REVIEWS

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Theodoricus, *De antiquitate regum Norwagiensium. On the Old Norwegian Kings*, ed. and comment. Egil Kraggerud, trans. Peter Fisher, The Institute for Comparative Research in Human Culture in Oslo, Oslo, 2018, XCVIII + 394 pp.; series B: Skrifter, 169

The edition under discussion has a long history, if not prehistory, behind it. Researchers specialising in the chronicle by Theodoricus monachus had for decades to satisfy themselves with the Gustav Storm edition from the collection *Monumenta Historica Norvegiae*. While that edition certainly satisfied the highest standards of nineteenth-century editorial work, as set by the *Monumenta Germaniae Historica*, it obviously became obsolete long ago – be it because it was based on the incomplete manuscript resource.[1] After Storm's death, two other copies were found which he had not managed to make use of. True, they do not diametrically alter our idea of Theodoricus's text, and have themselves been quite well described in the literature. The fact remains, though, that preparation of a new edition, based on all the four manuscripts, was a must – a long-discussed thing. An opportunity came in the mid-1990s with Lars Boje Mortensen's initiation of a series of Latin sources regarding the history of mediaeval Norway. The *Historia Norwegie* and the *Passio et miracula beati Olavi* was to be the responsibility of Inger Ekrem; Karen Skovgaard-Petersen was to take care about *De profectione Danorum in Hierosolymam*; Egil Kraggerud, a classical philologist from Oslo, would prepare an edition of *De antiquitate regum Norwagiensium*. As Ekrem died suddenly while working on her edition of the *Historia Norwegie*, Mortensen completed her work[2] whilst the *Passio et miracula* was published independently by Lenka Jirouškova.[3] The edition of *De profectione Danorum* is still underway;[4] Theodoricus's chronicle was published in August 2018.

[1] *Theodrici Monachi Historia de antiquitate regum Norwagiensium*, ed. by Gustav Storm, ser. Monumenta Historica Norvegiæ. Latinske kildeskrifter til Norges historie i middelalderen udgivne efter offentlig foranstaltning (Kristiania, 1880), I–XIV, 1–68.

[2] *Historia Norwegie*, ed. by Inger Ekrem, Lars Boje Mortensen, trans. Peter Fisher (Copenhagen, 2003).

[3] Lenka Jiroušková, *Der heilige Wikingerkönig Olav Haraldsson und sein hagiographisches Dossier. Text und Context der Passio Olavi (mit kritischer Edition)*, i–ii (Leiden–Boston, 2013).

[4] For the time being, the following have been published Karen Skovgaard-Petersen, 'A Journey to the Promised Land. Crusading Theology' in *Historia de profectione*

The work on the latter edition lasted for some twenty-five years, for which the editor is somewhat repenting (pp. VII ff., XII). This was a fruit-bearing time for research on the chronicle. Kraggerud managed to publish a series of contributory articles which gave a foretaste and an idea of the work being prepared (for a breakdown of these articles, see pp. 389 ff.). An English and a Czech translation of the chronicle were issued at that time, along with a Russian edition, while the undersigned managed to successfully defend his doctoral thesis on the chronicle and get it printed.[5] The edition under review, which we have finally received, ought to be considered in the thus-outlined context. Does it meet our expectations? Can it replace Gustav Storm's version of the *Monumenta* and definitely close the editorial work on the historiographical monument in question?

Let us take a closer look at the edition then. A foreword (pp. VII–XIII) and an extensive introduction open the book, introducing the chronicler's biography and the questions of any and all sorts which are usually posed in connection with source texts (pp. XV–XCVIII). The edited text is accompanied, on odd-numbered pages, by an English translation done by Peter Fisher (pp. 1–129). Then follows, as an appendix, a Book-Norwegian (*bokmål*) translation by the editor (pp. 333–78). There is an index of names of the historical figures appearing in the text (pp. 131–51), a philological commentary (pp. 153–332), a list of abbreviations and a bibliography of references (pp. 379–94), all making up approx. 500 printed pages.

Let us start these considerations with the chronicle as the basic text. The new edition could not limit itself to correcting and complementing G. Storm's edition (as Sergey Agishev did it a few years earlier; pp. XII ff., XCVII ff.). Theodoricus's text had to be presented in all its glory, in the context of today's knowledge and with the full awareness that what we deal with is a mediaeval work preserved in modern manuscript copies. Has Egil Kraggerud done a good job? Let us bear in mind that we deal with a classical philologist who boasts himself on his website with several dozen conjectures introduced in Virgil's or Horace's poems. This triggers concerns about his

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Danorum in Hierosolymam (c. 1200) (Copenhagen, 2001); *eadem*, 'Et håndskriftfund i Lübeck ca. 1620. Om den spinkle overlevering af to norske nationalklenodier', *Fund og Forskning*, xli (2002), 107–27.

[5] Theodoricus Monachus, *An Account of the Ancient History of the Norwegian Kings*, with an introduction by Peter Foote, trans. David McDougall, Ian McDougall (London, 1998); Sergey Yu. Agishev, *Teodorik Monakh i ego "Istoriya o drevnikh norvezhskikh korolyakh"* (Moskva, 2013); Vladimir P. Polách, *Historie o starých norských králích. Středověké Norsko a Skandinávie v kronice mnicha Theodorika* (České Budějovice, 2014); Rafał Rutkowski, *Norweska kronika Mnicha Teodoryka. Północna tradycja historyczna wprowadzona w nurt dziejów powszechnych (koniec XII wieku)*, Monografie FNP (Toruń, 2019).

possible intent not only to 'reconstruct' the original text by the Norwegian historiographer but also to correct his Latin according to classical standards.[6] As he remarks, the present-day editor of a mediaeval text can play the role of a modern editor, never knowing for sure whether he is facing a scribe's or the author's error. Hence, cautiousness is recommendable when making changes in the text – and, once they have to be made, they should be transparent for the reader. Let us approach it not only in terms of an exposition of the editorial rules but also as the editor's alibi.

Relative to the Storm edition, nearly eighty essential modifications were made with respect to selection of textual variants, punctuation, conjunctions and prepositions, word orders and spelling of names proper (pp. XCIV–XVI); a number of variants were selected from copies M and L, unknown to Storm (pp. XCI–XCIV). Also, the spelling and internal divisions of the text were altered. The diphthong {ae} was consistently replaced by the single letter {e}; {j} turned into {i}; the German {ß} into {ss}; and {v} was replaced by {u}. Kraggerud gives no grounds for this operation, which is regrettable since spelling gives important information on a text and the condition in which it has survived. Instead, he would explain the reasons behind the said new divisions (pp. IX, LXXXV–LXXXVIII). As has been remarked, titles of chapters were not necessarily the chronicler's and might have been inserted later on (for instance, the agnomen Hardrade only appears in the title of chapter 25, while the phrase *frater beati Olaui* appears in the text – p. LXXXV, 92). In Kraggerud's opinion, the very division into chapters is secondary, hence their numbers and titles are put in square brackets. What the editor did, is a division, following the editors of the *Historia Norwegie* and the *Passio et miracula*, into smaller paragraphs, each corresponding to, more or less, one sentence. This will certainly make it easier for future researchers and readers to deal with the text; a funny thing, though, is that Kraggerud loses himself at times in his own numbering of the paragraphs (e.g. the correct numbering is 21,15, rather than 21,14 – p. LXXV; 31.11 instead of 31.10 – p. XCIV; 32.6 rather than 32.7 – p. 329).

The chronicle is published under the name of Theodoricus; Kraggerud supposes that this form is the most appropriate of the three that have come down to us by way of manuscript tradition (the other two being Theodricus and Theodericus; pp. XVII–XXIII). He quits the traditionally accompanying title of *monachus*, which – as could be guessed before – was ex-post attributed to the chronicler (p. XXXV).

In contrast to Gustav Storm, Kraggerud took into account the title *Ecclesiastica Historia Norwagiensium*, appearing in the incipit of the prologue, in three extant manuscripts (p. 12). He considers it primary compared to the somewhat artificial *Historia de antiquitate regum Norwagiensium*. The cover and title page

[6] http://egil.kraggerud.no/home [Accessed: 15 June 2019].

feature the title *De antiquitate*, and not *Ecclesiastica Historia*, though. As the editor explains, the former title better fits the content of the work as it deals with deeds of Norwegian monarchs. However, he argues, convincingly and in concord with the undersigned's opinion, that the segment '*Historia*' was added on a secondary basis. The chronicler never calls his work an '*historia*': the terms he uses are '*schedula*' or '*series rerum gestarum*' (pp. XXIII–XXVIII). Let us add that a number of 'ecclesiastical histories' appeared in antiquity and the Middle Ages, whereas the title *Historia de antiquitate* became popular only in the modern era (examples including *Bernardini Scardeonii Historiae de urbis Patavii antiquitate*, 1559; *Nicholai Cantalupi Historiola de antiquitate et origine Universitatis Cantabrigensis*, 1721; and *Historiola controversae recens motae, de antiquitate regni Sueo*-Gothici, 1751). As Kraggerud aptly points out (p. XXVII), the title *Historia de antiquitate* is attested for the Theodoricus's work in the earliest available testimony – namely, the Lübeck library catalogue, which contains a description of the lost codex based on which the later copies were made. This is not to say that the said title also appeared in the codex mentioned there; instead, it could have been coined by adding the word *Historia* to the prologue phrase *de antiquitate regum Norwagiensium* (p. 4) – in the image of the (*Historia*) *De profectione Danorum in Hierosolymam*, known from these same codices.

The disappointing thing is that the title *De antiquitate regum Norwagiensium* is consistently translated as *On the Old Norwegian Kings* (title page), or *About the Early Kings of Norway* (pp. 13, 129). Indeed, a classical philologist would undoubtedly be aware that *antiquitas* is an adjective rather than a noun. As it seems, the title was better rendered by those before Kraggerud and Fisher, namely: *An Account of the Ancient History of the Norwegian Kings*.

In line with his earlier announcements, Kraggerud emends the quote from Boethius's *Consolatio philosophiae* appearing in the prologue (lib. II, prose VII) regarding the oblivion extending to the men whose deeds were not immortalised in chronicles: *quos nimirum, ut ait Boetius, clarissimos suis temporibus uiros scriptorum inops deleuit opinio* (p. 4). He replaces the subject *opinio* (fame), reproduced in all the copies, by *obliuio* (oblivion), as he finds it better fitting and meaningful, plus in concord with the quotation's source. Let us admit that not only does the *opinio* seem to make much sense in its specific context (otherwise, 'fame' has apparently erased a famous man) but it also has no analogy in Boethian manuscript tradition or in mediaeval historiography. It has been transmitted, though, by all the known copies of the chronicle – and this very fact should itself prevail, and the word should have been left as it was. Is Kraggerud editing and publishing the chronicle by Theodoricus, or the treaty by Boethius?

A similar remark can be put forth in respect of the information on anti-Christian works by Julian the Apostate; as the chronicler remarks, referring to St Jerome, on his expedition to Partia the emperor "sex libros contra Christum

euomuit" (p. 30, plus note 146). Comparing this piece of information with the respective Jerome's letter, we can see that the wording is identical or similar, the word order somewhat different, whereas the number of Julian's works is definitely different: seven, rather than six. Consequently, Kraggerud corrects *sex* into *septem*, ascribing the alleged error to the copyists. Again, are we dealing with an edition of Jerome's letters, or the chronicle by Theodoricus? As Kyrre Vatsend and Espen Karlsen point out, though, the number of works ascribed to Julian was fluctuating in the Middle Ages. According to Peter Damiani (*De sancta simplicitate*), the Apostate generated as many as eight.[7] Hence, there is apparently no need to correct Theodoricus at this point, while the view of the number of Julian's works was not established in the mediaeval period.

Valuable comments from the editor concern ancient pieces of verse with which Theodoricus decorate his narrative. The point is not the altered context, which Kraggerud often finds glaring: in fact, the chronicles has cut off the quotations in a manner that makes their literal comprehension problematic. This concerns, for instance, the quote from Lucan in chapter 26 (p. 96, 306 ff.); also the analysis of quotes from St Paul and Proba (p. 175 and 316 ff., resp.) appears interesting.

One would willingly say that Kraggerud goes too far when approaching a request to Archbishop Eysteinn for correcting the work: "Vestre igitur Excellentie potissimum presentem schedulam examinandam misimus, cuius peritie certissime scimus nec ad resecanda superflua quicquam afore nec beniuolentie ad ea, que recte prolata sunt, comprobanda" (p. 6). In Fisher's translation it reads: "So I have submitted this little document here for your Excellency to peruse, in preference to anyone else, for I know for sure that your skill will not fail to cut away superfluous parts nor will your generosity refuse to give approval to those sections which have been properly presented" (p. 7). Kraggerud sees two difficulties here. First, he considers whether *certissime* is an adjective or an adverb: in other words, does it describe the addressee's expertise or skill (*peritia*), or the conviction (*scimus*) with which the author expresses his attitude toward this skill? The editor believes it is an adverb (pp. 169 ff.). Second, generosity (*beniuolentia*) in the further part of the sentence is, in his opinion, a superfluous surplus that spoils the harmony of the whole thing. What Theodoricus says is he is certain that, for one thing, the hierarch has a sufficient skill to cut out what is superfluous or irrelevant in his text; for another, he would have the generosity to approve what is appropriate or relevant. However, is the skill not sufficient to perform both operations? Is generosity necessary to do the latter one? Admittedly, the sentence is somewhat verbose and complicated. How to prove, though, that Theodoricus himself did not write it down just like this? Let us take

[7] Espen Karlsen and Kyrre Vatsend, 'On Theodoricus Monachus' Use of Late Classical Authors', *Collegium Medievale*, xvi (2003), 254 ff.

a distanced look at the attempts to emend it. It is as something if Kraggerud took on the role of the archbishop and satisfied the humble historiographer's request to have his work rephrased.

Egil Kraggerud must obviously assume a position amidst the undecidable debate on the date Harald Fairhair established himself as a ruler. As it is known, the date is different in almost every copy of the manuscript and editions (p. 12, incl. notes 46–8). The suggested years are 858, 862 and 1052 – the last of them being definitely too late, emended by modern scholars into 852.[8] Contrary to the earlier researchers, Kraggerud does not seek to confront this date against any other source or the internal chronology of Theodoricus's text. What he does instead is to decompose it into consecutive decimal orders and coming to the conclusion that *octingentessimo* is better attested than *millessimo*; *quinquagessimo* than *sexagessimo*; and *secundo* compared to *octavo*. He consequently opts for the year 852, stipulating, however, that the lost Lübeck codex, which was the basis for the other copies known to us today, must have included a two-hundred years later date – as absurd one as it can be anyway (pp. 172 ff.).

The textual options are meticulously commented on, though minor flows do appear. The spelling 'Hologaland' (with a 'd', rather than 't', at the end) in chapter 31 is confirmed by the Berlin copy also, not just by the three other copies (p. 116, note 604). If the *ut* in chapter 22 was omitted in MS A, it ought to be remarked that it appears in the three other copies, rather than in all the copies in general (p. 84, note 435). Doubt is triggered by the way in which chapter 5, referring to imparting by Otto II's of secular vassals to ecclesial dignitaries, is edited. The emperor is reported to have a vision of an angel who accused him of intoxication of the Church: *uenenum abdidisti ecclesiae* (p. 22). Kraggerud rightly follows Gustav Storm who emended the verb *abdidisti*, appearing in the manuscripts, into *addidisti* (note 108, pp. 192 ff.). When however the chronicler describes Otto as "the child being almost better than his exemplary father" („de optimo parente pene melior proles", pp. 22 ff.), Egil Kraggerud neglects that the adverb *pene* was omitted in the *editio princeps* text: thus, Otto II was better than his father. (This has nothing to do with the fact that such assessment is overtly opposed to the image that emerges from the description of the emperor's actions – or, to be more precise, the

[8] Usually the second editor, Peter Frederick Suhm, is mentioned in this context; however, he was preceded by Thormod Torfæus, *Historiæ Rerum Norvegicarum*, ii (Hafniae, 1711), 72 ff.; Johann Philipp Murray, 'Abhandlung von dem ältesten Norwegischen Geschichtsschreiber dem Mönche Theodrich', *Deutsche Schriften von der Königlichen Societät der Wissenschaften zu Göttingen*, 1 (1771), 300. They were followed by Vegard Skånland, 'The Year of King Harald Fairhair's Access to the Throne according to Theodoricus Monachus', *Symbolae Osloenses*, 41 (1966), 125–8; however, cf. Kraggerud's apt remarks (p. 173).

fruit they have yielded.) Second, in the *editio princeps*, the word *pheodatos* is followed by the parenthesis *feudatos seu vasallos*, omitted by Kraggerud.[9] This is undoubtedly a gloss from the first editor, which does not contribute much to the text itself, but the principles of modern editing make taking note of such minor moments obligatory. It is regrettable, in general, that many an erudite marginal gloss used by modern editors of the chronicle has been neglected.

In chapter 13, Theodoricus lists varied opinions on the circumstances of St Olaf's baptism, which reportedly took place either in Norway or in England, or, in Normandy. While distancing himself from any decisive resolution, the chronicler concludes that regardless of the actual place, it is certain that he was advanced in years the moment he died a martyr's death, as confirmed by credible sources: "Sed siue Rothomagi siue in Anglia baptizatus fuerit, tunc constat eum fuisse prouectioris etatis [quando martyrio coronatus est], quam illi dicunt, quibus maxime in huiusmodi credendum est" (p. 42). Storm was the first to note that the phrase "quando martyrio coronatus est" is out of place here: what did Olaf's age at the moment of his death have in common with the circumstances of his conversion? Also Kraggerud is of opinion that this is an interpolation, though he would not follow Storm's suggestion that the phrase should have been replaced with 'quando baptizaretur': this would have been a redundant addition of what is pretty clear from the sentence in question.[10] As Gudrun Lange and myself have observed,[11] there are two other such 'leaps into the future', heralding Olaf's sanctity (pp. 42, 48); another, a similar one can be found, regarding Magnus of the Orkneys (p. 120). Yet another doubt concerns the connective {c} preceding the phrase of credible sources, which appears different depending on the source (p. 42, note 216). Let us neglect the *cum* in copy M. *Quomodo* and *quemadmodum* from the Berlin copy and the *editio princeps*, respectively, only confirm Olaf's age, regardless of whether it concerned his baptism or death. Similarly to Storm, Kraggerud considers *quam* to be the proper one, signalling that Olaf was older than people considered credible in this respect. Does it make any sense? Why should the chronicler have named someone 'credible' and then correct a piece of information that this person had given?

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[9] *Commentarii Historici Duo hactenus inediti*, ed. by Bernhard Caspar Kirchmann (Amstelodami, 1684), 11.

[10] The emendation was not included by Storm in his edition but mentioned in a contributory pamphlet; see Gustav Storm, *Om Haandskrifterne af Thjodrek Munk* (Christiania, 1875), 4; cf. work reviewed, 223–5.

[11] Gudrun Lange, *Die Anfänge der isländisch-norwegischen Geschichtsschreibung* (Reykjavík, 1989), 101; Rafał Rutkowski, *'In illa terra, ubi nullus antiquitatum unquam scriptor fuerit*. Historia jako składnik tożsamości ludu cywilizowanego (na przykładzie opowieści Mnicha Teodoryka o okolicznościach chrztu Olafa Świętego)', *Studia Źródłoznawcze*, 53 (2015), 12 ff.

The sentence concerning the jealousy jarl Sven began to entertain toward his own brother includes the conjunction *ueromet*: "Paucis deinde transactis annis coepit inuidere fratri Erico Sueino, eo quod ille duas haberet partes Norwagie, ipse ueromet tertiam tantum et illam comendatam" (p. 46); what this means is: "After a few years had passed Svein began to envy Erik because he held two-thirds of Norway while he himself had only a third, and even that was in fief" (p. 47). As it seems, *ueromet* is a strange form of *uero*. According to Kraggerud, we have to do with a spoiled structure *ipse uero partem*. Is it not clear enough that the number of Norway's provinces held by both brothers is the important thing here? The earliest of the known copies gives the option reading *ipsemet uero* (note 239). Not only correct and economical but also having its analogy in chapter 26: when Mithridates had his wives and children poisoned to death, he took poison himself (*ipsemet*) – to no avail, though (p. 96). But this is a different story.

Passing on to the description of Erik's death, the chronicler says that he bled to death in England after "sibi pituitam fecisset abscidi" (pp. 46 ff.). The verb *abscido* basically means to cut (something) out, hence we could expect the sentence's object to specify the name of some anatomical organ. The point is, though, that *pituita* stands, simply, for phlegm. As we otherwise know, Erik died resulting from an operation on his uvula (called uvulotomy). Hence, Jens Hanssen was of the opinion that the Lübeck codex included an abridgement which was erroneously interpreted: "due to his phlegm, he cut his uvula out" ("<propter> pituitam <uuam>").[12] Kraggerud suggests a simpler solution: what the jarl cut himself off was actually his 'phlegm uvula' (*pituitosam uuam*, p. 46, note 240).

As to the spellings of names proper, Kraggerud did not try to unify or correct them, taking note of their inconsistency across the text and in each of the copies. This is true also for the declination of Norwegian names, which is Latin here and there and vernacular elsewhere (pp. X, LXIX–LXXXIV). However, the editor made a single exception: Wirtzlauus (Wicklauus/Wirchlauus/Wirklauus, depending on the manuscript), a king of Rus', reappears in the chronicle four times. The name points, by all indications, to Yaroslav the Wise (pp. 56, 62, 82, notes incl.). Gustav Storm consequently suggested that the name be emended into 'Iaritzlauus'.[13] As a side remark, it is astonishing that Sergey Agishev treats this form, without a comment, as if it originated from the twelfth-century chronicler, rather than a nineteenth-century publisher.[14] What is Kraggerud's position, then? He proposes the form 'Iertzlauus'.

[12] Jens S.T. Hanssen, 'Observations on Theodoricus Monachus and his History of the Old Norwegian Kings, from the End of the XII. sec.', *Symbolae Osloenses*, 24 (1945), 174 ff.

[13] *Theodrici Monachi Historia de antiquitate regum Norwagiensium*, 30, 35, 45.

[14] Agishev, 'Teodorik Monakh i ego', *Istoriya*, 254.

However, as Jens Hanssen observed dozens of years ago, attempts at emending the name Wirtzlauus bear tracks of editorial nonsense. He has found several figures bearing a similar name, appearing in Scandinavian sources.[15] There is no need, after all, to render Wirtzlauus's title into English as 'prince', while Theodoricus consistently names him a *rex* (p. 57).

A similar remark can be made with regard to the spellings of place names: there is no pressing need to rename Suoln, the venue of Olaf Tryggvason's last battle – as all the copies have it – into Suold (p. 44, note 230 incl.). I am referring to the spelling only, not touching any attempt to identify the place. The Fruar into Fitiar emendation is less controversial in this respect (p. 18, with note 84).

The chapter of the death of jarl Håkon the younger contains an explanation about Charybdis, as it was in the depths that his life came to an end. This phrase differs by copy (p. 56, incl. note 294). The version published by Gustav Storm, reading "in illam imam uoraginem" – "in this deepest whirl" – comes from two of them. The variants "in illam mari uoraginem" and "in illa in imam uoraginem" can also be found. Clearly, the text must have been not--quite-legible in this place, hence the differences in the variants. Egil Kraggerud proposes another, fourth, solution: "in illam immanem uoraginem", meaning "in this horrid [or, enormous] whirl". By the way, the latter adjective has an analogy in Paul the Deacon: "immanissimum illud barathrum" (p. 247 ff.).

The Charybdis thread develops in the subsequent chapter. Theodoricus enumerates there the authors who spoke of that sea whirl, Pliny in the first place: "De natura charybdis … antiqui scriptores reddunt huiusmodi rationem, Plinius uidelicet Secundus, Naturalis Historie uir prudens ac doctissimus, Chrysippus philosophus et multi alii" (p. 58). There is something missing, it is not being clear what Pliny's attitude to the aforementioned *Naturalis historia* was. In Storm's opinion, the word *scriptor* is missing, but if present, it would look awkward as the *scriptores antiqui* appear within the same sentence. Kraggerud believes that *author* would have been a better solution (note 298; pp. 251 ff.)

This being the case, naming Pliny a *uir* seems superfluous.

Another problem concerns Chrysippus of Soli, who is mentioned right after the author of *Naturalis historia*. Why him? Kraggerud's explanation seems overly backbreaking: while Seneca, a stoic, was antiquity's highest authority, then Chrysippus, co-founder of this same philosophical school, must have come as a 'pretty obvious' association. (Why, then, the name of the author of *Naturales quaestiones* is not mentioned there?) Or, perhaps, the name Chrysippus might have been a spoiled name of Charybdis; thus, not only would Pliny be a learned author of the *Naturalis historia* but also a 'Charybdis philosopher' (p. 252). Yet, such a suggestion is objectionable. First, would the number of titles granted to Pliny not be excessive? Second, should not Theodoricus have

[15] Hanssen, 'Observations on Theodoricus', 173 ff.

mentioned one more name after Pliny, before indicating that also *multi alii* wrote of Charybdis? And, third, attaching a specified view to Chrysippus (be it preposterous, from our standpoint) is an important piece of information on mediaeval reception of ancient philosophy.

Chapter 31 tells us of the appointment of a certain Harald as Norway's 'pseudo-king' (p. 116). One manuscript has this word in a mixed Latin-Greek form, featuring the *psi* letter (*ψeudoregem*). An analogous element can be found in chapter 18, where the word *είμαρμένην*, an equivalent of the Latin *fatum* (in vocabulary terms), is written with use of an epsilon and a iota (*eímarmenem*; p. 66, incl. note 352). Kraggerud follows here Hanssen's suggestion[16] whereby the whole *pseudo-* segment might be written with the use of Greek letters – that is, psi, epsilon, ypsilon, delta, and omicron (*ψενδοregem*).

We owe to Egil Kraggerud a significant correction concerning the editions of Storm and Agishev. Chapter 32 tells us that during his expedition to the British Isles, King Magnus the Barefoot stopped at the Orkney Islands, the territory under his rule, where he took with him (*secum*) his son Sigurd, later on to become a Norwegian ruler nicknamed the Crusader (p. 122). The problem is, G. Storm had the word *suum* printed in this very place.[17] Decades later, Vegard Skånland interpreted this as a haplography of *filium suum*.[18] Awkwardly enough, Skånland did not confront the edition's text with any of the manuscripts where the correct *secum* appears; only Gudrun Lange, and afterwards Kraggerud, noticed it.[19] However, Agishev's edition makes the situation somewhat complicated: the said passage is not commented on in it; instead, it looks different again. As we can read, Magnus took with him 'Siwardum serum' (a late Sigurd, perhaps?).[20] Let us take a look at the Berlin copy: the {c} in the preposition *secum* may at first glance look like an {r} and, possibly, hence the 'unblessed' *serum*. The *Orcades* in the very next line enables us to see the way Johannes Kirchmann would handwrite the {r} and {c} letters.[21] Did the Russian editor find the Latin neography *so* problematic? In any case, Kraggerud, as well as myself, have pointed this fact out.[22]

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[16] Jens S.T. Hanssen, 'Theodoricus Monachus and European Literature', *Symbolae Osloenses*, 27 (1949), 93.

[17] *Theodrici Monachi Historia de antiquitate regum Norwagiensium*, 64.

[18] Vegard Skånland, 'Einige Bemerkungen zu der "Historia de profectione Danorum in Hierosolymam"', Part I: 'Mandant und Verfasser', *Symbolae Osloenses*, 33 (1957), 147 ff.

[19] Gudrun Lange, *Die Anfänge der isländisch*, 184, fn. 21; work reviewed, 122, note 630.

[20] Agishev, 'Teodorik Monakh i ego', *Istoriya*, 436, 607.

[21] Berlin Staatsbibliothek, ref. no.: Ms. lat. fol. 356, 45r.

[22] Work reviewed, XCVII ff.; Rafał Rutkowski, a review of Sergey Yu. Agishev, *'Teodorik Monakh i ego "Istoriya o drevnikh norvezhskikh korolyakh"'* (Moskva, 2013), *Studia Źródłoznawcze*, lv (2017), 264.

Now, about the English translation penned by Peter Fisher, who already has to his credit the works by Saxo Grammaticus and the *Historia Norwegie*.[23] The translation of Theodoricus's work was made in the 1990s based on the Gustav Storm edition, with comments added here and there by the editor (p. IX ff.). The translation is of good literary quality; there are fragments, though, which would have been translatable in a more literal manner. Olaf Tryggvason sentenced jarl Håkon's slave to hang "in reward and for the crime (*pro premio et scelere*) against the lord, rather than "as the recompense for the atrocity" (p. 35): a distinct piece of irony from Theodoricus. A similar crime, one of cutting off a duke of Luchan's head, was committed by During, a Serb from Cosmas's chronicle – and he received another such 'donation' (*donativum*) from a Bohemian prince who told him to hang himself.[24]

Standing out in the English text is the misspelling in the title of chapter 7, where we can read '… again Olaf Tryggvason' (*sic*) instead of 'against Olaf Tryggvason'. Since the error reappears, the copy-paste rule must have been at work (pp. 9, 25). A similar typo is made in the Introduction (p. XCVII, note 72). When talking of technicalities, one of the text-related notes is printed in an over-bolded font (p. 40, note 196).

The translation of the information, drawn from Hugo of Saint Victor, about the Normans setting their sail at Lower Scythia for Galia may be open to doubt. Concerning the name *Scythia Inferior*, the chronicler tells us that Hugo "illam procul dubio uolens intelligi superiorem, quam nos Suethiam appellamus" (p. 4). How should this be understood? According to the authors of the previous English translation, Theodoricus identifies Lower Scythia with Sweden, explaining that whereas Hugo considers it to be *Lower* Scythia, it is *Upper* Scythia, as a matter of fact.[25] Could, however, the Norwegian chronicler believe that Normans originally came from Sweden, rather than Norway? The context suggests the opposite: as we can learn, there were combative men in that country (*in hac terra*) in ancient times, as is illustrated by the Hugo quote. In Peter Fisher's and Egil Kraggerud's version, the phrase should be understood thus: Theodoricus identifies the lower part of Scythia with Norway, just adding that, consequently, the land's upper part is identical with Sweden (pp. 5, 162 ff.) – the Scandinavian Peninsula being Scythia in its entirety. Such a translation allows avoiding stating that Normans descended from Sweden. However, such an argument could be questionable for philological reasons. As Kraggerud remarks, the word order is responsible: once shuffled, it makes the message clearer; it would namely read as follows: "illam, quam

[23] *Historia Norwegie*, *passim*; Saxo Grammaticus, *Gesta Danorum. The History of the Danes*, ed. by Karsten Friis-Jensen, trans. Peter Fisher, i–ii (Oxford, 2015).

[24] *Cosmae Pragensis Chronica Boemorvm*, ed. by Bertold Bretholz, MGH SrG n.s., ii, Berolini 1923, lib. I, cap. 13, 29–32.

[25] E.g. Theodoricus Monachus, *An Account of the Ancient*, 1.

nos Suethiam appellamus, procul dubio uolens intelligi superiorem". It is clear now that *illa* does not refer to the *Scythia interior*, which precedes the phrase, but to the subsequent *Scythia superior* instead.

The sentence referring to the skill of entering into and maintaining friendships by Charlemagne: "Et ne aliquod uirtutis uestigium sublimitatem eius [Karoli] preteriret, in parandis amicitiis et retinendis pene cunctos mortales anteibat" (p. 116) is not an easy-to-translate piece; its second segment is a free paraphrase of a sentence coming from a life by Einhard.[26] Problematic is its preceding rhetorical locution coming from Theodoricus himself. What is actually meant here? Is it about, following David and Ian McDougall's suggestion, missing no single manifestation of Charlemagne's virtue in the description of such marvellous a man?[27] Or perhaps, as in Peter Fisher's opinion, Charlemagne's grandeur could never renounce any manifestation of virtue (p. 117)? Lastly, as Kraggerud suggests, his brilliance ought not to overshadow the manifestations of his virtue (p. 326)? In any case, the first-mentioned solution seems to me the most convincing.

Let us briefly point that the phase appearing in Harald Bluetooth's fake matrimonial offer made to Gunnhild in order to trick her into coming to Denmark, is correctly rendered by Fisher: as the Danish king persuaded the queen, it is her (*illi*), rather than him, that ought not to seek for a husband among young men (pp. 24 ff.). The sentence tended to be misunderstood by some previous translators who linked the remark to the letter's author (p. 194 ff.).

The *more suo* phrase from Pallas's epitaph ("Filius Euandri Pallas, quem lancea Turni | Militis occidit more suo, iacet hic"; p. 70) has been rendered as "Pallas … lies here in his own fashion" (p. 71). However, as the punctuation in the text and Kraggerud's commentary (p. 268 ff.) suggest, rather than the way in which Pallas lay in his grave, the phrase refers to how he lost his life; this particular point seems ambiguous and hard to resolve, though.[28]

Peter Fisher's only severe error, not identified by Kraggerud, appears in chapter 8. As we can read, the young Olaf Tryggvason, when about to travel to Norway, took with him Bishop Sigurd and a few other of the clergy with the intent to Christianise the country. The chronicler says that it was already then that the king decided to subdue the whole of the country to Christianity so that not a single pagan remains. He actually followed Emperor Jovian in this attitude (p. 28). It is therefore astonishing that P. Fisher attributed the

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[26] *Einhardi Vita Karoli Magni*, ed. by Oswald Holder-Egger, MGH SRG in us. schol. (Hannoverae et Lipsiae, 1911), cap. 19, 24.

[27] Theodoricus Monachus, *An Account of the Ancient*, 49.

[28] The most recent article on the finding of Pallas's grave is worth noting: Ryder C. Patzuk-Russell, 'The Legend of Pallas's Tomb and its Medieval Scandinavian Transmission', *Journal of English and Germanic Philology*, cxviii, 1 (2019), 1–30.

intent to eradicate paganism to Sigurd, rather than Olaf (p. 29). Is it possible, though, that Theodoricus would have compared a bishop, rather than the king, to an ancient emperor? Very doubtful, it seems: it was the king who initiated the project, the bishop merely delivering his intents and designs (cf. p. 201). The only explanation would be that Theodoricus used an implied subject which was erroneously identified by Fisher.

Lastly, let us briefly comment on the content-oriented commentary attached to the edition in question. Kraggerud describes himself as a debtor of Peter Foote and the McDougall brothers; twenty years ago, the latter prepared an excellent English translation of the chronicle, furnished with an erudite commentary. He consequently declares that his intention is not to repeat the pieces of information they have already given (p. XII) – all the more so that their work is legally accessible on the Viking Society website.[29] Hence, one finds the size of his own commentary (almost three hundred pages!) astonishing. What do we find in it, then? In the introduction, Kraggerud persuasively, and more strongly than anyone before him, identifies the chronicler with Thorir, the Archbishop of Nidaros (d. 1214). What attests to it is the idea of a relationship between *regnum* and *sacerdotium*, emphatically expressed in the work – interpreted at times as Gelasian, and some other time as Gregorian. The question whether Theodoricus was aware of the title *rex perpetuus* as ascribed to St Olaf – which never appears in his work – is perhaps discussed too much at length (pp. XLIII–XLVII). The editor moreover discusses the sources used by Theodoricus, the spelling of proper names appearing in the chronicle (as aforementioned), and the manuscript tradition behind the work; the last aspect is discussed extremely concisely. The edition's text is followed by an extensive and interesting commentary in which Kraggerud summarises the content of the consecutive chapters, points to erudite parallels, discusses the selection and relations of the specific words used by the chronicler. He makes frequent references to the comparative material including the *Historia Norwegie*, *De profectione Danorum in Hierosolymam*, and *Passio et miracula beati Olavi*.

Several resolutions proposed by Egil Kraggerud give rise to doubts or even objections. On the other hand, though, the editor's task was not an easy one, and the way he approaches Theodoricus's text is transparent, leaving room for other scholars to make autonomous decisions. The book is meticulously edited, furnished with an erudite commentary and a very good English translation. It can be regretted that no facsimile has been provided of at least a few pages from the copies of Theodoricus's work; also, the text has not been page-numbered according to Storm, which would have facilitated mutual consulting of both edition on the reader's own. To have at hand something more ideal than the long-awaited Egil Kraggerud's edition, the

[29] http://www.vsnrweb-publications.org.uk/Text%20Series/Theodoricus.pdf [Accessed: 16 June 2019].

mediaeval manuscripts of the chronicle would need to be discovered – or, new editorial principles elaborated.

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Martin Faber, *Sarmatismus. Die politische Ideologie des polnischen Adels im 16. und 17. Jahrhundert*, Deutsches Historisches Institut Warschau, Wiesbaden, 2018, 526 pp.; series: Quellen und Studien, 35

Based on Martin Faber's habilitation thesis submitted to the University of Freiburg, Germany in 2013, the book is the first monograph of Sarmatism. Owing to the degree of substantive and methodological complications, research into the phenomenon calls for an interdisciplinary approach and extensive methodological competencies. The author formally has such competencies, mostly in the fields of philosophy, theology, and philology. He took an interest in modern history as he prepared his doctoral dissertation, under the tutelage of Wolfgang Reinhard; his interest in Polish history and culture dates back to around 2002.

The title 'Sarmatism' (*Sarmatismus*) is misleading in the case of this monograph, as the latter does not cover the phenomenon in its entirety but focuses, in line with its subtitle, on the political ideology of Polish nobility in the sixteenth and seventeenth centuries, being but an element of what is termed 'Sarmatism'. Such reduction of a complex phenomenon that encompassed multiple aspects of culture and mentality, mores and morals, and – yes, though never exclusively – a political ideology of the nobility of the Kingdom of Poland (colloquially called 'the Crown', after the Union of Lublin, 1569) is debatable as to substance though pragmatically legitimate. It basically enables one to put aside the artistic aspects of Sarmatism – while the latter tends to be approached by Polish historiographers as a variety of the Baroque and a political phenomenon in one. Faber aptly points to the imprecise use of the term 'Sarmatism' in the research of Poland's modern era; to my mind, such use is mainly based on the assumed symbiotic association between Sarmatism and Baroque, with the result that the former is primarily treated as a morals/lifestyle-related and artistic phenomenon. One example is the statement by literary historian Krzysztof Koehler, quoted by Faber: "Sarmatism is, probably, the most disputed notion of Polish Baroque".[1] The attempt at (re)arranging the notions and ideas constituent of Sarmatism and, in particular,

[1] Faber, *Sarmatismus*, 13, fn. 23.

to move away from regarding it as a Polish variety of Baroque,[2] made by the Chair of Old Polish Cultural History, Institute of History, University of Warsaw, at a conference at the Institute of Literary Research, Polish Academy of Sciences (IBL PAN) in 1994, was to no avail.[3] This is not to say that defining the term 'Sarmatism' in more precise terms is impossible at all: it *is* possible, the condition being that aesthetic criteria be dismissed in research into social manifestations and political consequences of Sarmatism.

The German Historical Institute of Warsaw recommends the book as "a **new look** at the role of Poland in the pre-Partition period and in Europe, this being the prevalent aspect in understanding the history of Poland [emphasis mine – U.A.]".[4] Martin Faber himself repeatedly stresses that no monograph on a 'Sarmatian' ideology has ever been published.[5] It can be accepted that the very publication of such a monograph is a value in itself; this, however, does not directly imply its novel quality in terms of methodology and substantial content.

Defining the purpose of his research even more precisely, Faber critically evaluates the studies on Sarmatism's ideology penned by Polish scholars, accusing them (and he is largely right in doing so) that they tend to limit themselves to describing a 'Sarmatian' (noble) ideology while neglecting in-depth analysis of internal connections between its constitutive elements. He argues in the introductory section that ideology can be vital in understanding Sarmatism and its culture; this is not to say that it would exhaust the problem. If no Polish historian has yet decided to write a synthetic study (rather than a monograph) of Sarmatism, this is not because of lack of knowledge or negligence, but it results from the awareness of how thematically and methodologically complex such research problem would be. It would namely extend to the mentality, mores and morals, aesthetic tastes and a specific understanding of the Polish-Lithuanian Commonwealth as a community of '*panowie bracia*' (noblemen equal to one another), and the need to apply a methodology drawing upon diverse humanistic areas, from history of art and history of ideas to political science.

Martin Faber notices that one of the reasons that no monograph on the Sarmatian ideology has yet been written is that in-depth work with historical records is indispensable for the compilation of such a study (p. 25). His observation that existence of a nobility's (Sarmatian) ideology is often treated by scholars as an undeniable fact, without referring to source-based examples

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[2] *Ibid.*, 13, fn. 13.

[3] The discussion referred to by Faber based on a publication in the Polish humanities quarterly *Ogród*, vii, 4 (1994), 48–107.

[4] https://www.dhi.waw.pl/pl/aktualnosci/detail/news/nowa-publikacja-sarmatismus-die-politische-ideologie-des-polnischen-adels-im-16-und-17-jahrhunde.html [Accessed: 29 Jan. 2019].

[5] Faber, *Sarmatismus*, 7, 154, 197, 222, 266, 267, 268, 399, 404.

of how it actually functioned in the period's society, is apt. Moreover, Polish historians have often tended to refer to manifestations of ideology when discussing other issues – primarily, crucial political and military events. Hence, Faber had to do preparatory work for studying the Sarmatian ideology based on documents and narrative texts from a two-hundred-year period. This is, doubtlessly, this author's contribution to the research on Sarmatism, though his interpretation of the sources is frequently disputable owing to a relational approach, without a more in-depth interpretation. For its most part, the bibliography points to printed sources, which are easier to find and read as compared to manuscripts, of which merely eight are mentioned (one of the Polish Academy of Sciences' Library in Kórnik, five from Princes Czartoryski Library in Cracow, one from the Ossoliński Library in Wrocław, and one collection from the University Library in Vilnius). A critical assessment of the output of Polish historiographers should have been confronted against the achievements of younger-generation scholars who have taken an interest in handwritten materials, especially handwritten chronicles of the nobles, which indeed form a new source for research in Polish nobility's ideology and mentality.[6]

Let us note that the book under review was written not on the basis of research done from scratch; instead, it is prevalently based on the existing historiography. As far as German historiography is concerned, there is the habilitation thesis by Hans-Jürgen Bömelburg, which had presented the issue of Sarmatism in a broader East-Central European historical and historiographical context.[7] Faber unsatisfactorily refers to the findings of this scholar – Bömelburg's publications on Sarmatism are only quoted or referred to in the notes. Other works on Sarmatism are treated in much the same manner – referred to, or even extensively cited, in notes. Their names are only mentioned in the index if they appear in the core text. Contrary to the accepted editorial standards, they are neglected if referred to or cited in the notes, which blurs the importance of the studies used by the author.

Martin Faber's monograph is challenging to review owing not only to its enormous volume (526 pages, incl. 71 of itemised bibliography) but also an overly dismembered narrative. Such a structure was probably meant to consistently and comprehensively explain the development of the Polish Sarmatism ideology by presenting the circumstances of its emergence and the ways of expressing it in various contexts. In practice, such dispersed argument makes the reading rather tricky. The core text is burdened with

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[6] For example, Joanna Orzeł, *Historia – tradycja – mit w pamięci kulturowej szlachty Rzeczypospolitej XVI–XVIII w.* (Warszawa, 2016).

[7] Hans-Jürgen Bömelburg, *Frühneuzeitliche Nationen im östlichen Europa. Das polnische Geschichtsdenken und die Reichweite einer humanistischen Nationalgeschichte (1500–1700)* (Wiesbaden, 2006).

pretty extensive notes, as is customary with German historiography; however, this makes the text at times secondary to the references and accompanying commentaries.

The book's structure is problem-based and chronological, its axis being the important events in the history of the Polish-Lithuanian Commonwealth. The author follows Stanisław Cynarski's conception of setting the consecutive stages in the development of the 'Sarmatian' ideology based on critical political events "being shocks that undermined the monotony and stability of the political and cultural life. There is no doubt that free elections, wars, religious and social struggles, particularly noble rebellions, were all such shocks".[8] Chapter II interprets the sixteenth-century executionist movement as an upheaval event; Chapter III – the first two interregna and Mikołaj Zebrzydowski's rebellion of 1606–7; Chapter V – the Khmelnytsky uprising and the elections of King Michael I (Michał Korybut Wiśniowiecki) and John III Sobieski.

A brief review cannot discuss in detail all the issues touched upon in such a dense text; hence I will describe the monograph in general terms, pointing to its major findings, both important and innovative as well as controversial ones. Two chapters will be discussed that directly deal with theoretical issues, namely, the idea of Sarmatism as an ideology (Chapter I) and the most important aspects of its content (Chapter IV).

Chapter I presents the principal (in the author's view) traits of the Sarmatian ideology and their interpretations in the existing historiography, which gives him a background for explaining the assumptions of his own concept. In his explanation of the notion of Sarmatism and in his quest for a possibly most exact definition, Faber takes into account the contributions of Polish historiography and presents the phenomenon in a broader context of the European one. Regrettably, when summoning to search for the original meaning of the word 'Sarmatism' (p. 11, fn. 15), he shuns formulating his own opinion.

Faber reminds us of the commonly known fact that the word 'Sarmatism' appeared only in the Enlightenment to describe anachronous customs characteristic of the nobility of yore, with a negative connotation – as something opposed to modernisation; a positive connotation was added to the term, as an impersonation of patriotism, for political reasons. Before then, the term appeared in belles-lettres and oratorical pieces as well as in colloquial parlance in the adjectival form which with time became purely conventional. It is worth reminding at this point that doubts have long been expressed with respect to the uniqueness of Sarmatism as a Polish nobility-specific aesthetic and cultural phenomenon, the quality otherwise emphasised by Faber. Endre (Andreas) Agyal, not mentioned in the bibliography, pointed out to common features of the 'Slavic Baroque', which not only indicated a community of aesthetic

[8] Faber, *Sarmatismus*, 24, fn. 64.

tastes and customs but also the ideology of the East Central European nobility, with regards to mores and morals as well as 'purely' ideological aspects.[9] Janusz Tazbir, who otherwise is regarded as a propagator of Sarmatism, did not consider it as a unique product of Polish culture.[10]

I have serious doubts regarding the assumed homogeneous noble ideology across the territory of the state referred to by Martin Faber and other West European historians (Robert Frost among them) as 'Polono-Lituania',[11] which can be regarded as inspired by the phrase 'Ukraine-Rus'/Ruthenia' used by Ukrainian historiographers.[12] The author is not consistent when referring to the Grand Duchy of Lithuania: he names it a 'Polish-Lithuanian republic' (p. 19), or 'Polish-Lithuanian state' (p. 27), but when referring to the nobility of the whole country, Lithuania becomes its province (p. 31). It is erroneous to state that Grand Duchy's citizens soon grew Polonised after the Union of Lublin of 1569, adopting a 'Sarmatian' political ideology.[13] A utilitarian linguistic Polonisation in the sphere of communication inside the political elite did not necessarily mean a cultural Polonisation nor an ideological Sarmatisation.[14] Based on the recent studies by Lithuanian and Polish historians who emphasise a difference in the genetic lineage of Lithuanians as direct descendants of the Romans, as reflected in Lithuanian and Old Byelorussian chronicles from the fourteenth century onwards, it follows that the hierarchy of ideological values of the two nations of the Commonwealth was different.[15]

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[9] Endre Angyal, *Die slawische Barockwelt* (Leipzig, 1961).

[10] Janusz Tazbir, 'Synkretyzm a kultura sarmacka', *Teksty*, 4 (1974), 43–57; *id.*, *Kultura szlachecka. Rozkwit – upadek – relikty* (Warszawa, 1983), 12.

[11] Faber, *Sarmatismus*, 11, fn. 15.

[12] For instance, Natalya Jakovenko, *Druga strona lustra: z historii wyobrażeń i idei na Ukrainie XV–XVII wieku*, trans. Katarzyna Kotyńska; ed. by Teresa Chynczewska-Hennel (Warszawa, 2010).

[13] Faber, *Sarmatismus*, 49.

[14] Faber, S*armatismus*, 107, fn. 173.

[15] Darius Kuolys, *Asmuo, tauta, valstybė Lietuvos Didžiosios Kunigaikštystės istorinėje literatūroje: Renesansas, Barokas* (Vilnius, 1992); Artūras Vasiliauskas, 'Antika ir Sarmatizmas', in Vytautas Ališauskas, Liudas Jovaiša, Mindaugas Paknys, Rimvidas Petrauskas, and Eligijus Raila (eds), *Lietuvos Didžiosios kunigaikštijos kultūra. Tyrinėjimai ir vaizdai* (Vilnius, 2001), 13–31; *Lietuvos istorija*, iv: Jūratė Kiaupienė and Rimvidas Petrauskas, *Nauji horizontai: dinastija, visuomenė, valstybė. Lietuvos Didžioji Kunigaikštystė 1386–1529 m*. (Vilnius, 2009), 489–90; *Lietuvos istorija*, v, 580–1; Jan Jurkiewicz, *Od Palemona do Giedymina*: *Wczesnonowożytne wyobrażenia o początkach Litwy*, Part I: 'W kręgu latopisów litewskich' (Poznań, 2013). The best known such case is the *Bykhovets Chronicle*, being a version of the *Chronicle of the Grand Duchy of Lithuania and Samogitia* written down in the 16th c. by an Orthodox Ruthenian author, probably on commission of the Gasztołd family and their relative families of Holszański and Olelkowicz-Słucki.

The cultural and political consequences of the early adoption of the Latin option in the sixteenth century for Grand Duchy's residents before the Union of Lublin were recently discussed by Jūrate Kiaupienė in her book *Between Rome and Byzantium*.[16] Social consolidation of noble citizens of both states forming the Polish-Lithuanian federation essentially came to an end only in the eighteenth century, whereas the sense of cultural and political distinctness of the Lithuanians (called 'Lithuanian separatism' in the earlier historiography) denies the argument of their fast Polonisation, vel 'Sarmatisation', shortly after the 1569 Union of Lublin.[17]

Faber's hunting out for the origins of a Sarmatian ideology in the 1530s is arguable. While it is legitimate with respect to a liberation ideology, is a 'Sarmatian' one really the case? In giving grounds for the dating, he argues that the executionist movement was the main driver behind the formation of a political ideology. During the first Interregnum, Polish nobles and secular senators taught themselves how to communicate and collaborate. The new awareness of the nobles developed in parallel with its altered position in the state and their gained sovereign status. This created an environment that, referring to the Sarmatian genetic myth created by historiographers, gave Sarmatism a new shape and a new communication/ideological function. It would be proved by the fact that canvassing pamphlets and leaflets from 1572–3 revealed all the main elements of Sarmatian ideology, which in the later years were extended and taken advantage of for political purposes. 'Polish freedom' excelled among them, Faber believes.[18] There is nothing new in this statement, for freedom, or liberty, has long been recognised by Polish historiographers as a fundamental feature of the nobility's ideology.[19] However, justification of this credibility, be it in respect of the Ruthenian voivodeships incorporated in the Crown based on the Union of Lublin, would require the use of dietine records, which are more authoritative that the propagandist texts written in Polish. Moreover, the argument is certainly not applicable to the nobility of the Grand Duchy of Lithuania, where the impact of the executionist movement was minimal, owing to a different social and economic structure.

Chapter IV – 'Inhalte der Ideologie', especially its first part entitled 'Gesellschaftliche Voraussetzungen des Sarmatismus', quite extensive as it is (pp. 159–359), forms the monograph's most important segment, as far as

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[16] Jūratė Kiaupienė, *Between Rome and Byzantium: The Golden Age of the Grand Duchy of Lithuania's Political Culture. From the Second Half of the Fifteenth Century to the First Half of the Seventeenth Century*, trans. Jayde Will (Boston, 2020).

[17] Faber, *Sarmatismus*, 107, fn. 173.

[18] *Ibid.*, chap. III, 67–157.

[19] Recently, Anna Grześkowiak-Krwawicz, *Regina libertas: wolność w polskiej myśli politycznej XVIII wieku* (Gdańsk, 2006).

the problem in question is concerned. Since its Chapter expands on the issues touched upon in the previous three chapters, it contains some repetitions, as in the fragment of the origins of nobles' freedom and privileges (pp. 173–4).

Faber assumes that Sarmatism, once stabilised as an ideological system, persisted almost unaltered over two hundred years. In line with the narrative pattern adopted for the whole monograph, the argument begins with a survey of the historiographers' stances and opinions, spanning from the *Annales* School and the *longue durée* concept to the 'new intellectual history' and a survey of Polish studies by historians of ideas. The fact that scholars specialising in research on the second half of the seventeenth and the eighteenth centuries prevail among them is significant (pp. 159–65). The difference between the realities and the rhetoric is demonstrated on the example of Polish peasants subservience to the nobility and restrictions to the ennoblement of burghers (pp. 164–9) who favoured the illegal penetration of plebeians into the noble estate; the noblemen's dependence upon the magnates and the consequences of clientelism in the parliamentary activities (diet and dietines) are shown, among other minor aspects (pp. 169–71).

Too frequently Faber exceeds the chronological framework indicated in the title, as (for instance) when he discusses the oligarchisation of the Commonwealth's political life and degeneration of republican values on an ex-post basis, from the standpoint of the Wettin-rule period (pp. 232–3, 241–2) or even the age of Stanislaus Augustus's reign (pp. 236–7). As it seems, it is such a reversed standpoint that leads to a negation of the 'noble equality' – as an ideological and ethical value rather than social pragmatics – in as early as the former half of the seventeenth century: an illegitimate view, to my mind. Faber reduces equality (pp. 194–6), legalism, termed by him 'fictitious' (pp. 236–7), and the popular 'rule of the law' phrase, to a rhetorical convention used instrumentally for propagandist purposes. The phrase and its use was not something peculiar to Polish realities but was used in anti-absolutistic propaganda in other European countries as well. He does not conclude from his own observation that a phrase of this sort first appeared in the Kingdom of Poland in the 1530s, under the rule of the hereditary House of Jagiellon and its use continued under the election monarchs. The question remains open whether we have to do with a (set) phrase or rather, with systemic/constitutional principles in altered political realities; what the role of freedom is (as a cliché or expression of genuine convictions shared by noble political elites); and, what the association is between the ideology of freedom and 'Sarmatism'?

In any case, highlighting the discrepancies between the declared values and their actual observance is nothing new. The difference between ideological principles and socio-political realities is, likewise, obvious and timeless: apart from Poland-Lithuania, it appeared and continually appears in other European countries too. In this context, an interesting thing is the discussion between

Michael Müller and Robert Frost, recalled in the subchapter on the so-called magnate oligarchy (*Magnatenoligarchie?*, pp. 177–89), on instability and lack of solidarity among the magnates in respect of politics (pp. 180–1), contrary to their shared interests and familial connections across the Polish-Lithuanian Commonwealth, which would allow to treat this particular social stratum (following Henryk Litwin's view) as a separate estate (i.e. class).

Faber rightly observes the unilateral submission of the nobles to the magnates, along with the interdependence of these two strata, rooted in the pragmatics of coexistence. The indispensability of magnate patronage in Poland-Lithuania ensued, *inter alia*, from the social and legal specificity of the Grand Duchy, a factor that the study neglects.[20] It can be regretted that the author did not attempt to compare the patronages in Poland and Lithuania: this phenomenon, apparently identical, had different consequences in these two countries, due to their different legal systems, methods of administering, and dependencies between owning land and enjoying full civic rights.

Then, the idea of freedom is broadly discussed,[21] treated (as mentioned above) as the greatest good and supreme value of the nobility's political ('Sarmatian') ideology, one that expressed itself in specific rituals, parliamentary rhetoric, and propaganda (pp. 216–20). Faber points to a duality in conceptualising the origins of this fundamental value of noble ideology as a royal bestowal (as in Jan Długosz and Marcin Kromer) or as originated in nature. The latter concept was adhered to by the nobility, a group that Faber perceives (after H.J. Bömelburg) as the main carrier of liberation ideology (p. 196). He stresses the association between political and religious freedom, the 'Polish toleration' added to it (p. 225), noticing the other side of the coin as well: the *liberum veto* privilege was perceived by the contemporaries (as well as by Faber himself) as an extreme manifestation of egoistic self-interest (pp. 226–36). Following Henryk Olszewski[22] and Emanuel Rostworowski,[23] he points to a formal similarity between the nobility's freedom rhetoric to the West European liberal rhetoric. The obvious reflection that the similarity was external, whereas assignation of pre-liberal traits to it is discussable has been left for the reader to consider, without the author's commentary. This is typical of the monograph indeed: rather than formulating his own substantive opinions, Faber makes critically references to the literature. In this particular case, he takes note of Bömelburg's apt observation whereby the 'transitory' elitist culture of the seventeenth and early eighteenth centuries

[20] Urszula Augustyniak, 'Specyfika patronatu magnackiego w Wielkim Księstwie Litewskim w XVII wieku. Problemy badawcze', *Kwartalnik Historyczny*, cix, 1 (2002), 97–110.

[21] Faber, *Sarmatismus*, 189–97.

[22] *Ibid.*, 196, fn. 175.

[23] *Ibid.*, fn. 176.

tends to be perceived as a 'Sarmatian culture' that encompassed the active plebeians – or, in fact, burghers; yet, he would not tell us whether he agrees with this concept.[24]

The subsequent subchapters discuss issues that are well described in Polish historiography; so, let us just enumerate them. Subchapter IV.2 focuses on the nobility's conservatism and objecting to changes. IV.3 deals with the role of the king in the Commonwealth's political system, as a necessary keystone of the latter and as a point of reference. The two subsequent sections (IV.3, IV.4) discuss two models of Polish nobleman: the knight (pp. 269–90) and the landowner (pp. 291–302). Subchapters IV.6 ('Gute Gesetze, schlechte Sitten') and IV.7 ('*Polonia defensa*'), the latter addressing the idea of *antemurale* and the Commonwealth's role as a 'bulwark of Christianity', deals with the two stereotypes from the noble ideology, which have been repeatedly described in the literature. Having read fragments of Chapter IV on common subjugation, one might ask whether the societies of Western monarchies were less captive and whether Polish and Lithuanian magnates were indeed unique in Europe?

Faber's characteristics of these essential elements of the nobles' ('Sarmatian') ideology is based primarily on the now-classical studies by Władysław Konopczyński and Władysław Czapliński (whose statements on an exclusively destructive character of magnates were challenged by the later studies) as well as those of Adam Kersten, who had noticed certain constructive elements of the magnates' programme. The author all too rarely offers his own interpretation of the ideology's fundamental values, their mutual relations or hierarchies, treating all the elements of the nobility's ideology, save for freedom, as equivalent.

The monograph's fifth chapter discusses the consecutive stages of transformation of the Sarmatian ideology up to the end of the seventeenth century, marked by the breakthrough events in Polish-Lithuanian history: V.1 – 'Ruhe in der Sturm – Zwischen *Rokosz* und Kosakenaufstand' (1608–48; pp. 359–64); V.2 – 'Die Entwicklung der Ideologie bis zum Ende des 17. Jahrhunderts' (pp. 364–91); V.3 – 'Der sarmatische König': the reign of King Michael I (1669–73; pp. 391–422); and, V.4 – Der unsarmatische König': the time of King John III Sobieski (1674–96).

These chapters essentially form a review of facts based on the literature. It is debatable whether the political facts were indeed directly decisive as to the changes in the nobility's mentality and ideology. In any case, the advantage of this part of the book is that, in contrast to the lengthy arguments in the preceding chapters, the author now demonstrates a skill of synthetic presentation of the events concerned. The last two subchapters (V.3 and V.4), compare King Michael's reign, as a Sarmatian monarch, against King John III, who is (rightly) considered a European, rather than 'impersonation of Sarmatism' – contrary to the centuries-old historiographical tradition.

[24] *Ibid.*, 197, fn. 178.

A brief conclusion (*Schlusswort*, pp. 453–63, marked as Chapter VI) complements the monograph. The author assumes a position with respect to Polish historiographic tradition (particularly, the so-called Cracow School) which saw in the Sarmatian culture the actual genesis of the late-eighteenth-century partitions of Poland-Lithuania and ascribed self-defeating inclinations to Polish seventeenth- and eighteenth-century nobility (pp. 453–63), and, to an extent, accepts the standpoint previously criticised in the Introduction. A list of abbreviations and an extensive bibliography follow (pp. 465–525).

In sum, the proposition of a new approach toward the seemingly exhausted issue of Sarmatism through reducing it to a political ideology, as formulated in the monograph under review, will perhaps inspire Polish historians to dispute or accept such an exploratory perspective. From the content-based standpoint, the chapters discussing the political realities influencing the changes in the nobility's ('Sarmatian') ideology at the consecutive stages of its development essentially form a review of facts-based findings of Polish historians, particularly those made after the Second World War. These sections can be deemed valuable primarily to the German reader, since their Polish counterparts would find in them no original interpretative ideas. Apart from Faber's reluctance mentioned above to unambiguous formulation of his own opinions, a weak point of the monograph under review is also the assumption that the 'Sarmatian' culture and ideology were homogenous and absolutely dominant across Poland-Lithuania between the beginning of the sixteenth and the end of the seventeenth century. Unification of the attire, aesthetic tastes, customs and morals should not be taken for a collective mentality and ideology. A synthetic image of nobles' ideology in one monograph does not, perforce, reflect its diversity. The nobility in the different parts of the Commonwealth – apart from the Grand Duchy of Lithuania, which was federated with the Crown, the provinces of Royal Prussia, Ruthenia, or even Masovia should be taken into account – preserved a sense of a distinctive historical tradition and local patriotism until the unifying reforms of the Great Sejm of 1788–92. The 'Sarmatian' ideology, as depicted in Faber's monograph, approached as a cohesive system, is one-dimensional, with all its details. The assumption whereby the changes in the ideology directly reflected those in the social and/or political realities determined by the 'important events' is a simplification that does not explain the importance of Sarmatism, in all its scopes, for the gradual integration of the nobility and the plebeian social strata in Polish-Lithuanian Commonwealth.

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Morgane Labbé, *La Nationalité, une histoire de chiffres. Politique et statistiques en Europe centrale (1848–1919)*, Presses de Sciences Po, Paris, 2019, 382 pp.

The recent studies on the nation have mostly emphasised the liability of this quite specific entity: 'nation' eludes defining and blows apart interpretative patterns, whereas the scholars investigating such problems are expected to display an exceptional methodological sensitivity. Morgane Labbé, who has authored a study (under review) on the statistics and nation in Central Europe (the region being exemplified by the former territory of Polish-Lithuanian Commonwealth in the years 1848–1919), seems to go against the tide, trying to identify particular aspects of the entity/notion in question. She namely argues that 'nationality studies' researchers can encounter in the archives the whole series of figures, maps, and breakdowns for the period concerned, produced out of the statistical research conducted at the time. On the other hand, Labbé does not approach these found data as a key to determine the sizes or numerical force of the region's national/ethnic groups. On the contrary, statistics serves her as yet another model; or, putting it more emphatically, as an instrument in an identifiable political game related to (the) nation and nationalism.

Labbé is a demographer and historian with the background of the École des hautes études en sciences sociales (EHESS); she specialises in the history of populations, states, and nationalism in nineteenth- and twentieth-century Central Europe. The book under review takes a closer look at how the three partitioning powers used statistical studies in order to determine the number of members of the nations inhabiting former Poland-Lithuania. As can be expected, such actions were in nowise intended to satisfy the curiosity of these states' administrative apparatuses. On the contrary: the data acquired for statistical purposes instantly turned into instruments and arguments usable in political struggle.

Interestingly, however, the empires that occupied the Polish territory attached different purposes to such actions. In the German context, the maps which showed the distribution of the ethnic/national groups, as well as the statistical analyses, were primarily treated as tools with which to create a cohesive German nation through implementation of specifically defined mechanisms (to recall the activities of the Colonisation Commission). Austria and, then on, Austro-Hungary, mainly sought to preserve the coherence of a multination empire (p. 30). In Russia, for a change, the figures primarily served as a means for the authorities to control the population. As it however seems, statistics played a lesser role in Russia than in Prussia or Austria; for example, the only general census in the period concerned took place in 1897 (locally, some censuses were held before then, on the initiative of the local authorities). To compare, the first general census in Austrian Galicia was held in as early as 1857.

The book is arranged into four chronological sections, each subdivided into chapters and subchapters. The first section deals with visibility of the Polish nation in the statistical breakdowns compiled in the nineteenth century by the apparatuses of the three partitioning countries. A specially large portion of source material used in the proposed analysis originates from Prussia and, subsequently, the German Reich: it was this country that had general census methods developed to the most significant extent among the three partition powers, and put them into practice the most frequently. Section two shows statistics as a peculiar technology of power, as part of which the quantified communities gained visibility within the order of numbers or figures and, thereby, in politics. Of high interest are considerations on Polish counter-statistics which was used to articulate Polish independence-oriented aspirations. The third section deals with the ways and methods in which statistics were used in the Polish territory during the First World War, which was due to the necessity to plan the warfare and administer the occupied territories. Lastly, section four discusses the peace conference at which decisions were made as to the shape of post-war Europe. The author shows the role of statistical data in the diplomatic games and the designing of a new order for Europe. This last section seems to be less interpretative and more reporting-oriented than the other ones.

The primary asset of this book is an excellent idea about how to do research. Instead of one more analysis (or series of analyses) of how 'nation' is understood in Romanticist poetry, or one more interpretation of the emergence of modern nationalism at the century's end (these issues have so far been covered a great deal), a new refreshing perspective has been proposed. What is more, the definition of a research problem of this sort implied the need for international comparisons, along with a focus on the region that formed a veritable national/ethnic, cultural, and linguistic mosaic. As Labbé repeatedly points out, this mosaic posed a problem to the statisticians of the time, in particular as regards selection of the criteria underlying the decisions as to which of the groups ought to be regarded as a nation, and what it is that determines belonging to a nation. Based on the proposed analyses, statisticians were wont to apply the linguistic, religious, or ethnic criteria, occasionally combining them in diverse proportions. Besides, in some special cases – one of them being the Russian partition territory – diverse measures were applied to examination of national (self-)identification of the different social strata: the nationality of the nobility was mainly assessed through the prism of religion; the peasants were identified by use of the blended criteria of language and ethnic background (p. 45).

The chronological framework of 1848–1919 is thoroughly convincing. The former date, the year of the Spring of the Nations – a "national *and* emancipative" event (p. 12) – marks the beginning of attempts at representing nations in numerical terms. The final date, 1919, clearly refers to the Paris Peace Conference

and the Treaty of Versailles. The study is based on broad source material, primarily consisting of German-language records (incl. materials of the Prussian Statistical Bureau). Moreover, Labbé analyses the extensive (mainly, French-language) sources related to the diplomatic games of the time (used mostly in section 4). The narrative is rooted in the extensive literature; however, no reference to the now-classical studies by Miroslav Hroch on the formation of nations in East-Central Europe[1] is somewhat surprising.

The language used by the author is clear and vivid, which makes the reading smooth and comprehension easy. The book is structured logically and transparently – although, as usual, one might discuss whether certain fragments should perhaps be relocated, if not shortened or expanded; such problems and questions, however, is something every author is familiar with; as it seems, apt decisions were mostly made in this particular case. One weak point to be possibly remarked is scarce reference to the legacy literature that wrestles with the problems of emerging nations and nationalities in the nineteenth century. Labbé's study offers considerable contribution to the debates in this field, especially in respect of nation-forming processes at the threshold of modernity; hence, a more explicit expression of her own theoretical position, preferably in the final section, would have made the book even more powerful.

The study under review is a good illustration of the fact that any measure alters the object of measurement – let alone an object as sensitive as nation. It is interesting that the statistical research – though subordinate to the objectives of the modern invasive and possessive empires – eventually revealed, to a growing degree, the fact that the Polish nation did exist (p. 85). This significantly averted the prospect of complete erasure of this entity; the reverse might have been the case (considering the period's concepts of 'nation') in the late eighteenth and early nineteenth century, by means of the very fact of the decline of the Polish-Lithuanian Commonwealth. (A trace of such thinking remains in the Polish national anthem: 'the Poles' were expected to emerge only after they have 'crossed the Vistula and the Warta', this being an epitome of regained independence.)

The perspective assumed by the author, based on insight into grand state machineries, on the one hand, and diplomats and politicians who used the data and maps is interesting but may imply a vital threat: after all, as we read this book, we do not learn much about how the development of modern statistics concerning nations influenced the perception of nationality/nationalities and defining identities by social (or, ethnic/national) groups studied in such a way (including with the use of questionnaires). This particular issue

[1] In particular, the bibliography should have mentioned Miroslav Hroch's, *Social Preconditions of National Revival in Europe: A Comparative Analysis of the Social Composition of Patriotic Groups among the Smaller European Nations* (New York, 2000).

should have been more distinctly indicated, even if this had led to proposing some hypotheses or formulating further research questions.

In conclusion, the author aptly points to statistics as one of the nation-building factors (p. 361), thereby making it part of a broader landscape of the formation of modern state structures. Instead, the actual conclusion is rather cursory (pp. 361–5), which is, effectively, to its disadvantage. At this particular point, there was an opportunity for broader considerations – for instance, a comparison between the effects of statistical research done by the empires in other parts of Europe or worldwide in relation to their subordinate territories forming an ethnic mosaic. I should think that individual regions of Austro-Hungary might provide numerous examples for such considerations. The conclusion might also have been the place for a broader theoretical reflection on the mechanisms of creation of nations in the region (i.e. in Central Europe).

All in all, we have received an important and interesting book, and a coherent and well-thought-out study, which certainly fills in the gap in our existing knowledge. In spite of its minor weak points, it is recommendable to all the scholars specialising in the history of the long nineteenth century, nations and nationalism, and – as a broader concept – the emergence of modernity in Central Europe.

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Balázs Trencsényi, Michal Kopeček, Luka Lisjak Gabrijelčič, Maria Falina, Mónika Baár, and Maciej Janowski, *A History of Modern Political Thought in East Central Europe*, ii: *Negotiating Modernity in the 'Short Twentieth Century' and Beyond*, Part 1: *1918–1968*; Part 2: *1968–2018*, Oxford University Press, Oxford, 2018, 472 and 392 pp., selected bibliography, indices

This two-part history of political thought in our region of Europe follows up the volume once discussed in this periodical (*APH* 116). This fact releases me from the duty of reporting on the main methodological assumptions of this ambitious multi-author synthetic study. Let me just highlight the central thing once again: what the reader receives is a book written by as many as six authors, but nobody would ever tell who of them is responsible for which particular chapter or section. The team's effort to put into making the narrative coherent and homogenous is impressive, and in this respect, the two-part second volume is by no means inferior to the first. Actually, the differences, if any, are rather slight and based (as will be made more explicit below) on

the character of the sources concerned and, perhaps, the authors' attitude to the history they can remember, rather than on a new research strategy. Although the authors tell us at the very beginning (Part 1, p. vii) that some reviewers of volume I accused them of unsatisfactory use of relevant secondary literature in one respect or another, they defend the initially adopted method of source-based work, stressing the practical premises. A book that would seek to enter into a well-informed dialogue, let alone polemic, with multilingual histories of political thought in East-Central Europe would have had to be unforgivably voluminous, or simply, never-ending.

This last aspect is, incidentally, one of the study's really strong points. The proposed narrative stretches as far as it can, ending in the year 2018. It moreover observes continuity, emphasising the references and genetic associations of figures and concepts, which becomes particularly expressed in the final chapters, with their numerous references to the earlier-discussed traditions. What is more, the patient reader who can read the entire book (or who, in their impatience, would look at the last pages at once), will receive something like a universal formula encompassing the history of the region's political thought from the eighteenth century until the present. Here comes the quote, and I am citing it, hoping sincerely that this would not discourage anybody from reading this detective story:

> Such liminality can be depicted in terms of repeated attempts at the internationalization of the norms of political modernity, alternating with periods experiencing the implosion of the reformist emancipatory/liberal democratic system. The anti-Western turns usually occurred under the pressure of disaffected masses, left out of the benefits of political and socioeconomic modernization, whose resentment came to be articulated by cunning political entrepreneurs with anti-institutional political rhetoric. This opened the gates for the competing factions of the elites pushing for a redistribution of symbolic and material resources, seeking to remove the erstwhile champions of Westernization from their power positions. All this might well be described as a 'metahistorical' pattern of recurrent cycles of 'catching up' and 'alienation' (vol. ii, p. 326).

As may be inferred from the above quotation, the book's conclusion would not instil optimism in those attached to liberal democracy, or such who are merely accustomed to basic civic freedoms. Before, however, the authors have led the reader all the way to the cycle of historical spiral that we are experiencing at the moment, they interestingly and sure-footedly describe and organise a bottomless bunch of political directions, groups, and individual thinkers, making at times astonishing excursions to the regions of arts and sciences. The two volumes comprise a total of three parts (the middle one is split into two), of which the first analyses the interwar ideologies revolving around renewal and regeneration of society and state. Its seven chapters discuss as follows: (i) nation-state building, problems of minorities

and critics of nationalism; (ii) (debilitating) liberalism, in its versions, focused on economic and worldview questions; (iii) the Left, in its diverse trends; (iv) 'third path' ideologies, mostly assuming the form of agrarian populism; (v) the conservative revolution, fascist and fascioid currents at home; (vi) technocratic and biopolitical concepts of the 'New Man'; (vii) the Second World War. Part two, which more precisely sticks to a chronological sequence, covers: (i) the transitory period between the year 1945 and the Stalinisation of East-Central Europe; (ii) Stalinism and de-Stalinisation; (iii) attempts at reforming the communist system; (iv) consolidation and 'senile decay' of the real socialism; (v) political thought in émigré circles; and, (vii) dissident movements. Lastly, the third (and shortest) part three rearranges the material in question into three major issues, namely: (i) the transition; (ii) problems with which ECE countries struggled in the subsequent decade, mainly including constitutional debates and minority issues; and, (iii) 'culture wars' and diverse ways of parting with democracy in the last dozen-or-so years.

The material is so enormous that any attempt at summarising it would miss the point. The authors' erudition deserves all the greater respect that they usually (though not everywhere) manage to speak clearly. Similarly, as in the first volume, the literariness of style has been sacrificed on the altar of precision, which is visible in the above-quoted citation from an end section. Taking into account a textbook character of the study, this strategy is reasonable – and is only disturbed by two decisions of the authors. First, they fairly consistently neglect the event-based history, even if such facts would have been helpful in understanding the political lines in question. As a result, ideological conceptions are basically considered out of the symbols they made use of. The separatism of the Sudeten Germans is only presented based on the analysed writings of their intellectual elite; the reader is not told of the fundamental role of the 'March victims', that is, the German demonstrators who were shot dead by the Czechoslovak troops in the spring of 1919 and subsequently used in propelling the political campaign against the state. There are more such examples. (One of the very few exceptions, where the exhaustive historical context appears just-in-time is the chapter on etatism.) The authors clearly endeavour to purify the political ideas of their direct material context and the social emotions that generally accompany them. While this might be an appropriate approach, it never makes the reading any lighter, at times, merely inhibiting the understanding of the argument.

Another inconsistency appears in the final sections, particularly in the chapters on real socialism and democratic (or, undemocratic) opposition. The rule of sticking to the political thought is clearly treated in these fragments more loosely. There appear quite numerous, and fairly extensive, references to cultural, literary, cinema, popular-music and youth-subcultural texts. Scientific references are present too: sociological theories function on a similar basis as the Tartu Semiotic School, or scientific futurology. Such a measure definitely

adds colour to the story – while also evoking the question of why it is not applied in the other parts of the book, to a similar extent.

The merit of this book is that it has introduced – hopefully for good – a whole series of previously-missing links into international academic discourse. These include figures overshadowed by their students and/or continuators who have been luckier in their striving for international recognition. Among such unveiled figures are Marceli Handelsman, František Kutnar, and many, many others. The authors point to the Central European genesis of entire intellectual traditions of importance to the global twentieth-century culture. They bring into daylight, for instance, the local roots of the theory of nationalism which usually tend to be ignored by historians who use formulae edited and proposed by Hans Kohn, Karl Deutsch, Eugen Lemberg, or Ernest Gellner. The same happens with research into totalitarianism that originally dates back to interwar East-Central Europe, and even with postcolonial critique whose germs are identifiable, according to the authors, in the Ukrainian anticommunist underground right after the end of the Second World War.

Obviously, it is not the case that an East-Central European historian reading this book is much better informed than his or her Western colleagues. Rather than that, the gaps in his/her knowledge emanate somewhat elsewhere. As a standard, s/he is knowledgeable of his/her own country's intellectual traditions, rather than those of any of the adjacent countries. Also, a reader of this sort will find in this book a whole lot of astonishing and intriguing pieces of information and comparisons. For the undersigned, the passages on Bessarabia (the Soviet republic of Moldova), in both the interwar and post-war periods, were a source of recurring astonishment. Another reader may be taken by surprise by any of the innumerable threads of this book.

Complementation of the legacy image of the history of political thought by the long-neglected yet essential elements is another strong point that goes to the authors' credit. No less important is, seemingly, the arrangement of local traditions according to a new key deducted from comparative analysis. This is particularly evident in the chapter on the extreme rightist ideology, which demonstrates with analytical precision for what reasons one can speak in East-Central Europe of local fascisms, instead of looking for naming substitutes. Another example is the description of local varieties of Stalinism: they are so different that it becomes clear that Moscow had only imposed a general framework that was filled with content by the local communists. It was owing to this diversity, *inter alia*, that (as the authors argue) socialism remained intellectually vivid even after the year 1968.

With all the similarities between volume one and (two-part) volume two, certain differences between them are discernible. They are not methodical; as it seems, they attest to some exhaustion of the authors who, this time, took a more straightforward – if not overly simple, at times – path to their target compared to what they did in the first volume. Hence, probably, specific

logical or facts-related gaps (not quite serious ones, though) occurring in the argument. For example, what the reader learns about the agrarian reforms after 1918 is that they would frequently become an element of a nationality policy. In most cases, ethnically, religiously or culturally alien landowners were expropriated. Which is true, to be sure, but not completely. In interwar East-Central Europe, reforms were carried out and (even more importantly from the authors' viewpoint) debated outside any ethnic criteria as well. It would be interesting to learn more about this particular aspect – just because this fact eludes a simple ethnicity-based interpretation. It would give no lesser pleasure to read about the real (in contrast to so many others discussed in the book) autonomy of Silesia, a worthy-of-note thread that has been neglected by the authors. Inevitably, not all the personal choices made by the authors seem to be obvious. For example, why is Polish interwar sexuality progressivism represented by Tadeusz Boy-Żeleński, rather than Irena Krzywicka? The representation of feminism is, overall, one of the book's conceptual problems. The authors handle the situation by 'deploying' the feminists depending on their position on the political arena. Feminists are mostly covered in the chapter on liberalism and, to a slightly lesser extent, in the chapter dealing with the Left; they are much poorly represented in the chapter on the agrarian movement, while they appear a bit in the fragments on rightist radicals. While such a decision is entirely legitimate, specific threads of the story are in exchange getting looped.

There are not too many linguistic errors; rather light mistakes as to facts appear incidentally (the Polish 'March Constitution' was adopted in 1921, rather than 1922; p. 24). The overly synthetic depictions of some of the phenomena under analysis are somewhat annoying – probably a side effect of the character-focused narrative. Some political thinkers are 'dealt with' using a single sentence, be it compound but not saying anything in specific about their views. In such cases, the names should perhaps have been quit, rather than embedded with one-sentence definitions.

Fortunately, the inclination toward epitomising diminishes as the book nears its end: in the final sections, it goes beyond a synthetic historical study. It gets read as a sort of guide to our day and age, which helps one to understand the issues that are basically known to us and to identify the roots of the phenomena we encounter and deal with today. One hardly avoids cracking a bitter smile while reading (how topical!) the description of the essentialities of the populist politics pursued by Vladimír Mečiar, penned by Marián Leško. As he wrote in a 1996 book, "There are politicians who do not know how to lie. There are politicians who do not know how to tell the truth. There are politicians who do not see what the difference is. Mečiar belongs to the latter" (vol. ii, p. 252). The discovery of the prototype of the leading statesmen of today's world will hopefully convince those who might still be having doubts about whether it is really worthwhile to read this weighty work.

As I have mentioned, this book is not one of those emanating optimism. The perspective adopted by the six-headed author is a sober-minded analysis, and a stoical peace indeed, in the face of even the most extravagant products of the human spirit. This is not to say that no optimistic conclusions can be drawn based on it. The reviewer can think of two. First, although it might have long been doubted whether so ambitious a study would ever get compiled, the design has been successfully completed, without compromising quality. Second, as the authors seem to communicate, while we basically remain helpless with respect to the political upheavals occurring the region concerned, we are no more *intellectually* helpless.

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Joanna Hytrek-Hryciuk, *Między prywatnym a publicznym. Życie codzienne we Wrocławiu w latach 1938–1944* [Between the Private and the Public. Everyday Life in Wrocław in the Years of 1938–1944], Via Nova, Wrocław, 2019, 319 pp.

The very title: *Between the Private and the Public. Everyday Life in Wrocław in the Years of 1938–44* conveys the main message and reflects the content of this book. Thus this review will focus on its analysis and interpretation, rather than traditionally beginning with a detailed presentation of the structure and content of the monograph.

Let us start with the dates, which may seem to be shifted back by one year, taking into consideration the chronology of the stark events of the time in world history, European history, and German history. The period of 1939–45 would correspond with the duration of the Second World War, but the author's choice of dates clearly shows she does not consider it to be most relevant to the local history of Wrocław (or rather Breslau[1]) of the era. The first bombings of the city took place in August 1940, but the real airstrikes started only in spring

[1] The changing name of the city has been carefully traced by Norman Davies and Roger Moorhouse in *Microcosm; Portrait of a Central European City* (London, 2002). In the entire period presented in this book the city belonged to Germany under the name of Breslau, and thus this version will be used throughout this text despite the fact that Joanna Hytrek-Hryciuk chose to use the Polish Wrocław, the official name of the city after it became part of Poland as a result of the decisions of the Big Three conferences in Yalta and Potsdam. The Polish version was used also earlier in the Polish language, just as was the case with other European cities, and not only in Polish, e.g. London/Londyn/Londres, Brussels/Bruksela/Bruxelles/Brussel, Braunschweig/Brunswick/Brunszwik, Lwów/Lviv/Lvov/Lemberg/Leopolis.

of 1944, and even they did not immediately change Breslauers' life struggles. Following the airstrikes, people visited affected places mostly out of curiosity, to see what the ruins looked like (p. 75). Thus, the military events were not the turning point in the history of the city. In fact, a man in the street would have had difficulty choosing the turning point or identifying the changes that sparked or gave radical impetus to the chain of events that led eventually to the near-total destruction of the city, the expulsion of its civilian population, its subsequent devastation – and then repopulation and reconstruction within the framework of the Polish, not the German, state. This is exactly the process that Joanna Hytrek-Hryciuk aptly illustrates: how with the coming to power of the Nazi ideology (which was generally welcomed with enthusiasm by the citizens of Breslau, who expected development and the modernization of their city) the citizens of Breslau watched their lives change; and the changes gradually became overwhelming. They were introduced first by peaceful means, and then as the result of the war – with Poland, with Western Europe, and especially with the Soviet Union. She analyses such instances of Nazi influence as greetings in formal and informal correspondence (with or without *Heil Hitler*); scientific research (i.e. who was allowed to conduct it and on what topics); religious rituals, including traditional holidays such as Christmas or Easter (with special rationing procedures increasingly put in place on these occasions, and ultimately a Christmas without Christmas trees in 1944); transition rituals, including marriages (e.g. the question of the racial origin of a prospective spouse needed to be answered before marriage was allowed), contraception (or rather the prohibition thereof) or (in)tolerance towards homosexuals (which varied depending on whether gays or lesbians were concerned).

The author's contention, clearly reflected in the title, is that at least two crucial events which took place in 1938 marked the beginning of the changes. On the one hand, the *Kristallnacht* was an outburst of open violence, this time addressed towards the Jewish population (already persecuted earlier in various forms), and on the other hand, the Munich agreement propelled Germany directly on the path to war. In summer 1939 the Polish intelligence services could already report from Breslau on the military preparations underway for the invasion of Poland, and in September the first families had their sons sent to the front.

The author notes the impact of the war on the divergent aspects of life. Initially, the changes were not very painful, such as the shrinking of tourism and of leisure activities, or the fear – based on the experiences from the First World War – that there might be no chocolate and other sweets available in the shops as the war would progress. But gradually actual food shortages changed the habits and everyday activities of the majority of the city residents – with special cookbooks published, vegetarian days, and ersatz products introduced along with other strategies of survival, either adopted by the society or imposed by the government. Even before the airstrikes

started, people had to put special curtains in their windows, and the bulbs in the streets were replaced with blue ones as a form of protection. The approaching end of the war was marked by the evacuation of museums and other institutions. Even before the Soviet offensive reached Breslau, living in the city and doing business as usual, was no longer possible: a result of the lack of manpower due to army conscriptions; rationing cards; insufficient healthcare, especially in the face of the growing needs (soldiers evacuated from the front; cold and starving people); a general lowering of capacities in all areas of life; and ubiquitous death and the feeling of defeat with limited means to express grief and condolences. By the end of 1944, Breslau began to turn into a military stronghold, with the civilian population to be evacuated, which clearly justifies the choice of 1944 as the closing date for the book dealing with the topic of everyday life – a notion that has not been clearly defined but which by intuition includes the variety of activities undertaken by the citizenry in relation to securing their health and good standing and overall happiness and fulfilment.

In her presentation of daily life in the period covered, the author includes detailed data on the prices of various products and their changes (or stability), on meals, clothes, even stationery (for example the lack of fine thin paper and having to make use of whatever was available to write on reflected the deteriorating economic situation of the city's population); on health issues (illnesses and their treatment) and mortality; on sexual practices, including those promoted by the authorities and those forbidden but nevertheless existing (prostitutes, homosexuals); as well as sports, press, radio broadcasts, the book market, theatres and movies, childcare and toys – among others. The book contains an enormous amount of detailed findings – the result of the meticulous archival, press, and library research – which are presented in a readable and engaging manner. Hytrek-Hryciuk avoids referring to pure statistical data; her book contains no tables or charts with rows of numbers. Instead, she portrays flesh and blood characters and their experiences, such as Anna Wieczorek, who was renting an apartment with no heating (p. 81); or the 21-year-old aristocrat Fritz von Schaffgotsch who lost his life in Poland in September 1939 and had a truly aristocratic funeral, which is contrasted with the brief condolences sent to a family of an ordinary Hans (p. 115–16); or Selma Rosemann, a German woman who saved her Jewish friend (p. 202) – to name only a few. The book brings life to the story and the story to life. It engages the reader emotionally and presents everyday life from a bottom-up perspective, as it was experienced by the people of the time, who were obviously not just statistical cases. The book addresses important universal questions regarding people's reactions to the extraordinary circumstances and their choices and justifications for their actions, from the enthusiastic crowds welcoming Hitler to the perpetrators of *Kristallnacht* and the Jewish victims of this and other atrocities.

The title *Between the Private and the Public* also refers to the role of the totalitarian state, which tended to impact each and every aspect of its citizens' activities and left little if any space for the private. This can be observed in the examples already mentioned concerning matters as intimate as greetings and procreation, religious life, and medical treatment. The author pays special attention (and rightfully so) to the impact of 'the public' (mainly the Nazi ideology) on the fate of the Jewish population of Breslau. She devotes a whole chapter to this group, but the Jewish stories are also placed in the context of the other issues discussed in the book. Both methods are fully justified; first by the nature of the Holocaust, which put the Jewish population in a special position in the city; and second by the sources for re-constructing everyday life in Breslau, many of which originate from the Jewish memoirs. In the Jewish case, the presentation of individual stories works perfectly well, and a reader becomes not only informed but also involved, e.g. in the biography of Willy Cohn or the Lasker sisters, but also feels compassion towards the people who are mentioned only once and whose fates are largely unknown except for fragments documented in individual sources.

The extent to which sources form the narrative is one of the big issues of historiography as such, and one that can also be asked in the case of Hytrek-Hryciuk's book. The author clearly did her best to explore a wide variety of sources, located not only locally but also in other parts of Poland, in Israel, Germany, the USA, as well as texts available on the Internet – written, oral, iconographic; containing material created during the war and ex post. However, in the case of research on everyday life, one could probably never say that the set of sources taken into consideration is exhaustive and that nothing can change the picture, or that particular sets of sources did not over-emphasise particular aspects of research. In the case of *Between the Private and the Public*, the collections from the archive of the University of Wrocław provide insights into the local academic community to an extent unavailable for any other professional community of the city. The author is well aware of this, but nevertheless has decided to include these materials in a separate subchapter devoted to academia (p. 170–92). The readers can only praise her choice. The same can be said about the section devoted to the communist movement in Breslau, which reflects Hytrek-Hryciuk's research into a collection of documents from the Institute of Marxism-Leninism in Berlin-Lichterfelde (p. 60–2).

Another general historiographical question to be asked refers to the impact of a researcher's own background and interests on his or her research. In this case, one can ask if the space devoted to the Polish community of the city does not exceed the position it occupied in the history of the city during the period in question. The book is undoubtedly a piece of Polish historiography, which is also reflected in the Polish name of the city used in the title: it is Wrocław, not Breslau, notwithstanding the fact that it was Breslau throughout the entire

period presented. However, the Polish readers, to whom the book is primarily addressed, almost certainly share these interests and preferences with the author.

Here we come to another important feature of the monograph. Without compromising its academic rigour in terms of the archival research involved, offering the scholarly bibliography consulted (references included), and meta-narrative remarks (e.g. on the difficulties or limitations of her research), Joanna Hytrek-Hryciuk did not limit her audience to the academic community. From the choice of the publishing house through to the form of the book – the paper, the typeset, the choice of endnotes and not footnotes, and the abundance of photographs – to the vocabulary used and the narrative constructed (with emotional involvement in the individual characters and their life stories) – the book appeals to the more general public interested in the past, especially in the past of the city of Wrocław/Breslau.

The author often refers to particular places within the city and presents them in the photographs with a kind of familiarity. She uses many toponyms, especially street names, but also names of industrial objects – predominantly in German, with Polish translations provided – in an index placed in the very last pages of the book. This is yet another solution aimed at making the book both readable, accessible, and accurate.

The only aspect that somewhat lacks accuracy and raises some doubts are the photographs' captions. They do provide the necessary information on the content of a particular picture or focus viewers' attention on the necessary details to put them into the spatial and temporal context. However, the vast majority of them are referred to as coming from a private collection of the author. One may ask what this means. Did the author acquire a set of photographs from Breslau? Is she familiar with the provenance of her collection? Who took them, or each of them – when, in what circumstances, and for what purpose (private/official; for a file or for publication or exhibition)? Is the author the owner of their only copies, or are they also available elsewhere? These are only some questions that may be asked, and that could affect the interpretation of the pictures and make their re-use subject to further examination, including for other research topics. Or perhaps the formula of using the author's private possessions is justified by legal copyright issues, which in the recent years occupy the attention both of researchers and publishers?

Altogether however, Joanna Hytrek-Hryciuk's meticulous research has brought us closer to understanding the everyday life in the city of Breslau under Nazi rule. The multifaceted picture she draws contributes to Breslau's/Wrocław's political, social, economic, local, gender, Jewish, Polish, German and transnational history (and the list remains open). The form of the book's narrative makes it an excellent example of public history.

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Barbara Engelking and Jan Grabowski (eds), *Dalej jest noc. Losy Żydów w wybranych powiatach okupowanej Polski* [Night Without an End. Fate of Jews in Selected Counties of Occupied Poland], 2 vols., Stowarzyszenie Centrum Badań nad Zagładą Żydów, Warszawa, 2018, 871 + 835 pp.

Reviewing a sensational book two years after its publication has advantages and disadvantages. On the one hand much has already been said, so critiques and thoughts that have been expressed can be used; while on the other hand it is difficult to be original. This is especially true if – as in this case – the book is also politically controversial. Whenever it comes to the question of the Polish participation in the Holocaust – concerning both help for or the betrayal of Jews in hiding during the war – Poland is divided: while some see only the heroic resistance and view Poland as the Righteous among the Nations, others want to emphasise an inherent Polish anti-Semitism and crimes against Jews. In many ways, this polarization, together with its accompanying blind spots, makes it impossible to deal objectively with the topic. It almost seems as if a sober point of view is only possible for outsiders, but even they too have to free themselves from the Polish emotional debate, which has long since arrived with force in countries like Israel and the USA and is being fought out there by the supporters of the two camps.

Against this background, a new publication by the Polish Center for Holocaust Research (Centrum Badań nad Zagładą Żydów) of the Polish Academy of Sciences (PAN), clearly assigned to the "critical" camp, promises a substantial, source-saturated contribution to the micro-history of the Shoah. In the two volumes totalling 1,700 pages, researchers led by Barbara Engelking and Jan Grabowski present studies on the fate of the Jews in nine rural Polish *powiats* (counties). The numbers make it clear that this is by no means a classic anthology of smaller essays; on the contrary, the individual contributions are on average 150 pages long and have the character of small monographs. What they have in common is their questions and analytical procedures, which focus primarily on the conditions of Jewish survival – and why this was so rarely possible.

All the texts are based on a massive amount of archival sources. The authors rely on different types of documents and also evaluate the available literature. The details brought to light are impressive, mainly because the names of victims and perpetrators on the local level are mentioned to an unprecedented extent – a register of 90 pages bears witness to this. It also becomes clear that by no means can one speak of passivity on the part of the victims – on the contrary, they were active human beings with their own agendas, which of course were primarily aimed at their own survival. This played a role in particular after the big *Aktions*, the deportations to the extermination camps

of "Aktion Reinhardt". This period, between early summer 1942 and the liberation by the Red Army – which for the persecuted Jews was a liberation without ifs and buts – is the main interest of the researchers of these two volumes. As a kind of common conclusion, the editors emphasise that help for Jews took place more in the villages rather than in the small towns of Poland. They also say that two-thirds of the Jews in hiding did not live to see the end of the war: they mostly died because of denunciations or unwillingness on the part of their Polish neighbours, and not seldom directly by their hands.

It is hardly surprising that such findings have prompted criticism on the part of the "heroic" camp of Polish historiography. This criticism, especially from researchers at the Institute of National Remembrance (Instytut Pamięci Narodowej, IPN), has been as extensive as is detailed.[1] And indeed some errors can be found in this collaborative work of the Polish Center for Holocaust Research. At the same time, despite the efforts to take care and be thorough, such errors can hardly be avoided in a work totalling 1,700 pages. Even if highly specialised scholars examine every footnote and every single statement, there will still probably be things to complain about in any given research publication on the topic – here only he or she who is without guilt should be allowed to throw the first stone.

The main question seems to be what these – in the end rather marginal – corrections fundamentally mean. Research thrives on discussion, and of course, it is as legitimate as it is reasonable to correct mistakes and errors. However, the thrust here is political, because it is more about discrediting some overall statements and conclusions through criticism of details. This is a popular defamatory discourse strategy along the lines of: 'If footnote 1376 is incorrect, everything else must be wrong as well'. This procedure lends itself to being used to avoid an actual discussion of the overall findings and analysis, and usually is not even aimed at one. And this is very unfortunate, because beyond all the polemics it is well worth addressing both the procedures used and the interpretation of *Dalej jest noc* in a serious and non-political manner.[2]

[1] For 72 pages of such criticism, see Tomasz Domański, *Korekta obrazu? Refleksje źródłoznawcze wokół książki Dalej jest noc. Losy Żydów w wybranych powiatach okupowanej Polski* (Warszawa, 2019); Tomasz Roguski, 'Dalej jest noc. Losy Żydów w wybranych powiatach okupowanej Polski', *Glaukopis*, 36 (2018), 335–56; Piotr Gontarczyk, 'Między nauką a mistyfikacją, czyli o naturze piśmiennictwa prof. Jana Grabowskiego na podstawie casusu wsi Wrotnów i Międzyleś powiatu węgrowskiego', *Glaukopis*, 36 (2018), 313–23; Dawid Golik, 'Nowatorska noc. Kilka uwag na marginesie artykułu Karoliny Panz', *Zeszyty Historyczne WiN-u*, 47 (2018), 109–34.

[2] See e.g. Jacek Chrobaczyński, 'Osaczeni, samotni, bezbronni... Refleksje po lekturze książki *Dalej jest noc. Losy Żydów w wybranych powiatach okupowanej Polski*', *Res Gestae*, 6 (2018), 266–301; Karolina Koprowska, 'Nocne i dzienne historie. Doświadczenie Zagłady na polskiej prowincji (O książce *Dalej jest noc. Losy Żydów w wybranych powiatach okupowanej Polski*)', *Wielogłos*, 36 (2019), 161–74; Beth

For example, the methodological access using micro-history has received too little attention. In many respects, one would have to ask about its scope. It is, of course, legitimate to examine rural and not urban regions.[3] But what is not discussed is to what extent the selection made should be deemed representative: eight of the nine *powiats* were located on the territory of the German General Government, and one in the *Bezirk Bialystok*. It is not clear why the western Polish regions with *Warthegau*, West Prussia and East Upper Silesia remain completely excluded – and the *Kresy* of the Nazi *Reichskommissariate Ukraine* and *Ostland* mostly excluded – all the more so as they could have been telling with regard to ethnically heterogeneous areas.

Bielsk and Złoczów are at least two *powiats* which were occupied by the Soviets until 1941. The authors of the respective articles concerning these *powiats* devote a separate subchapter to this period, but the differences between these two cases and those territories that were fully German-occupied after September 1939 are not explicitly mentioned; the comparison is left to the reader, who is usually unaware of this fact. Naturally, an edited volume cannot provide a synthesis, but these open questions make it very clear why this is so painfully lacking – the classic by Czesław Madajczyk is still the most important reference and an excellent book in this regard, but it is more than 50 years old and does not take into account the Soviet occupation at all.[4]

Furthermore, what also raises doubts is the inconsistent definition of a *powiat* which mixes both the pre-war and post-war borders of these administrative units (which only in exceptional cases coincided with the new structures created by the occupiers). This has very serious implications, because the local dynamics of the Holocaust depended heavily on the German administration and police personnel, a fact clearly demonstrated already a few years ago.[5] In effect, the articles in this two-volume work construct

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Holmgren, 'Holocaust History and Jewish Heritage Preservation. Scholars and Stewards Working in PiS-Ruled Poland', *Shofar*, xxxvii, 1 (2019), 96–107.

[3] The focus on rural Poland is a continuation of earlier works by the Center with a similar collaborative approach, e.g. Barbara Engelking and Jan Grabowski (eds), *Zarys krajobrazu. Wieś polska wobec zagłady Żydów* (Warszawa, 2011).

[4] Czesław Madajczyk, *Polityka III Rzeszy w okupowanej Polsce*, 2 vols. (Warszawa, 2019) [reprint; first ed. 1967]. Another very helpful work but devoted **only** to the territory of the German General Government, is Dariusz Libionka, *Zagłada Żydów w Generalnym Gubernatorstwie* (Lublin, 2017).

[5] As regards the General Government, see the classic studies: Dieter Pohl, *Nationalsozialistische Judenverfolgung in Ostgalizien 1941–1944. Organisation und Durchführung eines staatlichen Massenverbrechens* (München, 1997); Bogdan Musiał, *Deutsche Zivilverwaltung und Judenverfolgung im Generalgouvernement. Eine Fallstudie zum Distrikt Lublin 1939–1944* (Wiesbaden, 1999); Markus Roth, *Herrenmenschen. Die deutschen Kreishauptleute im besetzten Polen – Karrierewege, Herrschaftspraxis und Nachkriegsgeschichte* (Göttingen, 2009).

new contexts, which have never existed historically. At the same time, the question of what relevance the *powiat* borders had for, e.g., Jews seeking refuge or for Poles helping them remains unanswered. This seems almost absurd when Jan Grabowski, in his text on the Węgrów county (*powiat węgrowski*), examines the Treblinka extermination camp, which in fact was and is located in the Sokołów Podlaski county (*powiat sokołowski*) – whereas the Germans had combined the two *powiats* into one joint *Kreishauptmannschaft*. The attached map shows the *powiat* borders correctly, but locates the Treblinka I labour camp in the village of the same name, even though the actual location between the hamlets of Wólka Okrąglik and Poniatowo (at that time Kutaski, as correctly labelled in the map) could easily be identified via its railroad branch, which runs (once again correctly on the map) outside the *powiat* border.

Because of these ambiguities, the given statistics and tables should be treated with caution with regard to their (geographical) completeness. There may be important reasons for the tailored case studies in terms of sources, but it makes it incomparably more difficult when trying to present the full picture, which the authors clearly strive for. Of course, all of this is more of a nuisance than a reason to doubt the key findings of this impressive work. Overall, the studies paint a very clear picture of the Jews' desperate attempts to survive. With the exception of the few courageous helpers, they were largely left on their own and surrounded by deadly threats.

In this sense, the methodological problems should not distract from the actual and highly fascinating interpretations. Here is where the real grounds for discussion lie, which is why *Dalej jest noc* does exactly what important research always does: it stimulates thinking and inspires, rather than preach pretended truths that historical science cannot deliver anyway. For instance, Joanna Tokarska-Bakir questions Barbara Engelking's findings in her study on the *powiat bielski*,[6] according to which the only people who rescued Jews during the third phase of the Holocaust in this *powiat* were peasants from villages surrounding a manor house. In fact, there exists no class history of the Holocaust, not even dating back from communist times. At the same time, however, the perspective is entirely legitimate and fruitful. But this approach should not tempt one to dismiss anti-Semitism mainly as a problem of some backward or even socially-retarded peasants from the countryside. The intelligentsia and the wealthy classes – often from the former gentry – were by no means exempt from it. It would make more sense to ask about the reasons and the form of class-specific hostility toward Jews, but this is actually a completely different research question. In any case, it would be inadmissible to make an analogy if such interpretations were projected into the present and then politically exploited. In other

[6] Joanna Tokarska-Bakir, 'Błąd pomiaru. O artykule Barbary Engelking *Powiat bielski*', *Teksty Drugie*, 5 (2018), 166–94.

words, the Holocaust is not about the current Polish government's supporters and/or the opposition.

Another aspect seems even more important to me, and it is not without irony that I formulate this as a German researcher of the Holocaust: the importance of German politics and perpetrators for the Holocaust. Of course, it is perfectly obvious to all authors of *Dalej jest noc* that the Shoah was a German crime carried out on Polish and European soil. But in the book these clear categories become blurred, the concentration on the Jews fleeing and in hiding, who were at the mercy of their Polish neighbours, obscures the fact that all this happened only because of the conditions created by Germans. In the end, probably some 99 per cent of Poles during the Second World War were neither heroes nor *szmalcowniki* (informers and murderers). In contrast, the study – polemically speaking – leaves the impression of a "Holocaust without Germans" (although none of the authors of the volume would ever claim such nonsense).[7]

On the one hand this is due to the blurred responsibilities, which would have been clearer if the articles had been oriented towards the German administrative borders; while on the other hand it is due to the focus on Polish-Jewish relations. The Germans appear primarily as murderers together with Polish blue policemen (*policja granatowa*), or as executioners alongside Polish informers, which ultimately places all these participants side by side and on an equal footing. It is, of course, worthy of merit that the work identifies for the first time these German perpetrators by name and thus shows the everyday murder of the Jews was committed by the "normal men" of the occupation – and was not just the work of the officials, commanders and desk perpetrators. But this obscures the intentions, structures and hierarchies – and diminishes the industrial scale of the mass-murder in the death camps and during the "Holocaust by bullets". This is directly related to the failure to take into account the interdependencies between the different crime complexes and victim groups, which could have applied to, e.g., the *powiat* Biłgoraj, where combating resistance and burning villages took place on a huge scale. From the point of view of the perpetrators, this was inseparable from the Holocaust, since the Jews were considered a central pillar of the resistance.

In many ways, we are dealing here with an intentionally political book that aims to have a myth-destroying effect by emphasising the Polish contribution to the genocide. But by questioning the supposed Polish uniqueness in helping Jews, it also strengthens the myth of a special Polish part in the Holocaust. However, both would only be possible to assess by comparison at the national

[7] Similar tendencies can be observed in earlier books by the two main editors: Jan Grabowski, *Judenjagd. Polowanie na Żydów 1942–1945. Studium dziejów pewnego powiatu* (Warszawa, 2011); Barbara Engelking, *Jest taki piękny słoneczny dzień… Losy Żydów szukujących ratunku na wsi polskiej 1942–1945* (Warszawa, 2011).

level. This, on the other hand, leads to a competition that is as senseless as it is macabre – with the unwanted consequence of relativising German responsibility for the murder of the European Jews. Of course, genocide on this unprecedented scale would not have been possible without the participation of the occupied and the collaborating nations. It is certainly necessary that this be said for the political education of responsible European citizens. However, the basic requirements – the perpetrators and their actions – must not be pushed into the background. In all of this, the overwhelming German responsibility must be clearly stated. Or to get back to class history: "Men make their own history, but they do not make it as they please; they do not make it under self-selected circumstances, but under the circumstances already existing, given and transmitted from the past".[8]

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Magdalena Ruta, *Without Jews? Yiddish Literature in the People's Republic of Poland on the Holocaust, Poland and Communism*, edited by Jessica Taylor-Kucia, Jagiellonian University Press, Kraków, 2017, 452 pp.; series: Studies in Jewish Civilization in Poland, 2

As we all should recall from school, one should not identify the lyrical ego in a literary text (especially one in poetic form) with its author. This is why historians are rather cautious about using literary texts as historical sources (except perhaps for autobiographical texts). But where is the border between one's autobiography and one's literary creation? And can the analysis of novels, stories, and poems – whose authors had, after all, various life experiences – help us to understand these experiences better?

The latest book by Magdalena Ruta, a literary scholar from the Jagellonian University and one of the leading Polish scholars in Yiddish literature, analyses the Yiddish literary output (both poetic and prosaic) in Poland in the 1945–68 period. As the author explains in the Preface, her research is "based on a closed body of literary output which includes both the relatively numerous texts from the interbellum and war (written in Poland and the USSR) but published (often for the first time) after liberation, and texts written in the years that followed the end of the war and published contemporaneously in the press and/or in book form. The vast majority of their authors are Jewish writers who survived the war in the USSR, but also the small group of those who survived in Poland" (p. 19). Among the authors, we can find those who

[8] Karl Marx, *The Eighteenth Brumaire of Louis Napoleon, 1852*, online at http://www.marx2mao.com/M&E/EBLB52.html [Accessed: 1 Apr. 2020].

are commonly associated with post-war Poland, such as Binem Heller, Leyb Olitski, Elye (Eliasz) Rajzman, Hadase Rubin, David Sfard or Kalman Segal. But there are also those who remained in Poland for a really short time (usually after having returned from the USSR), sometimes so short that it is hardly mentioned in their biographical notes. To name but a few, they include Chaim Grade, Reyzl Zhikhlinski (Żychliński), Avrom Sutskever and Rokhl Korn.

The book is entitled *Without Jews? Yiddish Literature in the People's Republic of Poland on the Holocaust, Poland and Communism*. Interestingly enough, in the period under consideration, there were at least two poems in Yiddish published with the very title – *On yidn* [Without Jews]. One poem, by Jacob Glatstein (Yankev Glatshteyn), talks about the Jewish God dying together with His people (p. 90). Another one, by Binem Heller – who is one of the main protagonists of Ruta's book and one of the most outstanding (if not **the** most outstanding) of the Yiddish poets in post-war Poland – talks about Jews being forgotten by the post-war world after the Holocaust (p. 200). There is, however, one fundamental difference. Both Glatstein and Heller in the titles of their poems state a fact, while Ruta adds a question mark, thus suggesting a polemic with the widespread opinion that after the Holocaust there were no longer any "authentic" Jews in Poland; that is Jews deeply attached to their Jewishness, including the Yiddish language and culture. Luckily, in the past ten years or so this opinion has been gradually called into question. And this is in great part thanks to such scholars as Magdalena Ruta.

Having said the above, one must admit that such an opinion was justified to a certain degree. The post-war Jewish community in Poland in its best – and shortest – period, i.e. in 1945–9, was subject to constant fluctuations. Memoirs from this period often use the metaphor of Poland as a railway station, where some people disembark but even more board trains in order to leave and never come back. The truth, however, is that in contrast to the interwar period, the Yiddish culture was officially recognised and supported by the Communist authorities until 1968: there was a State Yiddish Theater (run by the distinguished actress Ida Kamińska), the Yidish Bukh publishing house (which printed books in Yiddish), the Jewish Historical Institute, Yiddish periodicals, and there were also state-sponsored Jewish schools (teaching in Polish but with Yiddish and Jewish history courses in the curriculum). At the same time, the first post-war generation was rapidly assimilating to the Polish language and culture, and in the 1960s the young generation of Polish Jews – which was to be directly affected by the anti-Semitic campaign of March 1968 – was more or less fully acculturated. Despite that – or maybe precisely because of that – the post-war period in Polish Jewish history deserves a comprehensive analysis. Thus Ruta claims, with respect to the post-war Yiddish literature: "For this literature, as a testimony to an immensely dramatic period, deserves to be heard, the more so that aside from its target audience, nobody has ever really listened to it before" (p. 19).

The book's structure itself suggests a strongly autobiographical interpretation of the texts analysed. Having explained her goals in the Preface, Ruta discusses her research in five chapters. Each of them begins with an ample historical introduction (usually several pages long) based on the existing scholarship and allowing the reader to see the literary texts in their proper context. Chapter 1, 'Response to the Holocaust', deals with how the Yiddish writers reacted to the Holocaust. Most of them, in fact, were not Holocaust victims *per se* since they survived the war in the USSR – "like the brethren of Job, they were victims, but without the immediate experience of the Holocaust" (p. 40) – which made their experience different from those who survived in Nazi-occupied Poland. By analysing various texts, including, e.g. B. Heller's famous poem *In varshever geto iz khoydesh nisn* [It Is the Month of Nisan in the Warsaw Ghetto] (pp. 78–9), Ruta shows that the first post-war years were crucial in shaping the Holocaust memory and fixing a particular way of commemorating the Shoah and that the Holocaust became then and remained the central theme of post-war literary output. By discussing the differences in war experiences between those in Poland and those in the USSR, she rightly notes that many of the latter had had some previous affiliations with Communism in prewar Poland, which explains their enthusiastic reactions to the Soviet reality (let us bear in mind, however, that non-enthusiastic reactions were not possible, at least in print, and definitely dangerous) and their perception of Stalin and the Red Army as the only forces able to destroy Nazism (p. 57).

Chapter 2, 'The Image of Poles and Polish-Jewish Relations', analyses texts which are often mentioned in the context of Polish-Jewish relations right after the war, like the long poem *Tsu Poyln* [To Poland] by Avrom Sutskever, as well as texts which come across as surprising at first, such as short novels published in the spirit of Socialist Realism. The Yidish Bukh publishing house launched a series called *Bibliotek fun shlogler un ratsyonalizatorn* [Library of Champions of Labour and Rationalisers], which showed Jewish 'men of work' rebuilding the country side-by-side with their Polish fellow citizens and focusing on their productivity. These novels can be read today as a vision of a 'future Jew' – a Jewish person who is not afraid of hard physical work in a predominantly non-Jewish milieu. They can also be read as an expression of wishful thinking, as we know from other sources that many Jews, especially the less assimilated ones, still preferred to live and work together. This motive is further elaborated in Chapter 4 as well (p. 304).

Chapter 3, 'Poland as a Jewish Homeland', deals with the dual image of Poland in the Yiddish literature. On the one hand, it is the 'Jewish Poland' or a 'Jewish homeland' (the latter form referred to in the title of the chapter); while on the other hand it is 'Poland of the Jews' Polish neighbours' (p. 253), which is sometimes unrecognizable and sometimes unwelcoming after the Shoah. "I walk around my native town / and do not recognise the place", wrote poet Mendel Man after a visit to his native Płońsk in 1945 (p. 207) – and

these words from a poem are a common leitmotiv repeated in the memoirs and other sources from the period. Ruta discusses here another of the leading themes in Yiddish literature, that is, the (former) Jewish *shtetl* (p. 233), now inhabited practically only by non-Jews. She also devotes her attention to the post-war Jewish geography: "Owing to the war, most survivors had lost their private homelands, and as mature adults were building links with new places … thus new place names appear on the literary map of Poland" (p. 227), such as Wrocław, Dzierżoniów, Katowice, and Szczecin, to name but a few. While some of these cities became important also for the history of Polish post-war literature (Wrocław, Szczecin), others – like Dzierżoniów – remained important for the Jewish community only. This geographical shift, which is one of the most prominent features of the post-war Jewish community, was expressed in the Yiddish literature as well, as poet Elye Rajzman testifies in his poem *Baym Oder* [On the Oder] written in Szczecin: "I stand on your banks, gazing into the distance / As I once did by the Pripet, the river of my native region" (p. 228).

Chapter 4, 'The Identity of the Jewish Communist', aims to reconstruct the literary image of a Polish-Jewish-Communist identity and its evolution under the specific historical circumstances. In fact, it would be hard to write a book on Yiddish literature in post-war Poland without mentioning the topic of Jewish Communists (let us be precise here and note that Ruta focuses only on those who identified as Jews and Communists at the same time). Not only because many of the analysed authors (such as Sfard or Heller) were party members, but also because the main goal of the Yiddish-speaking Jewish Communists was the emergence of a new Yiddish culture: secular, progressive, and being an equal partner with the Polish culture. Ruta notes here that "detached from its religious roots, Yiddish culture was of limited attractiveness to the broad Jewish masses, which chose assimilation instead" (p. 257), and while one could agree with her statement, especially for the first half of the 1950s when Yiddish was just supposed "to provide an ethnic package for ideological content" (p. 295), one should not forget that assimilation was, at the same time, a common phenomenon in such countries as France or the United States, both with unlimited access to the more religious side of traditional Yiddish culture. Historians, as we all know, should refrain from discussing 'what if' questions, but here one really cannot help the feeling that if not for the anti-Semitic campaign of 1968 and the final massive emigration of Polish Jews, the Yiddish culture in Poland would simply slowly have died out, together with its last representatives.

In Chapter 4, Ruta shows – based on the example of the aforementioned Binem Heller – a typical path of a Jewish Communist: from firm believing in a better world (she analyses poems written by Heller in Soviet-occupied Białystok in 1939–40, p. 287) to a deep disillusionment and disappointment following the October thaw of 1956 and Khrushchev's famous speech on

Stalin's crimes. Heller's poem *Akh, hot men mayn lebn mir tsebrokhn* [Ah, My Life Has Been Broken] (p. 32–4) is the most famous response to this event, being both a confession of one's own sins of blindness and an accusation against those who kept lying for years about a better world to come.

Chapter 5, 'The Social and Cultural Identity of the Survivors', discusses literary testimonies of how the Yiddish writers dealt with their identity after the war. Here the author refers to the available sociological research (e.g. by Małgorzata Melchior). One of the leading motifs Ruta explores is the difference between personal war experiences – i.e. between those who survived in the USSR vs those who survived in Nazi-occupied Poland. Another topic she explores is that of adopting an exclusively Polish identity (often based on the Holocaust experience in Poland, when one had to hide one's Jewish identity) or a dual Polish-Jewish identity. The latter path was chosen by the writer Kalman Segal, who wrote in both Yiddish and Polish, and while all the Yiddish writers in post-war Poland were, in fact, bilingual (or even multilingual), it was probably only in Segal's case that this bilingualism and biculturalism were expressed so openly and in so many forms.

Magdalena Ruta's monograph constitutes proof of the in-depth fastidious diligence, academic honesty and research skills of the author, who has been able to interestingly analyse literary sources which to many have seemed uninteresting and cliched. Despite their literary character (which should always be kept in mind), they allow us to supplement what we know from strictly historical sources and/or to see these historical sources (such as personal memoirs) in a different light, especially when it comes to discussing the memoirist's former political engagement. My only real objection is that Ruta seems to take maybe too seriously poetic declarations of involvement and to expect consistency on the part of the authors of such texts. For example when she writes: "Only in the late 1970s did Sfard, who in 1948 had written a poem entitled *Dermonung vegn Moskve* [Reminiscence of Moscow], which in glowing words recalls the capital of the Land of Soviets and Stalin's concern that the light of the new dawn vanquish the 'gloom of the West,' reveal his own experience of what was common knowledge on the subject of the 'leader of nations' and the methods he had used" (p. 47); one feels tempted to say: yes, but isn't it normal that people change their minds? Isn't it normal that people try to play down their earlier choices if they perceive them later as embarrassing? It is perhaps a collateral damage effect of being a writer that one's earlier statements, even in a literary form, remain in print and can be used as arguments in confronting the authors with their past, while many of those who actually shared the same feelings and participated in the same events seem to evaporate from the collective consciousness simply because they have left no written testimonies of their involvement.

Having said that, I find Ruta's book to be one of the most insightful books written in recent years on the Polish Jewish post-war experience. And one

must add that together with the Polish version of this monograph (which appeared in 2012), Magdalena Ruta published a trilingual anthology *Nisht bay di taykhn fun Bovl/Nie nad rzekami Babilonu* [Not By the Rivers of Babylon],[1] which contains some of the poems analysed in her monograph and many others, thus giving a comprehensive picture of Yiddish poetry in post-war Poland. The Yiddish scholarship in Poland owes her a lot, and I am looking forward to her next academic contribution.

proofreading James Hartzell

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[1] *Nisht bay di taykhn fun Bovl. Antologye fun der yidisher poezye in nokhmilkhomedikn Poyln / Nie nad rzekami Babilonu. Antologia, poezji jidysz w powojennej Polsce*, selected, translated and prefaced by Magdalena Ruta (Kraków, 2012). The preface, footnotes and author notes are in English.