The book contains some overstated claims: Stackert argues that P is strongly anti-prophetic, that it "effectively undermines the prophetic religion it rejects" (p. 184), when what emerges from his discussion is that P simply doesn't make an issue of prophecy. Stackert himself notes that, for P, "prophecy is an idiosyncratic relic, valued only for its legacy" (p. 192). P clearly adopts the prophetic characterization of Moses common to all the sources, but Stackert does not show us the polemics we would expect to find in a text with a strong anti-prophetic stance—the kind amply demonstrated for E and D. The discussion is also at points skewed to Stackert's interest in the prophecy-law dynamic: He claims that the sources present Moses more as a mediator of divine messages (law) to the Israelites than as intercessor (p. 55), but this is a curious claim in light of the complaint episodes, where Moses often does initiate an intercession on behalf of the people, and one wonders what an account of prophecy in the Pentateuch would look like if it dealt with these cases more fully.

All that said, Stackert's advocacy for the idea that characters and plot developments in pentateuchal narrative embody arguments about social institutions, situated in a narrative fiction set in the distant past, is hugely promising. Like Wellhausen, Stackert moves the discussion forward by asking the right questions and giving us much to contend with as we strive to answer them.

Stackert takes as the starting point for this study the Neo-Documentary Hypothesis—a powerful, even compelling reorientation of the Graf-Wellhausen version that corrects many problems with it. But Wellhausen's synthesis was also powerful and compelling, which had much to do with its broad acceptance in spite of reservations from the start. We should be equally cautious here. Sweeping syntheses are inspiring and motivating and, as such, play an important role in scholarship, but they are no substitute for detailed literary-critical studies of individual texts. Stackert argues that biblical studies should find its academic home in religious studies in order to benefit from the descriptive, analytical, and historical study of religion. I would add that biblical studies as a whole—and not just its synchronic corner—should also become bedfellows with literature and literary theory, since everything hinges on whether the readings that differentiate the sources are the most compelling readings, not whether the model as a whole is compelling.

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Ramesside Inscriptions: Translations. Vol. 7. Addenda. By K. A. Kitchen. Walden, Mass: Wiley Blackwell, 2014. Pp. xxxvi + 274. $400.

Ramesside Inscriptions. Vol. 4. *Merenptah and the Late Nineteenth Dynasty: Translated and Annotated, Notes and Comments*. By Benedict G. Davies. Malden, Mass.: Wiley Blackwell, 2014. Pp. xl + 397, maps, charts. $400.

The translations of Kenneth A Kitchen's *Ramesside Inscriptions* have recommenced. In volume 7 of his series the expert has provided once more fresh, and in many cases new and useful, renditions of the addenda texts that concluded the first series (*KRI*) of this so important arduous work. Once more, the author has also provided literary analyses of key texts, e.g., the account of Userhat-Hataiu-Peniya, a chief sculptor but also a wise and erudite composer of praise to deities. The example chosen is particularly revealing as it demonstrates Kitchen's deep understanding of Egyptian prose as well as poetry—see his *Poetry of Ancient Egypt* (Jonsered, 1996). This should indicate that these translations are not mere mechanical ones, akin to oft-repeated new editions into modern languages of ancient words belonging to a far-away and dead tongue.

But non-poetical materials, or perhaps better put—those of a non-literary nature—are also analyzed with deliberation and consideration upon the original set-up. The oft-consulted Papyrus Butler, to take a case in point, an account that records an offering to Osiris by a military cohort, is neatly presented in columnar format so as to allow the reader and investigator a relatively easy understanding of this

important social and economic document, one that directly concerns the Egyptian military in the Nineteenth Dynasty.

Volume 7 is a heterogeneous collection of additional sources not covered in the first six volumes of *KRI*. As such, the reader must carefully page back to the relevant historical sections and combine the first series of copies with this last one. Fortunately, page and line references to the hieroglyphic editions are provided, a policy that Kitchen earlier used with great perspicacity. Thus the user will find the "Addenda to the Addenda" somewhat perplexing unless that scholar is conversant with this immense time period. Hence, on page 32, for example, we skip from *KRI* VII p. 44 to pp. 405 and following. This edition, therefore, is composed in a historical fashion that integrates *all* of the supplemental addenda into the "addenda volume."

Of significant import is the presence of two "complementary addenda," both of which add yet new material to the original presentation of *KRI* 7: the re-edition by Grandet of the Death of King Siptah and the Year 4 Karnak Stela of the High Priest Bakenkhonsu are included. These two cases are of such great importance for the reconstruction of the chronology and history of the late Nineteenth Dynasty-early Twentieth Dynasty that I was most pleased to see their presence here.

There are a few slips, which are to be expected, but they do not mar this excellent work. I came across "4167:10" (p. 236) and "Translatiions" (p. xxxiii).

But the importance of the volume must be emphasized further. For any study of the Ramesside Period, Kitchen's immense labor, devotion, and perseverance cannot be doubted. His expert handling of various Egyptian compositions and their scripts plus useful palaeographical commentary can be found throughout the entire *KRI* series. Here, speaking only with regard to the complementary translation group, I cannot but reiterate the same impression. The entire undertaking was, and is, a lifetime's work, and a task solely conducted and executed by a single person. This result, superb from the first go, proves Sir Alan Gardiner's dictum in the introduction to his *Ancient Egyptian Onomastica*: experts alone—few at best—should be in charge of long-ranging projects. It moreover appears in Egyptology that a single person, with love and devotion, can accomplish much more than a vast herd of state-funded practitioners.

Is this a by-product of our "fiery desire," to reword Gardiner's blissful remarks in the Preface to his *Egypt of the Pharaohs*? I suggest: yes. (The life's work of Rainer Hannig is another example. Even the ill-fated *CAA* supplies proof, but of a negative nature.) Egyptologists are not necessarily solitary beasts, but surely they are—as proven by Kitchen's life and career—perhaps "Seized by a Strange Desire" (Sundaram Chandrakalaadhar). I believe so, and this final volume of translations provides me with yet further evidence of the author's love of pharaonic Egypt.

* * *

With the appearance of volume 4 of the Notes and Comments platoon of the *KRI* "army"—this time provided by Benedict G. Davies—we have also leaped over an intermezzo during which it appeared that the entire Ramesside project, founded by Kitchen, was no longer operating. In this case the orientation of the volume is very different from that just described. Here, we encounter a wealth of up-to-date scholarly opinions and evaluations concerning the texts in *KRI* 4. The end of the Nineteenth Dynasty (Merenptah and his successors) is the defined theme. Davies provides an extraordinary depth of scholarly research, especially if we consider that the entire "shebang" is a motley assortment of royal and private texts, publicly monumental and privately assigned inscriptions, hieroglyphic and hieratic copies, and of diverse genres. Going over some of the material that especially interested me, I discovered a wealth of background information that I had cursorily scanned or overlooked. For any historian this volume will prove to be an excellent vademecum. I cannot but recommend it.

How can one review such a work? I believe, and this is my present orientation, that it is best to concentrate upon two major historical texts and extend the *argumentum* into new vistas. To be specific, I was most happy to see how Davies used Colleen Manassa's lengthy re-edition of the Great Karnak Inscription of Merenptah as well as the "companion piece," the Israel Stela. And therefore I shall take this section of the work in a paradigmatic fashion.

Davies' interest is definitely historical. His able and lengthy commentary centers on the historical background, the specific geographical and social details of the king's western campaign, as well as the plunder list. In a nutshell, Davies' analysis gives a succinct analysis of the king's Libyan war as derived from the major historical text at Karnak.

In contrast, one can read the study of Merenptah's Israel Stela with equal interest. There, Davies has been able to supplement his previous commentary on the Karnak inscription with more than enough additional details. I must commend the author for this very helpful follow-up on the "Libyan situation" in the late Nineteenth Dynasty.

Both useful commentaries slight the means of presentation, the language employed, the linguistic registers chosen by the authors, and the question of poetics. For one, the Israel Stela is not just a hymn or a "Triumph-Hymn." I would argue that it is a combination of a newer approach to narration, one that now used a developed "Late Egyptian" narrative presentation in which a decided switch from earlier models has taken place. Granted, this aspect is ancillary to Davies' procedure. Indeed, it is not the main purpose of this volume or, indeed, of any of the *KRI* Notes and Comments editions. Yet surely it would have been worthwhile to adumbrate the literary aspects of this narration in the Israel Stela and, in fact, to mention the different narrative procedures used in the Great Karnak text of Merenptah.

Both of Merenptah's accounts turn from the style of the war records of the Eighteenth Dynasty. The new, longer style eschews the simple ephemerides approach that is still present in the Kadesh Poem. Instead, the grammatical formations that express narrative are rooted in a newly coined monumental Late Middle Egyptian style. The Israel Stela has gone far in its presentation of the verbal constructions, ones that Edward Wente saw already in 1959 as prevalent in the Medinet Habu narrations of Ramesses III, the historical section of P. Harris, and even in the Late Egyptian Miscellanies (*Syntax of Verbs of Motion in Egyptian*, Chicago, 1959). This was barely commented upon by Thomas von der Way in his useful 1986 volume on "Holy War" (*Göttergericht und "Heiliger" Krieg im alten Ägypten: Die Inschriften des Merenptah zum Libyerkrieg des Jahres 5*, Heidelberg, 1992), but the issue remains extraordinarily significant in light of the historical background of the Egyptian monarch, court, and high-level society at this time.

If I have concentrated upon these two texts of Merenptah it is to show that more than a "pure" diplomatic-military-political-historical approach could have been taken with regard to all the royal historical sources. One must investigate the literary method chosen by the composer within the contemporary Egyptian society in order to reveal more clearly the intent of the battle reports, the differing methods chosen for public showing, and the aim of the whole schema or single text: Why the Israel Stela? Why the Karnak Inscription?

By and large, Davies' careful historical approach has led to a commentary that is a tad too conservative for my liking. Assuredly, specific references to von der Way's reflections on the Israel Stela plus Ramesses III's Year 5 Inscription were needed. Why the switch in style and presentation occurred at this time *is* historically significant, especially in light of the attitude of the Egyptians towards the Libyans. It appears that the latter were considered to be far more dastardly than the Asiatics, the Nubians, or even the Sea Peoples. Both of these pharaohs' accounts are as somber and vicious in attitude as they are tactile and sensory in approach. I would claim that this attitude, rooted in a different expectation of royal proclamation—and publication—is a product of an age different from those earlier.

I am not quibbling with the wealth of historical scholarly information supplied by the author. For this I shall be always grateful. As a case in point, I was happy to see that Davies has provided a good and salutary analysis of Sue D'Auria's suggestion that there were two battles between the Egyptians and the Sea Peoples/Libyans (p. 7). Moreover, I greatly appreciate the industry shown by the author with respect to the multitudinous source material emanating from Deir el Medineh, and I would like to single out pp. 70–72, which are concerned with Qenhirhhopshef. In addition, Davies' able commentaries on the viziers and, for me at least, especially on the Viceroys of Nubia, have proved remarkably prescient.

Perhaps linguists, literary scholars, and historians need to interact more closely. True, the so-called field of "Modern Egyptology," a horrendous term in my opinion, has undergone great fragmentation amongst its participants. I am not arguing for a reintegration, on a higher level to be sure, of the dated role of the "Master Egyptologist." But consider what could have been added to these, admittedly

solitary, but nevertheless highly significant two pieces of the historical record. Historians and literary people need to come back to each other.

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Persia's Imperial Power in Late Antiquity: The Great Wall of Gorgān and Frontier Landscapes of Sasanian Iran. By Eberhard W. Sauer; Hamid Omrani Rekavandi; Tony J. Wilkinson; and Jebrael Nokandeh. British Institute of Persian Studies Archaeological Monographs. Oxford: Oxbow Books, 2013. Pp. xvi + 711, illus. $150. [Distributed by the David Brown Book Co., Oakville, CT.]

This book reports the results of a multi-year study of the Great Wall of Gorgān, in the northeast corner of modern Iran to the east of the Caspian Sea and just south of Iran's border with Turkmenistan. Although the Gorgān Wall was an enormous edifice, it is little known even among specialists, so this collection of linked studies is welcome for what it reveals about the wall's setting, construction, and fortification. The book is more important, however, for how the authors use their study of the wall to explicate the power, organization, functioning, and collapse of the Sasanian Empire that built it.

The Gorgān Wall was actually a complex assemblage of features and structures. The wall itself, some two meters wide, at least three meters high, and over 195 km long, was built of fired bricks. It was connected to a second, shorter wall—the 11 km long Tammisheh Wall—at its western end. Over thirty forts were integrated into the main wall's structure and a wide trench along the wall's northern face was actually a canal rather than a simple ditch. There were large arched kilns every thirty to a hundred meters along the length of the wall, used to fire the bricks the wall was built of. And a series of large enclosed settlements south of the wall are interpreted as campaign bases serving to support the garrisons along the wall if they came under attack. The enormous effort required to build and maintain this complex demonstrates both the power of the Sasanian Empire and the degree of threat they faced in this corner of their territory.

The first question this book resolves is the dating of the wall's construction and abandonment. It was built during the reign of the Sasanians, who ruled a vast swath of the Middle East from the early third to the mid-seventh centuries AD. More specifically, the wall appears to have been built during the fifth or possibly early sixth century and to have been abandoned somewhere between 612 and the 650s, during the time of the Islamic conquest. The date of construction has been determined through carbon dates from charcoal in the kilns along the wall and optically stimulated luminescence (OSL) dates from bricks, pottery, sediments in the kilns, and soil beneath the wall. The abandonment is dated by carbon dates from excavations inside Fort 4 near the eastern end of the wall, and supported by both historical records and the absence of typologically later material in surface collections from forts along the wall.

The bulk of this bulky book is devoted to a diverse set of field studies by a collection of researchers. The first study examines the landscape of the Gorgān Plain, emphasizing that the wall marks a natural frontier between arable lands to the south and the dry grasslands of the Turkmenistan steppe to the north. Beyond that observation, this section is most interesting for its discussions of the paleochannels of the Gorgān River and the massive fluctuations in the level (and shoreline) of the Caspian Sea, which ranged from 6 m higher than today in 500 BC, 2 to 4 m lower than today when the wall was built, and 4 m higher than today around 1200 AD.

The core of the book—the section on field research—begins with the archaeological survey, which was based on an analysis of Landsat (digital) and CORONA (photographic) imagery supplemented by visits to sites or features recognized on the satellite data. Sixty-two sites were visited and mapped, and "represented diagnostic wares were collected" by the surveyors. Notably, the research did not include walking surveys of the wall or systematic walking surveys to establish what the satellite coverage might have missed. In addition, a significant proportion of the settlements identified from satellite