At the same time, a mostly implicit, in places explicit, periodization undergirds Mairs' arguments: The book proposes two key historical moments, first, the creation of Achaemenid satrapal Bactria, when for the first time the region was incorporated into a stable pan-Near Eastern imperial system of rule, and, second, the satrapy's emancipation from Seleucid rule and emergence as an autonomous regional kingdom. As a result, the Alexandrian and Seleucid empires are all but absent from the weave of Mairs' arguments.

Certainly, this can provide a persuasive historical texture, as in the discussion of administrative continuity in chapter one; but on occasion it gives pause. Take, for instance, the Mesopotamian groundplan of Aï Khanoum's temple; Mairs proposes that the form derives from earlier Achaemenid satrapal traditions in Bactria. Despite good evidence for the deliberate employment of a hybrid or international style in the Seleucid empire's new settlements, such as at Failaka/Icarus and Seleucia-on-the-Tigris, Mairs' Graeco-Macedonian colonists are allowed responsibility for only the most stereotypical of Greek buildings. The book, of course, acknowledges that there is as yet no material support for this hypothesized Persian background; it can only be hoped that future research will pin things down more clearly.

That is but a minor quibble. For, overall, the book's four chapters, really, self-standing studies of discrete historical questions, are united by Mairs' respect for the *ars nesciendi*. The analysis is consistently cautious and sensitive to the dangers of overenthusiastically combining disparate and scanty evidence, whether historical and archaeological data on imperial exploitation or Chinese and classical historians on second-century population movements. Moreover, the book very successfully combines a post-colonial vocabulary and sensibility with a keen awareness of disciplinary perspectives, assumptions, and limitations. Its clarity, sophistication, and accessibility will, if I am not mistaken, make the Hellenistic Far East freshly attractive and more widely accessible.

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Historical Linguistics & Biblical Hebrew: Steps toward an Integrated Approach. By Robert Rezetko and Ian Young. Ancient Near East Monographs, vol. 9. Atlanta: SBL Press, 2014. Pp. xx + 699. $89.95 (paper).

This is a formidable book. It continues the authors' previous major effort to call for a fresh investigation of the dating of the original composition of the writings of the Hebrew Bible (Young, Retzetko, and Ehrensvärd 2008). That two-volume work raised questions about the general consensus on this issue. The present volume continues to challenge that consensus from the perspective of historical/diachronic linguistics. The massive response to their earlier work (especially represented in print in the 2012 *Diachrony in Biblical Hebrew* [hereafter DBH]) indicates that it was taken seriously (see here pp. 2–3 and nn. 12–15 for a survey of the enormous response, including multiple conference sessions).

The present volume also deserves serious consideration. In it the authors have marshalled extensive historical linguistic evidence and presented it systematically. The focus of the book is on the inferior nature of the evidence in the Masoretic text (hereafter MT) for the reconstruction of the history of ancient Hebrew on the basis of current critical scholarship and textual criticism of these writings and on the use of historical linguistic methodology in such an undertaking. Their conclusion is already anticipated in the introduction, where they propose "a new perspective on the language of Biblical Hebrew (BH): not only is the linguistic dating of biblical writings unfeasible, but the distribution of linguistic data in the Masoretic Text (MT) of the Hebrew Bible suggests that EBH (Early Biblical Hebrew) and LBH (Late Biblical Hebrew) are better explained in general by a model of co-existing styles of literary Hebrew throughout the biblical period" (HLBH, p. 2).

In the first paragraph of their introduction the authors very briefly summarize the so-called "maximalist" and "minimalist" controversy that reached a high point in the 1990s (and has not subsided until today), but they make no further reference to it. One of them did edit a volume of studies (Young 2003)

bringing together both perspectives, in which all three authors in their contributions to that volume already anticipated their parts in the 2008 publication and the viewpoint of the volume under review. The authors' claim is that the evidence we have in the MT does not support the widely held view of a periodization by which these books (or parts of them) can be segregated into early, middle, and late periods of ancient Hebrew language development. They find a consistency throughout the MT in what they term "Standard Classical Hebrew" and an inconsistency in what they term "Peripheral Classical Hebrew," with the consistency in the former and the inconsistency in the latter both supporting their argument.

In the introduction the authors summarize the background leading up to their book, the issues they will address, their objectives, and their terminology. As to issues, they argue that there can be multiple complications in even establishing periods in the development of any language and many more in placing specific compositions within such a framework, depending on the nature of the evidence. This is the central question this book raises. What kind of evidence does the MT offer, and what kind of investigation can it support? Critical questions in this regard revolve around such issues as: is the evidence composite; is it datable, and can its provenience be identified; how much change has it undergone over time (both through copying errors and editorial changes); is it literary or non-literary (e.g., documentary); can authorial styles be distinguished among the various authors (only some of whom can be identified at best in this case); what is its text type, genre, and degree of poeticality and/or orality? The authors have gone to great lengths in this book to demonstrate that they believe the MT to be poor evidence for dating the original composition of any of its contents.

In a second chapter they offer an introduction to historical linguistics in its objectives, attention to sources, study of variation, and work on language periodization. In their discussion of the objectives of historical linguistics (pp. 14–21), they make a point to say that "'linguistic dating of texts' is seldom if ever on the mind of the historical linguist" (p. 15). I doubt this, but, even if so, I believe any effort to date the original composition of any material under historical linguistic investigation is admirable. That said, the strength of their argument does not depend on their disclaimer. It rests rather on the nature of the evidence (which would determine in every case the success or failure of any effort to date sources) and the distribution of linguistic data within the MT. In their discussion of the nature of sources that can serve as evidence in historical linguistic investigation the authors cite telling quotations from three historical linguists (Fischer, Lightfoot, and Schneider) that combine to suggest that the MT is bad evidence for such an investigation. In the heart of their book (chapters 4–9) they purport to demonstrate this.

But first, in a third chapter, they survey the field of the textual criticism of the Hebrew Bible, with primary reference to the third edition of the work by Emmanuel Tov (2012). They call into question the general, unspoken assumption that "the language of the MT represents in detail the language of original authors" (p. 68), grounding this denial on the critical and text-critical consensus that the language of the MT is the result of a long historical process of redactional/editorial activity. With special reference to the Qumran scrolls they review the current consensus of text critics that the biblical text was in a fluid state from its beginning into the first century of this era. Drawing together current views on text-critical and compositional history of the Hebrew Bible and summarizing the work of Person, Carr, Hurvitz, Holmstedt, Joosten, Polak, Zevit, and others they conclude this survey and anticipate the book's conclusion by asserting that "no reliance can be placed on any of the manuscripts currently in our possession that they provide us specific information about the particular linguistic usage of any of the authors of the individual compositions in the corpus" (p. 115).

With this groundwork laid, the authors offer in the heart of their book in chapters 4–9 their analysis of the language of the Hebrew Bible through two methods of historical linguistic investigation: Cross-Textual Variable Analysis (CTVA, chapters 4–6) and Variationist Analysis (VA, chapters 7–9). Chapter 4 introduces CTVA, by which they mean a comparison of variables in different versions of the same text. To illustrate the method they cite examples from German, English, French, and Spanish literature; survey the work done by others that could in a general way fit in this category; and summarize the kinds of questions they consider relevant and, for the most part, so far unanswered.

In chapter 5 they study parallel passages using CTVA with a single variable approach and a multiple variables approach, in the latter with a focus on the MT (and some attention to Qumran texts) of

2 Samuel 22 and Psalm 18, 2 Kings 24–25 and Jeremiah 52, 2 Kings 18–20 and Isaiah 36–39, and 1 Kings 22 and 2 Chronicles 18. Their conclusion is the following: "Large-scale and basic features of Classical Hebrew only relatively rarely show variation. Less common features of Classical Hebrew are highly fluid, and the current distribution of such forms (i.e., in MT) cannot be relied on as evidence of the language of particular authors at particular times and in particular places. Nevertheless, it is precisely these less common features that have played a big role in historical linguistic and linguistic dating studies of BH writing" (p. 168).

In chapter 6 they undertake CTVA of the Samuel manuscripts in MT, Qumran (DSS), and the Septuagint (LXX). Their focus in the DSS variants is on directive HE and iterative WEQATAL and in the LXX on a wide variety of variants in 1 Samuel 1–2. A final section looks at the accumulation of "late" language between books that are clearly early and clearly late, with attention to multiple variants in 2 Samuel 11–12. Their conclusion for this section is that "All the biblical texts we have studied have at least some 'late' Hebrew, and often the frequency of this 'late' language is higher in the 'earlier' than in the 'later' writings" (p. 196). For this chapter and all three using CTVA they conclude: "*any historical linguistic research on BH that simplistically assumes the integrity or 'originality' of the MT as a primary source, and downplays or disregards the substantial quantity of linguistic variation between the surviving textual witnesses to the biblical writings, is unsound in theory and ill-conceived in method*" (p. 210; emphasis theirs).

Chapter 7 introduces Variationist Analysis (VA) as a sociolinguistic, in this case historical sociolinguistic, methodology, discussing it as integrating historical linguistics, histories of individual languages, corpus linguistics, philology, discourse studies, socio-pragmatics, and dialectology. VA is essentially the description and exploration of "patterns of variation in language as they relate to times and places and individuals and groups" (p. 212), i.e., a study of actual change in a language over time. Kinds of changes and measurement of innovation via an s-shaped diffusion curve are discussed as background to chapters 8–9, which apply VA to the history of the Hebrew language seen in MT, DSS, and Ben Sira (with only slight attention to the Samaritan Pentateuch in chapter 8 and even less in chapter 9).

Chapter 8 applies VA to this corpus with lexical studies of the verb דרש meaning "to study," and then ten so-called "late" lexemes and their so-called "early" variants, what many would call synonyms: חפז, מהר ("early") and בהל, דחף ("late") for "hasten"; רגז, פחד, ירא, חרד ("early") and בעת (*Niphal*, "late") for "be frightened"; הלך (*Qal*, "early") and הלך (*Piel*, "late") for "walk"; צעק ("early") and זעק ("late") for "cry"; קבץ, אסף ("early") and כנס ("late") for "gather"; קצף, אף-/+חרה ("early") and כעס ("late") for "be angry"; קום (*Qal*, "early") and עמד (*Qal*, "late") for "arise"; לקח ("early") and קבל (*Piel*, "late") for "receive"; קום (*Hiphil*, "early") and קום (Piel, "late") for "raise"; and משל ("early") and שלט ("late") for "have power over." The "late" noun מלכות and its "early" counterpart ממלכה for "kingdom" are also studied at the outset of the next chapter. At the beginning of chapter 8, six criteria for the selection of the variables studied in chapters 8–9 are discussed and objectives are given. At the end of the chapter they draw the conclusion that in the use of the variables they have surveyed, in contrast to the appearance of early and late variables that has been claimed, "conscious selection or stylistic variation is at work" (p. 325).

Chapter 9 consists of grammatical studies in the same corpus of abstract nouns ending in ות with a focus on the specific noun מלכות, the pronominal endings סְם and יהֶם, and the directive הָ. There follows a discussion of the issue of periodization of the language reflected in the biblical writings, with this conclusion: "The linguistic profiles of the biblical writings, are not reducible to their dates of earliest origin but reflect the entire history of involvement of a multiplicity of editors and scribes who processed those writings and handed them down through time" (p. 400); ". . . the common linguistic features of BH appear relatively stable in the biblical sources—both within the MT and between the MT and other texts" (p. 402); and ". . . it is often difficult or impossible to know the precise reasons behind the linguistic variations in the Hebrew Bible" (p. 403).

In their conclusion the authors summarize their work and restate their call for a new approach based on the tools of historical linguistics, with five suggested methodologies that they believe will add to what they have done (further CTVA and VA, diachronic typology, identification of associative patterns, and clarification of the idiolects of individual writers and writings).

Three appendices follow, the first (30 pages) dealing with variants in parallel passages in the MT, the second major appendix (139 pages) detailing linguistic variants in the Masoretic and Qumran texts of Samuel, and the third (8 pages) offering "some more not-so-random thoughts" primarily in response to Zevit's criticisms of their 2008 volume published in DBH. A bibliography, index of modern authors, and index of biblical and related texts close the volume. Helpful as these indices are, an additional index of general subjects would have been desirable, as would section notices at the top of every page. Many of the footnotes gather extensive bibliography on important topics related to the subject of the book and greatly enhance its value.

The authors are due thanks for prodigious research and careful writing. Those who take a different view of the dating of the language of biblical writings will need to fault their data or their conclusions, or reconsider their own position. The book's challenge cannot be safely ignored.

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Sexual Violation in Islamic Law: Substance, Evidence, and Procedure. By HINA AZAM. Cambridge Studies in Islamic Civilization. New York: CAMBRIDGE UNIVERSITY PRESS, 2015. Pp. xi + 270. $95.

This is an extremely careful and detailed study of Islamic legal discourses on male sexual violation of free women, from their inception in the seventh century CE until the emergence of what we recognize today as classical Islamic legal doctrine on rape,[1] which the author situates in the twelfth century. Primarily conceived for those with an interest in premodern Islamic law, the book has also been written to assess understandings of classical Islamic law on which basis sexual violence is treated in a number of contemporary states and to challenge a practice in which "legal institutions [. . .] function to promote violence against women in systematic ways [. . .] providing legal cover for males who perpetrate violence against females." By engaging in a critical evaluation of classical Islamic jurisprudence on rape, Hina Azam wants to judge whether contemporary injustices can legitimately be attributed to the classical legal system (see pp. 1–7). This explains why female slaves and free non-Muslim victims of rape have been left outside the scope of the book (p. 12).

Contrary to what the label "classical" might suggest, Islamic legal doctrine on rape admits a wide variety of approaches to define sexual violation and establish its punishment, embracing distinctions between divine and human claims, sexuality and property, and volition, legal capacity, and legal liability. By virtue of this internal variety, mutually conflicting results are possible, which might severely compromise a victim's capacity to prosecute perpetrators and obtain compensation, to the point of impunity. The coexistence of contradictory views is testified in all the equally authoritative schools of

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1. Like the author, I use the term "rape" for practical reasons and to avoid the longer but more accurate "sexual violation" option. As she notes (pp. 16–18), there is no exact equivalent in the sources she used to our modern concept of rape, which is based on notions of individual autonomy and on the inviolability of the female body; not everything we consider to be sexual violence today, e.g., marital rape, was deemed as such by premodern jurists, and to them sexual violence was not always synonymous with sexual violation.