future, and not in the U.S.

If early-medieval China studies are to live into adolescence in the West, then the West will need the following kinds of activity, even if piece-meal:

1. an increase in courses on historical linguistics (sadly lacking in the U.S.), from which can grow such specialties as Buddho-hybrid Sanskritics, proto-Türkic, and the various Silk Road languages;
2. individual scholars' translations of standard-history treatises and/or monographs on the formation of those treatises, and translations of other primary as well as secondary literature on history and institutions. This is a somewhat old-fashion endeavor, but one that must be revived; it will continue the work of Etienne Balazs, Ho Peng Yoke, B. J. Mansvelt Beck, and Yang Lien-sheng.
3. Grants for long research visits in mainland China, where the bulk of freeflowing study and discussion is underway on archeological work and institutional history.
4. Individual scholars must contribute time to making English translations of selected Japanese, Russian, and Chinese articles, and then seek out publishing venues for them in U.S. and European journals, and also in special issues like the one Brill will produce this year. The journal *Early Medieval China* has been an excellent help in this regard, but others can follow suit. Perhaps even more important is for Chinese publishers to gather up translations of the best of U.S., European, and Russian works on early-medieval China and get them translated into Chinese. This ought to go hand in hand with increased teaching of English as an academic language in China. There exists an imbalance, and it would be better if Western-Chinese communication reached a parity, in order to further the enormous potential for international exchange of ideas.
5. A necessary tool is to have ongoing series that deliver archeological reviews and bibliographic reviews as *Early China* does in their issues, and as we have done here and will do more of in the upcoming Brill version.
6. The financially secure universities (anywhere in the world: and probably we should be looking to China) might sponsor two specific projects: A) a four-year editorial project that will bring together authors to make a two-part Volume 2 of the *Cambridge History of China.* It is my opinion and that of others, I am sure, that through the present birth of early-medieval China studies in the West, such a volume will impel scholarship and writing to higher levels; B) set up a database for early-medieval Chinese lives and careers, primary texts, visual objects, and administrative geography, such as the ongoing projects at Harvard are doing in relation to later Chinese history.