A Conceptual Appendix to "The Politics of Historical Knowledge"

Article *in* Journal of Asian History · January 2019

DOI: 10.13173/jasiahist.53.2.0309a

1 author:

Tay Jeong

A Conceptual Appendix to "The Politics of Historical Knowledge"

Tay Jeong*

Introduction

My original article, which was published in volume 52.1 of this journal, presented three broad descriptive arguments about the study of the historical geography of Old Chosŏn: overconfidence, convergence, and external influence. I think what has agitated many people about these broad descriptions is the worry that they may be used to derive unacceptable or radical normative arguments. In this article, I present what I think is a reasonable interpretation of the descriptive arguments. First, the record of overconfidence and external influence reminds us of the subrational origins of the field and the possibility of path-dependent biases that have survived over time. Second, the record of convergence resists the view that the rational reconstruction of scientific historiography is solely populated with elements from orthodox research.[1] In the final part of the article, I respond to Andrew Logie's criticisms, starting from the conceptual criticisms and ending with some noteworthy empirical issues.

1 Declaration

"Personal opinions in politics are completely irrelevant, unless you're a person of great moral authority," Norman Finkelstein, a political scientist and critical historian of Israel, once said in a lecture when an agitated audience pressed him to declare his personal stance on

* McGill University Department of Sociology. Tay Jeong may be reached at jeong.tay@gmail.com.

1 While the lexical meanings of the terms "orthodox" and "heterodox" are simply based on conventionality [according to Oxford Dictionaries], I am using them to refer to the broad but real traditions of historical research that started in the colonial times and persisted throughout a large part of modern historical scholarship. See Jeong 2018a for historical details. The terminology was inspired by widespread usage in the field of economics. It must be stressed that the orthodox/heterodox division is conceptually independent of quality or scientificity: I am not making any direct claims about a theory's soundness by using these terms. For example, calling a certain geographical proposal "heterodox" is not an act of legitimization just as calling Marxist economics "heterodox" is not necessarily a defense of the scientificity of Marixist economics.

various controversial topics in Israeli history. Although he was talking about issues in politics, this statement reflects our common norm in science in general, where criticism remains internal to directly expressed arguments rather than encompassing aspects about the researcher such as his motivation and judgments on issues that bear no direct relevance to the premises and inferences that constitute the argument.

My original article focused on procedural rather than technical evaluations.[2] By the latter, I mean that I did not attempt to directly weigh the empirical evidence and make a substantive judgment about the relative plausibility of rival hypotheses for each topic. Instead, I devoted my attention to social evidence, or what social epistemologists like to call "evidence of evidence". The idea is that examining things like the level of expert support, past track records, and various external biases can give us useful information about the plausibility of scientific theses.

As technical judgments on the key topics in historical geography were outside the scope of my original arguments, I had consciously refrained from making them explicit – doing so would have been no different than expressing my personal opinion on these issues, which is irrelevant. Yet, at the risk of committing what would generally be considered bad practice in academic communication, I will start this supplementary article by declaring my personal judgments on the key empirical issues that I discussed in my original paper. It is my bet that doing so will help some of my agitated critics read the rest of this article (and the original, if they decide to go back to it) without making unnecessary hostile speculations about me.

My personal judgments on issues that pertain to pre-Qin historical geography simply follow one version that is popular among South Korean historians and archaeologists. I think that Chosŏn first rose as a political entity between the eighth and seventh century BCE based on the mandolin-shaped dagger culture in the Daling River basin area. It spread eastward to Liaodong starting from the fifth century BCE and, a couple of centuries later, lost its western territory to Qin Kai's conquest. Manpanhan, the easternmost extent of Qin Kai's conquest, is likely to have been somewhere in the Qianshan Mountains in eastern Liaoning Province. My judgment about the location of Paesu, the river where the Han Empire made its retreated border with Chosŏn during the Qin-Han transitional period, also follows the recent trend of identifying it as the present-day Hun River. I have not made a firm judgment about where Wanghŏm was – I think it could have been in Pyongyang or somewhere in southern Manchuria. Locating it at Pyongyang with certainty had been an old (near-)consensus, but since the early 2000s, we have seen a number of serious experts treating this as an open question. Finally, regarding the notorious topic of Lelang, I abide by the standard view that locates it in Pyongyang. I think the empirical evidence, as well as the social evidence, are so strongly in

2 Hereafter, "original article" refers to Jeong 2018a.

favor of this location that it could now be plausibly classified as confirmed historical knowledge. I do, however, have some reservations about accepting the standard thesis that Lelang continuously persisted as a Chinese commandery in this region up until Koguyro's invasion in 313CE as confirmed beyond doubt. Recent research revealed several anomalies for this time frame, and I think it remains to be seen if simply explaining them away is the best response.[3]

It should be clear whether my technical judgments on these issues are closer to and informed by the recent findings and proposals from authoritative South Korean experts or the historical reconstructions advocated by Lee Dukil. Contrary to some speculations, I have rather little knowledge of the projects of politically active extra-institutional historians.

2 The Main Descriptive Arguments and Their Epistemic Implications

The main message of my original article was predominantly descriptive: it focused on describing certain notable patterns in the history of historiography rather than making claims about normativity or historical truth. The argument of the first main section of the article was that, regarding a number of key topics related to the historical geography of Old Chosŏn, our confidence in the important theses that had been considered uncontestable in the past has tended to weaken, rather than strengthen, with the accumulation of new evidence and the growth of professional scholarship. And often, that weakening meant getting closer to (if unintendedly, and if for not exactly the same reasons) those heterodox or "resistance" geographical reconstructions that were considered squarely outside the realm of reasonable historical research. The degree to which such a pattern occurred varies by topic, of course, and I tried to make this clear in the historiographical review that constituted the first main part of the article.

The main content of the second section was that, at virtually all stages of modern historical scholarship, the study of ancient Korean historical geography has been permeated with external influences, namely political, ideological, and factional interests.[4] What my analysis aimed to show was that mainstream theories – those that were later more solidly confirmed as well as those that were questioned – have been by and large far from devoid of external influences. This was predominantly the case during colonial times, but external concerns continued to exert a significant influence on ancient history scholarship even long after independence and national division. This kind of argument is just typical practice in the sociology of knowledge, for which the modern scholarship on Korean antiquity provides suitable material.

3 See footnotes 83 and 84 of the original article.

4 For reasons of convenience, I will ignore philosophical arguments that there can be no objective demarcation between internal and external elements of science.

I will call these three arguments (the first two from the first part and the last one from the second part of the original article) "overconfidence", "convergence", and "external influence". Many people found these descriptions provocative, but I don't think that these descriptions per se express anything new or radical. The first two simply describe certain noticeable patterns in the history of historiography. The third may involve a greater degree of interpretation, but it is certainly not a description that stands out as idiosyncratic. The strong presence of external influence in modern scholarship, even on those works that fall in the camp of orthodox historiography, is something that has already been expressed by scholars multiple times in the past and present, and remains fairly widely acknowledged even by people who do not like to talk about the subject. The bits of research that constitute these three broad descriptions are nothing new or arcane – it is just that the patterns they collectively form previously existed in a state of relatively unspoken, and unorganized, silence. The immediate goal of my original article was to bring these phenomena to the surface and express them in a structured and detailed manner. It was my judgment that these macroscopic patterns and characteristics of modern scholarship merited explicit expression.

In my view, what has agitated many people about my article has to do with the epistemic and normative implications that these descriptive facts about historiography contain. "So what?" seems to be the inevitable question. I initially found Logie's repeated efforts to speculate about my motivations rather surprising, as this is not the kind of criticism one would normally expect.[5] But this is likely just an expression of anxiety that these descriptions may be interpreted to derive unacceptable normative conclusions. Likewise, when Logie said that my description of convergence was a "false analogy" (138), I believe he was pointing out that the normative or epistemic conclusion that (he thinks) I am trying to get across is false.[6] An Jeongjun, who wrote a critical response to my Korean language article, also expressed, "I do not understand why Tay Jeong brought up the phenomenon where a new theory about the location of Paesu is presently receiving attention in the academia" (my translation).[7] Am I trying to imply, for example, that Lelang is just as likely to have been in Liaoxi as early Chosŏn, or that Lee Dukil's opinions are just as sound as any majority position among authoritative scholars? Do my descriptions gesture towards "post-truth"?[8]

The epistemic implications were largely left unspoken because, apart from the fact that they were outside the immediate focus of the article, I thought there was a good deal of interpretive uncertainty. Now, in the face of all the radical normative arguments that I have been accused of making (or implying), I will propose an interpretation that I, upon further reflec-

5 Logie 2019.
6 Logie 2019, 138.
7 An 2018, 360. This was a response to Jeong 2018b.
8 Logie 2019, 146.

tion, have found reasonable. I will say two things: First, the study of ancient historical geography started from a subrational state, and there is a possibility that its irrational legacies may still linger in the professional habitus of its researchers. If this is true, then the discipline (or at least some parts of it) might be able to benefit from some added intellectual caution and humility. Second, some works in the heterodox tradition can claim a legitimate spot in the rational reconstruction of scientific historiography. This second interpretation has more to do with our retrospective evaluation of modern scholarship than an analysis of the activities of the present body of scholars. The rest of this section will be devoted to explaining how these epistemic interpretations may be drawn from the three descriptive arguments presented in my original article.

2.1 Confidence, Bias, and Rationality

The record of overconfidence demands a critical adjustment of the level of confidence we deem appropriate for the mainstream theories in the field. The basic reasoning rests on a simple and commonsensical Bayesian inference: the fact of having been wrong or wrongly confident in the past reduces the likelihood that your next confident judgment will be true or safe. Similarly, a currently prevailing thesis in a field with an excellent record of corroborating the beliefs it held with high confidence in the past will *prima facie* enjoy a stronger epistemic status than a majority opinion in a field where confidence has frequently been betrayed by further evidence and reasoning.

Here, one may argue that getting things wrong or developing the wrong levels of confidence in our theories may just be a result of intellectual growth rather than any shortcoming in scientific practice. People had full faith in geocentrism until they developed the tools and methods for coming up with a better theory, heliocentrism. That is surely a record of error, but it seems that there is nothing immediately irrational in this history: it could just be that, at each moment in history, astronomers supported the theory which explained the available evidence with maximal rationality. As scientists are never perfect in reality, the more refined critic may admit that there are sure to be occasional elements of irrationality in conventional scientific theories in each period. But he/she would then argue that, while it is generally true that scientists need to be keenly aware of the possibility of irrationality and overconfidence, there is nothing in the history of historiography of ancient Korean geography which makes it more problematic than any other healthy research program. My description of the record of overconfidence has no epistemic implications other than completely trivial reminders that are common to all sciences.

The strong external influences that permeated modern historical scholarship of Korean antiquity decrease the plausibility of such a response. It is commonly assumed by practicing scientists as well as philosophers and historians of science that bias, or the influence of external factors, generally impedes rational judgment.[9] The error-conducive effects of bias are already well accepted with relation to the works of heterodox scholars. Lee Chirin, easily one of the most influential heterodox historians, spoke with full confidence that, for example, Lelang was not in Pyongyang and Wanghŏm was in the Liaodong Peninsula.[10] That confidence was surely mistaken – the available evidence was arguably determined to support the rival hypothesis in the case of Lelang while the evidence is still inconclusive in the case of Wanghŏm. Yet, why did he make these assertions with such unwavering confidence as if the evidence was clearly determined to support his hypotheses? It must have been the case that external motivations, such as political motivations, induced him to inflate the plausibility of the theories that he found agreeable and understate the plausibility of those with which he was uncomfortable. I believe this much of my argumentation should face no objections even from my strongest critics.

The crux of the second part of my original article was that in retrospect, the same can be said of researchers in the orthodox camp in the past. We can now very plausibly say that the location of, for example, Paesu remained largely underdetermined for the most part of modern scholarship. Yet, why was it that, until relatively recently, virtually every professional historian on ancient Korea in South Korea and Japan spoke with conviction that Paesu could not have been anywhere north of the Yalu? Or, to take another example from my article, why was the thesis of the centuries-long presence of Chosŏn in Liaoxi widely considered unreasonable among the absolute majority of scholars, despite documentary records indicating that this was the case and despite the absence of any decisive reason to believe this could not have been the case? The most plausible explanation lies in elements that are external to the rational content of science. In my original article, I identified colonial path dependence and the existence of heterodox adversaries as the most likely sources of collective bias.

The records of unwarranted confidence in traditional geographical theses are attributable to a non-trivial degree to biased scientific practice. At this point, the critic may complain that all my examples (perhaps with the exception of Wanghŏm, where serious revisionist arguments have begun to be presented relatively recently) are from the past. My response is that

9 In this article, I will abide by this assumption without questioning it, although it must be mentioned that there is a philosophically controversial but noteworthy argument – notably advocated by feminist epistemologists – which hold certain forms of bias to be either constitutive of or conducive to knowledge. These works argue that taking the standpoint of the marginalized groups makes science more objective or truth-conducive. Considering the strong political power dynamics that have been intertwined in almost every part of the history of historiography of Korean antiquity, such feminist epistemological concerns – if one decides to take them seriously – are likely to have implications for our evaluation of historical knowledge. Such an extended application of epistemological theory is beyond the scope of this article, and my argument can be made sufficiently clear with the standard assumption of bias as an epistemic nuisance.

10 I am sure that interested readers can think of more recent examples. I just do not want to pinpoint anyone who is currently active in the field.

this is just a typical feature of reasoning from history. A historiographical evaluation of a track record can only be made in retrospect, and current cutting-edge research will need to wait until the tribunal of time allows us to evaluate them procedurally with the advantage of hindsight. But then, how can my descriptive arguments about the history of historiography imply any prescriptive demand for *current* research in the field?

There is surely a problem of external validity here. If we think in deterministic terms – that is, if we reason only with chains of inferences where the conclusion certainly follows the premises – my descriptive arguments about overconfidence and external influence would need to end just as a historiographical argument that pertains only to relevant cases of past research. And frankly, I would have been fine with leaving my argument there. If we actively seek out the prescriptive demands that can be derived from those descriptions, however, and if we think probabilistically rather than deterministically, I think it should be possible for us to make certain tentative prescriptive arguments. Again, the logical principle behind this is just Bayesian induction: The existence of bias up to the relatively proximate past in mainstream historical research presents an uncertain but serious possibility that past legacies may still remain in the present at least in some subfields and among some researchers. This reasoning accords with recent *technical* research that has forcefully argued that path-dependent biases exist in the study of Korean antiquity.[11]

2.2. Deviant Theories in the Rational Reconstruction of Science

The patterns of error and overconfidence in modern scholarship on the historical geography of Chosŏn differ from those in most other research fields in that the conclusions of deviant or fringe hypotheses have turned out to be closer to the truth or at least less implausible with the accumulation of more advanced research. The key question here is whether this fact is something to be taken seriously or something that is to be explained away. Can at least some deviant research from the heterodox tradition claim a legitimate place in the rational reconstruction of scientific historiography?

Epistemologists commonly identify two main intuitions about the concept of knowledge: (the absence of) luck and (cognitive) ability.[12] I find these two intuitions useful for analyzing the epistemic significance of convergence. Instances of convergence can be ignored to the extent that they can be reduced to sheer luck. Deviant research will be able to claim a spot in the rational reconstruction of science to the extent that their correct or reasonable predictions are a result of ability.

11 The most prominent recent proposal comes from Jung In-Seung, who has identified the presence of sticky and subrational frameworks inherited from the colonial period in our archaeological theories of the early Iron Age Korean Peninsula. See also footnote 104 of the original article.

12 Pritchard 2010 contains a good concise overview of the two intuitions.

There is no doubt that measuring the proportion of luck and ability of a certain thesis or argument is an empirical judgment that resists top-down generalization. I do not wish to argue that all heterodox theories in the history of modern historical research whose geographical inferences approach the proposals of recent cutting-edge research can claim an element of ability. Nor do I wish to downplay the role of luck, which I believe is almost universally present: Often in the study of Korean antiquity (as well as that of antiquity in many other parts of the world), the evidence is too scarce, ambiguous or mixed to allow for complete determination of historical knowledge, so the potential truth or plausibility of inferences made in such research is likely to contain an element of luck. But it is my firm judgment that at least some deviant geographic proposals whose plausibility is retrospectively verifiable contain a significant degree of ability as well. Ability in this context consists of both positive and negative terms. On the positive side, it is about paying attention to the evidence, which, albeit scarce and not fully determined, adds weight to a certain possibility. On the negative side, it is about being able to critically deal with underappreciated problems in prevailing theories.

The examples I mentioned in section 2.1 exhibit elements of both positive and negative ability, which is reflected in the works that lay out some of the most prominent heterodox geographical theories. The existence of an early territory of Chosŏn in Liaoxi hypothesized in the works of pre-1990s North Korean scholars was not just a product of wishful thinking but was also based on an interpretation of widely-used Chinese textual sources that pointed to this location, as well as the newly available evidence provided by the mandolin-shaped daggers. Consider, also, the territory of late Chosŏn demarcated by the Paesu. Neither the textual nor the archaeological evidence has, at any point in the past, unequivocally favored the traditional locations of the Chŏngchŏn or Yalu Rivers. Even looking at the most basic source – *Shiji*, "Chaoxian liezhuan" – alone, the only description of the size of Chosŏn's territory is just not compatible with the identification of the Paesu as the Chŏngchŏn and only very dubiously so as the Yalu.[13] Basic compatibility issues regarding cardinal directions can be found in other key primary sources like "Weishu" and "Dongyi zhuan" in the *Sanguo zhi*.[14] The problem gets really compounded when we think of the compatibility with other conventional geographic reconstructions.[15] Yet, the fact is that almost all authoritative historians had decided to explain away these and kindred problems until relatively recently without paying due attention to the historiographical uncertainty they pose, and it was mainly schol-

13 *Shiji* 115.2985 (眞番臨屯, 皆來服屬, 方數千里).

14 *Sanguo zhi* 30.850 (燕人衛滿亡命, 爲胡服, 東度浿水, 詣準降, 說準求居西界, (故)[收]中國亡命爲朝鮮藩屛. 準信寵之, 拜爲博士, 賜以圭, 封之百里, 令守西邊). It is hard not to wonder whether one really would have recorded that Wiman crossed the river eastwards to Chosŏn and defended its western border if the Paesu was the Chŏngchŏn or Yalu.

15 See, for example, Park Jun-Hyoung 2016, 96–98 for details.

ars influenced by the heterodox tradition that chose to deal with them seriously before the late 1980s. At least in the context of the historical geography of Chosŏn, we cannot simply reduce the relative similarity between major heterodox geographical reconstructions and the proposals of recent cutting-edge research to sheer chance. It is a mistake to think that the rational content of the history of historiography on this topic is exclusively populated by elements from orthodox research.

3 Response to Logie

The catalyst for writing this sequel to my original article was in part Logie's critical review of it. I will devote the remainder of this article to addressing his criticisms and other notable aspects of his paper that have not already been addressed.

3.1 Think in Degrees, Not Kinds

> However, rather than viewing this [the fact of having staved off erroneous colonial historical theses such as Mimana Nihonfu] as evidence of South Korean scholars' agency, and by extension their acceptance of a P'yŏngyang location for Lelang and Wanghŏm likewise, Jeong frames it as a potential "intimation" of a gradual process of decolonization.…[16]
>
> This [the fact of recent convergence] is nothing but a manifestation of the fact that many different theses normally coexist in academia, and that the presentation of minority theories is not suppressed.…The cases that Tay Jeong mentions merely indicate that the minority theories that are published in academic journals through normal procedures sometimes receive attention and may even become the majority theory depending on the progress of subsequent research[17] [my translation].

One conceptual criticism that I have often received is that I have not interpreted the fact of having overcome distorted or biased theories in the past as indicative of the intellectual openness and agency of present South Korean academia. In principle, I have no opposition to this interpretation – I think it is also a natural message that we get from the history of historiography. There is no doubt that present South Korean academia is open enough for minority or even "seemingly heterodox" proposals strongly supported by evidence to receive serious attention. But this fact poses no contradiction to the interpretation that the experience of constantly having corrected past errors or biases could be taken as a reminder that modern scholarship did not begin in a rational state, and irrational path-dependent biases have been present for the greater part of the history of modern scholarship even as they were presuma-

16 Logie 2019, 143.
17 An 2018, 361. This quote is from An's critique of my Korean-language article.

bly corrected or weakened one after another. This points to a very real possibility that unwanted biases – although not as explicit and overt as they once were – may still persist in a non-trivial portion of the professional habitus. If anyone thinks that these two interpretations are contradictory, it is just a result of thinking in types (as one or the other, open or repressive, rational or irrational) rather than in degrees. The current situation in Korea is that there are quite a number of otherwise respectable scholars who are refusing to appreciate the latter interpretation. Such an attitude is only reinforced by the worry that it may be seen as a weakness against aggressive populist adversaries. The recent anti-pseudohistory campaign is just an organized manifestation of this attitude.[18]

3.2 Hidden Motivations

Logie spends much effort trying to uncover my hidden motivations. Most of these efforts rely on one-dimensional similarities between my arguments and those of the "pseudohistorians". For example, Logie takes issue with my frequent citation of Jung In-Seung, saying that Jung's argument about the location of Wanghŏm was cited by a supporter of Hwandan Gogi.[19] Citing Jung in multiple locations in my article was only natural considering the outstanding contributions he had made to the discipline with his thorough and bold hypotheses, many of which rely on novel data. His well-evidenced arguments supporting the existence of remaining colonial legacies in Korean archaeology also provide a useful reference for my descriptive argument about external influence. And still, Jung's works had only minor representation among all the sources I cited in the historiographical review which laid out the arguments about overconfidence and convergence.

The same can be said about Logie's criticism of my treatment of Yi Pyŏngdo. Logie said, "As is common to the polemical narratives promoted by Korean pseudohistorians, Jeong highlights only Yi Pyŏngdo as being representative of establishment historiography, while making no mention of other professional South Korean scholars or the ways in which they sought to decolonize narratives of Korean history."[20] Regarding this point, it seems clear to me that the activities of the "pseudohistorians" have a much larger presence in Logie's mind than mine. Yi is sometimes branded as an archvillain in popular historical narratives in South Korea, and, at least in some of the more radical polemics, the entirety of South Korean insti-

---

18 I am referring to the recent series of organized journalistic, academic, and political activities by some South Korean historians that condemed the irrationality and danger of the "pseudohistorians" far more aggressively than most historians had before. The special editorial sections in the 2016 and 2017 volumes of *Yŏksabip'yŏng*, which contain more than a dozen contributions specifically aimed at criticizing various elements of "pseudohistory", are one noteworthy attempt.

19 Logie 2019, 125.

20 Logie 2019, 137.

tutional academia is portrayed as being mindlessly subservient to Yi. But there is no reason that I should pay attention to such issues. Yi was by far the most important historian of Old Chosŏn in the couple of decades after decolonization (1945) (there weren't even that many people specializing in this topic back then), and if there is one person who should appear in such a brief historiographical review to represent the works of the early South Korean historians on this topic, it is Yi. I still cannot think of any other South Korean historian from that period whose addition to the historiographical review would have made any important difference for the topics that I was discussing. In addition, I made it abundantly clear that subsequent generations of South Korean researchers moved further and further away from colonial precedents. Such efforts to portray me as a covert supporter or ally of aggressive populist movements and as a person whose "broader motivation may be to throw a lifeline to pseudohistorians" (129) do not even qualify as arguments.

3.3 A Note on Terminology

The only normative argument I really wanted to forcefully advance had to do with the use of the word "pseudohistory". The basic argument is that this term is better used as a predicate than as a name in the context of the study of Korean antiquity. This is an important difference. A name refers to objects in the world; a predicate simply ascribes properties to an object already referred to by a name. Simply put, the use of the term "pseudohistory" as a name is undesirable because its referent is too vague for a serious scientific language. This causes immediate problems with the clarity of communication. For example, Logie's statement that I "portray[ed] the dispute between pseudo and professional historians and archaeologists as a vigorous debate between scholars of opposing factions in which no side has absolute evidence"[21] is borderline unintelligible. I expressed clearly that the "factions", as much as they are a historical reality, have gradually dissolved over time.

In addition, almost all the scholars I cited were professionals. In a number of cases, the disputes are between professional scholars with no noticeable or proclaimed ties to the tradition of what I have called "resistance historiography". To illustrate this point with an example, the people who appear in my review of the historical discussion surrounding the location of early Chosŏn range from Jeong Yak-yong, Inaba Iwakichi, and Jeong Inbo active in precolonial and colonial times to Lee Chirin, Yi Pyŏngdo, and Ch'ŏn Gwanu of the 1960s and 1970s and to Lee-Hyunggoo, Bok Gi-dae, Zhang Boquan, Wu'en Si, Song Ho Jung, Park Junhyung, and Ishikawa Takehiko from the period after the 1990s, just to randomly grab a few names from the bibliography. Exactly which part of my historiographical review is Andrew Logie calling a "portrayal of the dispute between pseudo and professional historians and archaeologists"? I do

21 Logie 2019, 128.

not mind commentators calling any specific personmentioned in my articles a pseudohistorian (i.e. using the term as a predicate) as long as they present a cogent case for it. It is just that Logie's statement is unintelligible and scientifically unfit because of his use of the word "pseudohistory" as a name. The same can be said of any other topic I mentioned in my article with equal plausibility, even the least controversial topics, such as Lelang.

Here one may ask: is the problem with the referential use of the term "pseudohistory" merely that it adds a vague name to our scientific vocabulary, thus hindering efficient communication? It might seem to some that this is not that big a deal. Yet, I think the negative implications go far beyond that, and explaining those further negative implications of the referential use of the term "pseudohistory" in the context of the study of Korean antiquity was the topic of my Korean language article. As this usage seems to be spilling over to scholars working in the English language, I shall briefly reiterate my arguments below.

In my Korean language article I classified the negative implications into epistemic, practical, and ethical aspects. To avoid excessive redundancy, I will just briefly outline the main logic of the epistemic aspect, which I think is the most relevant for the present article. The problem is that there is a theoretically very plausible and empirically attestable danger that the users of this name will be compelled to implicitly bring in a criterion that is external to the lexical meaning of the word "pseudohistory" to make up for the excessive vagueness of its reference.[22] In ancient Korean historical geography, that external criterion is most likely to be the property of being a deviant hypothesis that depicts a larger territory than the traditionally accepted view. Or perhaps, it could be the property of having cited certain canonical works or arguments that are generally recognized as belonging to the heterodox tradition. The essential problem with both criteria is that they are not sufficiently reliable indicators of pseudohistory; there is no reason to believe that these two properties are coextensive with the property of being pseudohistorical. The epistemic consequence is that it may render scholars unable to maximize the opportunity to engage in a fruitful deliberation and discussion of the material which may prove useful for further scientific development, especially when it comes to overcoming path-dependent biases.

3.4 Partiality (Representativeness)

One of the main objections that Logie raises against my article is that the review of historiography was partial and biased. I can think of two kinds of partiality in a historiographical review: qualitative and quantitative. The former has to do with representation, given the fact that a certain thesis was selected for discussion. As I was committed to avoiding technical judgments about the quality or plausibility of each thesis, the only fair way for me to present

22 Jeong 2018b, 441–443.

each theory was to introduce the key supporting arguments as well as criticisms, in addition to information about the level of support each theory receives from experts. One could probably still criticize me for more subjective things like nuance or tone, or singular anecdotal cases where I did not mention the flaws of a certain minority thesis in detail. At this point, I can only respond by saying that I made an effort to maintain qualitative fairness within the constraints of avoiding technical judgments.

Let us consider quantitative partiality. What I mean by that is that the amount of attention and coverage that is devoted to each theory included in the historiographical overview is not proportional to its historical visibility in the academia.[23] For example, if one theory had a visibility of 9 and another theory 1, quantitative parity will demand that I write roughly nine sentences about the former theory and one sentence about the latter. I never made an attempt at quantitative representativeness in the first place, nor was it necessary or even appropriate. A critic could make the technical judgment that at least some of the hypotheses and research trends I mentioned are of such low quality that they did not deserve inclusion in serious academic communications and worry that mentioning them may lead the innocent reader to believe that they are actually good. I do not think we need to agree on exactly which constituents of my historiographical overview are good and which are bad, or which hit the "legit history" mark and which do not. The paper was presented as an original research article in a journal frequented by historically literate people. The risk of potentially misleading naïve readers is an ethical non-issue.

3.5 Irrelevance between Topics?

Logie argues that my paper "conflates the question of the location of P'aesu, Wanghŏm, and Lelang with the separate topic of Chosŏn's earlier, pre-Qin Kai territories.... In terms of culture-historical associations, [the] 'movement hypothesis' ... has no impact on the pertinent questions regarding historical P'aesu, Wanghŏm and Lelang, which all pertain to post-Qin Kai, and indeed the post-Warring States period."[24]

The degree of interconnectedness between topics is not crucial to my descriptions. The baseline coherence of the topics I discussed was given by their relevance to Chosŏn and Lelang, and they collectively constitute my broad descriptive arguments. However, it just happens to be the case that all these topics, especially the ones regarding the pre-Qin Kai and post-Qin Kai territory of Old Chosŏn, are closely related – in fact, they have often been

23 Here, visibility should be compatible with a number of definitions, such as the number of publications, the number of citations, or even hard-to-measure concepts like cultural-political influence, or whatever. It does not matter for the present discussion.

24 Logie 2019, 130.

discussed in conjunction with each other.[25] It appears to me as common sense that a certain polity's territory after a recorded major loss is not unrelated to its territory before the loss. In fact, Logie shows that he knows this himself when he says, "Unaddressed by Jeong, the necessary premise to support the assertion that Chosŏn continued to occupy Liaoning would either be that the Yan expansion never occurred, or that Chosŏn's original territory had extended even further west."[26] Although this sentence is quite vague – the modern administrative district of Liaoning is vast, and it is not really intelligible what the exact criterion for "even further west" is – it is clear that the latter option expresses some kind of relationship between pre- and post-Qin Kai territory. For example, if one wants to consider the Sejungni-Lianhuapu culture as belonging to Chosŏn in its later stages, then it makes sense to say that its territory had likely extended to Liaoxi before the major loss.

3.6 Lelang Seals

This one is comparatively trivial but still worth pointing out. Logie says,

> [T]here is certainly a debate specifically concerning the authenticity of clay seals associated with the Lelang excavations, as first highlighted by Chŏng Inbo, and the conspiracy theory of Japanese forgery remains a favourite of pseudohistorians. … Lelang graves have continued to be uncovered by North Korean archaeologists, which constitutes the strongest evidence that the Japanese did not fake their excavations. It is precisely through this method of introducing the reader to contradictory information, however, that Jeong works to construct the idea of an ongoing debate.[27]

This is just a typical case of conservative overconfidence (plus, if I may guess, Logie's own ignorance of the literature). In most cases, an excavation is unable to provide strong proof of the authenticity of another excavation conducted by another team at another time. More concretely in the context of Lelang seals, North Korean archaeologists never reported any Lelang seals in their excavations; they have only reported features and artifacts that are not seals. I do not see how one can ever argue (or has ever argued) that the vast number of seal-less features reported by North Korean archaeologists provides strong evidence for the authenticity of the Japanese excavations of Lelang seals.

25 This makes me wonder whether, his unwavering trust in "professional" research notwithstanding, Logie is really all that familiar with the historical and archaeological literature on the historical geography of Chosŏn. His bibliography is wanting when it comes to the sources on this topic that have been written in Korean or other East Asian languages, which are the languages where the majority of the cutting-edge discussion (or just about any discussion) has been taking place in the past 100 years.

26 Logie 2019, 131n.

27 Logie 2019, 135.

According to recent research, it is unlikely that the Japanese excavators intentionally forged the seals that they claimed to have unearthed. This conclusion was primarily based on an examination of the long-forgotten excavation records and the memos left by participating excavators and researchers.[28] However, as is well known, debates about seal forgery are not just about the possible dishonesty of Japanese excavators – they are also (and perhaps to a greater extent) about the possible interference of antique traders, for whom the high price of Lelang seals provided a motivation for forgery and other illicit activities. At present, there is no decisive evidence that the more than 200 Lelang seals that we know of were all or mostly authentic, nor is there decisive evidence that they are all or mostly fake. This was the conclusion of the most thoroughly evidenced and researched publication on this topic by Jung In-Seung. The fact remains that there is an ongoing debate and continuing uncertainty about the authenticity of the Lelang seals.

Bibliography

An, Jeongjun 안정준. "'Saibiyŏksahak'kwa 'singminsahak'e taehayŏ: t'ei chŏng, 「'saibisahak' pip'anŭl pip'anhanda」e taehan nonp'yŏng" "사이비역사학"과 "식민사학"에 대하여: 테이 정, 「"사이비사학" 비판을 비판한다」에 대한 논평, *Yŏksabip'yŏng* 역사비평 125 (2018), 353–376.

Jeong, Tay. "The Politics of Historical Knowledge: The Debate on the Historical Geography of Old Chosŏn and Lelang Commandery", *Journal of Asian History* 52.1 (2018), 43–82. [Jeong 2018a]

——— "'Saibisahak' pip'anŭl pip'anhanda" "사이비사학" 비판을 비판한다, *Yŏksabip'yŏng* 124 (2018), 438–469. [Jeong 2018b]

Jung, In-Seung. "Iljegangjŏmgi t'osŏngnit'osŏng (nangnangt'osŏng)ŭi palgulgwa ch'ult'oyumul chaegŏmt'o: pongniwa nangnangyegwanŭl chungsimŭro" 일제강점기 토성리토성 (낙랑토성)의 발굴과 출토유물 재검토: 봉니와 낙랑예관을 중심으로, in the proceedings of the conference "2016 nyŏn sanggosa t'oronhoe: Han'guk sanggosaŭi chaengjŏm: wanggŏmsŏnggwa han'gunhyŏn 2016" 년 상고사 토론회: 한국 상고사의 쟁점: 왕검성과 한군현, Korea Press Center 20F international conference room, Seoul, March 2, 2016, 97–132 [online: www.nahf.or.kr/gnb03/snb03_01.do?mode=view&page=2&cid=52088&hcid=27671. [Jung In-Seung 2016b]

Logie, Andrew. "The Politics of Feigned Impartiality: A Critical Response to "The Politics of Historical Knowledge", *Journal of Asian History* 53:1 (2019), 117–150.

28 Jung In-Seung 2016. See also footnote 64 of the original article.

Park Jun-Hyoung 박준형. "Kojosŏn p'aesuŭi wich'i" 고조선 패수의 위치, in the proceedings of the conference "Han'guk sanggosaŭi chaengjŏm: kojosŏn'gwa hanŭi kyŏnggye, p'aesunŭn ŏdiin'ga?" 한국 상고사의 쟁점: 고조선과 한의 경계, 패수는 어디인가?, 20F, Press Center, Seoul, June 21, 2016, 89–104 [online: www.nahf.or.kr/gnb03/snb03_01.do?mode=view&page=1&cid=55740&hcid=27671].
Pritchard, Duncan. "Anti-luck Virtue Epistemology", in: Pritchard et al. 2010, 48–65.
———, Alan Millar, and Adrian Haddock (eds.). *The Nature and Value of Knowledge: Three Investigations*. Oxford University, 2010.
Sanguo zhi 三國志, by Chen Shou 陳壽. Beijing: Zhonghua, 1959.
Shiji 史記, by Sima Qian 司馬遷. Beijing: Zhonghua, 1959.