BOOK REVIEWS

Archaeology and Emerging Kabikolan
Andrea Malaya M. Ragragio
2012. Quezon City: University of the Philippines Press
Contribution to Archaeology Series

Review by Reynaldo R. Avellana
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This book is the latest addition to the scant but growing publications on Bicol archaeology. It is the most comprehensive body of work to date dedicated to the study of Bicol Region's ancient past. If Beyer's *Outline Review of Philippine Archaeology* (1947), which is national in scale, were to be localised at a regional level, without a doubt, this book is its equal.

While tracing the regional identity of the Bikolnon, Ragrario provided a general survey of Bicol's antiquity by utilising mainly archaeological materials along with a number of historical and ethnographic accounts. Covering Bicol's six provinces across various times, the book contains a grand review of related literature and studies on Bicol archaeology spanning from the early European scholars' antiquarian interest of the 1800s up to the recent underwater archaeology attempts in Catanduanes.

Ragrario's *magnum opus* is the published form of her master's thesis, a result of dedicated graduate-level research at the Archaeological Studies Program of the University of the Philippines – Diliman (Ragragio 2010). Published by the nation's premier university, the book commands authoritative integrity among its readers. Colleagues in the archaeological discipline will find reading the book easy; general readers may find reading it a bit harder despite the writer's effort to simplify her discussions.

Before the main critique of its content, I will first tackle the minor technical details of this publication. A reliable *Index* is provided and arranged neatly with alphabet heading. *About the Author* is located at the last page instead of the back cover. Nevertheless, it informs the readers on the academic qualifications and professional experiences of the author.

The *References,* consuming 23 pages, contained more than 300 bibliographic entries. The *List of Illustrations* has a total of 36 plates, which are either black-and-white or greyscale. The *List of Figures/Tables* contains seven maps slated as "figure" and one table on comparative prehistoric chronologies. Aside from typical formatting and typological errors that are forgivable, no major editorial flaw is alarming.

After evaluating the trivial parts of the book, I will proceed with the major assessment of the main content. The bold declaration at the back cover posed provocative questions on the ontological significance of "Bikol archaeology." It further challenges prospective readers to contemplate on the meaning of "Bikol" using epistemologies of archaeology on top of history and anthropology. The formulaic purpose of archaeology as seeking the past identity of the present is acknowledged for its ability to chart the future. In the *Preface,* the series editor, Dr. Victor Paz, reaffirms the same call for unraveling regional ethnic identity. The *Contents* serves as the outline of the book's content.

The main body of the book is divided into four parts. Part I provides the background of the archaeology of Bicol Region and its reflexive relations with national archaeology. Moreover, it sets the objective of the book, which is, to find the meaning of Bicol archaeology and the Bicol social boundary it creates. My only comment for this part is that the methodology by which the concept of "identity" can be analysed must be explained with specific parameters or criteria to concretise "identity" which is an abstract concept.

Part II provides historical documentation of colonial and post-colonial accounts on "Kabikolan," a term she prefers instead of "Bicol", probably for a more indigenous feel. Further integrated in Part II is the praise-worthy synthesis of history of archaeological research in Bicol patterned after Paz's (2009) history of archaeology for the Philippines. The use of terms "accidental, committed, directed, and reflective archaeology," respectively for the history of Bicol archaeology recalls to mind the sequence proposed by either Willey and Sabloff (1993) or Trigger (2006). If there is no local alternative to this Western paradigm, then she might have made the right choice. Little emphasis was made in relating this to the development of Bicol identity. She closes Part II with discourses on culture history, time, and heritage. These are "heavy" concepts that need elaboration. The first two concepts can be the opening

for Part III as these concepts are fundamental to Bicol archaeology. The third concept should either be explained in Part I or Part IV.

Part III comes with a heading "The Archaeology of the Bikol Region" but mainly provides an inventory of archaeological sites as geographically located namely waterways, caves, open sites, as well as underwater and coastal sites. As an archaeologist, I am critical in this way of presenting the archaeological sites as this spatially distribution deprives temporality. It would be best if time control is shown alongside the spatial distribution especially that a regional chronology was made available for Bicol by herself. The occasional thematic topics incorporated in the geographical discussion of type-sites seemed out of place when in fact this can be the heart of her discourse.

Part IV concludes the book with the wrapping up of the matters culminating "towards a regional archaeology." In fact, the sole chapter here bears the same title as the book. This chapter features a table of comparative chronologies for Philippines and Bicol. The chronology on Bicol on page 206 indicates "(After Paz 2008)," however, this is not found in the *References*. This table in my humble opinion should be presented much earlier if not first similar to how Junker (2000) presented the chronology for Bais-Tanjay Area Regional Chronology. The brief parting words are played safe and sounded cliché with its open-ended statement on "Bikolness."

In closing, I provide few general points that can be improved on should a revised edition be warranted. The existence of Bicol identity in the past is Ragrario's major line of inquiry. Though the title bears "archaeology," Ragrario referred not exclusively to archaeology to prove her point; it can be a double-edged sword depending on the evaluator. Identity remained to be an elusive concept, that is, without recognisable face.

How social identity or boundary is archaeologically observed over time and space is the second line of inquiry demanding concrete explanation to fully satisfy the book's main goal. As an anthropologist, the author is well aware of the dynamic and complex character of social identity that is constantly negotiated across time and space and affected by various factors like environment, subsistence, and politics. However, this is not concretely reflected in her discourse. My impression is that a solid regionalist identity homogenously exists since the earliest times. While it is true that convergences are observed every now and then,

specific and distinctive smaller group traits likewise exist. She could have provided input on the ethnographic diversity of groups that compose the region. One angle is the dialectal or linguistic differences that can aid in defining social boundary. She can turn to linguistic studies considering that modern day residents in Bicol show strong affinity to a specific dialect—a form of identity that has grounding in the past.

Moreover, as an archaeologist, she could have capitalised on the materially observable traits among the artifacts in asserting specific identity in the manner or format done by Solheim in his pottery studies (Solheim 2002). Limited researching time might have prevented her from accomplishing similar feat; nevertheless the complexity of her research problem demands equivalent efforts. The use of informal sources like haphazard museum collections needed sufficient disclosure and explanation to avoid painting a wrong picture to the average readers. Lastly, the real people behind the archaeological record, the subject of any prehistoric reconstruction, should receive more attention for an identity discourse that truly goes beyond the level of "culture history" (Willey and Sabloff 1993).

Defining social boundary is a daunting task Ragrario accepted. Her efforts in contributing to the definition of a regional identity deserve heartfelt commendation. Future archaeologists, researches, and scholars will find reading this reference material worthwhile.

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The Origin of Our Species
Chris Stringer
2011. London: Allen Lane

Review by Rob Rownd
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The Origin of Our Species by Chris Stringer is an old guy's book. But it's far from tired.

Patient in tone and deliberate in pace, the book has been mildly chastised by some scientists for allotting too much space to discussion of some 'flighty' scientific ideas (Hawks 2011). It has also been mildly chastised by some popular intellectuals for not living up to its title and summing up the history of humans in half a dozen declarative sentences (Forbes 2011). But the key word in both cases is mildly. Even people who don't accept his ideas or admire his work, respect the civility and openness Chris Stringer brings to its defense. Still, I doubt Stringer would worry much about either opinion even if they were harshly expressed. This book, like his other writing, his public lectures and video interviews is always focused on the work and the ideas behind it.

Since joining the research staff of the British Natural History Museum in 1973, Stringer has acquired over a yard's worth of credits for journal articles and written 10 books for general audiences, all while serving as the very public face of the out-of-Africa theory of Modern Human origins. This is someone who is very comfortable contextualising

new ideas for non-specialists and the interested general public. Stringer's prose rolls off the page as easily as his many public lectures roll off his tongue. Common sense doubts and admissions of the incompleteness of certain ideas are treated with the same understatement that is used to stress the things he is relatively sure of. His simple concise descriptions of complex theories and scientific processes are fair, well balanced and easily followed even when he does not agree with them. He also has a nice way of doubling back to clarify or reinforce points that never seems pedantic or unnecessary.

What makes this all the more impressive is that *The Origin of Our Species* is a significant modification of his previous very publicly stated views on the Origins of Modern Humans. In the book, he addresses the overwhelming evidence that we are not entirely Out-of-Africa after all. The bigger, more detailed and most likely more accurate picture of how we came to be us is a work in progress. Genetic evidence from the last decade has backed up the common place observation that we look kind of different from each other and those differences seem to be based on where our immediate ancestors came from. It turns out that we are genetically different from each other but only very, very slightly. Does it matter? Stringer does not think so but he is not sure. It's going to be an interesting few decades while we figure out what *slightly* actually means.

The book is laid out as a personal history, a genre a lot of scientists seem to find tricky. But Stringer's restraint serves himself and the material well by keeping the book centered on how the ideas about human origins came up and were modified over the past 40 years rather than veering into his personal reaction to them or the people who developed them. There is a good deal of personal opinion and reflection here but it is used to tie single ideas into threads or mark the contrasts in shifts of opinion over time. This is science as we all know we were supposed to be doing it.

After summarising the dominant theories of the origins of homo sapiens circa 1970 (when his career began), the narrative begins with describing how the introduction of room sized mainframe computers (less powerful than the current IPhone) changed the study of skulls. Suddenly comparing multiple measurements across dozens and then hundreds of samples was possible if one put a couple of years into the project.

Stringer did. Lacking the precision and speed of the scanning and number crunching software that is so cheap and easy to use in the 21st century,

the new method of the 1970's seems almost quaint to those of us who were not there at the time. But the ability to compare so many sample skulls on so many points was a great leap forward.

It was also the beginning of the end for any serious consideration of Neanderthals as the species who developed into early modern humans. They are just too morphologically different from modern human to be a possible immediate ancestor. Those differences became unarguably apparent when the data derived from the computer aided morphological studies became public and the idea was dead and buried within a decade.

Stringer freely acknowledges that a lot of his best ideas and observations began as vague hunches. He saw something was amiss in the then current thinking and wondered what it was. But he considers himself lucky to have been begun his career when the "standard approach [was the same one that] had been in use since before the time of Charles Darwin" (Stringer 2011: 86) and to have then been able to take advantage of the development of much more precise ways of measuring nearly everything involved with Archaeology. It was the 'pictures' that emerged in the computer age, the high resolution representations of the material (whether expressed as numbers or graphs or images) that we have access to now which changed things.

Even if we limit the discussion to new high resolution images of the original Neanderthal and Cro-Magnon 'type' fossils that were found before the turn of the 20th century, the images of them simply tell a completely different, and more accurate, story than was readable prior to the 1980's. There are, of course, additional finds that add to the narrative in hugely significant ways and individuals who have done some amazing work but to hear Stringer tell it, it is the discipline as a whole that has undergone the sea change. The explosion of understanding that occurred in the last 40 plus years is as much about the tools now being brought to site and into the lab as it is about the people using them. Vague hunches are just much more easy to confirm or negate now than they were before.

For example, back in the later 1970's, based on little more than one of those hunches and some long conversations, Stringer along with some American colleges began arguing for limiting homo sapien status to fossils that 'look' like us. By extension of the same idea, they argued that sets of fossils which look like each other should also be assigned to a common species even if they were found in disparate locations. The *Homo heidelbergensi* in Bodo and Broken Hill, their argument went, are the

same species as the German based name specimen even though they are some nine thousand kilometers apart. Flipping the same idea on its back to exclude non-like parts, Neanderthals were not directly ancestral to Modern Humans despite the fact that their remains had been found in different layers of the same cave in more than one location because they looked too different.

Using this new conceptual framework essentially threw out physical distance between finds as a significant consideration for typology and specification and replaced it with a morphological similarity/difference that was barely measureable at the time. And, with that shift in thought, Heidelbergensis was spread out across an area large enough and at an appropriate time in the past to be the potential immediate common ancestor of both Homo Sapiens and Neanderthals.

While the difficulty of measuring this 'significant difference' didn't make their idea untrue of course, it made it very difficult to substantiate. Hence it was a fairly risky and unpopular idea despite being internally consistent and working well as an explanation for the similarities between Neanderthals and Homo Sapiens. They shared a common ancestor but had developed differently from that common point.

Over time, the degree of difference and similarity between skulls has been much more observable as measuring techniques and number crunching abilities improved to their current state. Stringer's acceptance of a difference that was barely measureable at the time now looks like a combination of brilliance and blind faith in technology but its really neither. It is just a very careful consideration of how an artefact could fit into an extended context. Stringer seems to be able to think and consider finds in multiple contexts more easily than most. Further evidence of that the same willingness to follow a strange hunch to the place it logically leads in the meta-context of our collective data can be seen in his consideration of the 'hobbit' as possibly an Australopithecus afarensis gone a wandering and then surviving in isolation till 17kbp.

To contemporary eyes, this idea is at least as much of an outlier as the idea that Heidelbergensis had a range from Africa to Germany during its peak was in 1978. But since then we have uncovered Box Grove and Swanscombe, which only adds to their range and takes us even further away from reasonable doubt about how successful this 'species' was in its day. So the possibility that we will have out-of-Africa-1 reassigned to a creature that was pre-human in brain size and body type but somehow

learned to use tools to hunt stegodons should remain open for awhile. In 2015, *Nature* featured a description of evidence of human-like hand use in Australopithecus africanus. (Skinner *et al.* 2015). Hmmm, Looks like it is going to be an even more interesting couple of decades.

Stringer is far more interested in Anatomy (and Morphology in particular) than he is in Genetics, Tools, and, surprisingly, behaviour. But he gives all the elements their due in his story. The lengthy and balanced consideration of what likely went on in Africa, Europe and the Middle East between 200kbp and 25kbp is so wonderfully written, you wish it would have included more of the world and gone on for another 350 pages.

Stringer is not at his strongest discussing Theories of Mind, 'symbolic non practical' activities such as playing music and painting or the beginnings of pre-planned collaborative behaviours that do not have an obvious purpose. The two chapters on behavioural modernity are the weakest part of the book as he falls back on the discipline's long tradition of interpreting every found symbol to be a sign of communion with powerful things unseen. Was the late Pleistocene really devoid of boredom, joy, beauty, doodling and friendship? If so that would make it rather unique in the human adventure. Somewhere in the material record, there is probably solid evidence of somebody drawing an elk on a wall, playing the bone flute or smearing ochre on someone else just because they liked doing it. And it is probably being misread as being deeply, deeply, deeply symbolic. It is hard to believe that Neanderthals and early Modern Humans cared for their injured and maimed, buried their dead with grave goods or showed any kind of compassion towards each other without some memories of good times to bond them together. We see the jokes in the art from our era, even including art from Rembrandt to Andy Warhol. Yet, we do not extend that courtesy backwards in time. Rather Victorian of us.

Stringer's section on DNA however is a wonderfully concise and accurate description of some extremely complex material. We all "know" that we are connected to "Eve" and later to "Adam". We also know that everyone outside of Africa, has a little Neanderthal in them. And we know that Genghis Khan and Brian Boru and another half dozen super donors really got around. DNA does seem to be an amazing tracker of human interaction. However, as with the first version of carbon dating, workers have realised that there is something slightly off about the clock

and it is in need of calibration. That work has not been done yet. So, for now, we should take the dates it suggests with a grain of salt.

Stringer also spends time earlier in the book considering the limitations of the Biological Species Concept (BSC) that so much of the multi-regionalist argument is predicated on and then returns to it for his consideration of the meaning of the admixture in our own DNA. To paraphrase Stringer's already broad strokes, a species consists of the largest community of a group of plants or animals that breeds amongst itself but not with any other community . This is one of the old chestnuts of biology we all learn in high school and then have to learn a series of exceptions to in graduate school. One of the latest ones to be confirmed, polar Bears and grizzlies (Paabo 2014) is not discussed by Stringer but it sums up his assessment particularly well.

Over the past ten years, grizzlies in the wild have been observed migrating into polar bear only habitats in Manitoba, the Northwest Territories and even far northern British Columbia. Perhaps driven by various environmental stresses due to increased human populations in the area or simply in search of food, bear nature being what it is has led to the creation of a hybrid animal nicknamed 'the prizzlie'. Confirmations by DNA tests of hunter kills have even revealed a specimen that is a 2nd generation hybrid. This, of course, means that at least some of the first generation hybrids are fertile. A similar 'process' has be repeatedly induced between grizzlies and polars in zoos (although the use of Barry White classics during the 'inducting' process does tend to call the results into question).

While these cases should have multi-regionalists jumping up and down with joy for its apparent support of a continuum between species in the classic Darwinian sense, the simple math (including the DNA tests) actually points in the other direction.

There is ample evidence of hybridisation between related species occurring throughout nature. It simply happens. But it is always a small minority of the individuals in a species at any given point in time who are hybrids and that small minority decreases significantly in size as the organisms increase in complexity. Vascular plants feature 25% hybridization, butterflies 16%, birds 9%, and mammals 6% (Mallet 2008). We feature evidence of about 2-4%, hybridisation that has degraded over time to about half of its original strength because of the usual gene mutations. While we know that we need to further calibrate the genetic

clock to make it more accurate, this is in line with what one would expect for a fairly recent 'encounter' between higher mammalian 'species'.

Returning to the members of the Ursidae (bear) family, we find there is ample evidence of ancient admixtures between nearly most of them in the wild just as there is between Homo Sapiens and at least three archaic human 'species' (Kutschera *et al.* 2014; Miller *et al.* 2012). But that does not change the current reality that polar bears and grizzlies are two very distinct animals both physically and behaviorally who have adapted to thrive in completely dissimilar environments. Each of them is at the top of their respective food chains and there is no way you would confuse one for the other. It's certainly a surprise that they can successfully interbreed but, at the end of the day, does that actually change anything about the way we understand them?

Instead, it seems more likely that there is something more to being a type or 'species' of bear than who they can mate with. And by extension the same should apply to humans. For me, one of the strangest things about the discovery of the Denisovan genome is how quickly it turned up in our own admixture. It was literally within the month. It is as though as soon as they knew what to look for they found it. Stringer's take on the hominin admixing that has been documented in our own genomes is more of a polite shrug at the oddity of it than anything else. In his opinion we are, based on both the majority of our own genes and our peculiarly 'globular headed' morphology, mostly out of Africa. The things that are most significant and interesting about us are also mostly out of Africa too.

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Pangasinan, Pinablin Dalin: History, Culture, Development
Edited by Virginia J. Pasalo and Fe B. Mangahas
2015. Lingayen: Pangasinan Historical and Cultural Commission, 808 pp.

Review by Erwin S. Fernandez
Abung na Panagbasay Pangasinan

Edited by Virginia J. Pasalo and Fe B. Mangahas, then Commissioner of the National Historical Commission of the Philippines, the book was a product of Memorandum of Agreement (MOA) between the provincial government and the Pangasinan Historical and Cultural Commission (PHCC) in 2011, which led to a formation of a research team headed by Dr. Perla Legaspi. In August 2012, the last draft was submitted to the PHCC editorial board, which, then, decided to commission six new authors – myself included – to revise and add new chapters to the manuscript. Thus, in January 2013 I signed a contract to write two chapters for the "new" book. In the summary for Parts II and III, the

editors agreed to my assessment, which I wrote in my letter to the governor decrying the quality of the research, that the narrative mainly relied on secondary sources, on the three-volume history of Pangasinan by the late Prof. Rosario Mendoza Cortes (1974, 1990a, 1990b). As an attempt toward self-reflexivity, I will also point out some necessary clarifications in my work based on the additional research and readings that I did.

The book is divided in six parts and twenty chapters. For Part I, consisting of two chapters, which I wrote, begins with a discussion of the geologic origins of Pangasinan from the late Cretaceous period to the arrival of early humans in the Philippine archipelago tackling the flora and fauna, the formation of indigenous knowledge in astronomy, land and water resources, climate and temperature, typhoon, earthquakes, agriculture, fish and marine resources, forest and mangroves, wild fauna, plants, herbs and fruit trees, and gold and minerals, origins of villages, towns and province and finally origin and development of industries. One egregious editorial mistake is apparent in the following: "Spanish chronicles, like Fr. Lorenzo Cosgaya (1865) also diligently recorded these terms in Pangasinan-Spanish dictionaries" (32). I utilised Cosgaya's Pangasinan dictionary but I never stated that Cosgaya was a Spanish chronicler. He was not. The original text ran: "Terms in Pangasinan language have preserved this ancient knowledge. Early dictionary by Fr. Lorenzo Cosgaya (1865) recorded these terms." Unfortunately, personal names have been deleted such as my grandfather's name, Mariano Sison Soriano, when I referred to the case of nipa, an old industry, which he can still buy at a local village store in the 1990s. The personal anecdote was reduced to an anonymous, inert statement. In the second chapter, I traced the ethnohistorical development of Pangasinan nation from the various theories regarding their origins as well as my own to its participation in the international trade in the fifteenth century AD. There are assumptions here that I need to clarify or even delete given my exposure to an informed review by another scholar. Also, I was inconsistent with my use of Pinyin and Wade Giles style for the transliteration of Chinese texts.

Part II comprised of three chapters dealing with Pangasinan at the point of Spanish contact, Spanish colonialism and Philippine revolution. Part III, comprising of two chapters, tackled American rule and Japanese occupation. Part IV, consisting of three chapters, discussed contemporary history from the postwar to 1986. These are the main parts of the book that deserve further scrutiny as they deal with history.

The main weakness of these chapters as noted earlier is that they mostly did not consult primary sources. They mainly relied on secondary sources preventing the authors to verify whether the previous assertions about Pangasinan were true or not. They also perpetuated major errors such that Pangasinan came from *Panag-asinan*. This again is a recent invention for in the primary contact period accounts, which I was able to consult from the *Archivo General de Indias*, Pangasinan was really Pangasinan with one clerical error referring to the place as Pagasinan. In that document, Pangasinan was the name of a river, most probably Agno, the major river in the province (Anonymous 1572). Also, in the same dictionary by Cosgaya (1865:101), saltworks or saltbeds was given as 'Pangaasinan', and not 'Panag-asinan', which in a linguistic process called elision later evolved into Pangasinan. The references to a Luyag ed Dapit-Ilog as Caboloan and Luyag ed Dapit-Baybay as Pangasinan were based on a 1920 account in the Census of the Philippine Islands whose data can be questioned since it does not indicate sources.

Relying on secondary sources that dealt with Tagalog society, Fe de la Luna A. Andico, Shiela Marie M. Dasig and Ma. Cristina B. Daligcon in Chapter 3 "Ancient Pangasinan at Point of Contact" conflated Pangasinan society with Tagalog society flattening their differences by citing their similarities. They kept alive the story of Urduja, the so-called Amazon princess who ruled ancient Pangasinan, who was not from Pangasinan but a foreign historical figure (Cortes 1995). They maintained the assertion that early Pangasinans only practiced animism and nature worship when it was possible that Hindu-Buddhist practices must have filtered through their early beliefs before the arrival of Islam and the Catholicism in the Philippines (Fernandez 2014). They were ignorant of the existence of an extant manuscript bearing Pangasinan indigenous scripts in the *Archivo de General Indias* when they wrote: "samples of artifacts utilizing the Pangasinan language have yet to be discovered" (177) despite in 1599 a petition by the Mangaldan elite recorded signatures in that script (Villarroel 2008). Chau Ju-kua (Zhao Rugua), a Chinese commissioner of foreign trade, did not mention any polity called Ling-ya-mon as a reference to Lingayen as Andico *et al.* indicated. Encomienda was tackled in the later part of the chapter which could have been relocated to the earlier sections of the same chapter.

In many sections of Chapter 4 (191-235), written by Dasig, Daligcon and Legaspi, are generalisations not supported by primary sources on *reducción*, hispanisation, low status of women, the

confrontation between indigenous religion and Catholicism, education, the decline of precolonial economy because they based their assumptions on what is written in traditional historical textbooks such as those written by Agoncillo and Alfonso (1967), Agoncillo and Guerrero (1977), Constantino (1975), and Agoncillo (1990). Primary sources can be found in the Archivo General Indias and UST archives among others. The assumption that "there was no real Filipino participation and no representation in municipal governments" (230) during the Spanish period is, I believe, baseless. It lacked local examples on the interference of the friar on local affairs that can only be known if they consulted primary sources. In Urdaneta, for example, a teacher was removed from service due to the influence of a friar (Fernandez 2013).

Discussion on the Philippine revolution of 1898 by Legaspi in Chapter 5 is miserably short for there was no effort to use the many accounts on Pangasinan from the Philippine Revolutionary Records (see Fernandez 2013 for the primary sources used from the said records).

In Chapter 6, Dasig, Daligcon and Legaspi including the editors seemed to have been afflicted by the black legend, which demonised the Spanish contribution to Philippine civilisation by stating that "Americans started the modernization of Pangasinan" (269). It was Spain, which brought modernisation with the coming of reforms in education and the development of Manila-Dagupan railway (Cortes 1990a). The discussion on the Japanese occupation by Dasig and Legaspi has no introduction and no context focusing only on some towns. The insertion of Ferdinand E. Marcos in the narrative is suspicious (297). Although he was a guerrilla officer before the fall of Bataan, he was not a guerrilla leader during the Japanese occupation – he contrived he was the head of his fake Maharlika unit – but, in fact, he was a Japanese collaborator and a black market dealer (McDougald 1987).

Human interest was lacking in Chapters 8 and 9 written by Dasig, Daligcon and Legaspi and Andico and Dasig, respectively, for statistical tables supplied the data while in Chapters 10 and 13, both written by Legaspi, one cannot fail to notice that there are two styles, one written by Legaspi characterised by a periodisation based on national events and national laws as highlights that are somewhat irrelevant to the topic and the inclusion of provincial details that should be the meat of the narrative, which must be the intervention made by the editors.

The chapter that is the most problematic of the lot is Chapter 11 "Groundings and Expressions of Pangasinan Culture" by Celestino Cesar D. Joven which tackled Pangasinan language and literature, architecture, graphic arts, clothing and ornaments, and finally performing arts. It suffered from a surfeit of errors, typographical and factual, as well as unsubstantiated assumptions. Obviously Joven did not know anything about his subject while the editors who should know better as they are tasked to correct errors did not remove these. The highest god in Pangasinan pantheon is not Apoguley but Apolaki and no tribe is called Malasiqui (357), except a town, which had that name. The writing style is rather stilted and many times ungrammatically constructed.

Most disconcerting are the numerous unsupported assertions: the language was brought by people through waves of migration (359), the language is syntactically different from the rest of the Philippine languages when each language has its own syntax different from other languages (359), no pre-colonial oral tradition (361); the development of Pangasinan vocabulary stopped during the Spanish period (365) and Jose Palma was a Pangasinan who should have written *Filipinas* in Pangasinan rather than in Spanish (366) when Palma was a Tagalog born in Tondo. In the discussion on architecture (374-398), the focus was on the *bahay kubo* instead of *abung a nipa* or the *abung a bato*.

There must be distinction and difference between the two and it is up to the researcher to know and understand the Pangasinan *abung*. For example, what is a oanán, olóy abung, lusec, panocóng, panocsolán, silongay abung, sipi (Cosgaya 1865)? How different is an abung a simpóc from tinapin abung (Ibid.)? Again there are questionable statements such as that housing in early Pangasinan did not show architectural differences in design either through class or wealth (378), that there was no prehispanic religious architecture (379) when an early religious shrine called *anitoan* existed (Aduarte 1640/1690). No significant research was made on graphic arts, clothing and ornaments and performing arts throughout the different periods in Pangasinan history so that any assumption made is tentative, unwarranted and superfluous.

The only exception to the dull presentation of the preceding chapters is Chapter 12 by Ma. Crisanta Nelmida-Flores. It discusses Pangasinan thematically dealing with *Kabayawasan* tradition of indigenous educational practice under the guava tree for youngsters, Our Lady of Manaoag and the *manag-anito*_tradition, the connection between

indigenous priestess or shaman and the worship of the Virgin of Manaoag, Princess Urduja, cattle caravans, *anacbanua*, the local term for the indigenous elite, and Pangasinan literature and the arts. A number of these subjects, however, already appeared in the author's articles (Flores 1999, 2004, 2005, 2007), which actually came from her doctoral dissertation (Flores 2002).

In Part V, four essays by Florangel Rosario Braid, Anabelle E. Plantilla, Rodolfo Vicerra and Virginia J. Pasalo tackled education, environment, economic development and the future of Pangasinan respectively. While no doubt these essays were written by experts in their own fields, a significant opportunity was lost to have the views and perspectives of those who are working in the provincial government of Pangasinan. For example, the head of DENR Pangasinan, the provincial health, or the provincial administrator could have provided an insightful long-term programme they plan to do or leave behind with the facts and data they have at hand and the hands-on experience they knew from the grassroots.

Part VI gathered miscellaneous data on the province in three chapters. Chapter 18 by Cynthia P. Lopez and Irene A. De Vera basically repeats data discussed in the preceding chapters. Chapter 19 by the research team is a list of tables on various political, social and economic statistics regarding Pangasinan. Chapter 20 by Virginia J. Pasalo shallowly deals with Pangasinan women working as Overseas Filipino Workers (OFWs) through a listing of statistical tables. An index at the end of the book is provided.

The most controversial, however, was the book cover, which highlighted at the centre a bare-breasted warrior woman in the act of brandishing a sword who was no other than Urduja. As I have said earlier, Urduja was not a Pangasinan historical figure. She was already the subject of a national conference in 1990 (as cited in Magno 1992) in which the foremost lady historian of Pangasinan, Prof. Cortes (1995) has pronounced her as somebody who was not a real Pangasinan figure seconding her mentor, Prof. Nicolas Zafra (1952), on the issue. But the editors who were advocates of women's rights, short of being called feminists, wanted to maintain the falsehood because it jibed with their politics and advocacy. Also circumstances forced them to do so since the princess was named after the residence of the governor in the capital. Yet, if they are looking for a Pangasinan female historical character who must

have the same prowess if not greater than Urduja, it does not take time for her to be discovered, which this book could have done only if they did their job.

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