

REPORT ON:
HRUBÝ, JAKUB, 'THE SYSTEM OF TITLED NOBILITY OF THE WESTERN JIN
DYNASTY (265-316 AD)'
DOCTORAL SUBMISSION, UNIVERZITA KARLOVA V PRAZE, 2012.

Overview

The author sets as the scope of his enquiry the system of titled nobility developed under the Western Jin state (265-316 AD). The thesis divides into four main chapters; a brief introduction and a conclusion are also offered. It opens with a chapter, explicitly comparative for the most part, that seeks to define the character of the medieval Chinese nobility as it relates to European analogues. The author offers translations of key noble titles and, in so doing, includes a sketch outline of their functions. The chapter concludes with a brief discussion of sources. The second chapter traces the development of the system of ennoblement from Han times (206 BC – 220 AD; the author supplies an incorrect date for the establishment of this state), through Cao-Wei (220-265), and into the first year of Western Jin rule in 266. The account here follows a broadly chronological structure. Its effect is to trace the immediate antecedents of the system implemented by the Western Jin founder, Emperor Wu. The third chapter adopts a thematic arrangement. Different aspects of Western Jin's ennoblement system – the administration of the fief, the location and size of the apanage, the ennoblement of different classes of individual, and so on – receive treatment in turn. The final chapter accords particular focus to two features of the ennoblement system: inheritance; and the policy of special grace, through which exceptions to the standard ennoblement system were made. The topic of Western Jin's system of titled nobility is an important one: as the author notes in his conclusion (p.304) - though perhaps he could have made more of this point in the main body of his thesis - Western Jin practices set many of the terms of debates about noble status in the later medieval period. A close examination of the system therefore has the potential to contribute usefully to our understanding of medieval society and statecraft.

Strengths

The present thesis offers just such a close examination. It is clearly the product of a thorough and meticulous reading of a key source for this period, the seventh-century standard history of the Jin state, *Jin shu* 晉書. The author draws a wide range of examples from all sections of this work. Such attention to detail is increasingly rare in modern Sinological studies. Yet these are the foundations on which all else rests, and so the present author is to be applauded for taking the time and the care to work through *Jin shu* in this way.

The result is that, at its best, the thesis offers a sharp picture of the structure of the Western Jin titled nobility. It also sheds valuable light on the significance of individual titles and of what it meant for an individual to receive a particular status or fief, or to be transferred between one title and another. On p.53, for example, we have an insightful analysis of the unwritten implications of third-century statesman Sima Zhao's gradual move up the political and social hierarchy as he transferred between noble titles. A few pages later (pp.56-9), we are treated to an extended section on the manipulation of peerages for political ends, also under Cao-Wei rule; a further

example of this insightful style of reading appears for the same period on p.62. The author offers other such examples later in the thesis, as we move into his analysis of Western Jin. The political machinations of the early medieval imperial court come into view when we have such information to hand. To date, though, Anglophone scholars have paid relatively little attention either to this area or to the corollary theme of the import of certain official postings and transfers. (Incidentally, it would have been interesting to have heard more from the author on the actual and ritual connexions between noble titles, official ranks, and bureaucratic functions. Was there a meaningful pattern of correlation between these areas during Western Jin?) The author seems to have read and referred to all of the major contributions to the meagre body of Anglophone scholarship on these themes. There is a far greater corpus in Chinese and Japanese, though here the issue of the divergence between a title and its implicit significance often finds itself passed over in favour of accounts of institutional function. (The major Chinese contributions receive attention in this thesis; Japanese scholarship has passed entirely without notice, though.) Here, then, is one of the main areas in which this thesis might make a contribution to existing scholarship.

Weaknesses

Despite the evident care that has gone into the research of this thesis, it is badly let down by its final presentation. In this, it falls some way short of a professional level of finish. I shall point up five areas – one general, four more specific – for particular attention in this regard.

First, the thesis ‘feels’ long. This is not an indication of its actual length – its 360 pages are about average for a doctoral thesis – but rather of the impression that it leaves its readers. The author treats many points ponderously. Though close reading is undoubtedly the main strength of the piece, as I have suggested above, the sheer number of examples cited in support of any one argument is unnecessary and interferes with the pace and progression of the larger case under construction. For one example among many of this tendency, see the ten-page sub-section on pp.164-75 that deals with the auspicious naming of fiefs: ten examples (some of which stray into over-interpretation of the original text) are adduced in support of a relatively straightforward point. A single, well-chosen example for each argument would have resulted in greater economy and in a tighter piece. That, I suggest, is one cause of the ‘feeling’ of an over-long and slow-moving study. That ‘feeling’ also traces to a series of problems that will receive treatment in what follows.

Second, I shall question whether or not the structure of the thesis best serves its analysis. Its first part is based on a chronological structure, which is marked as extending from Han rule to 266 (though in practice, the author takes us into the 270s); its second part switches to a thematic structure, in which different aspects of the Western Jin titled nobility receive individual treatment. The potential value of this thesis, to my mind, lies above all in its analysis of the diachronic developments of the Western Jin system. Yet by adopting a mixed structure, the author has not drawn out these developments as fully as he might have done. A purely thematic approach would have enabled him to trace each aspect of the system of titled nobility through time, both in the centuries that preceded Western Jin and during the tumultuous events of that state itself. An exclusively chronological approach would have pointed

up diachronic change even more clearly. It would have enabled a stronger and more explicit examination of developments that occurred as a result of the so-called Rebellion of the Eight Princes in the late third and early fourth centuries (which, to my mind, indicates more than anything else a profound failure of Western Jin's system of titled nobility); or of the barbarian insurrections of the early fourth century; or of the first fall of the imperial capital in 311; or, expanding the chronological scope of the thesis, of the fall of Western Jin in 316 and the subsequent establishment of Eastern Jin. (I should note in connexion with this last point that the chronological boundaries of the present thesis are in any case loosely applied. A move into the 270s on p.86, for example, does not strictly conform to the section's purported focus on Sima Yan's accession in 265-6 and would have merited a new sub-section. More broadly, and more seriously too, despite claiming in his title a focus on Western Jin, the author frequently draws on examples from Eastern Jin for his analysis - see pp.158-9, 162, 183, 195-7, 259-64, 282-4, 287-9, 299-301.) Should the author choose to develop the research of the present thesis, I shall offer two possible suggestions for revisions of structure. First, that he limit more tightly his scope to Western Jin and remove those passages that deal with events after 316. Here, a more strictly chronological structure would serve him better than the mix of chronological and thematic organization on which the present thesis is based. Second, and I think in many ways preferable, would be to extend the present chronological scope to include in his analysis Eastern Jin and thus cover all the material contained in *Jin shu*. This would allow for a comparative study of the two polities and an analysis of the profound ways in which the composition and social image of the titled nobility changed with the territorial shift from north to south in the early fourth century. A final note on structure: the author would have greatly eased his readers' task at times by dividing the existing sections more finely, into a series of shorter sub-sections.

Third, those sections of the thesis that focus on Western Jin comprise long sections of quotation. These are archetypal of the thesis' main strengths and weaknesses as a whole. On the one hand, they offer evidence of an admirably close reading of primary sources. On the other hand, they are long and almost entirely redundant *as quotations*. On the whole, they fail to have argumentative force as quoted pieces of text and serve only to embellish a point already made. (I shall note here that the author has a habit of straying from the original text in his translations. In particular, he adds to his translations much that does not appear in the original. I assume that the intention here is to explicate detail for the reader. Such additions are only rarely necessary, though, and at times they run the risk of over-interpretation.) Rarely are such quoted passages followed by linguistic analysis or close critical scrutiny of the sort offered, exceptionally, on pp.87-8. As a result, it is not always clear why the author has chosen some quotations, and not others, to support his argument. On p.269, for example, why are the two quoted passages selected as particular examples of the use of the appellation 'Heir Apparent', when literally hundreds of uses of that term appear in *Jin shu*? And what does either offer as a quotation that might not have been conveyed by a simple footnote reference to the relevant passages? In sum, for all its demonstration of close reading, the mass of quoted evidence does not always appear to have been critically sifted. Many of the quotations on offer in this thesis are not integral *in their quoted form* to the argument being made: remove them, or alter their form, and the case would still stand. Why, then, include them as quotations? Where necessary, the author might have included such evidence in the form of a tightly-written paraphrase, drawing out only the most salient items from the source text, rather than include the

sprawling wording of the original. (At worst, we are given *both* a quotation *and* a paraphrase, which supplies the whole account all over again without contributing new analysis. In so doing, the author doubles the length of the example.) In most cases, a simple footnoted reference to the relevant passages of evidence in the source text would have sufficed. At a guess, I should imagine that the thesis might have been reduced by a third or more simply by following one of these two measures. The result would have gone a long way to reducing the impression of length.

Fourth, sections of narrative are loosely written and tend to meander. Repetition is often a problem here, too. This makes itself felt above all in the conclusion, which simply summarizes what has gone before without attempting to add a new, overarching perspective on the major themes of the thesis or to draw together its main arguments. Greater economy of narrative throughout the whole piece would have reduced its length - both actual and perceived - and, as important, would have maintained its pace. That would serve to deliver the reader more efficiently to the interesting sections of analysis and argument. As it stands, description threatens to overwhelm analysis and the overarching argument of the thesis risks becoming obscured as a result. On a smaller scale, several of these narrative sections contain sweeping statements that lack both precise references and footnoted documentation. This lack makes itself felt when the author engages in unsupported speculation about, for example, the motivations, thought processes, possible views, and likely conduct of historical actors. On what foundations does he develop such speculations? I am not suggesting by this that this sort of speculation is wrong; simply that, if it is to be convincing, it needs tighter documentation.

The fifth, and in many ways the most basic, of these points of criticism is the numerous linguistic and editorial errors that litter the present thesis. Given the present predominance of Anglophone culture in Western Sinology, I am sure that the author is judicious in his attempt to produce a thesis in English. The fruits of his labour will no doubt reach a wider audience as a result. This is important with the study of early medieval China – a field in need of international exposure and expansion. Yet, even allowing for the fact that the author has taken on the challenge of writing outside his native language, certain basic standards of professionalism must apply. The author's English is clearly of an advanced level: evidence of this suggests itself in a particularly strong lexical range, though against this the consistent failure to make appropriate use of such basic items as the direct article is surprising. Overall, though, the author is linguistically well-placed to meet what I have just called 'certain basic standards of professionalism' when writing in English. He does not meet them here, though. I have rarely read a typescript with so many linguistic, typographic, transcription, formatting, and other errors. There are inconsistencies between the use of American and British English, and between full-form and simplified Chinese characters – sometimes even within a single paragraph. Items from the bibliography do not receive reference in the main body of the thesis. And so on. I have included a small sample of representative pages from the typescript, chosen entirely at random, to show what I mean here. Such errors and inconsistencies suggest to me a long and careful process of research followed by a rushed write-up of the results of that research. That is a real shame: the quality of the final presentation seriously damages the reader's impression of things. At worst, it impedes understanding of the author's argument. The clumsy use of dependent clauses presents particular problems in this regard. I had to read some passages several times over before I felt that I had

understood the thrust of the point under discussion. The ‘flow’ of the piece is broken as a result. Here lies one of the contributing factors to the impression of excessive length that I note above.

Those are the weaknesses of presentation and professional finish. A number of methodological shortcomings also make themselves felt in the course of the thesis. I shall outline here only those I deem to be recurrent or of particular important to the author's larger argument. If required, I should be happy to supply further comments on individual points of detail to either the committee or the author, or both.

1. On pp.27-31, the author introduces the main primary sources for his thesis. ‘We are more or less dependent on official court or governmental compilations known as the dynastic or standard histories’, he suggests (p.27). He therefore limits himself to the standard history of Jin, *Jin shu*, and to *San guo zhi* 三國志, a history of the preceding period of the Three Kingdoms. Seventeenth-century scholar Wan Sitong's 萬斯同 *Li dai shi biao* 歷代史表 also appears in one footnote and in the bibliography (though the author's name has been incorrectly transcribed here). Yet the claim of being limited to these primary sources does not ring true to me. The dozens of fragmentary histories of Jin and its rival Sixteen Kingdoms, which fed into but were not wholly incorporated by the seventh-century *Jin shu* (the author is aware of the existence of these sources – see p.29); myriad anecdotes from contemporary and near-contemporary literary sources; poetic compositions; collected writings (*wen ji*) and jottings (*bi ji*) by men of the Western Jin; genealogies and so-called ‘family instructions’ (*jia xun*); a growing body of archaeological and epigraphic evidence; fragmentary evidence preserved in such later compendia as *Yi wen lei ju*, *Chu xue ji*, *Tong dian*, *Tai ping yu lan*, *Ce fu yuan gui*, *Zi zhi tong jian kao yi*: all of these might have offered alternative perspectives on the composition and, importantly, the perceptions of the Western Jin titled nobility. They would have had the obvious advantage of representing a contemporary take on the topic at hand; by contrast, *Jin shu* was only compiled under Tang, three centuries after Western Jin's fall. (Incidentally, on pp.28-9 it seems odd to have used Wilkinson, as well as Ng and Wang – both overviews rather than specialist works on medieval historiography – as authorities on the standard histories, when Denis Twitchett has done detailed work on the Tang context for official historiographic compilation.) All this begs the obvious question: why did the author choose *Jin shu* over other possible candidates for analysis of the period? Why reject the rich evidence that this other material might supply? And how does the author's choice to limit himself to *Jin shu* skew our final picture of the system of the Western Jin titled nobility – do we end up with a seventh-century understanding of this system rather than a third/fourth century one? A stronger source-critical discussion is essential here. Perhaps greater clarity over the scope of the research question would also be beneficial: is this intended as an account of the Western Jin titled nobility or of that titled nobility *as it appears in the standard dynastic record*?
2. On the same issue of sources, there is a need for greater clarity on the nature of some of the key secondary sources used. There are two points to make here. The first echoes a criticism already raised: at a number of points, the author makes use of secondary sources that are not the most appropriate tools for the

task at hand. The *Cambridge History of China* is no longer the most up-to-date account of the Qin-Han transition (p.32), for example. Rafe de Crespigny's monumental biography of Cao Cao would have been the obvious place to turn for information about this key individual, rather than Wang Zhongluo's survey history of the medieval period (p.42). I have already discussed above the preference for the surveys by Endymion Wilkinson and Wang and Ng, over the likes of Twitchett's specialist study of Tang historiography. Works with a bearing on the subject of medieval elites by Nicholas Tackett (albeit with a focus on the late medieval period) and of early medieval relations of vertical patronage by Andrew Chittick are absent. And so on. Similarly, in a number of cases, the author deploys what the Chinese call 'grandson quotations' 孫引 – quotations casually reproduced from quotations in intermediate sources, which quite possibly appear in modified form and shorn of important textual features – rather than trace them back to their original locations (p.79, fn.139 is one example of a broader practice). There are obvious critical pitfalls here. I am afraid that in both cases the choices of sources that the author makes at times seem arbitrary and, again, rushed. They do not appear to take account of the best available scholarship on a particular scholarship but simply of the works closest to hand at the time of writing. Second, there is a need for the author to define his work more precisely in the context of the secondary sources that he uses. True, the author attempts early on to differentiate between his own work and that of previous scholars. But he does so only in broad terms. How does the basic argument of his thesis actually differ – apart from in length and number of examples given – from one of his major and oft-cited secondary sources, Yang Guanghui's 楊光輝 *Han Tang feng jue zhi du* 漢唐封爵制度? This is not to say that there is no difference – there is, to my mind – but rather that the author need to clarify the ways in which he had built upon Yang's and others' researches and modes of analysis. A so-called 'literature review' would not be necessary to achieve this; a clearer expression of the author's critical engagement with secondary scholarship would do the job.

3. As a corollary, a failure to consult Japanese scholarship on this topic is a major lacuna in the present thesis. As I have suggested above, Japanese scholars have undertaken painstaking research into the structures of the medieval nobility. Their work far exceeds scholarship in Western languages. It is roughly comparable in quantity, and often superior in quality, to Chinese scholarship on the topic. Consultation of this body of secondary scholarship would have greatly profited the present thesis.
4. Despite the inclusion of a section devoted to definitions, the author assesses medieval Chinese noble titles and functions largely in comparative terms: technical issues of translation are uppermost here. This comparative approach, and the adoption of European terminology that it produces, is interesting yet full of problems for a precise analysis of early medieval China. Greater clarity is needed in marking out which observations concern the European system and which the Chinese and, by extension, which apply to both. Do the definitions of nobility as a legal grouping and of aristocracy as a sociological grouping that were worked out in the European context (p.10) have purchase in the medieval Chinese context, for example? This is particularly important when we hear that 'nobility as a hereditary quality did not exist in China'. So why use the English term 'nobility' in the medieval Chinese case? To my mind, a

lack of rigour in such instances risks undermining the potential value of this thesis – the location of the precise functions of the titled nobility within a specific historical period (even within Chinese imperial history, the composition of the titled nobility was by no means constant). The author is clearly sensible to such problems (p.13). Yet when he starts to consider such themes (on p.12, for example), he does not see them through but instead falls back on conventional terminology. The author would therefore have done well to elucidate the nomenclature of nobility and aristocracy in the context of his main source, *Jin shu*, of contemporary Jin discourses, and of the legacy that the Jin imperial state inherited. What specific terms were used for the concepts of nobility and aristocracy and how did their linguistic boundaries shift over time? In short, a fresh contribution to the (sparse) philological research that surrounds this topic would have been useful in the immediate context of the present thesis. It would also have been of great import to research on the later medieval period. In this spirit, I would also strongly urge against the use of *Encyclopaedia Britannica* as a source for defining such central terms as ‘clan’ and ‘lineage’ (p.24).

5. At several points in the thesis, the author drip-feeds his readers the historical background against which the Western Jin system of noble entitlements and enfeoffments was implemented. The result is a piecemeal account of the larger picture of Western Jin political and social history. This fragmentation makes itself felt, for example, in the treatment of the Rebellion of the Eight Princes on pp.207-44: the complex political maneuvering that took place during this period of internecine conflict appears only in episodic fashion. I suspect that it will be hard for readers without a detailed prior knowledge of the major events and actors involved - it is hard enough even for those of us with some background in the period - to follow the thrust of the author's analysis and argument. Above all, a clear outline of such contextual information will be crucial to the author's attempts to identify the underlying significance of the system of titled nobility at any particular point during Western Jin rule. A coherent sketch overview of the major events of the dynasty early in the thesis would have gone some way to remedying this problem.
6. By contrast with this, the author offers *en bloc* a useful overview of the Han and Cao-Wei precedents for Western Jin's system of titled nobility. Yet it would have been a service to the reader if he had added regular indications of when Western Jin practice converged with or diverged from that of earlier dynasties. He would also have done well to include regular comparison with canonical ideals, particularly as they were understood in the third and early fourth centuries. Such measures would have afforded a clearer sense of Western Jin innovation - how its system of titled nobility built upon and altered what had come before it, and with what effect. The Han precedent of conferring dignities on the basis of merit is traced on pp.99-100; it lends useful clarity to the immediate analysis. Yet, unfortunately, this is an exception rather than a rule.
7. Analysis of regional variations between fiefs, their relative sizes and significance, and their distribution through Western Jin territory – an important point that receives particular attention on pp.117-64 – would have been greatly aided by the provision of maps. I would also advise that the several tables that appear in this section find a place in an appendix, rather than in the main text, to avoid disruption to the flow of the argument. With

this section as a whole, I wonder if there is a need to address each province, or geographical grouping of provinces, in turn? Here, again, is a case of the author's deployment of numerous examples, when a few well-chosen cases would have made the point. The author might have achieved greater economy, without sacrificing acuity of analysis, by identifying only the main criteria that lay behind the selection of a fief's location, as well as the broad significance of the relative position of a fief in Western Jin territory. Pertinent examples might have been cited for each criterion, without the need to itemize in turn the particular circumstances of each region.

8. The issue of political legitimacy plays a crucial role in the system of noble entitlement and, as a result, it receives frequent mention in the present thesis. Yet the author's handling of legitimacy is simplistic: there is an assumption throughout that Western Jin held legitimacy; what concerns the author is merely how it achieved that legitimacy. I should like to see things in a more nuanced light, though. Jin's legitimacy, like that of so many polities of the early medieval period, was hotly contested. Similarly, the more limited reference to Cao-Wei as the 'link in the chain of legitimate succession going from the Han to the Tang' (p.42) – a chain of transmission that was likewise subject to debate. Both cases received particular attention from the seventh century on – in the context of *Jin shu*'s composition, in fact. These discussions have ultimately shaped the way in which we now understand the legitimacy of these states. They surely deserve consideration in the context of the present study. A collection of these legitimacy debates appears in Rao Zongyi's 饒宗頤 *Zhong guo shi xue shang zhi zheng tong lun* 中國史學上之正統論 (Hong Kong, 1977). The author would do well to consult them, with a view to lending greater nuance to his discussions of this theme, so crucial to any treatment of the period between Han and Tang.
9. The author fails to trace a number of allusions in translated passages of quotation that would have shed valuable light on the nuances underpinning such texts. One example among several will make the point: repeated allusions to the canonical *Shang shu* in an imperial edict cited on p.67 contain important implications for claims being made of political legitimacy. These pass without notice in the author's translation or subsequent analysis, and the argument is weaker as a result. Similarly, relevant references to individuals and places that appear in translated passages would help the reader to grasp more fully the significance of those passages and their contribution to the argument of the thesis.

Recommendations

It is my recommendation that the candidate be allowed to defend this thesis. Despite several serious methodological and other shortfalls, which I have outlined above, the thesis represents a careful and close reading of a source, *Jin shu*, that is central to our understanding of early medieval China. More particularly, the thesis addresses a theme that has received inadequate attention in earlier scholarship in Western languages. The detailed research that underlies the present thesis and its potential contribution to the field are, to my mind, worthy of the award of a doctorate.

Yet, as I have also suggested in my comments, the final presentation of this research falls some way short of professional standards in the field. Since doctoral work is, in

part, a process of training individuals to meet those professional standards, I would strongly urge that the present thesis be passed only on condition of certain revisions. At the very least, the numerous linguistic and editorial errors that I outline above should be addressed. They number in their thousands. For reasons of convenience, I have not included an itemized list of all such errors with the present report. Instead, I have marked them up on a hard copy of the thesis. I should be happy to supply both the committee and the candidate with a copy of this annotated text to facilitate the process of making suitable corrections.

A handwritten signature in black ink that reads "Mark Strange". The signature is written in a cursive style with a long horizontal line extending to the right from the end of the name.

Mark Strange
Canberra, 26 June 2012

