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Book Reviews

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BOOK REVIEWS

HANSON, VICTOR DAVIS, ed. *Makers of Ancient Strategy: From the Persian Wars to the Fall of Rome*. Princeton: Princeton, 2010. Pp. 278. Cloth. \$27.95. ISBN: 9780691137902.

Since published by Princeton University Press in 1986, *Makers of Modern Strategy from Machiavelli to the Nuclear Age* has become a seminal work of military history. Composed of twenty-nine essays addressing topics and military figures from Clausewitz and Mao, this 950-page behemoth has set a daunting standard for all other compilations of its type. It is in the spirit of *Makers of Modern Strategy* that editor Victor Davis Hanson—calling in such illustrious figures of ancient military history as Donald Kagan, Barry Strauss, and Adrian Goldsworthy—has sought to create an ancient world parallel in the form of his recent compilation *Makers of Ancient Strategy: From the Persian Wars to the Fall of Rome*.

From the outset, *Makers of Ancient Strategy* is more of a warm-up exercise or test run for something far more grandiose—perhaps it may be Princeton's way of mimicking the development of its modern parallel, which was originally published in 1943 and contained only seven essays (which obviously had little to say about the nuclear age). Whatever the intent behind its publication, *Makers of Ancient Strategy* is nevertheless a comparatively short work, containing ten essays spread over 278 pages. Also different from the standard set by *Modern* is the at times peculiar directive established by Hanson in his introduction. He indicates that "*Makers of Ancient Strategy* not only reminds us that the more things change, the more they remain the same, it also argues that the classical worlds of Greece and Rome offer a unique utility in understanding war of any era" (3). As such, Hanson and his collaborators have attempted,

with varying levels of effectiveness, to select topics that allow them to show the sometimes remarkably similar military situations that can be found between the ancient and modern worlds.

The introduction to Tom Holland's "From Persia with Love" (11–30), for example, executes the ancient to modern connection rather brilliantly:

The invasion of Iraq, when it finally came, was merely the climax of an ongoing period of crisis and upheaval in the international order. The stand-off between the two sides had been a geopolitical fixture for years. Both had surely long suspected that open conflict was inevitable. As the invaders crossed into Iraqi territory, they would have known that they faced a regime that was hardly unprepared for war. It had been assiduous in stockpiling reserves of weaponry and provisions; its troops, massed along the border, blocked all the roads that led to the capital. . . . Yet all the regime's defenses, in the final reckoning, might as well have been made of sand. What it confronted in its adversary was nothing less than a superpower, the most formidable on the planet. The task force brought to bear by the invaders was a quite devastating display of shock and awe. Those of the defenders who were not left corpses by the first deadly impact of the enemy onslaught simply melted away. Even in the capital itself, the population proved signally unwilling to die for the sake of their beleaguered leader. A bare few weeks after hostilities had begun, the war was effectively over. So it was, on October 12, 539 BC, that the gates of Babylon were flung open "without a battle," and the greatest city in the world fell into the hands of Cyrus, king of Persia. (11)

To modern readers familiar with the recently concluded Iraq War, this passage sounds quite similar to the newscasts and newspaper articles of April 2003—an effective technique illustrating that technology changes only a few things in the nature and circumstances of warfare. Likewise, Hanson's own "Epaminondas the Theban and the Doctrine of Preemptive War" (93–117) makes a number of allusions to the American instigation of the Iraq War. For example, Hanson points out that the Theban military's invasion and liberation of Messenia from Sparta in 370–369 B.C.E. was a decision that was met with much of the same derision and scorn that has been directed at proponents of the modern Iraq conflict. Hanson concludes that while the invasion of Messenia was controversial among Thebans, Messenia soon became an established city-state that not only provided (relative) liberty to thousands of its inhabitants, but the city also served as a strategic check on the greatly weakened Sparta. As such, Hanson implies that the war in Iraq could yield similar results for the United States in the future.

Other selections, such as Susan Mattern's "Counterinsurgency and the Enemies of Rome" (163–84) and Ian Worthington's "Alexander the Great, Nation Building, and the Creation and Maintenance of Empire" (118–37),

struggle to find parallels with the modern world, and they appear forced and unnecessary. Some selections also seem to sacrifice depth while bridging the gap between past and present; as a result, especially in the case of “Alexander the Great,” they read like textbook entries from survey courses. While a helpful leg-up for the casual or nonspecialist reader, this practice falls far short of the standard set by *Makers of Modern Strategy* and diminishes the intended significance of its ancient world counterpart. Thus, while the modern applications featured in *Makers of Ancient Strategy* are sometimes apt and informative, the book would have been more effective in its original goal if they were treated more as the novelty that they are, rather than as the general theme of the entire work.

Makers of Ancient Strategy is a work that aspires to something greater than the sum of its parts in attempting to supply evidence to prove to the modern world that the ancients were not so different from us and that their experiences with warfare and international politics can serve as useful case studies for our own entanglements. Though some of its essays hint at the potential of a volume of similar depth and quality to parallel that of *Makers of Modern Strategy*, it seems that we will have to wait yet a while longer for such an arrival.

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ROLLSTON, CHRISTOPHER A. *Writing and Literacy in the World of Ancient Israel: Epigraphic Evidence from the Iron Age*. Atlanta: SBL, 2010. Pp. 144. Paperback. ISBN 9781589831070.

In his new book, Dr. Christopher A. Rollston, professor of Old Testament and Semitic Studies at Emmanuel School of Religion, expands the understanding of the origins of the Israelite script and literacy by examining epigraphic evidence from ancient Israel. Rollston's excellent work presents many epigraphic sources that illuminate our understanding of Israelite literacy. In fact, in November of 2011, the American Schools of Oriental Research presented Rollston with the Frank Moore Cross Award because of his scholarly contributions. In the course of his book, Rollston addresses an educated audience who would easily comprehend the Hebrew language as well as technical terms associated with ancient studies. However, he means the book to be utilized as a tool for expanding basic knowledge of the paleography of abecedaries and scribal practices.

Rollston builds his argument by creating stepping-stones in his themes, including (1) the national scripts used in the Levant succeeding the Phoenician script, (2) the status of scribes during the Iron Age in Israel, and (3) the literacy among Israelites. Ultimately, Rollston concludes that the textual evidence left by literate Israelite scribes proves that there were standardized educational systems present in Israel during the Iron Age, despite opinions to the contrary.

Rollston begins with a comprehensive study of the origin of the Old Hebrew script that would become the national script of Israel in the ninth century B.C.E. He explains that the earliest Northwest Semitic scripts were not standardized: the letters varied in stance, writing direction was undecided, and there was a far greater number of consonants than later settled upon in the Phoenician alphabet. These factors were standardized in the second millennium, forming the early Phoenician alphabet (19).

During this segment, Rollston performs an extensive review of Phoenician texts that illustrate the developments throughout the ninth and eighth centuries B.C.E. By studying the lengthening in the strokes of the letters *samek*, *mem*, *nun*, *tet*, and *kaf*, specifically in the Ahiiram Sarcophagus inscription, Azarba'al inscription, and the Yehimilk inscription, the author argues that a text can be dated according to stroke lengths (21). More importantly, after close inspection of these alterations in scribal techniques, Rollston provides a convincing argument concerning when the Old Hebrew script was first born. Through his assessment of the nuances between similar Phoenician letters, Rollston shows with his hand-drawn copies of the inscriptions, that Old Hebrew was not developed in the tenth or ninth centuries as previously thought, but rather in the late-ninth to early-eighth centuries B.C.E. (35, 42). He argues that the distinct epigraphic developments in the Phoenician abecedarly are evidence for an institutionalized educational process for the literate elite.

Alterations in the morphology of the Old Hebrew script, as Rollston points out, are careful and purposeful “national statements” which illustrate a distinct break from the *Mutterschrift* rather than “an evolutionary development” (44). This suggests that careful thought was given to the script provided by the scribes. Rollston uses the second half of his book to demonstrate that the scribes in the Levant would have been treated as elites or come from an elite family line.

He offers support of the preceding claim by presenting textual evidence discovered in Egypt, Mesopotamia, and Israel. The Egyptian Papyrus Lansing “extols the scribal profession by affirming that the scribe. . . persists in training and becomes a scribe so that ‘you may become one whom the king trusts; to make you gain entrance to treasury and granary (translation taken from

M. Lichtheim)" (85–86). Additionally, Rollston cites the Satire of the Trades and the Papyrus Anastasi II which both confirm the prestige that accompanies the elite status of scribes because they are saved from manual labor (86). Similarly, Mesopotamian and Hebrew texts laud the scribal profession and all the wealth and success it brings. While there are less Old Hebrew epigraphic texts, Rollston states that the canonical First and Second Temple writings "discern that the scribe was an esteemed member of elite society" from the many mentions of royal scribes that suggest their closeness with the palace (88). These textual evidences reinforce Rollston's theory of the scribal status in the Levant.

Along with demonstrating the elite nature of scribes, the book expresses the theory that literacy was contained solely among the elite in most cases. Rollston proposes that literacy is "the possession of substantial facility in a writing system . . . with minimal errors of composition or comprehension" (127). He argues that while some Israelites might have been semi-literate, the evidence does not point towards a high literacy rate (127–28).

With these limitations on the definition of literacy, Rollston is able to narrow the scope of the literate to, once again, the elite. He substantiates his theory by focusing on the cultural atmosphere that gave the elite families a higher likelihood of literacy. Speaking specifically of those in scribal families, Rollston brings forth the idea that they would have been accustomed to seeing documents, and thus their "print exposure," as he calls it, would create an environment conducive to a higher literacy rate in the next generation (124–25).

By focusing on primary textual evidence from Egypt, Mesopotamia, and Israel, Rollston presents the argument that extreme literacy would have been needed for scribes in high positions in order to be consciously aware of alterations made within their abecedary. Such literacy would only have been achieved in standardized educational institutions (ch. 6). During this time, schools were present in both Egypt and Mesopotamia. However, even though there has been no substantial evidence for such institutions in Israel, paleographers are of the opinion that such establishments must have existed. Rollston shows this possibility through Israelite records that could be student practice texts similar to those found in Deir el-Medina where more experienced handwriting stands next to an obvious novice's.

Likewise, Rollston's argument is drawn back to the origin of Old Hebrew where, it seems, a conscious decision was made to deviate from the Phoenician script (44). Through charts showing specific letters in the alphabet and how they differ between Hebrew, Aramaic, and Phoenician (shown on pp. 98–99, 101–2), Rollston convincingly reveals that "the Old Hebrew epigraphic record

reflects synchronic consistency *and* diachronic development is significant because it necessitates a mechanism, namely, formal, standardized scribal education” (103).

Rollston provides a clear explanation of ancient Israelite writing and literacy by arguing that the two were developed through standardized educational institutions. He builds upon his argument in a manner that is easy to follow. Further, his book is balanced by plenty of opinions that express both sides of the argument before he presents his view, allowing the audience to form their own opinions before being swayed by the author’s. While I would have liked more explanations of why he chose his viewpoints in some instances, this well-written book gives new insights into Northwest Semitic records and scripts, creating a thirst for more knowledge on the subject. Dr. Rollston’s book would make an excellent read for anyone with the desire for a more thorough understanding of the scribal impact on ancient Israel.

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LICONA, MICHAEL R. *The Resurrection of Jesus: A New Historiographical Approach*. Downers Grove, IL: InterVarsity Press, 2010. Pp 718. Paperback. \$24.00. ISBN: 978-0830827190.

Prior to reading this book, I had a well-formed opinion against the central premise of the work—a historical proof for the resurrection of Jesus. After reading it, my thoughts have changed. The author, Michael R. Licona, a research professor of New Testament at the Southern Evangelical Seminary, received his Ph.D. from the University of Pretoria. Licona’s book, *The Resurrection of Jesus: A New Historiographical Approach*, is an outgrowth of his doctoral dissertation and discusses whether the resurrection of Jesus can be “proven” historically. Licona’s work is by far the most thorough discussion of the methodology, historiography, and texts surrounding Jesus’s resurrection from the dead. It’s difficult to say how much of his approach is “new,” as many scholars before him have made similar arguments. What Licona does bring to the table is his exhaustive collection of all the arguments, theories, texts, and interpretations relevant to the question of whether or not Jesus was resurrected. In that regard, he has made the most thorough and complete argument of any scholar for the historicity of the resurrection of Jesus. Licona’s fundamental thesis is if a historian “brackets the question of worldview, neither pre-supposing nor

a priori excluding supernaturalism, and examines the data, the historical conclusion that Jesus rose from the dead follows” (608).

Without question, the most important part of Licona’s work is the first few chapters. He argues that every aspect of the historical method, particularly for ancient history, is colored by a historian’s worldview, what he calls a “horizon.” A horizon refers to the “knowledge, experience, beliefs, education, cultural conditioning, preferences, presuppositions and worldview” of a historian (38). Licona rightly credits the horizons of historians for producing so many theories about the resurrection of Jesus, theories ranging from historicity, to psychological delusion, to deceit, to forgery and simple confusion. By understanding the role horizons play in the writing of history, Licona notes that every historian’s horizon will permeate the entire analysis of the texts surrounding Christ’s resurrection. The very existence of horizons undermines historical objectivity. Though historians can establish some historical truth, they can never be absolutely sure of anything in the past because the method of history is colored by subjective elements related to the historian himself.

Following these initial and broad statements about historical inquiry, Licona outlines important assumptions for historical methodology: criteria for weighing and adjudicating between differing historical hypotheses. He presents five different categories that all hypotheses should be judged by, although he notes that there is widespread disagreement about which of the five should be given priority. The five criteria are: (1) Explanatory scope, or how many facts are accounted for by a hypothesis; (2) Explanatory power, or the quality of the explanation a hypothesis gives, how clearly it explains the facts; (3) Plausibility, meaning that if a hypothesis is implied by accepted truths, it is preferred to one that is not; (4) Less *ad hoc*, meaning a hypothesis with fewer “non-evidenced assumptions [that] go beyond what is already known” is preferable (110); and (5) Illumination, where a hypothesis provides a solution to some other historical problem. Licona admits that he values “plausibility as the most important criterion, followed by explanatory scope and power, followed by less *ad hoc*” (113).

The final discussion prior to actually examining the evidence is by far the most important. In the second chapter, Licona discusses the role of miracles in history. In doing so, he outlines and critiques the theories of philosopher David Hume and scholars Behan McCullagh, John P. Meier, Bart D. Ehrman, and A. J. M. Wedderburn and James D. G. Dunn. These scholars discuss a historian’s ability to establish miracles in history, the role of probabilities when establishing historical fact, and the use of horizons in justifying a historian’s

worldview. Bart Ehrman's arguments, which have become particularly popular, are discussed at great length by Licona. Ehrman makes five arguments to deny miracle claims in history, but one is more convincing than the rest. It can be summarized as follows:

1. By definition, history tries to establish what probably happened. Miracles, by definition, are the least probable outcome in any given situation.
2. Therefore, by the nature of the historical method, miracles can never be proven because in every possible situation they are the most improbable explanations.

Licona critiques Ehrman's argument by objecting to his second premise, "Why must a miracle hypothesis necessarily be the least probable explanation?" (174). According to Licona, premise two is based incorrectly on the notion of prior probabilities. In other words, Ehrman argues that because no person has ever risen from the dead, then Jesus did not rise from the dead. But, as Licona explains, this line of thinking "excludes the possibility of an external agent" who could alter the probability of an event occurring (175). For example, if God did exist and wanted to resurrect Jesus, then the probability factor dramatically shifts. The millions of other people who have never been resurrected play no part in calculating the likelihood of Jesus's resurrection because, presumably, God only wanted to resurrect Jesus. Therefore, if we allow for the possibility of an external agent, such as God, the question changes dramatically. But, as Licona notes, "The challenge for historians, of course, is that they cannot know ahead of time whether such a god exists" (175). However, in Licona's opinion, this should not of necessity lead to historical naturalism: "Instead of presupposing or *a priori* excluding it [the possibility of God's intervention], which *a priori* renders one's hypothesis as *worldview dependent*, historians ought to adopt a position of openness and let the facts speak for themselves" (175).

Licona adopts a historically neutral methodology, where miracles are granted as possibilities and God's intervention is not *a priori* excluded. Using this method, Licona thoroughly examines all of the relevant texts that discuss the resurrection of Jesus. After a 409-page discussion of these texts and the probable hypotheses to explain the historical facts, Licona concludes that the only likely hypothesis, the one which meets all five of his criteria listed above, is that the resurrection of Jesus did occur. In fact, for Licona, not only does the resurrection hypothesis become the most likely candidate among its competitors, "it outdistances them by a significant margin" (606).

Licona's book has been particularly eye-opening for me. Prior to reading it, I agreed with Bart Ehrman that miracles could not be established through the historical method. I felt that miracles were simply concepts outside the realm of history. But, Licona's arguments have changed my opinion. The crux of the entire problem starts here: should the possibility of God's intervention be *a priori* excluded from historical method? In my mind, the answer is no. Naturalism has just as many improvable assumptions as theism. Naturalism posits that God never intervenes as opposed to a theistic worldview where God can and does intervene. Why should one hypothesis be preferred over the other? Certainly neither can be proven superior on historical grounds. As such, what *a priori* basis do we have to dismiss miracle-claims as unreliable or outside of the historical discourse? If, to avoid these problems, we accept methodological neutrality, a new problem arises: if we allow for the possibility of God's intervention, how should we recognize it? How are we to be sure that some miracle is the doing of God and not a pantheon of deities, the work of an evil demon, or perhaps an aberration in the natural laws of our planet? Again, historical issues seem unable to suggest which of these "supernatural" entities is responsible for a possible miracle.

A related problem comes from Licona's discussion of horizons. If a historian is convinced, based on non-historical factors, that God exists and that he acts in history, should this historian be allowed to use that belief in historical explanations? If not, then why not? What precludes it? Every historian uses their own intuitions, reason, and biases to establish historical fact—this is the whole point of discussing horizons. The problem is especially acute in a field such as ancient historiography where an abundance of data is lacking. As such, if we are convinced of some truth, such as God's existence, why should we not apply it to our historical analysis? The opposite edge of the sword also applies: if a believer is given the right to insert God into history, a disbeliever has an equal right to remove him completely.

The question of Christ's resurrection strikes at the heart of history: what historical method should scholars accept? If we accept methodological neutrality, then any number of supernatural entities can be used to explain unusual events. Zeus, Father Time, Ahura Mazda, and Osiris could each be used to explain abnormal "miracles" in place of God. Why should God be the only possibility? Does any other "supernatural" explanation hold the same legitimacy as asserting God as the cause of unlikely happenings? Do we have some method for adjudicating between them? Whatever method is chosen, it would not be based on historical evidence but philosophical reasoning. If

methodological skepticism is adopted, then we have once again reached an *a priori* bias in the discipline that cannot be adequately defended. Why reject supernatural intervention outright? Certainly no historical event can supply us with methodological skepticism, so what philosophical or theological reasoning dictates its precedence?

If the excessive questioning and outright confusion has not explained it well enough, I am not sure where to stand on this debate. In my view, history has not developed an adequate and complete methodology to account for these problems. The entire question of Jesus' resurrection opens doors to fundamental topics in the foundations and philosophy of history. Due to the absolute importance of these debates, Licona should be praised for his ability to open the reader's mind to these questions throughout his book. The sheer exhaustiveness and accessibility of Licona's work makes these problems impossible to ignore. Whether one agrees with Licona or not, his work is commendable because it provides a strong argument for the resurrection hypothesis and it utilizes so many relevant materials. The book will remain a must-have for any historian who wishes to adequately understand the role of axiomatic truths in history and especially for any Christian wishing to understand the nature of their faith.

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