Early Modern Biblical Criticism and the Republic of Letters

N. Hardy, *Criticism and Confession: The Bible in the Seventeenth Century Republic of Letters* (Oxford, 2017); D. Van Miert, *The Emancipation of Biblical Philology in the Dutch Republic, 1590–1670* (Oxford, 2018); J. Toubert, *Spinoza and Biblical Philology in the Dutch Republic, 1660–1710* (Oxford, 2018).

The publication of these three books is a landmark moment, for it confirms a trend of the last two or three decades, in which the study of early modern biblical scholarship – and more generally the relationship between erudition and religion – has become one of *the* pre-eminent areas of pre-modern intellectual history.¹ The three books under discussion here all present brilliant, detailed, and highly sophisticated accounts of the development of biblical criticism between the sixteenth and eighteenth centuries, which move the conversation forward in numerous ways.² All of them offer accurate, precise readings of some hugely difficult texts, many of them written in convoluted humanist Latin and containing advanced Greek and Hebrew philology. All of them reject simplistic models in which ‘progressive’ humanist scholars (good) are ranged opposite ‘conservative’ theologians (bad). And all of them operate within the ambit of what we might call the ‘new’ history of scholarship developed first and foremost by Anthony Grafton, which does not seek to tell a linear story of progress towards ‘modern’ critical method, but rather focuses on recreating early modern scholars at work, and placing them in their immediate scholarly, religious, institutional, and political contexts. At the same time, none of them take this approach to self-indulgent excesses: they do not revel in the discovery of a marginal annotation for the sake of it, or harp on about ‘information management’. Rather, they use their discoveries to contribute answers to fundamental questions about intellectual and religious change.

All that being said, the most dangerous thing for any successful sub-field is to rest on its laurels. With that in mind, this discussion will seek to do three

- 1 Interested readers may wish to compare with my comments nearly a decade ago, D. Levitin, ‘From Sacred History to the History of Religion: Pagans, Jews and Christians in European Historiography from the Reformation to “Enlightenment”’, *Historical Journal*, 55 (2012), 117–60.
- 2 I should say at the outset that all three authors are well-known to me: Hardy is a former colleague and collaborator, and Van Miert and Toubert are acquaintances with whom I have worked. No doubt I am incapable of being impartial about their work; nonetheless, in what follows I have tried my best to convey what I personally take to be the strengths and weaknesses of their books. The amount of space devoted to each is reflective of their respective lengths.

things. First, it will summarise the achievements of these books, both in terms of empirical content and interpretative innovation. It is hoped that this proves particularly useful to those who are not fully familiar with this quickly developing field, not least because other reviewers have not had the chance to convey the full empirical richness and interpretative complexity of these books. Second, it shall seek to put the books in dialogue with each other, and to see where their theses might be compatible, and where they probably are not. Finally, it will suggest some potentially fruitful avenues for further research.

Before diving in, it is worth making a few conceptual distinctions that might help to clarify the main interpretative issues that are at stake. The central such issue is the relationship between scholarship and religious belief. Slightly more narrowly, it is that between scholarship and the *confessionalised* forms of Christianity that crystallised in the second half of the sixteenth century. Here already we encounter the need to make an important distinction between the structural impact of confessionalisation on any one scholar's work, and the impact of that scholar's beliefs and allegiances on their actual scholarship. Let me give an example from a field separate from, but not entirely foreign to, the one under discussion: oriental scholarship. A book like Thomas Hyde's remarkable history of Persian religion, the *Historia religionis veterum Persarum* (1700), was undoubtedly the 'product' (in a broad sense) of confessional forces, above all the investment of the Laudian and Restoration church in oriental learning. But I think it would be wrong to describe the text itself as particularly informed by Hyde's confessional allegiances. To be sure, it is not a work of secular scholarship. God plays a role in history; the ancient Persians are said to have had vague foreshadowings of the Trinity; most specifically, the organisation of their priestly caste appears quasi-episcopalian. But I would resist any interpretation that argued that Hyde wrote the book primarily to defend the English Church; indeed, I might even go further and suggest that these statements were largely *ex post facto* additions to a work that was written largely to sate Hyde's curiosity. That is not to say that there was anything intrinsically dangerous or anti-Christian about the book, or that Hyde had to 'disguise' his true aims with pious inferences. Rather, it is simply to point out that at this point in European history – and perhaps at all of them – scholarship cannot be reduced to the ideological forces that informed it. The flip side of that is that confessional investment in erudition produced many forms of knowledge that were not narrowly 'confessional' (just as the modern investment in military research produces much pure mathematics that has no military applications).

However, in early modern Europe the study of Persian religion was nowhere near as ideologically significant as the study of the Bible. That leaves us

wondering about the extent to which early modern Biblical scholarship *is*, at various points, explicable not just as a product of structural confessionalisation but also of the confessional agendas of the scholars who practised it. In turn, this raises a second major issue about how we study intellectual change. All three books under discussion are concerned with what we might for the time being loosely call a process of 'historicisation' (although Hardy in particular questions the linearity of any such process). Here it might be sensible to draw a distinction between what we might call 'qualitative' and 'quantitative' historicisation. The former involves specific intellectual moves. For example, an early modern scholar might argue that not all of the biblical text is inspired, but that the books which are purely historical were the product of the historically situated minds of the human actors who composed them, and need to be investigated as such. Or they might suggest that a specific practice, such as the Sabbath – or even a whole set of practices, such as Hebrew Ritual Law – emerged from contingent historical circumstances, perhaps so as to argue that it is no longer binding. Such moves are relatively straightforward to study: one simply has to read everything and look for the arguments – usually one can find them because they stimulated opposition and controversy. However, studying such moves as part of a process of change brings dangers. A historicising move made in one context need not be symptomatic of a general 'historicism': the same theologian who historicises the Sabbath might insist dogmatically on the trans-historical significance of a doctrinal or ecclesiological pericope (and that's before we even get to their readers).³ If we begin with the knowledge of what 'higher criticism' came to be, we run the risk of looking for its proleptic adumbration in the seventeenth century, and thus of misreading what our seventeenth-century scholars were themselves seeking to achieve.

Moreover, the study of specific controversies cannot exhaust the history of intellectual change (a lesson that the more extreme social historians of science have consistently failed to learn). Here is where *quantitative* historicisation comes in. For it might be the case that a theological issue that had previously been approached for the most part without the use of philological or historical tools very gradually came to be discussed via the use of those tools without generating much in the way of controversy or debate, probably because the shift had no immediate doctrinal implications. Studying such change is very difficult. First of all, intellectual history is not a quantifiable discipline. (One can

3 For a study of exactly such an attitude not from the perspective of the history of scholarship but of religious power, see P. Lake, 'The Laudians and the Argument from Authority', in *Court, Country and Culture: Essays on Early Modern British history*, ed. B.Y. Kunze and D.D. Brautigan (Rochester, NY, 1992), 149–76.

hardly say, for example, that in 1600, 20% of theology was 'historical', whereas in 1670 the proportion had gone up to 75%.) Second, we as a scholarly species are, perhaps understandably, not very good at charting such nearly imperceptible change. Rather, we extrapolate our narratives from individual texts and controversies. The quality and convincingness of those narratives depends on the quantity of evidence presented, and on the nature of the inferences made. There is no getting around this, but it is worth bearing in mind when we discuss the shifting nature of early modern biblical criticism. For as we complicate our stories by dropping unfeasibly simplistic or whiggish categories of explanation ('radical enlightenment', a non-confessional 'Erasmianism', etc.) we are left with the problem of assessing the relative impact of individual developments, and of balancing qualitative and quantitative change against each other.

Hardy's is the most ambitious of the three books, for it is concerned not with developments in one country, or even in one confessional space, but rather with biblical criticism in both the Catholic and Protestant worlds, covering texts and ideas produced between the 1580s and 1650s. Although the book's contents page might make it seem that Hardy is focussed narrowly on a canon of the usual suspects – Scaliger, Casaubon, Selden, Grotius, Cappel – each chapter in fact includes a great amount of detail on many other figures, above all the theologians who read these scholars' works. In particular, Hardy makes brilliant use of the archives of the Roman Inquisition and Index to chart the processes that went into censoring (or not censoring) works of biblical criticism, and to explore the extent to which Roman scholars were willing to collaborate with counterparts elsewhere.⁴ He has also taken the step – still very rare among intellectual historians – of reading his way into the history of doctrinal theology, especially the history of Reformed theology as it has been rewritten over the last three decades by Richard Muller and others.

Hardy's overriding aims are twofold. First and foremost, he has sought to 'produce a radically different account of the history of seventeenth-century biblical scholarship, illustrating its close ties to academic theology and the strong confessional commitments of its most famous practitioners' (16). In this regard, he seeks to challenge those historians of scholarship who have 'routinely and carelessly cast aspersions about the difference between scholarly

4 Since I have only made limited use of these archives, this is one aspect of the book I am less qualified to judge. For some fuller comments, see the review by J.-L. Quantin, *Revue de l'histoire des religions*, 237 (2020), 413–22.

and theological uses of the Bible without actually reading what academic theologians wrote, much less engaged in genuine, even-handed comparison of the two sides' (13–14). This is combined with a broader thesis concerning not just ideas, but also the world in which they operated: a challenge to the view 'that the republic of letters allowed scholars to transcend their particular confessional identities, despite the fact that they lived in an age of intense hostility and even armed conflict between representatives of the competing Christian confessions of faith' (5). Hardy is too careful a scholar to create any straw men, but if there are particular historians with whom he is in repeated disagreement then they are Anthony Grafton, Henk Jan de Jonge, Henk Nellen, and Peter Miller (although he also always acknowledges his debts to them, especially the first three). More generally, he sensibly challenges the indiscriminate back-projection of Arnaldo Momigliano's thesis about eighteenth-century 'antiquarianism' into the seventeenth.

The book begins with a Prelude on 'The discourse of *critica* in the late Renaissance', which will prove hugely useful to all students of the period (whether or not they agree with Hardy's broader thesis). He notes the many technical innovations of late Renaissance humanism, including the collation of manuscripts and the development of palaeographical and codicological techniques, but tempers this with the observation that 'early modern scholars never settled on a definition of criticism that excluded value-judgements and universalising, prescriptive norms, even though some of them tended to avoid such things in practice' (41). Late humanism was not *Altertumswissenschaft*.

The rest of the book effectively seeks to prove this by showing how scholarship was still deployed for theological ends by some of the most famous names of seventeenth-century erudition. The first of these to be considered, in a chapter that takes up a quarter of the book, is the great French scholar Isaac Casaubon. Hardy has uncovered a mass of new information about Casaubon (to which I cannot do justice here), and much of this chapter takes the form of biography. Its purpose is to show that as his career progressed, Casaubon became more and more enmeshed in confessional issues – and theological reading – above all in the wake of the Eucharistic debates at the Conference of Fontainebleau (1600), the constant attempts to convert him (not least by Cardinal Du Perron), and the dispute over jurisdiction that broke out between Venice and Rome in 1606, in which Paolo Sarpi played such a large role. Sometimes aligning and sometimes conflicting with these confessional forces was the more basic force of material considerations: as Hardy shows, the possibility of being paid by the Venetians, and the (better documented) desire not to lose his French pension once in England, may explain certain argumentative techniques deployed by Casaubon.

As for Casaubon's actual scholarship in the famous *Exercitationes* (1614), composed in England against Cesare Baronio's *Annales*, Hardy aims to show that 'to seek a consistently "critical" or "historical" approach to antiquity' in this book 'is to put the cart before the horse, since Casaubon's methods varied depending on the particular *locus* of doctrinal controversy in which he was intervening' (127). Casaubon's refusal to engage with elaborate explanations of predestination, for example, may have been designed not as a statement of scholarly irenicism, but rather as a covert act of support for the Arminian cause (122). Most powerfully, Hardy shows the theological implications of discussions of the timing and nature of the Last Supper not just by Casaubon but also Joseph Scaliger and others. Evidently, the focus on this stems in part from the fact that it has been put forward by Grafton and others as an exemplar of the power of philology to generate new conclusions in religious issues, in this case the idea that Christian practices were culturally Jewish. In a detailed discussion Hardy shows that the timing of the Last Supper was a theologically charged topic, since its typological character dictated whether it was a sacrifice of thanksgiving or a full-out sacrificial offering designed to obtain deliverance from captivity, as the sacrifice of the first paschal lamb had been. In turn, this had major implications for Eucharistic doctrine, which Catholics considered a propitiatory and expiatory sacrifice to obtain deliverance from sin. Here as elsewhere, a major strength of Hardy's approach is his familiarity with the secondary literature in the history of theology.

Hardy's treatment of Casaubon sets up the interpretative framework for the rest of the book, so it is worth dissecting the nature of his arguments in more detail. For example, in the context of the discussion of the Eucharist, Hardy touches on one of the most famous sections of the *Exercitationes*, in which Casaubon suggested that several elements of early Christian practice were deliberate imitations of pagan mystery cults. Hardy is critical of those who have seen in this section a drift towards 'historicism': 'rather than placing the study of early Christianity on a new comparative and contextual footing, Casaubon's treatment of the early Christian "mysteries" allowed him to distinguish between the elements of them which he regarded as permanent and necessary; those which were not fundamental, but nonetheless worthy of imitation; and those that may have served some purpose, but could or should now be discarded' (148). That the section had an implicit anti-Catholic dimension cannot be doubted. But it might still be legitimate to ask whether that should necessarily stop it from being part of a story of 'historicisation'. As far as I know, no one had ever made the case Casaubon was making as systematically as he did; moreover, the fact that he was by vocation a scholar rather than a theologian surely made him more likely to make it. Certainly that does not mean

that he treated *all* of early Christianity in such a historicist manner: perhaps Hardy's most important point is that we should not generalise a non-existent 'method' from such individual examples. Nonetheless, is this not a case when sophisticated philology – deployed with theological ends in mind – brought something new to the table? This ambiguous state of affairs is reflected in the historiographical claims that have been made, and with which Hardy is in dialogue. For example, he identifies as one of his targets Jonathan Smith's *Drudgery Divine*. But in that book, Casaubon's account of mystery cults is not presented as the work of a neutral scholar, but rather as part of a Protestant campaign to accuse Catholics of 'pagano-papism'.⁵ This is no less an 'ideological' reading of Casaubon than that offered by Hardy (although a far less sophisticated one). Grafton has also noted that in this section Casaubon deviated from his practices elsewhere.⁶ In other words, a claim made for reasons that were not limited to disinterested scholarly curiosity should not necessarily disqualify it from a larger story of historicisation, albeit one stimulated and informed by confessionalisation.

In this regard, it might be worth dividing Hardy's main historiographical case into three. First, there is the point that the world of early modern biblical criticism cannot be parcelled up into progressive scholars (good) and conservative theologians (bad), with all historicist moves coming from the former. I see no way in which anyone could argue against this, not least given the amount of new evidence Hardy has supplied.

Second, there is the point that the scholars who were not professional theologians – not least Scaliger and Casaubon – sometimes had theological agendas of their own. Again, I do not see how this could be denied, especially after Hardy's demonstration of the theological reading done by Casaubon in particular. (See e.g., the discussion of Casaubon's reading of Daniel Chamier at 81.)

Third, and more controversial, is a tendency towards ideological reductionism in Hardy's language. I say 'language' because I do not at all think his real aims are reductionist; rather, they are to correct the triumphalist excesses of some of the secondary literature. As he notes at the end of the book, 'to argue that scholarship and theology were closely associated is not, however, to collapse the two categories into one another', giving the example of the fact that

5 J.Z. Smith, *Drudgery Divine: on the Comparison of Early Christianities and the Religions of Late Antiquity* (Chicago, 1990), 55–8.

6 A. Grafton, 'Comparisons Compared: a Study in the Early Modern Roots of Cultural History', in *Regimes of Comparatism: Frameworks of Comparison in History, Religion, and Anthropology*, ed. R. Gagné, S. Goldhill, and G. Lloyd (Leiden, 2018), 18–48, at 23. (Of course, this was published after Hardy's book was complete.)

‘while Daniel Heinsius’s biblical commentaries and those by André Rivet or Johannes Cocceius are all in some sense “theological”, there are, nevertheless, significant disparities in their style as well as their content’ (401). Here Hardy hints at the issue of quantitative historicisation I raised earlier. But since these caveats come on the second to the-last page, readers of the preceding chapters might wish that they had come in the Introduction. For example, when Hardy argues that Casaubon’s interpretation of early Christian mysteries may have been designed to temper the reception of the book in France, he comes close to implying that Casaubon’s ‘real’ aims were theological and the philology was just a mask designed to conceal them, an impression accentuated by relatively frequent use of terms such as ‘evasive’ and even phrases such as ‘Casaubon’s supposed turn to history was insincere and incomplete’ (101). But Hardy’s meticulous biography itself provides examples of moments when Casaubon’s position was swayed by philology rather than theology, not least when Casaubon reported to Daniel Tilenus that ‘in the matter of the sacraments... Bellarmine sometimes appeared to have the upper hand over recent Protestant treatments of them, not least because he appeared to be supported by the consensus of the Fathers’ (81). When combined with Casaubon’s very real disdain for Reformed theologians who he thought besmirched Christian antiquity in favour of individualistic scriptural interpretation, such as Pierre Du Moulin and Conrad Vorstius (96), do we not see something akin to a Casaubonian ‘worldview’ (if not a ‘method’), albeit one that certainly cannot be identified with any kind of totalising ‘historicism’? And since Hardy has shown that many theologians were also willing to engage in very serious philology, might it not be the case that the phenomenon we are charting is not the ‘replacement of theology with the history of theology’⁷ but rather the historicisation – never complete – of theology?

I shall return to that issue later. In any case, whatever we think on that score, Hardy’s argument against generalising any broader ‘method’ from individual examples – and for realising the confessional import behind many of the most interesting such examples – seems spot on. Almost all of us have probably been too ready to take the most ‘modern’ looking elements of late humanist criticism as representative, and simultaneously to deny the same interpretative generosity to contemporary theologians. Again and again, Hardy shows that that is unfair. Scaliger’s discovery of Hellenistic Judaism, which itself may have been motivated by a desire to historicise Catholic allegoresis and admiration of apocryphal books of the Old Testament such as 2 Maccabees, came to be deployed

7 J.G.A. Pocock, ‘Historiography and Enlightenment: a View of their History’, *Modern Intellectual History*, 5 (2008), 83–96, at 87.

mainly for theological-exegetical purposes, although Claude Saumaise was an exception in this regard (188). When Scaliger himself applied conjectural emendation to the text of the New Testament, it was often to negate favourite Catholic proof-texts, such as Mark 9:49, used to defend purgatory (190). Scaliger certainly never claimed that all New Testament passages that alleged the fulfilment of Old Testament prophecies were later interpolations (a view incorrectly attributed to him by previous commentators (190–191)). As Hardy shows, this acceptance of typology was nearly ubiquitous among both scholars and theologians, and the real debate was about the meaning of specific typological readings. In fact, typology was a particularly fertile ground for scholars engaging in theology: ‘typological connections between the Old and New Testaments’ appealed to them ‘because they were grounded in philological and historical evidence’ (66).

In a short chapter on Selden, Hardy argues that the Englishman was an exception to this rule. This is the only section of the main body of the book where I think he might have failed to observe his own strictures against generalisation. To be sure, Selden did not use typological arguments in his *Historie of Tithes* (1618), and Hardy shows convincingly that plenty of clerics and laymen who opposed him believed themselves to be no less philological than him while still deploying typological arguments. But Selden’s non-use of typology in this work seems to have been a function not of his consistent historicism, but rather of the needs of the specific controversy; as Hardy himself notes, it resembles the arguments made by Reformed theologians such as David Pareus (172–173).⁸ Despite this, Hardy generalises Selden’s approach in the *Historie* to posit a Seldenian method: ‘For Selden, the task of philology was to provide greater precision, bringing glosses and parallels to bear in order to make the singularity of each example apparent’ (167); ‘in Selden’s hands, history posed a direct threat to the universalising modes of exegesis found in typology and natural law alike’ (172); ‘this is not only true of the question of tithing: Selden’s studies of pagan idolatry, similarly, were much more reluctant than those of G.J. Vossius or Herbert of Cherbury to search for a *consensus gentium* that demonstrated the universality and legitimacy of certain religious beliefs and practices’ (179). But while it is true that *De Diis Syris* (1617) is not obviously a work of apologetics or theological argumentation, Selden did there insist that pagan

8 Selden’s broader attitude to typology is worth investigating further: see esp. *De synedriis & praefecturis iuridicis veterum Ebraeorum* (London, 1650), 41–7, discussing 1 Cor. 10:1 and possible foreshadowings of Christian baptism in the Old Testament, but ultimately reigning himself in with the phrase ‘Sed nec definitio, nec contendam; nec Theologum heic ago sed Historicum’ (46). I find it difficult to establish Selden’s aims here, or when he insists that Jewish baptismal practises had pagan parallels (29–30).

idolatry concealed a latent monotheism, an argument that Vossius later developed at great length in his *Theologia gentilis* (1641), and for which he explicitly signalled his debts to the Englishman.⁹ Accordingly, Selden's position would be criticised by Jean Le Clerc later in the century¹⁰ – this is particularly relevant because Hardy cites Le Clerc's criticisms of Grotius on this score as proof of Grotius's universalising tendencies (392). Similarly, in the second edition of *De Diis Syris*, Selden even used his scholarship to support the idea of pagan salvation, drawing on the recently published work of the Italian Catholic theologian Francesco Collio.¹¹ In his earlier discussion of Casaubon's much more tentative postulation of pagan salvation, Hardy took that as a tell-tale sign of Casaubon's confessional allegiance (112–113). Finally, I'm not sure that Selden's work on natural law can really be taken as a challenge to universalisation, since the whole point of *De jure naturali* (1640) is to prove that the Jews also had a concept of a universal natural law (despite its total absence from authorities such as Josephus and Philo) available to right reason (or the conscience) as it partakes in the divine intellect, God having instilled it in man in the first place (i.e., the old Thomist doctrine of *synderesis*).¹²

In challenging his claims about Selden, I am in effect supporting Hardy's overall thesis about the lack of any full 'historicism' in this period. Perhaps the most daring argument he makes to that effect – and the one which will probably elicit the most pushback – is that concerning Grotius's exegesis of the New Testament in the 1640s. While acknowledging the oft-repeated fact that Grotius offered an unprecedented challenge to Reformed notions of the inspiration of the whole text, Hardy emphasises the lack of consistency in Grotius's approach, explaining it by positing ideological explanations for some of the Dutchman's most famous claims. Specifically, Hardy points to Grotius's drift towards Catholicism – or at least a belief in the possibility of a reconciliation – and consequent willingness to accept Catholic interpretations of certain

9 Vossius to Selden, 1 December 1644, in G.J. Vossius, *Opera Omnia*, 6 vols (Amsterdam, 1695–1701), iv.368.

10 *Bibliothèque Choisie*, 7 (1705), 122–3.

11 *De Diis Syris... editio altera* (Leiden, 1629), 72–3, and then *De jure naturali & gentium* (London, 1640), 32, 832–4, citing Collio's *De animabus paganorum libri quinque* (Milan, 1622), later a key source for Herbert.

12 J.P. Sommerville, 'John Selden, the Law of Nature, and the Origins of Government', *Historical Journal*, 27 (1984), 437–47, correcting an influential misreading by Richard Tuck. Note that Selden thought this truth was available even to pagan reason: see e.g. his citation of Hierocles's *Golden Verses* on this score at *De jure naturali & gentium*, 97. At around this time, Hierocles had become a favourite source for those making natural law arguments of this sort, not least Grotius: 'Hierocles de Deorum usu', in Hugo Grotius, *Philosophorum sententiae de fato* (Amsterdam, 1648), 1–17.

pericopes. As Hardy puts it, Grotius's famous 'commitment to "relativising" Christianity by comparison with paganism was not a consistent scholarly programme. Rather, it was a limited, tactical manoeuvre which he used only in specific generic contexts, with specific apologetic or ecumenical purposes' (199).

In particular, Grotius's discussion of the Last Supper, in which he suggested that the Eucharist was accommodated to both Jews and gentiles, reveals his continued commitment to typology and to sacramentalism; with his interpretation, 'Grotius was leaving open the possibility that the Eucharist, as instituted by Christ, was a genuine sacrificial offering, as it was for Bellarmine and other Roman exegetes' (202). Perhaps even more valuably, Hardy consistently reads Grotius's commentaries alongside the much more openly polemical pamphlets he was composing at the same time, using the latter to shed light on the former. In these pamphlets, Grotius again used typological arguments to defend an anti-Calvinist understanding of the relationship between works and justification, and argued that Christ had maintained certain Jewish practices and traditions such as prayers for the dead, monasticism, and purgatory. Often, these arguments involved interpretative manoeuvring that understandably made orthodox Reformed theologians such as André Rivet – himself a very able scholar – suspicious about Grotius's confessional allegiance. As Hardy concludes, Grotius seemed to be using 'them as props for the post-Tridentine Church, rather than sources for a rational or historicising minimalism' (211). In a section drawing copiously on material in the Inquisition and Index archives, he shows that Grotius's commentaries were not placed on the Index because many Catholics, especially in the circle around Cardinal Francesco Barberini, saw in them a powerful anti-Reformed (and anti-Jansenist) tool, and continued to hope for Grotius's conversion.

Hardy's third section turns to Old Testament criticism, focussing in particular on Jean Morin and Louis Cappel and the dissemination of, and reaction to, their ideas. Hardy begins with a crucial point, one which no one before him has made with anywhere near the same force: the limits of acceptability were very different in Old Testament criticism, and we should not take practices in that field to be representative of what was being done with the New. Where in New Testament studies 'criticism' was largely exegetical, more significant innovations had been made in the study of the Hebrew Bible: 'editors and critics of the Old Testament were starting to study the manuscript evidence of it in an unusually systematic way, and producing vivid, wide-ranging demonstrations of the ways in which it had been corrupted and transformed over time' (242). In part, this was because the Old Testament inevitably had less theological significance. But it was also because the inter-confessional disputes it did stir up were

often philological in nature; this was especially the case regarding the issue of which version – Masoretic Hebrew, Greek Septuagint, or Latin Vulgate – was to be preferred.

Hardy identifies the real start of this process with the Sixtine Septuagint (1587), whose ‘creation and reception were accompanied by waves of scholarship that threatened the competing confessions’ assumptions about the genesis, transmission, and meaning of the biblical text’ (250). As Hardy shows, the subsequent decades saw much hand-wringing among Catholic theologians and scholars about the best version of the Old Testament. But in the 1620s, Jean Morin, whom the French Assemblée du Clergé commissioned to oversee the reprint of the Sixtine text, ‘offered a remarkably sharply focussed case for the preeminence not only of the Septuagint, but of a single edition of the Septuagint, which was based almost entirely on its supposed closeness to the original text of the Old Testament’ (257). In his Preface, Morin developed a blatantly anti-Protestant line of reasoning for the unreliability of the Masoretic Hebrew version, and the concomitant need for versions produced and endorsed by the Church; via some rather convoluted arguments, he was able to claim that the Septuagint enjoyed such a status, and that the Sixtine Septuagint came close to the ‘authorised’ version used by Origen. It was also to these ends that he deployed the Samaritan Pentateuch. By following Morin’s practices of collation, Hardy argues very convincingly that such important innovations were made with confessional polemic in mind. It might again be slightly excessive to conclude that ‘Morin’s work was not driven by scholarly curiosity about the history of the Old Testament’ (263). As Hardy himself charts, Morin’s approach appeared original to many Catholics, and the Oratorian developed ‘methods for gathering and analysing textual criticism of the Septuagint into a more specialised, technical endeavour than it had previously been’ (275), and ‘even began to disentangle textual criticism from theological exegesis’ (270).

The truth seems to be that confessional interests were promoting new scholarly methods and ideas, not least because Morin’s confessional opponents, such as Patrick Young and James Ussher, could use the same scholarly tools that he had developed to undermine his claims for the primacy of the Sixtine Septuagint (276ff). However, if this is a case of scholarly tools crossing confessional boundaries, it did not precipitate actual trans-confessional cooperation. Hardy’s case study of the attempts by Young, the Royal Librarian, to produce a new edition of the Septuagint based on the famous Codex Alexandrinus shows how the Republic of Letters could work against any such cooperation. After a valuable section on Young at work (esp. 283–284), Hardy charts how he was unable to secure manuscript readings from Catholic scholars who were his

friends and to whom he had previously provided assistance, Lucas Holstenius and Jacques Sirmond, apparently because they were unwilling to support a project that would help defend the Masoretic Hebrew against the Sixtine Septuagint. As far as I can see, no one has ever previously produced such a detailed exposition of the way religious interests prevented the Republic of Letters from living up to its putative ideals. Presumably, not everyone will agree; indeed, I predict that this will be one of the most interesting areas of historiographical discussion in coming years.¹³ If that is the case, I also predict that the most fruitful conversations will be conducted using the traditional tools of scholarship used by Hardy: that is to say, reading as many letters as possible and placing them in their intellectual, biographical, and institutional contexts. The reason I make this point is that it is time, I think, to say openly what many have long been thinking: that the very generously funded large-scale digital projects that have promised to transform our understanding of the Republic of Letters, while they sometimes provide extraordinarily useful scholarly *tools*,¹⁴ have so far failed to live up to their grandiose interpretative promises, and the money might be better spent on individual doctoral and postdoctoral researchers.

Morin, Young, and Ussher had all largely eschewed conjectural emendation in favour of reconstructing the version history of the Septuagint. As Hardy shows, a much more conjectural approach to the Old Testament was taken by the Saumur-based theologian Louis Cappel, not least because Cappel simply did not have the same access to manuscripts as some of his contemporaries. Cappel is well known for having denied the integrity of any extant version of the Old Testament, and for questioning the antiquity of the Masoretic vowel points. This has given him a reputation for being 'critical' and non-confessional. Hardy, in contrast, argues that Cappel's account of the text of the Old Testament, as offered first in his *Arcanum punctationis revelatum* (1624) and then the *Critica sacra* (1650) was 'far from theologically neutral' (330). First, it was obviously an attempt to defend Protestantism by affirming the possibility of recovering the original Hebrew, at least in terms of fundamental theological meaning. Second, it may have been a product of the specific theology that had been promoted at Saumur by John Cameron and Moïse Amyraut,

13 For an equally innovative approach that comes to very different conclusions, see D. Stolzenberg, 'The Holy Office in the Republic of Letters: Roman Censorship, Dutch Atlases, and the European Information Order, circa 1660', *Isis*, 110 (2019), 1–23. A broader dialogue between Hardy and Stolzenberg on the nature of the Republic of Letters would be illuminating.

14 See above all Early Modern Letters Online, at <http://emlo.bodleian.ox.ac.uk/>.

and which emphasised the obscurity of the Old Testament's proleptic adumbration of Christian truths. This issue is both very important and very complex, and it is not clear that it had a direct impact on Cappel's conclusions about the biblical text; Hardy is himself cautious on the matter. Nonetheless, he is surely right to stress that a reading of Cappel's disputations from Saumur proves that Cappel himself was not an un-theological scholar: 'the *Critica sacra* was clearly meant to be used in the service of exegesis and dogmatic theology, rather than turned against them' (334). It was its belated publication in Paris by the Catholic Morin – who hoped to use it to destabilise Reformed scripturalism – that ensured its controversial status among Protestants. This is another occasion on which Hardy's study of the relevant archival materials has paid great dividends.

'It was th[e *Critica Sacra*], and not anything published by Grotius, Scaliger, or indeed Daniel Heinsius, that made some theologians... start to speak about "criticism" as though it were inherently detrimental to theology, rather than serving as its handmaiden' (242). This, I think, is a very important conclusion. It ties in to the wider thesis that it was only in the middle of the seventeenth century that the Reformed world started to see an open methodological battle structurally similar to that which had been raging in the Catholic sphere for a century and a half, between self-consciously 'scholastic' and 'positive' theologians.¹⁵ But as Hardy shows, the most eminent of Cappel's Reformed opponents who sought to defend the Masoretic tradition were hardly anti-intellectualists or philologically incompetent. Many of them – not least Buxtorf and Ussher – insisted far more than Cappel on the importance of understanding the specific features of individual manuscripts. A different approach was taken by Brian Walton in the *Prolegomena* to the famous London Polyglot (1656). Given the importance of this text, it is strange how little serious scholarship has been devoted to it, perhaps because of the surprising paucity of surviving evidence concerning its composition. Hardy challenges – to my eyes very convincingly – Peter Miller's thesis that it was the product of an 'anti-quarianisation' of biblical scholarship grounded in doctrinal 'minimalism'. (Indeed, Miller's position seems to be a textbook example of the confused conflation of social and intellectual change.) Walton was no 'liberal' – his aim was to buttress the role of clerical authority in interpreting the text while

15 I have developed this thesis in the section on the Reformed world in my 'Confessionalisation and Erudition in Early Modern Europe: a Comparative Overview of a Neglected Episode in the History of the Humanities', in *Confessionalisation and Erudition in Early Modern Europe*, ed. N. Hardy and D. Levitin (Oxford, 2019), 1–94.

remaining with the bounds of Protestantism.¹⁶ He accepted many of Cappel's arguments about the validity of textual criticism, but combined them with a much higher role for ecclesiastical tradition in preserving and giving credit to the various ancient versions. This leads to Hardy's most powerful conclusion: 'the story of early modern criticism was never that of a heroic struggle between theological and anti-theological forces' (368).

Given this conclusion, the 'Coda' with which the book finishes comes as a surprise. It argues that the biblical criticism of the late seventeenth century, especially as deployed by Richard Simon and Jean Le Clerc, was significantly different from that of the confessional criticism charted in the rest of the book. Here, I think Hardy does not grant this later period the complexity he finds in his own (although it is important to note that he himself describes his arguments in this section as provisional). Specifically, he abandons the hermeneutic of suspicion that he deployed throughout. For example, he claims that Le Clerc used Lockean philosophy not just 'to codify or defend a nascent historical-critical method', but 'to neutralise the confessional disagreements which the late Renaissance erudite practices that passed for "criticism" had so often helped to foment' (396). But why believe Le Clerc's irenic rhetoric when so much scepticism has been applied to Scaliger, Casaubon, Grotius or anyone else saying similar things? In reality, Le Clerc's works again and again ended up using 'criticism' to defend Arminian doctrine, exactly the sort of thing that Hardy consistently calls 'tendentious' use of philology in the case of earlier figures. This was the case throughout Le Clerc's career: from his writings of the 1680s, in which attacks on Reformed theological method were plainly designed to defend the Arminian position on grace; to his French New Testament (1703), condemned by many Reformed divines for its Arminian tendencies despite its ostensibly 'neutral' method; through to his *Historia Ecclesiastica* (1716), whose loud polemics against philosophising in theology were designed not only to uphold Le Clerc's tolerationism but also to defend yet again the Arminian position on grace. Indeed, I would be prepared to go further, and to say that, if we are to measure people on a scale from non-confessional to confessional, Le Clerc was more confessional than Scaliger, even if his overall method was more generally historical. This was a function of the institutional backgrounds from which they emerged: Scaliger was one of the last great products of the patronage-funded humanism of the sixteenth century; Le Clerc, in contrast, always in contact with the institutionalised humanism of the seventeenth

16 Hardy here builds on S. Mandelbrote, 'The Authority of the Word: Manuscript, Print, and the Text of the Bible in Seventeenth-Century England', in *The Uses of Script and Print, 1300–1700*, ed. J.C. Crick and A. Walsham (Cambridge, 2004), 135–53.

century, simply could not avoid engagement with confessional issues for any part of his life.

As for Simon, Hardy acknowledges his obvious desire to defend Catholic traditionalism, but claims that he 'put very little flesh on the bones of his argument for tradition' (382). But Simon's *Histoire Critique du texte du Nouveau Testament* (1689) was abundantly clear about his confessional aims, often deploying the most violent anti-Protestant language. Its application of critical method seems no less tendentious than that of most seventeenth-century predecessors. For example, Simon challenged patristic arguments when it suited his case, but was quick to deploy the old claim that deviant patristic interpretations were just 'private opinion'. In fact, both Simon and Le Clerc seem to offer perfect case studies of the very phenomenon Hardy has traced: scholars being 'forced to circumvent the dangers of direct involvement in doctrinal controversy by pretending to have dealt solely in matters of fact' (151). The recent work of Timothy Twining, some of it already published in this journal,¹⁷ illustrates the ways in which Simon emerged from the world of confessional scholarship, and how his reception as a particularly 'dangerous' critic was no less shaped by confessional forces than Cappel's.¹⁸ However, Hardy's Coda does raise the crucial question of how and when criticism was 'generalised'. One wonders if part of the answer is to be found in the deployment of the approaches previously applied only to the Old Testament (as Hardy has shown) to the New – the names of John Fell and John Mill in England come to mind – but also the programmatic expansion of the accommodationist hermeneutic to Christ, already being strongly advocated in some circles c. 1670.¹⁹ In all these cases, this was being done by scholars and theologians with a confessional agenda, one that found repeated manifestation in their work. Even a century later, the first articulation of the Synoptic Problem in the German universities, by Gottfried Lessing, Johann Gottfried Eichhorn, Johann Jakob Griesbach, and others, was still in part conducted as a debate

17 T. Twining, 'Richard Simon and the Remaking of Biblical Criticism', *Erudition and the Republic of Letters*, 3 (2018), 421–87.

18 T. Twining, 'Publishing a Prohibited Criticism: Richard Simon, Pierre Bayle, and Erudition in late Seventeenth-Century Intellectual Culture', in *The Worlds of Knowledge and the Classical Tradition in the Early Modern Age: Comparative Perspectives*, ed. D. Levitin and I. Maclean (Leiden, 2021), 336–65.

19 See e.g. Etienne Gausson, 'De natura theologiae', in *Quatuor dissertationes theologicae* (Saumur, 1670), 88–92. This subject deserves further study. The interesting recent account by H.J. Lee, *The Biblical Accommodation Debate in Germany: Interpretation and the Enlightenment* (Dordrecht, 2017) finds precedents to its eighteenth-century variant among the Cocceians only (23–61), which cannot be the end of the matter.

about the nature of Christ's messianic mission and the theological purposes of the earliest redactors of Christ's message, whoever they were.²⁰

Again, all this only serves to strengthen Hardy's overall thesis. It is worth stepping back for a second to place that thesis in broader context. *Criticism and Confession* is written very much with the secondary literature in mind. Accordingly, many of its interpretative conclusions are negative ('major scholar x was not as universally "critical" as has been claimed'), a fact Hardy himself recognises when he worries that his book might be perceived as 'overwhelmingly destructive, rather than constructive' (400). But that is certainly not how it should be received. Rather, I think it should be read as a fantastic case study of intellectual change within a highly pre-modern, confessional society. As Hardy puts it when summarising his section on Morin and subsequent Old Testament criticism:

it must be admitted that all of the scholars considered in this chapter were making important innovations that had something in common with each other. Although they may not have established a coherent, objective science of biblical criticism, they still came closer to doing so than scholars who, for all their radicalism or heterodoxy, operated within more traditional intellectual frameworks... Morin, Young, Ussher, and Walton... may not have been 'minimalists', but they nonetheless took more care to separate their arguments about the text of the Old Testament from the other theological controversies in which they were involved. They were, of course, still conducting a theological argument; but it was usually confined to the specific *loci* of controversy concerning the text, canon, and authority of Scripture, rather than the wider range of issues that had been at stake whenever Casaubon or Grotius applied themselves to the Bible (371–2).

Whether scholars agree with Hardy or not, we can only hope that they adopt the rigour he has displayed, and take on board the methodological lessons he has put forward. No one, I think, can now speak supinely of a constant culture war between 'critical' philologists and 'theological' divines, or presuppose any easily definable 'historicism' in the mid seventeenth century.

20 On Eichhorn, see G. Baltes, *Hebräisches Evangelium und synoptische Überlieferung: Untersuchungen zum hebräischen Hintergrund der Evangelien* (Tübingen, 2011), 168–73.

These comments hopefully serve to demonstrate that there is more in common between Hardy's book and Van Miert's *Emancipation of Biblical Philology* than one might at first imagine. Not only do both begin with a quotation from Scaliger, but both also emphasise that biblical philology regularly served as a handmaiden to theology and that it was just as often deployed by professional theologians as by those whom we usually recognise as 'scholars'. Where they differ is in some individual interpretations, and also the type of diachronic narrative that they offer. If Hardy has focussed on what I have called qualitative change, Van Miert is just as interested in a thesis about quantitative evolution. As he puts it, biblical philology did not have some intrinsic secularising force; 'however, the accumulation of philological evidence collected by various, and often adversarial, stakeholders made it more and more difficult to bring these results together into a unanimous Reformed interpretation of the Bible. Gradually, scholars had to acknowledge that the problems of establishing access to an "original" and "pure" Hebrew and Greek text stood in the way of defending the perspicuity and self-evidence of the Bible' (xxii).

Hardy would probably disagree with this formulation. He might also suggest that Van Miert's definition of his object of study – biblical philology defined as 'the study of a biblical text by means of textual criticism, linguistic analysis, and historical contextualisation' (xiii) – prejudices his conclusions. But before we place these two monographs beside each other, we need to appreciate the full range and scope of Van Miert's thesis. He begins by tracing the exceptionalism that he finds in the seventeenth-century Dutch Republic to the legacy of Scaliger: 'there is a certain "Scaliger school" which was not clearly geared towards a confessional agenda and at times was not even useful for theological reasoning, although Reformed visions of the past did offer a larger framework for Scaliger's approach to history' (11). Here, and especially in Van Miert's concomitant claim that because Scaliger 'focused on historical issues and largely ignored theological discussions' he 'potentially jeopardised the authority of Scripture', we see the first juncture at which Van Miert and Hardy diverge. I am not qualified to act as a judge, and I think this is a discussion that should be pursued further, not least with Grafton involved. However, it is worth pausing on the logic of Van Miert's words. One issue is what proportion of Scaliger's ideas stemmed from pure philological acumen and curiosity. But even if they did, does that mean that they 'jeopardised the authority of Scripture'? All subsequent scholars of these issues will now need to be aware that such questions cannot be answered by looking at the logic of the ideas themselves – history does not proceed logically – but only at their actual deployment in historical time.

In this regard, Van Miert proceeds with admirable caution. He turns out to be rather more circumspect about Dutch exceptionalism than some of his initial comments might suggest (e.g., 21). On this score, I would venture that any idiosyncrasy of the Dutch context owed as much to the uniquely strong position of the arts faculties in the newer universities, especially Leiden and Franeker, as to the influence of any one scholar.²¹ That is not to say that Scaliger was insignificant, and Van Miert's first chapter brings forward all the expertise developed over years of editing Scaliger's correspondence to chart the various forms of biblical criticism that appear in that correspondence, as well as the oral statements reported in the *Scaligerana*. For Van Miert, these show that 'Scaliger struggled with the tension between the trust in his scholarship and his faith in the Bible' (38). The best example for this is Scaliger's claim that 'the ancient Christians have added much to the New Testament' (the claim was made in the context of a discussion of the difference in the accounts of Herodias in the text and in Josephus) and the subsequent, famous assertion that 'there are more than fifty additions or changes to the New Testament and to the Gospels... If it were a pagan author, I would speak of it differently'.

Nonetheless, Van Miert also notes Scaliger's limits: 'when it comes to textual criticism in the limited sense of the constitution of the original text, Scaliger is restrained' (40); indeed, he preferred readings from the Church Fathers to those taken from manuscripts. Van Miert also notes Scaliger's admiration for Calvin's exegetical method, which – Scaliger claimed – contained 'no scholastic argumentations' (42). And like Hardy, he notes that Scaliger's historicisation of the Eucharist was rooted in an anti-Catholic agenda (43). An interesting passage offering a naturalised explanation for Christ sweating blood (Luke 22:44) shows that Scaliger in fact thought such explanations would 'contribute much to faith', since false miracles 'force Jews and heathens to recoil from the Gospel' (46). This is surely an important wider theme: how often were historicising or even de-sacralising moves the product of a desire to strip the 'true' faith of putatively superstitious accretions? Van Miert also discusses Scaliger's unpublished work on the supposed original Hebrew of Matthew's Gospel (now Bibliothèque Nationale de France, MS Latin 690), but he has here missed the confessional dimension: Catholics frequently asserted that Matthew wrote in Hebrew (or Aramaic) so as to defend early church tradition and to show that the Greek version used by Protestants was not the original; Scaliger's denial was in part targeted at these Catholic arguments. But all in all, I think it could be said that the most substantive difference between Hardy and Van Miert – at

21 Levitin, 'Confessionalisation and Erudition', 52–9.

least on Scaliger – is not one of overall interpretation but of approach. Hardy explores the specific confessional significance of some of Scaliger's arguments; Van Miert focusses on a more quantitative summary of many of those arguments. In the end, he even quotes De Jonge's statement that 'the undermining of Catholic conceptions was the goal of various exegetical notes by Scaliger' (51).²² It seems that no one really believes that Scaliger was a 'pure' scholar driven solely by curiosity.²³

Van Miert's next chapter, pointedly entitled 'Biblical philology: nothing radical', also seems to tie in well with Hardy's thesis, since it focusses on the philological acumen of two rival theologians: Jacobus Arminius and Franciscus Gomarus. Indeed, contrary to the claims of a long tradition of Arminian hagiography, Van Miert shows convincingly that Gomarus 'proved a far more accomplished philologist than Arminius' (53–54). The disputations conducted by Arminius's successor, Simon Episcopius, made much more room for philology than those of Arminius himself (62). Gomarus's philological acumen, meanwhile, was praised by both Scaliger (who entrusted him to re-edit his *Thesaurus temporum*) and Heinsius. Van Miert claims that Scaliger deeply inspired Gomarus, but the two examples he provides – the genealogy of Christ and the language of Matthew's Gospel – were such widely discussed issues that we need not assume that 'Scaliger's shadow loomed large' over Gomarus for him to discuss them (65); nor was the second issue 'theologically neutral' (67). Nonetheless, Van Miert supplies much excellent evidence for 'an entirely different portrait of Gomarus [from] the familiar image of a staunchly orthodox theologian' (67). Scaliger's influence is more convincingly posited in the illuminating discussion of Gomarus's study of biblical prosody, *Davidis Lyra* (1637). This chapter also discusses Johannes Drusius, who came into conflict with the theologian Sibrandus Lubbertus concerning the authority of philology. I think rather more could have been said about this – the claim that for Drusius 'the biblical text led to an orthodox point of view, not the other way around' (71) would apply to every Protestant, including Lubbertus. It may well be that Lubbertus attacked Drusius for theological rather than methodological reasons (as Van Miert himself argues at 73). Van Miert also mentions in passing

22 H.J. De Jonge, 'The Study of the New Testament', in *Leiden University in the Seventeenth Century*, ed. T.H. Lunsingh Scheurleer and G.H.M. Posthumus Meyjes (Leiden, 1975), 65–109, at 87.

23 Although in his review of Van Miert's book, De Jonge has written that 'practitioners of pure, disinterested biblical scholarship were rare in the period under discussion, but there were some: Scaliger and Drusius, and certainly Louis de Dieu': *Low Countries Historical Review*, 134 (2019).

that the Leiden Hebrew professor Gulielmus Coddæus ‘compiled a collection of New Testament quotations in patristic texts that differed from the available texts of the New Testament’, claiming that he ‘probably’ did so ‘under the influence of Scaliger’ (76): this interesting question would be well worth further investigation.²⁴

Van Miert’s next chapter provides the fullest account ever of the composition of the States’ Translation, commissioned at Dordt and published in 1637. As he rightly notes, this account only confirms that philology was practised by all theologians, and ‘was not deemed inherently subversive’ (78). In this respect, his work ties in with other recent scholarship on the scholarly dimension of the production of vernacular bibles, not least by Hardy.²⁵ Van Miert makes the important point that it was often the laity rather than the theologians who were a conservative force: ‘the people loathe even the slightest change when Bibles are reprinted’ complained one delegate at Dordt (81);²⁶ booksellers also hated being stuck with unsold stock of older editions (83). As for the scholarship behind the text, Van Miert makes excellent use of the so-called *Autographon*: the master-copy of the translation heavily annotated by the team of revisers who worked on it. (I cannot convey here the richness of Van Miert’s discussion.) But for all his emphasis on the historical-philological element of their work, he does not fail to note that their notes also offered theological guidance, much of which was obviously pushing Reformed interpretations (93), against not only Catholics but also Arminians (100). These were made without open polemic or naming the target: evidently, even theologians sought to soften the obviousness of their confessional agendas when not writing works of controversy. All this is so carefully argued that it comes as something of a surprise when Van Miert concludes that ‘the States’ Translation also proved to be a Trojan horse’ because it opened up biblical philology to the public (100–101). Given all the arguments for its philological sophistication and market success, would it not be equally or more convincing to suggest that it *bolstered* scriptural authority among the Dutch? To answer, we need to know more about its readers. The second, equally unexpected conclusion is that the act of making such translations and of issuing confessions of faith

24 Van Miert here follows up H.J. de Jonge, *De bestudering van het Nieuwe Testament aan de Noordnederlandse universiteiten en het Remonstrants Seminarie van 1575 tot 1700* (Amsterdam, 1980), 21–2.

25 N. Hardy, ‘Revising the King James Apocrypha: John Bois, Isaac Casaubon, and the Case of 1 Esdras’, in *Labourers in the Vineyard of the Lord: Scholarship and the Making of the King James Version of the Bible*, ed. M. Feingold (Leiden, 2018), 266–327.

26 A claim that has an illustrious precedent in Augustine, Ep. 71 to Jerome.

somehow undermined the principle of *sola scriptura* (101–102).²⁷ Such a reading would have been anathema to the translators, revisers, and the majority of their audience.

Van Miert's fourth chapter turns to the biblical studies of Daniel Heinsius as they manifested themselves in his huge *Aristarchus sacer* (1627) and *Sacrae exercitationes* (1639). Like Hardy, Van Miert is sceptical about finding any coherent method behind Heinsius's approach: 'his biblical philology [is] diffuse and difficult to characterise as a consistent effort with a clear programmatic purpose' (105). One of Van Miert's most valuable observations is that a key purpose of the *Sacrae exercitationes* was to show that the Church Fathers 'were very sloppy in their quotations from the Bible and that they cannot be trusted as witnesses to the early stages of the biblical text's constitution' (108); in this regard, Heinsius differed greatly from Scaliger. Van Miert couples this with the claim that Heinsius 'sought to avoid controversies', but one cannot help thinking that this was also an implicit case against Catholic traditionalism. As Van Miert reports, one celebrated reader of the drafts of the book, Constantin L'Empereur, thought that noting so forcefully the variations in the early textual tradition of the New Testament was illegitimate if there were no manuscripts attesting to the variant readings: a nice example of how even close colleagues could differ on the right application of 'criticism' (110). Drawing on Peter Van Rooden's classic study of L'Empereur, Van Miert suggests that there are clear instances when Heinsius would identify *textual* problems which L'Empereur would solve with *theological* reasoning. But then, Heinsius always had good theological authority to justify his approach: as he wrote, 'the great Augustine warns us that the first thing to do, when we get struck in the Sacred Scripture, is to diligently consider whether the manuscripts are corrupted' (111).²⁸ Moreover, the minimal conjectural emendations that Heinsius made were designed to save the evangelists from the charge of being mistaken by shifting the burden of blame onto the copyists (113). In any case, variants did not endanger doctrine, which the Holy Spirit never allowed to vary.

Among many other valuably detailed explorations, Van Miert also focusses on Heinsius's study of the 'Hellenistic' dialect and his criticisms of Beza. One of the most interesting aspects of the first is that it allowed Heinsius to show why

27 See also even more strongly 141: 'The Belgic Confession indeed stressed that the Bible was the testimony of the Holy Spirit, but the Synod of Dordrecht had made clear that the Bible alone was not sufficient: the Confession, together with the Heidelberg Catechism and the Canons of Dordrecht, were quickly establishing an authoritative human tradition in its own right'.

28 Van Miert does not identify the source, which is presumably *De doct. Chr.*, ii.11.

Nonnus, on whom he was commenting, had been a bad theologian because he had not understood the language of the New Testament: 'Heinsius defended the pursuit of biblical philology by stressing its use for dogmatic theology' (121). Likewise, there was nothing anti-theological about Heinsius's criticisms of Beza – after all, Gomarus had also planned to amend Beza's New Testament (124). Van Miert also discusses Heinsius's role in printing the second edition of Selden's *De diis Syris* – the strictly Reformed Heinsius seems also to have missed Selden's provocative ideas about the salvation of pagans (see above). Like Hardy, Van Miert notes the important differences of format favoured by various authors: Heinsius the commentary, Saumaise the monographic study (129). Perhaps the most interesting meta-observation in this chapter is that Heinsius, like other Dutch scholars, saw biblical interpretation as the culmination of a professional scholar's career. This is worth putting in comparative perspective. At this point, the institutionalisation of such lifelong careers in scholarship only really existed in the Dutch Republic. In England, for example, a professorship in Greek was an intermediate step on the ladder to clerical promotion (hence the importance of the fact that when it was eventually set up, the Laudian chair in Arabic was well funded and the incumbent was not allowed to hold a clerical post; its first holders thus stayed in the chair for a long time and produced monuments of scholarship comparable to those of Heinsius or Saumaise).²⁹

One Dutch scholar who was a product of this institutionalised world but came to function outside it was Grotius. In his longest chapter, Van Miert does not ignore the ideological element of Grotius's writings so highlighted by Hardy: one of the very first things he says is that 'unlike Heinsius's more neutral approach, Grotius's philological study of the Bible ran parallel to a sustained polemic over religious politics: he used the tool of biblical philology to back up his somewhat naïve ideal of religious ecumenism' (133). Like Hardy, he also notes the great importance of Grotius's polemical pamphlets of the 1640s (139–140). He identifies perhaps the most important point of Grotius's critique of Reformed theology, made in the annotations on Cassander (1642), which is worth quoting in full:

Not everything defended by Protestants is found literally in the Bible. They say consequences have to be accepted. But they themselves often

29 M. Feingold, 'Patrons and Professors: the Origins and Motives for the Endowment of University Chairs – in Particular the Laudian Professorship of Arabic', in *The 'Arabic' Interest of the Natural Philosophers in Seventeenth-Century England*, ed. G.A. Russell (Leiden, 1994), 109–27.

admit consequences stemming from a private individual mind. Hence the large number and serious character of controversies among Protestants, and their daily splits. (Trans. Van Miert, 141)

Van Miert does not quite convey the importance of this argument, which denied the methodological rubric – at that point almost universally accepted among the Reformed – that the logical consequences extracted from the biblical text could be recognised as not just doctrines, but as doctrines essential for salvation.³⁰ Here lay the very essence of much of Reformed divinity, which allowed it to defend central theological concepts such as a fully consubstantial Trinity and an elaborate doctrine of grace. In shifting the grounds for such doctrines from logical interpretation to the tradition of the early church (considered as witnesses to biblical truth rather than as an independent source of authority), Grotius – perhaps drawing on Catholic polemicists such as François Véron – was advocating a major change in theological method, one which was resisted by many of the Reformed.³¹

Van Miert does not offer a thesis regarding the extent of Grotius's crypto-Catholicism (142); rather, he focusses on Grotius's scholarly arguments. I can only convey some of his many findings and conclusions, not least the recognition that 'Grotius's textual criticism of the New Testament is more sophisticated than that of the Old Testament' (146), in part because of his use of variants from Codex Alexandrinus (collations had been sent to him by Young) – an interesting counterpoint to Hardy's discussion. When Grotius suggests an emendation to Romans 11:6, a classic proof-text for controversy concerning grace, I think he is rather more concerned 'to underscore a theological point' than Van Miert allows (146). As for the pagan parallels discussed when commenting on *Hoc est corpus meum*, Van Miert seems to differ from Hardy when he implies that they are 'contextualisations' rather than natural law arguments deployed to support a theological thesis; he brings forward Grotius's use of accommodationism to support the thesis (156–157).

On all these issues, it would be wonderful to see a forum (perhaps in this journal?) between various specialists on Grotius's biblical criticism and theology: not only Hardy and Van Miert but also De Jonge, Nellen, Sarah Mortimer, and others. The central dilemma is the extent to which Grotius's 'historicism' stemmed from a scholarly method or impulse (however vague and un-programmatic), or whether it was limited to those sections where Grotius wanted

30 R. Muller, *Post-Reformation Reformed Dogmatics*, 2nd edn, 4 vols (Grand Rapids, 2003), ii.493–502.

31 See e.g. Voetius's disputations cited in n. 34 below.

to score confessional points. As always, I suspect the truth will be found somewhere between the two extremes (but not necessarily in the middle). If Hardy has hyper-emphasised the confessional-polemical dimension of Grotius's commentaries, then Van Miert's claims that for Grotius 'the apostolic letters were written for specific audiences located in particular times and places, and... are not meant to serve as a corpus of doctrinal texts or rules to organise the Church' (158) and that 'the Holy Spirit seemed almost to have played no part in its [Scripture's] conception' (168) may go too far in the other direction. After all, Van Miert himself includes an excellent section on the Arminian doctrines scattered throughout the commentaries (159–162), and concludes that 'had Grotius remained consistently free of a theologico-political agenda, he might have produced a far more rigorously historical and philological commentary on the biblical text' (166; also 168). Perhaps most importantly, he suggests that Grotius's greatest significance was not qualitative but quantitative (166).

Van Miert's next chapter focusses on Claude Saumaise's participation in the 'Hairy War' of the 1640s: the dispute over whether men could wear long hair initiated by Dutch advocates for further reformation who had been inspired by English puritans. It offers a nice example of how anti-precisian campaigns served to generate new historical arguments designed to negate the deployment of scriptural injunctions in non-essentials. The question is whether such moves informed a wider process of 'historicisation'. Van Miert finishes his rich discussion with a generalisation: 'the future belonged to those who interpreted much of the Bible historically, and it was not long before Calvinist clergymen themselves started to wear wigs' (191). But need the victory of a growing bourgeoisie against a few puritans on something that even the latter did not consider an essential issue really be proof of a process of wider historicisation, let alone one that led to Spinoza (192)?

The next chapter, on the well-known pre-Adamite Isaac de La Peyrère, seems rather tangential to the main argument of the monograph. La Peyrère 'did not know Greek and Hebrew' (197) and was treated as a buffoon by those who bothered to respond to him; the more I read about him, the less important I find him to have been to seventeenth-century intellectual history. The interesting thing about pre-Adamitism is not its influence – which was virtually non-existent until much later – but rather the cultural-historical question of why a tiny number of laymen subscribed to it in the first place.³² The most novel claim made in this chapter is that 'La Peyrère... forced his adversaries to put biblical philology on the academic curriculum' (204), but even here the

32 On this score, see the brilliant recent study of another pre-Adamite, Francis Lodwick, by William Poole and Felicity Henderson: *On Language, Theology, and Utopia* (Oxford, 2011).

actual arguments seem to have been largely trivial. Much more intriguing are the Leiden disputations from 1657–1658 ‘specifically dedicated to textual matters’ which Van Miert discusses in his next chapter (216–217). It would have been good to have a longer discussion of these disputations, the extent to which their method was novel, and their exact place in theological education – was doctrine always at stake? Since so many of them are printed, they would make an excellent topic for an energetic PhD student.

Van Miert’s own focus in this section is on Isaac Vossius, and specifically on his well-known arguments in favour of the Septuagint chronology, in part to accommodate Chinese annals. Van Miert claims that these were ‘iconoclastic conclusions’ and that they prove that Vossius and La Peyrère ‘largely shared methodologies’ (222, 224), which I find unconvincing. More generally, this final section of the monograph, while it contains much interesting material, is slightly weaker than those that precede it. It is as if Van Miert suddenly felt the need to justify his title, and so there are quite a few vague statements about Cartesianism, the ‘destructive potential’ of biblical philology, and essentialising claims such as ‘theology was metaphysical and moral in character, whereas philology was a method of studying texts’ (233). There are also errors of fact: for example, Saumur is called a ‘Remonstrant stronghold’ (235). Van Miert even quotes the ultra-reactionary Catholic polemicist Brad Gregory about Protestantism generating an ‘individualistic, hermeneutical anarchy’ (Gregory has also argued that quantum theory provides further evidence for the necessity of a return to Thomism). Van Miert himself is aware that his claims for the ‘radical’ nature of mid-seventeenth-century biblical criticism are largely conjectural, admitting that the idea that such criticism ‘undermined biblical authority is easier to argue theoretically than to prove empirically’ (235).

Perhaps Van Miert’s talk of ‘emancipation’ should be read as a wave in the direction of the academic book market; it should not obscure the erudite, nuanced, and detailed themes that he has developed in the rest of his study. As I have noted, he is as concerned with the process of quantitative historicisation as he is with the individual leaps towards ‘higher criticism’ made by any one scholar. In this regard, his hints about the greater institutional role for biblical philology, and about its dissemination among the Dutch laity, will undoubtedly stimulate much further research. That is not, however, to detract from his readings of individuals such as Scaliger, Gomarus, Heinsius, Saumaise, and Grotius, all of which are some of the most sophisticated available, but are also conveyed with great clarity.

The many virtues of Van Miert's book are shared by Toubert's *Spinoza and Biblical philology*. Not only does he reveal many new details about the institutional context and lay reception of academic biblical criticism (often contained in Dutch texts largely unknown to Anglophone scholars), but he also makes excellent use of ecclesiastical records, especially consistory, classis, and synod minutes. More than either Hardy or Van Miert, he is concerned with excavating texts by scholars who are now long forgotten but whose works were widely read in the seventeenth century. The synthesis of such material alone makes the book an essential resource for anyone interested in the wider social impact of biblical criticism in our period.

Toubert's main thesis is that biblical criticism was a largely independent field of activity that need not be subordinated to philosophical categories. Spinoza's most important impact was not philosophical (in this regard his ideas were almost universally rejected) but rather philological, and even there among the *orthodox*: 'this book argues that one major area to be impacted by Spinoza's historical deconstruction of Scripture, unduly disregarded by modern historiography, was the practice of biblical philology among the Reformed Dutch' (5; also 15). In many ways he is very successful, although not always quite in the manner he intends: his work only seems to confirm Spinoza's marginalisation and even unimportance to the development of biblical criticism and intellectual history more broadly. In this regard, there is one point on which one wishes he had slightly nuanced his overall thesis. Repeatedly, he claims that after the publication of the States' Translation orthodox Dutch clergymen not only stopped caring about biblical philology, but even actively opposed it: 'the texts, history, and sense of the Bible's literal meaning had become immobilised by the adoption of standard editions, translations, and accompanying commentaries' (20); supposedly, such theologians, sometimes identified as followers of Voetius, espoused a 'dominant neo-scholastic theology that revolved around key theological concepts' and eschewed philology (78); built 'their theology on neo-scholastic foundations' (85); and disdained 'fresh textual investigation' in favour of reasserting 'the framework laid down by the Synod of Dordrecht' (190). In turn, this leads him to reify a group of 'scripturarians' who, he claims, opposed this 'dogmatist' tendency: 'for a dogmatist, the authorised translation, the accompanying annotations, and the Formularies of Unity were the essential tools for a Christian to read the Bible in a proper way. A scripturarian, on the other hand, was satisfied with only the biblical texts in their original languages' (23; also 86, 88, 92, 103, *passim*). Although these terms were sometimes used by contemporaries, I think Toubert pushes them too far: one would be hard pressed to find any orthodox theologian who disdained biblical

philology. For example, Voetius repeatedly emphasised the need for intensive philological training for theologians, even encouraging them to learn Arabic!³³

This is the sort of disdain for orthodox Reformed biblicism that Hardy is so keen to correct. Nonetheless, it is important not to reject Toubert's framework entirely. As I noted earlier, there *was* a methodological dispute in the Dutch Reformed world by the 1650s. However, it was not about the use of philology (everyone agreed that it was useful) but rather on the nature and organisation of theological teaching. Challenged by the Arminians – not least Grotius (see above) – Voetius and his allies now self-consciously defended the use of theological systems built on the logical implications of individual scriptural *loci*.³⁴ This was the 'Reformed scholasticism' that they espoused. It was a method that did not disdain philology or claim that the States' Translation was beyond reproach. However, it did limit the extent to which theologians should engage in philological disputes, which the Voetians saw as academic pedantry that did not contribute to the goal of further Reformation. So while Toubert's characterisation of the Voetians as unbiblical dogmatists is unfair, I think he *is* right to imply that, had this kind of precisianism triumphed even more, it would have caused a significant further decline in biblical scholarship, at least quantitatively. In this regard, one might say that Hardy's desire to rescue the Reformed from the charge of anti-intellectualism is entirely correct, but does not tell the full story. For example, when Hardy points out that John Owen's critique of the London Polyglot should not be read as a battle between two approaches to theology because 'Owen was quite up to speed with contemporary biblical scholarship' (Hardy 370), he is right to note that Owen was no narrow-minded 'scholastic', just as Walton was no 'minimalist'. But does it not remain legitimate to say that the theological-intellectual world that Owen inhabited would have never produced anything like the Polyglot? A structurally similar situation can be identified in the Dutch Republic, and in this regard a slightly tempered version of Toubert's framework seems most appropriate.

Before he turns to applying that framework to the mainstream debate, Toubert begins with a long chapter on Spinoza's *Tractatus* which argues 'that we should

33 Gisbertus Voetius, *Exercitia et bibliotheca, studiosi theologiae* (Utrecht, 1651), 45. This is in the section 'De cursus theologici intervallo primo' – Voetius also recommends reading Drusus's notes on the Old Testament (37) and even Arias Montano and Cappel (42). See further the important comments by the leading scholar of Voetius, Andreas Beck, *Gisbertus Voetius (1589–1676): sein Theologieverständnis und seine Gotteslehre* (Göttingen, 2007), 203–4.

34 See the two disputations 'De ratione humana in rebus fidei' (Feb 1636) and 'De theologia scholastica' (Feb 1640) in Gisbert Voetius, *Selectae disputationes theologicae*, 4 vols (Utrecht, 1648–67), i.1–29, esp. 5–12 for the validity of deducing logical consequences from Scripture via ratiocination.

not ignore the biblical scholarship of Spinoza by treating it as a mere smoke-screen for his radical ideas' (74). Drawing on the classic studies by Roberto Bordoli and James Preus, he places Spinoza in the context of the contemporary debate about the relationship between philosophy and theology in which Lodewijk Meijer played the central role. There follows a long discussion of possible Jewish and Christian influences on Spinoza's criticism. As is well known, this is a challenging issue, partly because of a lack of information, but also because Spinoza's aims were so blatantly instrumental (far more than those of the theologians against whom he argued), and all his ideas were subordinated to establishing the political message of the *Tractatus*. Spinoza had zero interest in textual criticism (63). And, as Touber acutely notes, he barely followed through on the methodological prescriptions put forward in the famous seventh chapter of the book (66–67). It would be interesting to investigate if there are any precedents for Spinoza's ideas about various Old Testament books – canonical and apocryphal (68) – in the heated debates about canonicity that followed Trent. As is well known, there was nothing new about the claim that Moses did not compose all of the Pentateuch; Spinoza deployed it not because he was a pioneering critic but because it suited his political argument (68–69). That argument, as Touber notes, should not be seen as the culmination of the genre of *respublica Hebraeorum* – after all, the reader had just been left 'with not much confidence in the accuracy and credibility of the Bible as a historical source' (71). In this regard, I think that Touber is quite right to challenge the high-profile but superficial and often unconvincing thesis developed by Eric Nelson in his *Hebrew Republic* (2010).

The next chapter turns to the initial responses by Dutch theologians to what Leibniz characterised as Spinoza's 'false arguments and his abuse of eastern letters' (76). The first figures to be considered are Jacob Alting, theology professor at Groningen, and Antonius Perizonius the younger, theology professor at Deventer. Very interestingly, Touber reveals that Perizonius sought to reform academic disputations by grounding them in the exegesis of individual scriptural passages rather than theological *loci communes* (80) – here we really do see a 'philological' challenge to established methods of doing theology. He reports this argument as it was made in a letter to Cocceius; it would be good to know more about the circumstances that led Perizonius to compose his *De ratione studii theologici tractatus ad eius emendationem praecipue spectans* (1669), one of the fullest arguments for the reform of theological method published in the seventeenth century, but – as far as I can tell – virtually unknown to scholarship (it is not cited here). Alting inspired Perizonius to investigate Spinoza's arguments for the corruption of the books of Ezra and Nehemiah; alas, Perizonius's refutation has not survived. Nonetheless, in his

correspondence with Alting we find a very relaxed attitude to the question of the historical formation of the Old Testament canon. The first published response to the *Tractatus* was by Johannes Melchior, a professor at Herborn. As Toubert shows, theologians such as Melchior – whom no one would ever call ‘liberal’, ‘enlightened’, etc. – were perfectly at ease with the method proposed by Spinoza in chapter 7 of the *Tractatus*; what they opposed was what they took to be its egregious misapplication. The same conclusion applies to Regnerus van Mansveld of the University of Utrecht, who happily deployed Grotius’s ideas against Spinoza (100), and the Collegiant Frans Kuyper. Theologians, in short, were not remotely reluctant to use philological or historical arguments. Meanwhile, after the publication of Spinoza’s *Ethica* in 1677 it became obvious that his whole approach was grounded in a metaphysics that seemed outlandish to virtually everyone, and Dutch anti-Spinoza writers turned to scoring into the open goal that was refuting monism. Toubert finds the return to philological anti-Spinozism only in Le Clerc’s writings of the 1690s, specifically his Pentateuch annotations (1693–1696). Toubert offers an excellent guide to some of Le Clerc’s ideas in these texts, going beyond all previous accounts, although I’m still not sure that those ideas are unique enough to say that they ‘signal the birth of a new period in biblical scholarship’ (121).

In the third part of his book, Toubert turns to what he calls ‘Biblical history and antiquarianism’. His focus is on the Old Testament, and his concern with ‘the reconstruction of ancient Israelite and neighbouring societies, in particular their laws and ceremonies, in order to improve the understanding of biblical texts and their contexts’ (127). He rightly notes that the ideas produced in this period have not received the same attention as those of Scaliger, Heinsius, and Saumaise, but were often no less interesting. Many of his examples here are virtually unknown to scholarship, and so are most valuable. For example, he charts some of the arguments of Johannes Leusden, who in his *Philologus Hebraeus* (1656) defended the antiquity of certain bronze coins – and the Hebrew script in which they were inscribed – by placing them in the wider context of thoughts on the ‘character of a monetary economy, on royal monetary policy, Jewish ceremonial, and ancient history as a whole’ (128). In general, he finds such biblical antiquarianism more prominent among Cocceians: Johannes Alting, Johannes Braun, and Cocceius himself. Braun wrote about the garments of the High Priest; he also sought to rebut John Spencer’s famous thesis about the Egyptian origins of certain Jewish rituals and practices, not least the mysterious Urim and Thummim. Spencer had developed his argument to defend the English Church (if God had allowed the Jews to adopt indifferent rites from the Egyptians, then why could the English Church not maintain some indifferent rites after the Reformation?). Such an aim was obviously alien

to the Dutch Reformed, and Toubert charts how Braun and others sought to respond: as he shows, 'it was not the nature of Spencer's philological, historical, or antiquarian arguments as such that offended' them, but rather 'the way the Cambridge don employed them to reach conclusions that seemed contrary to what was acceptable theologically' (142). On the other hand, Arminians such as Philippus Van Limborch and Adriaan Cattenburgh accepted Spencer's findings (145). Other Dutchmen became embroiled in a debate about the architecture of the Temple of Solomon, beginning with a discussion in Cocceius's commentary on Ezekiel (1669). The arguments were blatantly theological: Cocceius denied that the building described in Ezekiel's visions (Ez. 40:2–3) had anything to do with the Temple of Solomon, but rather was 'a metaphor for the structure of salvation history' (148). This was typical of Cocceius's combination of a very theological and typological view of history with impressive philological skills. But those who were not prepared to see types everywhere were more sceptical, most importantly Campegius Vitringa, theology professor at Franeker, who reasserted the identification with Solomon's Temple influentially suggested by the Spanish Jesuit Juan Bautista Villalpando. The debate then incorporated laymen such as the bookseller Willem Goeree, whose *Mosaïze historie der Hebreeuwse kerke* (1700) happily accepted the idea that some Jewish rites had Egyptian origins.

More sophisticated lay scholars also engaged with the Bible. The most interesting of these is the physician Theodorus Janssonius van Almeloveen, who finally secured an academic post in 1697 as professor of Greek at Harderwijk. Anyone who has worked on late seventeenth-century scholarship will have come across his name (not least through his many letters), but he remains a somewhat shadowy figure in need of much more research. Toubert only mentions him briefly; rather, he focusses on the sex-maniac Adrian Beverland, an outstandingly erudite philologist who found sexual allusions in all ancient texts, whether pagan or biblical, and sought to prove that postlapsarian humanity was dominated by its sexual urge. Beverland's career and abstruse Latin works have recently been subjected to a very thorough investigation by Karen Hollewand.³⁵ Toubert does not duplicate her labours; rather, he offers a detailed investigation of a notebook, composed c. 1678–1679, in which Beverland's ideas were recorded alongside those of others, obviously by someone who knew him personally and was barely less sex-obsessed than him.³⁶ Although the notebook contains statements about

35 *The Banishment of Beverland: Sex, Sin, and Scholarship in the Seventeenth-Century Dutch Republic* (Leiden, 2019).

36 For a full account and transcript, see P. Steenbakkers, J. Toubert and J. van den Ven, 'A Clandestine Notebook (1678–1679) on Spinoza, Beverland, Politics, the Bible and sex', *Lias* 38 (2011), 225–365.

Spinoza's biblical criticism, its author seems 'not to have actually read any of his works' (168). In fact, more radical and more interesting was the author's association of Moses with political imposture: 'Moses', he wrote, 'received his laws from the Egyptians and passed them on to the Jews as though he had received them from God' (171). Unfortunately, such statements are so disconnected from any wider context that it is hard to know exactly what role they played in the author's thinking. Nonetheless, Toubert's reconstruction of the circle of lay scholars who were interested in such matters is again extremely valuable, and yet another important contribution to the history of elite lay reading of the Bible at this time, comparable to the pioneering recent work by William Poole and Kate Loveman on the same phenomenon in England.³⁷

Since this is all so fascinating, I think it is a shame that this chapter concludes with some uncorroborated statements about the putatively destructive effect of biblical antiquarianism: 'the very endeavour of reconstructing the objects, customs, and institutions of the Jewish past threatened to end up in the same quagmire of doubt that would ultimately engulf the debates on the role of reason and faith in hermeneutics' (130). This seems to stem from Toubert's desire to connect the developments he has traced to Spinoza: 'Spinoza's unconventional analysis of the nature of prophecy, and his down-to-earth reconstruction of the origin of the biblical texts, seemed to fit in perfectly with the cultural idiosyncrasies of the chosen people paraded by some antiquarians' (129, also 175). But all the evidence he has gathered points to exactly the opposite. Orthodox Reformed clerics happily practised such antiquarianism; even Beverland's note-taking friend hadn't read Spinoza. The reality revealed by Toubert's account is that Spinoza, however fascinating in himself, was at the time a minor concern, and had virtually no positive influence on the actual practice of biblical studies.

The next chapter, devoted to 'The Bible in the Political Fabric of the Dutch Republic', is to my eyes the least convincing of the book because it likewise seeks to reveal the destructive potential of biblical scholarship without having enough evidence to do so. Toubert writes that 'historical contextualisation, both of the biblical texts themselves and of the events narrated in them, made shaky the biblical foundations of an ecclesiastical-political order presumed to be immutable' (177). Yet the evidence he brings forward tends to show the contrary. Toubert claims that those whom he calls the 'dogmatists' 'fenced Scripture within protective walls, consisting of pre-formulated historical and dogmatic

37 W. Poole, *The World Makers: Scientists of the Restoration and the Search for the Origins of the Earth* (Oxford, 2010), 36; K. Loveman, 'Samuel Pepys and "Discourses touching Religion" under James II', *The English Historical Review*, 127 (2012), 46–82.

interpretations' (183), and even that 'the States' Translation attained a status almost as inviolable as the Catholic Vulgate' (!) (185). But the one example he provides testifies to the opposite: the Synod of Utrecht found an error in Joshua 13:2, and the text was eventually changed. None of this had anything to do with Spinoza, and it seems far-fetched to say that 'the outrage caused by the *Tractatus theologico-politicus* and the fuss over a single omitted word in the States' Translation are connected' (186). Throughout this chapter, Toubert somewhat caricatures the approach of the orthodox theologians whom he has labelled 'dogmatists' (and even 'zealots'). He can only do so by reading 'implicit' messages into their political writings, such as the claim that they 'implicitly deny[ed] both that the Bible as a text had come into being in historical circumstances and that to make sense of it required a profound understanding of those historical circumstances' (203). There is little evidence that anyone thought this, and Toubert himself has provided much fascinating evidence to the contrary. The chapter finishes with a section on Frederik van Leenhof, a minor cleric in Zwolle who stimulated a short controversy there in the early 1700s. Recently, scholars looking for Spinoza under every bed have tried to appropriate van Leenhof for the 'radical enlightenment'; none of this is at all convincing. Toubert's account is more sensible, and he himself admits that Van Leenhof had no 'durable legacy' (273). But even he cannot resist seeing Spinoza where there is no evidence for him, for example in Van Leenhof's emphasis on 'the incommensurability of theology and philosophy' (218). As Toubert himself noted earlier in the book, such an emphasis was common among Cocceians and many others. Like them, Van Leenhof's works very frequently offered typological readings of the Old Testament.

Toubert is back in top form in the concluding chapter, which offers a *longue durée* account of Dutch disputes about the Sabbath. At the start of the chapter, Toubert tries to forge a connection between these disputes and Spinoza, but again, without much conviction (the *Tractatus* didn't even discuss the Sabbath); the association is quickly dropped in favour of a much more interesting story. That story begins with the disputes after Dordt, in which Gomarus made a sophisticated philological argument against the universality of the Fourth Commandment. The puritanisation of the Dutch Church in the 1650s, above all by Voetius and his allies, generated a backlash, which was in turn opposed by Cocceius and Abraham Heidanus. As Toubert notes, erudition was deployed by both sides: 'advocates and opponents of the morality of Sunday rest bombarded each other with historical and linguistic arguments' (248). In turn, he is able to provide fascinating insights about the way in which new scholarship was integrated into the disputes. Arminians such as Van Limborch and Le Clerc eagerly incorporated Spencer's ideas about the historical contingency of Jewish

rites into their anti-Sabbatarianism. This was a step that the Reformed were not prepared to take, although again Toubert is perhaps too ready to dismiss them as simple 'dogmatists': Hermann Witsius was certainly an able scholar and his advice on the self-formation of a theologian (not noted by Toubert) sounds squarely 'scripturarian' on Toubert's own terms.³⁸ As he himself concludes, 'neither side had a monopoly on philological scholarship, in the sense of textual, linguistic, and historical criticism and contextualisation' (264). Here as elsewhere, Toubert provides precious evidence of lay engagement with academic biblical scholarship. It is very much to be hoped that not only scholars of the Dutch Republic but anyone interested in European lay religion in this period read Toubert's account and thinks about its potential implications.

Toubert's conclusion evinces the same duality that characterises the rest of the book. On the one hand, he recognises that the copious evidence he has collected – much of it new – suggests that philological and historical approaches to the Bible were pervasive among all ideological factions, even if some were sometimes more likely to deploy them. On the other hand, he still characterises the orthodox as 'inclined towards rigidity' (274), and still insists that historical and antiquarian studies had an intrinsically destructive dimension (e.g., they 'threatened the status of the Old Testament Jews as a divinely elected people' (269)) that somehow brought them into contact with Spinozism. I think that the pressures of Dutch historiography may here offer an explanation: so great is the expectation of discovering secularising tendencies in the Dutch 'golden age', or of foregrounding Spinoza, that even evidence that points in the opposite direction becomes redeployed for that purpose. If anything, Toubert's brilliantly wide-ranging analysis captures something of a comparative decline in biblical scholarship in the Dutch Republic in the second half of the seventeenth century. At least when it came to textual criticism, the centres of gravity

38 Hermann Witsius, *Oratio inauguralis, exhibens speciem veri ac sinceri Theologi* (Franeker, 1675), 18: 'In arcanis scripturarum libris, neque uspiam nunc alibi, sacratoris sapientiae secreta panduntur'. Soon after (20), Witsius goes on: 'Dici non potest quantum nobis praepostera illa studiorum, quae nimis eheu! invaluit, methodo obsimus, quotiescunque ex humanis scriptis conceptus formamus de rebus divinis, quibus postmodum firmandis Scripturae aliquot loca inquirimus... quum ex ipsis immediate Scripturis ideas divinarum veritatum haurire debeamus, neque aliter humanis scriptis uti, quam ut indicibus, ea nobis loca ad singula Theologiae capita monstrantibus, ex quibus de sententia Domini erudiamur'. This is worth translating: 'I cannot express enough how much we hurt ourselves by that wrong-headed method of study – which, alas!, has grown so strong – which leads us to form our concepts of divine things from human writings, which we then support by searching for Scriptural *loci*. We should rather draw our ideas about divine truths immediately from Scripture itself, and use human writings only as signs showing us the places the individual headings of theology from which we can be taught the decrees of the Lord'.

seem to have moved to England and France. Scholarship remained a central tool of theological dispute, but the newest ideas were often imported from abroad. As per my previous comments, this may have been at least in part the result of the specific direction taken by Dutch confessionalisation, namely the success of an especially precisian form of theology.

In part to address this exact issue, I would have very much liked to hear more about the aforementioned Campegius Vitringa, who only gets a tiny walk-on part. In 1714–1720, after thirty years of work, Vitringa, a professor of theology at Franeker who until very recently had not received the historiographical attention his contemporary importance warrants, published his 1700-page (in folio!) *Commentarius in Librum Prophetiarum Jesaiae*. The book was undoubtedly one of the foremost works of biblical criticism published in Europe at the turn of the eighteenth century – Le Clerc was only one of many who admired it – and Utrecht tried to lure Vitringa with a Scaliger-esque salary.³⁹ In this regard, Vitringa and his book seem to form a bridge between Cocceius and the great Dutch biblical scholars of the eighteenth century, not least his Franeker colleague Albert Schultens, as well as Herman Venema (the latter in particular is virtually unstudied, despite being probably the leading Dutch scholar-theologian of the eighteenth century). Vitringa was a perfectly orthodox Reformed divine – he sought to strike a balance between what he saw as the typological excesses of Cocceius and the over-minimalism of Grotius. No one reading his book could claim that biblical philology had been ‘emancipated’ from theology. And yet, it deployed the full range of sophisticated philological techniques, sometimes emending the Masoretic text on the basis of the Septuagint, as well as placing Isaiah in historical context.⁴⁰

The reality seems to be that in the Dutch Republic as elsewhere, theology and orthodoxy were not intrinsically opposed to philology (but only ever to specific philological conclusions), and theologians were not particularly threatened by scholarship, but rather helped to promote it. However much our own secular-liberal impulse may be to sympathise with someone like Spinoza, and however fascinating the quest to figure out how he reached his idiosyncratic conclusions, the historical reality is that men like Vitringa were far more important to the history of biblical criticism than he was. It is in Vitringa and his successors – and not in Spinoza or La Peyrère – that we really find the Dutch legacy of Scaliger and Heinsius (if such a thing exists at all).

39 See now C.K. Telfer, *Wrestling with Isaiah: the Exegetical Methodology of Campegius Vitringa (1659–1722)* (Göttingen, 2016), 11–12; 67, n. 133 for Le Clerc; 25 for Utrecht’s seductions.

40 E.g. *Commentarius in Librum Prophetiarum Jesaiae*, 2 vols (Leeuwarden, 1714–20), i.83, here drawing on Samuel Bochart for an emendation; for historical context, see the examples in Telfer, *Wrestling with Isaiah*, 146–50.

These final comments give a hint of how much about early modern biblical scholarship remains to be explored, even after all the progress made by the books under review. Vtringa and Venema are not the only major individuals who deserves further study. For example, the influential English Hebraist John Lightfoot surely warrants a full investigation, not least because of his influence on the Continent and because an examination of his career would illuminate the central issue considered here: the relationship between scholarship and Reformed theology. More generally, Hardy, Van Miert, and Toubert have all pointed the way to another major area of inquiry: the scholarly background to vernacular bible translations, and to lay engagement with 'academic' biblical studies. In this regard, a fruitful dialogue may be forged with recent scholarship on what we might call the 'theological public sphere'.⁴¹ Commentaries are another genre that has not received the attention it deserves, probably because it is so difficult to characterise these sprawling texts. Finally, and most generally, we are desperately lacking synthetic studies on early modern New Testament scholarship. Each of the books discussed here focus on the Old Testament (although Hardy and Van Miert make important findings about some New Testament studies); in this regard, they reflect recent scholarly trends. This fascination with the reception of the Hebrew Bible is understandable, since early modern Old Testament scholarship dealt with some of the most striking issues in the history of European ideas: the age of the world; the relationship between Moses, the biblical Jews, and surrounding pagans; the development of language; the textual transmission of non-Graeco-Roman texts. Nonetheless, it was the New Testament that was most important both to scholars and to all literate Christians, and if we are to investigate the changing nature of biblical 'authority' and the role scholarship played in that process, we sorely need more work on everyone between Stephanus and Griesbach.⁴²

Most importantly, I think we need full studies of the institutionalisation of various types of historico-philological approaches to the Bible. If Hardy has taught us that it is futile to posit any one early modern scholar as the instigator of 'modern' critical method, Van Miert and Toubert have shown that the process

41 E.g. J.-P. Gay, *Morales en Conflit: Théologie et Polémique au Grand Siècle, 1640–1700* (Paris, 2011); id., 'Lettres de Controverse: Religion, Publication et Espace Public en France au XVIII^e siècle', *Annales*, 68 (2013), 5–41.

42 Two pioneering recent studies are J. Krans, *Beyond what is Written: Erasmus and Beza as Conjectural Critics of the New Testament* (Leiden, 2006) and G. McDonald, *Biblical Criticism in Early Modern Europe: Erasmus, the Johannine comma, and Trinitarian Debate* (Cambridge, 2016).

of quantitative change was often a granular one. Assuming we accept these claims, then the role of institutionalised pedagogy appears more important than ever. Rather than tell linear narratives of the ‘historicisation’ of the Bible, we can pinpoint individual moments of particularly intense investment in biblical scholarship: in the Dutch Republic in the first half of the seventeenth century; England in the second; and the German lands in the eighteenth.⁴³ If this work is to be done, it will have to be done in dialogue with historians of theology, whether we like it or not. In the wake of Hardy’s book – and more generally, the work of Jean-Louis Quantin – studying early modern scholarship without a proper grounding in doctrinal theology is starting to look akin to studying medieval philosophy without knowing about scholastic theology: a futile enterprise.

With that in mind, it is worth making one final remark. In a review of Hardy’s book, Kristine Haugen – whose work on Bentley is perhaps the most important recent contribution to the history of eighteenth-century scholarship – has noted that Hardy ‘takes part in a current scholarly conversation about what separates the Renaissance from the Enlightenment in intellectual history’, and expressed her desire ‘to hear much more about’ this theme.⁴⁴ For what it’s worth, my view on this matter is straightforward: ‘enlightenment’ is a unhelpful category in this field (and perhaps all fields), and only serves to confuse the issues. The idea of a radical intellectual shift beginning with Descartes was invented by Fontenelle and other French propagandists in the eighteenth century;⁴⁵ it was then adopted by Kant, Hegel, and their various followers to serve their own propagandist agenda, from where it was smuggled into English discourse.⁴⁶ It was also used to ‘explain’ the French Revolution, primarily by its opponents. It is, in short, a historical myth developed by philosophers and politicians for self-serving reasons. As a historian of philosophy myself, I can comfortably say that I see no major impact of Cartesianism (or any other

43 I have attempted a programmatic overview in this vein in my ‘Confessionalisation and Erudition’. In the same volume, Scott Mandelbrote and I offer a case study of how institutional developments help to explain the ideas of a famous individual: ‘Becoming Heterodox in 17th-century Cambridge: the Case of Isaac Newton’, 301–94. See also J.-L. Quantin, ‘A European Geography of Patristic Scholarship in the Sixteenth and Seventeenth Centuries’, *International Journal of the Classical Tradition*, 27 (2020), 300–31.

44 *History of Humanities*, 3 (2018), 421–5, at 423.

45 H.-P. Schütt, *Die Adoption des ‘Vaters der modernen Philosophie’* (Frankfurt, 1998); D. Edelstein, *The Enlightenment: a Genealogy* (Chicago, 2010), esp. 24–30.

46 J. Schmidt, ‘Inventing the Enlightenment: anti-Jacobins, British Hegelians, and the Oxford English Dictionary’, *Journal of the History of Ideas*, 64 (2003), 421–43.

philosophy) on the practice of biblical criticism.⁴⁷ Its significance to theology was that it gave a great impetus to methodological separationism: that is to say, the idea that theology should not be practised using the tools of philosophy, a theme pushed especially by Jansenists, Cocceians, Arminians, and some others among the Reformed (e.g., Louis Tronchin in Geneva) to such an extent that by 1700 it became orthodoxy among a majority of Protestants and also many Catholics, especially in France. This left philology as the premier 'handmaiden' to theology. The leading biblical critics of the eighteenth century – men like Charles François Houbigant, Johann Jakob Wettstein, Johann Albrecht Bengel, Johann David Michaelis, Johann Salomo Semler, and Benjamin Kennicott – were all professional theologians who benefited from this separationism: none of them were in any significant way influenced by Cartesianism, Spinozism, or any other philosophical movement. In this regard, I think Grafton remains right: our intellectual histories should tell the story of the period c. 1520–1800 as a coherent whole, and not privilege the eighteenth century for artificial reasons that have their roots in mythical genealogies of European modernity.

To talk of enlightenment is to mistake a social phenomenon for an intellectual one. Eighteenth-century Europe witnessed sweeping social changes, but none of these were the direct product of academic ideas, whether in philosophy or philology. If secular rulers had wanted to restore religious persecution, they could have done so easily, however much Le Clerc, Spinoza, or anyone else screamed and shouted (they would have been very quickly silenced). The reasons that these rulers chose not to persecute – and instead hoovered up church wealth to such an extent that the institution became increasingly irrelevant – have their explanations not in intellectual history, but in political and social history. By desperately hanging on to the category of enlightenment, early modern intellectual history is now perhaps the last field of modern historiography in which naked teleology and whiggism remains acceptable. But as the books discussed here so wonderfully demonstrate, historians of early modern intellectual life already work on some of the most interesting, difficult, and exciting historical questions concerning one of the most spectacular moments in the history of knowledge. We can afford to be modest, and to stop claiming that the ideas that we study somehow changed every aspect of the world.

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47 Apart from in individual debates that touched on natural philosophy, e.g. about the meaning of Genesis. But Aristotelians had been no less ready to read their philosophy into the text by using the hermeneutic of accommodation.