REVIEWS

Acta Poloniae Historica
112, 2015
PL ISSN 0001–6892

Radosław Kotecki and Jacek Maciejewski (eds.), *Ecclesia et Violentia: Violence against the Church and Violence within the Church in the Middle Ages*, Cambridge Scholars Publishing, Newcastle upon Tyne, 2014, 360 pp.

As has been the case with many a recently published collective-author book, also this one has been based on a research grant managed by Radosław Kotecki at the Casimir the Great University in Bydgoszcz, with an accompanying international workshop. The project is part of a new wave of study on violence in the Middle Ages, with increased focus on the Church: its institutions and its people – the clergy.

The book is co-authored by nineteen researchers, most of them young or middle-aged, several of them representing Poland (seven) and the USA (five), along with Russia (two authors) and one each from the UK, Hungary, Croatia, Spain, and Switzerland. This selection has taken into account not only the multiplicity of regions covered by the Catholic Church but also the medieval era as a whole. This broad view of the relationship signalled in the title is reflected in the book's structure, composed of four parts: (i) 'Violence against the Church'; (ii) 'Violence within the Church', (iii) 'The Church in a Violent World'; and, (iv) 'Cultural Perceptions of Violence'. The essays within each are arranged chronologically, thus also reflecting the research on violence against/inside the Church as concentrated, for a number of years, on the Early and, particularly, Central High Middle Ages, with (consequently, and quite understandably) a preference for the Frankian and French and, to a lesser extent, 'German' (imperial) territories and sources. Thence, although no French or German authors are represented, the monasteries and dioceses of France and of the Empire come to the fore in this narrative. The Spanish, Norwegian and Bohemian Churches (the issue of Hussitism, as far as the latter is concerned) are discussed in one article each, their Hungarian (of the Great Hungary, encompassing Croatia) and Polish peers being covered by two each (except of the Bydgoszcz-related case study – the essay by Milena Svec Goetshi); but even England, with its somewhat larger coverage (three essays), is approached as, inter alia, a part of the Plantagenet realm.

The four articles in Part I, focused on violence against the Church, cover the centuries from twelfth to fifteenth – somewhat contrary to what has just been said of the early medieval period's primary significance in research

terms. Considered are various regions, ecclesiastical structures, and situations: the dioceses (Salamanca, Zamora) of the Kingdoms of Léon and Castilla in the twelfth and thirteen centuries (essay by Esperanza de los Reyes Aguilar); the south of France in the era of the Albigensian Crusade and Church reform (Walker Reid Cosgrove); and, the Croatian Pauline monastery in the fifteenth century (Silvija Pisk). The common denominator for them, regardless of whether the analysis has been based on a single case study (the assassination of the papal legate Pierre de Castelnau, 1208, on the banks of the Rhône River), or a rich original material (the case of the Garić Pauline monastery), is investigation of the socio-political context of violent acts: the tensions between the various groups of power and the clergy. Stressing the fact that the anticlericalism in southern France, 'rampant' as it was since the mid-twelfth century, was targeted not against the Church as a religious institution but against the temporal power and authority of the Church in the region, Cosgrove poses the important questions: Was violence against the Church accepted (and in what ways, if so), and, how was this (accepted) violence defined? The fourth article in this same section (by Joëlle Rollo-Koster), discussing the violence during the papal and episcopal vacancies, is a little different in character: referring to her book *Raiding Saint Peter*[1], Rollo-Koster enters into discussion with its reviewers (Andreas Rehberg, Anna Modigliani) and with other scholars[2], re-establishing her stance with respect to the right of spoil, and her approach to the vacancies as 'liminal moments'.

Part II deals with violence within the Church, and features, with its six articles, the largest chronological span: the sixth century into the latter half of the fifteenth century. The diverse issues discussed and research methodologies applied make it difficult to define a common denominator to these subsections, save for the generalised phrase in the section's title. Thus, there is a gender-oriented study of the revolts that occurred in the late sixth century in the nunneries of Poitiers and Tours, instigated by persons of royal blood. Natalia Bikeeva focuses on the possibility that the women overtly partook in the violent acts, and in a revolt in particular. On the other hand, sticking to the monastic environment (monks, this time), based on the eleventh-century situation in the abbeys of Saint Gall and Reichenau, Michał Tomaszek proposes an analysis of violence story and its peculiar form – 'an imperfect lynching', as recounted by the chronicle of Ekkehard IV of Saint Gall, considering the structure of these stories, the part played by the officials in the violent acts (penalty awarding included), and certain 'implicit' references to the Rule of St Benedict (never expressly mentioned in the sources).

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[1] Joëlle Rollo-Koster, *Raiding Saint Peter: Empty Sees, Violence, and the Initiation of the Great Western Schism (1378)* (Leiden, 2008).

[2] Particularly, with Michail A. Bojcov and his articles on the *jus spolii*.

The subsequent three chapters deal with warfare and active participation of high-ranking clergy in military actions in the High Middle Ages. This, otherwise heavily exploited, subject-matter has become an opportunity for Daniel Gerrard ('Chivalry, War and Clerical Identity: England and Normandy c. 1056–1226') to dispute with Dominique Barthélemy and Timothy Reuter. Without focusing too much on the Three Orders concept, this author mainly concentrates on the ambiguity of attitudes and assessments of the clergymen's military activity, as is easy to spot in the period's texts, and – given the context – the clerical identity and the comprehension of sanctity. The ambiguity, which should warn against a temptation to categorise the medieval clergy into archetypes, is explainable, to Gerrard's mind, by the existence of two ways of understanding the clerical identity and sanctity: one, rooted in the clergy's (or, at least, the ideal cleric's) separation from the world, rejected "the clerical involvement in warfare", whilst the other assumed that at least some of the ecclesiastics were capable of simultaneously fulfilling both roles, and impersonate the different systems of values.

The article by Craig M. Nakashian on the military service rendered to Henry II by his baseborn son Geoffrey Plantagenet, Bishop-elect of Lincoln and Archbishop of York, elaborates, to an extent, on Gerrard's considerations. The rich source documentation has enabled Nakashian to embark on a detailed analysis of opinions and judgements expressed by the chroniclers who described the doings of Geoffrey, from his years as a young clerical student and loyal servant to Henry, up to his deeds as 'an ecclesiastical hero' who opposed the policies pursued by King John, Pope Innocent III's foe, the price he paid for it having been a death in exile. The author concludes that the political context and partisanship were key to the building of the ecclesiastical identity.

Analysing the records of a canonical trial brought in the early years of the fourteenth century against Cracow Bishop Jan Muskata, who was entangled in the conflicts over the Polish throne in the late thirteenth/early fourteenth century and in the politics of Wenceslaus II, both Bohemian and Polish monarch at the time, Jacek Maciejewski takes the reader to a journey into the domain of a distinct monarch–ecclesiastical hierarch relationship. In thirteenth-century Poland, "service at the royal [actually, *ducal*, since there was no king in Poland between 1079 and 1295] court was not put into the *curriculum vitae* of the bishops of Gniezno province" (to my mind, this perhaps being due to a lack of emphatic sources, rather than a really dissimilar relationship between the Church and the secular authority).

The closing essay of Milena Svec Goetschi, dealing with apostasy-related violent acts (the instances of abandoning the monastery as considered in the Apostolic Penitentiary in the fifteenth century), is related, to an extent, with the issues mulled over by Tomaszek. However, Svec Goetschi analyses her source with a different purpose in mind, which is to catalogue and classify

the reasons behind the apostasy, this being done in order to grasp the relation between apostasy (escape) and violence. A nice tribute to the workshop organisers is a document made by the author a point-of-departure for her study: namely, a petition from a Carmelite monk of Bydgoszcz, accused of having killed his fellow-brother, a refugee from the convent.

Not all of the Part III ('The Church in a Violent World') essays make the world outside the ecclesial institutions prominent. In any case, there is not much that these studies have in common, apart from a focus on the political context in which the Church operated. Michael E. Moore makes this context the focal point of his considerations: he revisits the Cadaver Trial – the posthumous trial brought against Pope Formosus, in order to analyse how the collapse of the Carolingian dynasty and the decomposition of the Empire, with the accompanying violence and disorder, influenced the Christian theory of the Carolingian system, with its legitimate violence and sanctioned links between violence and religion. The reference made in the conclusive section to René Girard's mimetic scapegoat seems, however, to be initiating, rather than concluding, the thitherto unfolding discussion. Jakub Morawiec, in turn, offers the reader a rather rudimentary exposition of political history and Church–monarchy relationships in Norway of the late twelfth and early thirteenth century. On a lower level, conflict-imbued relations of the convent (of Canons Regular, in this particular case) with a Silesian duke, Aleksandra Filipek analyses the chronicle of Abbot Ludolf of Sagan in terms of a monastery's aide-mémoire, of potential use in court trials. Lastly, the article by Anna Anisimova discusses the feuds involving the Augustinian monastery, the Archbishop of Canterbury, and the king, over the appointment of a rector for the parish church in Faversham, northern Kent, with the Pope intervening (and thus ought rather to have been included in Part I).

Part IV ('Cultural Perceptions of Violence') is the most 'voluminous' section, as far as the issues raised are concerned. It offers us an analysis (penned by Szymon Wieczorek), very closely related to the source text, of the visions drawn from the French tenth- and eleventh-century hagiographies (mainly, *miracula*) whereby a saint delivers corporeal punishments. There is also a more profoundly problematized discussion held by Radosław Kotecki with Lester K. Little, Richard E. Burton, and Geoffrey Koziol, founded upon complaints and requests from the French clergy (the Empire being taken into account, on a complementary and comparative basis) for protection against violence, submitted to rulers in the tenth to the thirteenth century. Kotecki's polemic mainly concerns an 'over-ritualization' of these complaints by scholars; he puts a stronger emphasis on their juridical nature. The article authored by Gergely Kiss, also dealing with protection of the Church, specifically – the Hungarian Church (eleventh to early fourteenth century), offers, for a change, a concise survey of the royal and ecclesiastical legislation. Finally, we come across an analysis (by David A. Traill) of two songs from

the *Carmina Burana*, featuring – quite untypically for this collection, detailed descriptions of violent sex and rape. Also, a rather superficial insight in the course of disputes and polemics instigated by or targeted at Hussites, with a brief by-the-way paragraph on "the use of the secular arm".

As is frequently the case with research and editorial undertakings of the like sort, as regards the issues raised and methodology employed, *Ecclesia et Violentia* is not a coherent collection of articles. The capacious Latin word *violentia* and its no less capacious English counterpart tend to be regarded in various ways by the authors. Most of them have used the period sources and research problems to pick up the events, situations, and broader phenomena associated with the exercise of physical force; yet, some of the contributors, including the editors, would follow a broader definition: „violence should mean deliberate actions that the people of the time believed to be against the law and which, at the same time, caused harm to a specific individual or group, property or valuable objects, or caused fear of harm (injury, destruction or theft)"[3].

The layout of the volume, apparently perspicuous, turns out to be not quite functional as it imposed divisions and classifications upon a phenomenon whose complexity bursts their limits. Consequently, the issues dealt with not always go hand in hand with the subject-matter as outlined in the respective section title. The essays by Gerrard and Nakashian from Part II ('Violence within the Church') might have as well, if not more legitimately, been comprised in Part III ('The Church in the Violent World'); the article by Maciejowski, in turn, definitely overlaps with the problems dealt with in Part I ('Violence Against the Church').

The editors express their view in the 'Introduction': "Violence against the Church and in the Church are phenomena which took very specific forms in the Middle Ages. … The medieval Church was immersed in violence"; hence the central issue, which was basically meant to organise the contents of this collective volume, was "how the violence of the period was unique". The many analyses and images offered to the reader would not confirm the editors' conviction, nor supply an unambiguous answer to the question about the unique character of violence in the Middle Ages. And this for quite obvious reasons: not only do the authors display different concepts of the notion of 'violence' but there is no comparative perspective proposed, one that would exceed the limits of the era. Furthermore, with their chronological, territorial and problem-related diversity, the considerations proposed give

[3] Jacek Maciejowski, 'Making War and Enormities: Violence within the Church in the Diocese of Cracow at the Beginning of the 14th Century', 145 f.; at this point, the author refers to: Piotr S. Górecki, 'Violence and the Social Order in a Medieval Society: The Evidence from the Henryków Region, ca. 1150–1300', in Balázs Nagy and Marcell Sebök (eds.), *The Man of Many Devices, Who Wandered Full Many Ways: Festschrift in Honor of János M. Bak* (Budapest, 1999), 92.

no sufficient basis for comparisons or generalisations within the period concerned, either. Nevertheless, the wide-ranging view of the Church and violence in the medieval times has produced, in a few essays, an important discussion with the picture that emerges based on the extensive, and recently fast-accreting, literature. Some other, basically exiguous, essays provide details enriching the picture, or adding informed penumbrae to it.

trans. Tristan Korecki *Halina Manikowska*

Jerzy Piekalski, *Prague, Wrocław and Krakow: Public and Private Space at the Time of the Medieval Transition*, Wrocław, 2014 (series: Wratislavia Antiqua: Studia z dziejów Wrocławia / Studies on the History of Wrocław, 19), 181 pp., ills., bibliog. [accessible at www.academia.edu]; Polish edn.: *Praga, Wrocław i Kraków. Przestrzeń publiczna i prywatna w czasach średniowiecznego przełomu*, Wydawnictwa Uniwersytetu Wrocławskiego, Wrocław, 2014, 256 pp., ills., bibliog.; series: Acta Universitatis Wratislaviensis, 355

The book is not its author's first synthetic study on the urban development of a medieval town in a comparative perspective. Ranking amongst the most outstanding Polish medieval archaeology scholars, Jerzy Piekalski published sixteen years ago a book *Od Kolonii do Krakowa: przemiana topografii wczesnych miast* (German edn., *Von Köln nach Krakau: der topographische Wandel früher Städte*, 2001), which proved impressive with its flourish. His own research is primarily concentrated on Wrocław and other Silesian towns, his other focus being the towns of Central Europe, particularly in Bohemia. His studies excel with their broad and penetrative use of the historians' research and, above all, with a permanent dialogue with the others' studies. There is no surprise then that the deepening in the last years of the historians' investigations of the civilisation changes occurring in Central Europe in the twelfth and thirteenth centuries (in particular, the town foundation process), which have reinterpreted at least some of the related phenomena, along with a significant expansion of archaeological stations in the very centre (the historical 'core') of the region's major cities: Prague, Wrocław, and Cracow, have become for Piekalski a definitely legitimate reason for attempting to mutually confront and synthesise the outcomes of those studies. Such confrontation of various assumptions and techniques clearly yields benefits to both domains, but also has a risk to it, especially when it comes to compiling a synthetic study. Using the historical research, Piekalski the archaeologist makes a selection of, primarily, the recent generalising concepts to reaffirm his own statements,

whereas the historian reader cannot follow a complete review of the results of the excavations – particularly those not fitting the image suggested by this author. In return, however, such reader receives the author's pronounced stance and some discussion-provoking statements.

Such is the case with Piekalski's most recent book. It is composed of a foreword, five chapters, and a conclusion. The first two chapters deal with town-planning concepts for the three aforementioned towns, initially as proto-urban centres (tenth to twelfth/thirteenth century) and, then on, as 'incorporated' cities. These chapters are closed by conclusion sections where essentially the related development models are proposed. The English edition has a not really apt (in my opinion) term 'incorporation' to denote the town's foundation[1], which to an extent neglects the scale of urban-development and social changes related to the delineation of a new area of the city – the territory of operation of the bestowed rights and freedoms. The following three chapters focus on a less fundamental problem of development of urban space, shown through analysis of the organisation of the burgage plot space, wooden and stone houses and, finally, the construction of streets and tidiness of the city. The discourse is concluded with considerations on the role of Prague, Wrocław and Cracow in the political system and, primarily, in civilisation transformations that extended to the European region in question, plus the influence of these changes on the development of the aforesaid cities between the tenth and the early fourteenth century.

A historian would naturally seek, above all, the development models presented in the first chapters, with a stronger stress put by the archaeologist on certain phenomena (like, e.g., the ironworking activity as "the key to the origin of settlement on the Old Town terrace", as part of discussion of Prague; p. 24). The first such model, the development of inland proto-towns outside of the limes, which performed the central political and ecclesiastical (and, consequently, economic) functions, is formed of the following findings and hypotheses: (i) The deliberate selection of the area was with respect to the site of the castle [*gród*], which was one of the several constituents of the polycentric proto-town. Determined by the geographical factors, the selection had initially a powerful bearing on the development of the hub (to give an example, with respect to a major river: "It may be assumed that this situation resulted from the interplay of economic and military considerations – river communication and transport on the one hand, and its control on the other",

[1] The source term is *locatio*, which in the first place denotes spatial organisation (an example being the 1261 charter for Wrocław, the so-called second foundation: "videlicet in macellis carnium et etiam hortis, ante civitatem jacentibus, infra fossata primae locationis"; cf. <https://www.herder-institut.de/no_cache/bestaende-digitale-angebote/e-publikationen/dokumente-und-materialien/themenmodule/quelle/2163/details.html> [Accessed: Nov. 10, 2015]).

p. 46); (ii) Contrary to certain proposed models, castles – the initial element of settlement agglomerations (for each of the hubs under analysis) – were not castle-towns, or burg-cities; (iii) The 'market theory' with respect to the origins of medieval town ought to be definitely rejected ("the market-crafts suburbia of Prague, Wrocław and Cracow developed as a result of the concentration of secular and church elites in castles with the concomitant demand for luxury goods, iron and articles of everyday use"; p. 46); (iv) The castle and the non-agrarian suburbia form an axis of the settlement complex, with monasteries, residences of the secular mighty and farming settlements or hamlets adding to its structure in a long-term process; (v) The proto-towns in question have no linear border; the earliest necropolises are situated, in all the three hubs, outside the populated zone. A significant difference between the towns under analysis, in the tenth to thirteenth century, are mostly related to the settlement activity of *hospites*, which first got stabilised in Prague. Jewish merchants began settling down in the hub on the Vltava since the eleventh century; in Wrocław, the Jewish, German, and Romance people first appeared in the late twelfth and early thirteenth century; in Cracow, the appearance of Jews is documented as from the eleventh century, the religious community being attested only for the thirteenth century onwards.

The model of the transition to the urban stage – the thirteenth-century town foundation process proposed by Piekalski – is somewhat less coherent or convincing. The author tends in this respect to quote the main positions proposed by historians, urban planners and archaeologists, and supports, rather cautiously, the one(s) of his choice. Of importance for him is the research done not long ago by Sławomir Gawlas, although this author based his findings primarily on the literature, rather than sources. Piekalski follows Gawlas (and, in fact, a number of other historians before him) in acknowledging the primacy of economic determinatives in the foundation process (the economic interest of the ruler: reinforced rule over alien merchants, market monopoly), remarking that "Legal, political and ideological conditions determined only the form of urbanisation" (p. 71). His strongly highlighted opinion whereby „The pre-incorporation, incorporation and post-incorporation phases of development cannot be separated with any accuracy" (p. 68) arouses no doubts (though Piekalski is not completely right in faulting historians for such disjunctive approach to the subsequent developmental phases).

Given the great and still discussed problem of the layout of founded/incorporated city, Piekalski's stance is clear and resolute: it never was a one-off act of shaping a 'finished' form, in a strict association with the legal incorporation: "It would seem that in each case we find an accelerated evolution rather than an incident with a definite timeframe. Ever more frequently, we have come to refer to incorporation as a complex process rather than the act of founding a town" (p.70). This conclusion is not fully confirmed, though, by the preceding detailed argument. Even Prague, in the

process of foundation of its multiple segments, experienced 'single surveying episodes' of the sort – to remind the foundation of the Havelské Město or the Malá Strana, which was associated with demolition of the local old buildings and removal of the local (that is, Bohemian) population. Cracow offers the most distinctive example: the 'legal regulation' carried out in 1257 after the area was devastated by the Mongolian invaders was interrelated with the removal of the old buildings and the laying out of a new spatial plan for the city. Years ago, Benedykt Zientara, who proposed the until-recently most important incorporation (or rather, town-founding) watershed model in Polish historiography, perceived the occurrence as a process but highlighted its constituent single radical actions. Among them, those related to urban planning and population development excelled: demarcation of the new area and importation of the settlers, the latter often connected with removing the previous population outside the area of the town being founded. Piekalski is right, however, in stating that the old method of reconstructing the founded city's plan, measuring and laying it out (assuming that these were 'one-off' actions or events), which was based on nineteenth-century cadastre plans, can no more be considered satisfactory.

With respect to the 'accelerated evolution' model proposed by this author, the disputable issue of 'first foundations', not quite clearly outlined by the sources, leaves a feeling of dissatisfaction. According to historians, this view being accepted by some archaeologists too, such foundations were carried out in all the three urban centres several dozen years before the foundation as confirmed by a charter or chronicler's note – this including the bestowal of the (Magdeburg) town law and spatial reorganisation. These early foundations have been most recently considered the act of legal singling out of a group (or, possibly, hamlet) of colonists (*hospites*). Piekalski is prone to see in it the formation of one more segment of the pre-incorporation towns – namely, a commune "of foreign, mainly German-speaking, *hospites*" (p. 69); yet, he gives no new and strong arguments in support of this hypothesis.

The two following chapters deal with the private space mentioned in the title: the size, regularity and organisation of the space (or, in stricter terms, buildings and other developments) of the burgage plot as well as of the merchant's and craftsman's house, in the foundation period and several dozen years afterwards. Archaeology has in the recent years contributed new data and helped solve the major differences noticeable in the housing developments as characteristic of the cities analysed in this book. In Prague, highly important were the structures developed in the pre-foundation period, as opposed to Wrocław, with its city delineated alongside the old crafts and trade settlement. In Cracow, in turn, the so-called full (72/144 feet) plots demarcated in relation to the 1257 foundation charter constituted, first and foremost, the fiscal calculation modules. Very few of them, solidified in the late thirteenth century with stone and brick buildings, remained an elitist

phenomenon, for plots tended to be divided into smaller parcels ever since the town became settled. Piekalski challenges on these premises the validity of the planimetric approach applied in the previous research, as it ignored the social and material (financial) diversification of the immigrants. The 'real' parcels were thus not of the same size: apart from the elitist 'full plot', half of such plot ought to be considered commonplace (still before stone and brick houses appeared).

Piekalski describes how a typical plot was organised, using the example of Wrocław, primarily because of the research advancement and opportunities (Prague being a much more difficult case in this respect). A plot was divided into five zones, from a front zone, featuring a glamorous building, through to the sanitary zone with wells and cesspools (or a garden, in less densely populated parts of the town).

The presentation of houses of merchants and craftsmen in Prague, Wrocław and Cracow is preceded by an extensive passage on residential buildings in the towns situated between the Rhine and the Oder, mainly in the eleventh/twelfth century (Zurich, Basel, Freiburg im Breisgau, Ulm; in Lower Germany: Dorestadt, Hedeby, Schleswig, Lübeck – the latter covered in detail). Prague once again appears to be unique in comparison to its neighbour cities. Although the one-room log or wattle-and-daub house was a common type of building in the Early Middle Ages there as well, timber post buildings (otherwise untypical to Wrocław or Cracow) were built there already in the twelfth century, whilst stone residential houses appearing locally much earlier on. Piekalski is ready to hypothesise that the twelfth-century Romance stone houses (some ninety having been discovered so far), erected on the right bank of the Vltava, were for the most part owned by rich merchants rather than the society's most powerful members (magnates). When considering the subsequent hypotheses in relation to the origin of such buildings (or, strictly speaking, the possible region of importation of the model), the author would not speak for any of them but concludes instead that the phenomenon in question was 'simply an original' one (p. 116).

A large subchapter dealing with residential buildings in Wrocław since the pre-foundation time is vastly based on Piekalski's own research. A 'new quality' was marked by the appearance in the late twelfth/early thirteenth century of timber-framed houses, which, some of them situated at the Market Square, were decisive to the character of the buildings already at the founded city stage (thirteenth century). In Wrocław, with its shortage of stone material, a considerable number of brick houses appeared in the residential blocks of the Market Square and its adjacent areas. At least some of them must have belonged to the rich town oligarchy. In Cracow, for which identification of the houses of the *hospites* settled there before the 1257 foundation still remains a task for researchers to deal with, the remnants of the residential towers at the Market Square are considered the earliest relics

of residential developments, which were erected in the already founded city, on surveyed parcels. Those best preserved, dating to the late thirteenth/early fourteenth century, are to be referred to the very top men in the society: the mighty *vogt* Albert and his brother, *vogt* Henryk. The ascription of residential towers to burghers remains hypothetical then, as does the interpretation of the nature of the stone buildings erected, under the Přemyslid rule of Cracow, deeper down the plot (like in the southern German towns, the towers might have possibly belonged to clergymen and knights). The stone houses situated in the plot's front section (since the late thirteenth century) are less mysterious and more numerous; indeed, they are even considered the city's dominant buildings until the mid-fourteenth century.

The chapter 'Sanitation and street surface construction' closes the analytical section of the book. Piekalski has long been preoccupied with these issues, and his learnedness in this area allows him, again, to set his analytical effort against a broader background. The sanitary conditions and the street construction method are set within the context of various natural conditions characteristic of each of the cities, which proved the least advantageous in Wrocław. The enormous increase of archaeological investigations in the recent years has enabled to negatively verify the hypothesis of overpopulation that would have affected also these cities in the period preceding their fast development on a vast scale in the fourteenth and fifteenth centuries. Cracow remained 'underpopulated', if anything, well into the end of the thirteenth century.

But the author is primarily preoccupied with streets, surface reinforcement methods, and the various materials used for the purpose in the towns under discussion: timber, river pebbles, and cobbles. These differences were due, to a considerable extent, to the access to materials available (in Cracow, limestone was employed in the twelfth to fourteenth century not only for construction purposes but also for hardening of street surface). Some corrections have recently been offered by archaeology with respect to the existing research findings. For instance, based on the dendrochronological dates, the opinion is now rejected that Wrocław, right after its foundation, had permanent timber street surfaces built: it has been found now that the material was, in fact, retrieved from the demolished wooden buildings and reused.

The closing chapter entitled 'Prague, Wrocław and Krakow – the towns of New Europe. Conclusion' concisely recapitulates the conclusions of the arguments proposed in the book, stressing the differences between the three cities in question. Striking in the general conclusion is the author's conformism, of a sort, with respect to the historians' findings prevalent in academic syntheses, which leads to an 'academic conservatism'. In Poland, this trend has not ceased to refer to Marxist-oriented economic and social studies (which otherwise still prove to be of the first water). Thus, the development and deep transformations occurring in these cities between the twelfth and

the fourteenth century were part of the social and economic change taking place at the time in Europe (following Gawlas's concept, Piekalski terms them a 'commercialisation'): "At the present stage of research and discussion, we may conclude that the acceleration of the urban process in East Central Europe was not so much the result of the will of political and ecclesiastical elites of the day as of the economic, social processes and population growth observed in the 12th–13th century in the region west of the Elbe. In other words, the economic prosperity of Cologne, Nuremberg or Lübeck was translated by way of trade into the development of the towns of 'new Europe'" (p. 159).

trans. Tristan Korecki *Halina Manikowska*

Martina Mařínková and Christian Zschieschang (eds.), *Wassermühlen und Wassernutzung im mittelalterlichen Ostmitteleuropa*, Franz Steiner Verlag, Stuttgart, 2015, 340 pp.; series: Forschungen zur Geschichte und Kultur des östlichen Mitteleuropa, 50

Intensified interest in milling industry in the Middle Ages and Early Modern Period has recently been seen in historiographical literature. This particular industrial branch, formerly of crucial importance, is perishing nowadays dramatically fast. When travelling across European villages, we come across numerous monuments of water or wind milling, not infrequently putrescent. But mills prove fascinating not only to historians (p. 7): ethnologists, anthropologists, archaeologists, and linguists are also interested. This large group of people has over the recent dozen-or-so years endeavoured to deepen our knowledge about this now-endangered 'magical' (as a book's title would have it) craft. Many grassroots organisations are emerging in Europe to popularise knowledge on mills and windmills, one of them being The International Molinological Society, or a Scandinavian organisation Danish Windmills. With no association of this sort yet set up in Poland, there are some hobbyists' projects such as the Rzeczpospolita Młynarska[1], or The Virtual Museum My Windmills – a Web project run by a retired Poznań city guide.[2]

Apart from popularisation activities, there are research units that delve into research in milling industry on a professional basis. The activity of one of them led to a conference held in 2013 under the auspices of the Geisteswissenschaftliches Zentrum Geschichte und Kultur Ostmitteleuropas

[1] See http://rzeczpospolitamlynarska.pl [Accessed: Dec. 10, 2015].

[2] For more on the project, also in English, see http://www.wbc.poznan.pl/publication/251856 [Accessed: Dec. 10, 2015].

(GWZO), at which aspects of the operation of watermills in medieval East Central Europe were discussed. The publication under review has come as an aftermath of the event.

The book includes an introduction and three main sections, of which the first, 'Wassernutzung im Mittelalter', contains four articles (two of them in English) on the use of water resources in the medieval German Reich. The second section deals with watermills within the landscape, economy, and how they were perceived in the Middle Ages. This definitely most abounding part of the book offers seven treatises – historical (i.a., Prof. Winfried Schich's *Die Bedeutung der Wassermühle für zisterziensische Klostergemeinschaft im 12. und 13. Jahrhundert*), onomasiological (by Stanisława Sochacka), an essay co-authored by Monika Chroś and Łucja Jarczak, and one by Christian Zschieschang. Also an article on mills in medieval theology of image (*Die Mühle in der Bildtheologie des Mittelalters*). The third, and last, section comprises five archaeological studies concerning relics of material culture related to the functioning of watermills in the medieval period, with two essays standing out. Jens Berthold discusses the most important outcomes of the recent West European archaeological research into watermill stations (*Mühlen im Befund – Eine Übersicht zu archäologischen Erscheinungsformen von Wassermühlen*). Gerson H. Jeute deals with popularisation of watermills in late medieval Europe, in light of archaeological research (*Zur Verbreitung der hochmittelalterlichen Mühlen aus archäologischer Sicht*). Colour illustrations related to the respective articles round off the book.

Onomastics, represented by the essays of Stanisława Sochacka (*Die Namen der Wassermühlen in Schlesien*) and the one co-authored by Monika Choroś and Łucja Jarczak (*Schlesische Orts- und Flurnamen mit dem Glied Mühle/młyn*) is one of the breaking-through topics. These authors investigate, from various perspectives, the names proper of mill facilities spread across the medieval Silesia. Both articles display high quality of analysis, which is based on collecting extracts or excerpts for individual mills, thus establishing a possibly broad chronological range, reaching in many cases as far as the twentieth century. Onomastic studies on Silesian mills have clearly shown the change in the naming of these objects after the Second World War, along with mutual penetration of the names between, for instance, physiographic objects and watermills (p. 169).

The classification of the names of mills as proposed by Sochacka (pp. 171–2) is worth discussing now. As she suggests, mills are named according to: (i) situation, as related to other mill facilities (objects) or legacy settlement space; (ii) size ('Large/Small Mill') or age ('Old/New Mill'); (iii) names accorded based on trees or other plants; (iv) type of construction; (v) type of ownership; (vi) type of raw material(s) processed. This concept calls for a commentary. First, research of names of watermills or other facilities related to the medieval or modern-age economy implies the need to analyse

the nomenclature in a rather broad context of settlement in the object's nearer and further surroundings. This is also, and indeed at times primarily, true with history of material culture. Given such a context, the name *Dembowy*[3] ('Oaken Mill') could be explained as located in the village called Dęby ('Oaks')[4], or situated close to an oak forest. A thorough analysis of the source shows, however, that the mill in question was in fact a fulling mill used to tenderise the cloth and grind oak bark, which was an important raw material used in leather tanning.[5] Hence, the basic meaning of the name of 'mill' has to do with the flora, whereas the material-culture context refocuses the name's semantics into the type of industrial production prevalent in the Middle Ages and Early Modern Period. Similarly for a mill called *Ważny*[6]: the association first coming to one's mind is a mill 'of importance, essential, weighty'[7]; but this link is misleading, since the name has once developed from the basic word *waga*, denoting – in this specific context – a special mechanism of mounting the waterwheel (on the scales, which is what the word *waga* denotes in Polish) enabling to adjust the wheel's level against the level of the water in the river.[8]

The aforementioned classification nowise reflects the onomastic changes related to the location of watermills. Based on the research published so far in Polish scientific periodicals, apart from the settlement structure context, the nomenclature of such facilities has been heavily informed by the regional conditions. In the fifteenth- and sixteenth-century Poland, the mills called *rudne* ('ore mills', devised for grinding and processing the bog iron ore), were named *Hamer* or *Hamernia* – using a Polonised form of the German *Hammer*

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[3] See the source text in Adam Mysłowski and Wojciech Graniczny (eds.), *Knigi pol'skoi koronnoi metriki XV stolětiâ*, i: *1447–1454* (Warszawa, 1914), 98–9, no. 114.

[4] In the area adjacent to the Czarnków settlement there was no village that would bear a name connected with a vegetation or forest place name. For more, see Marek Słoń (ed.), 'Tax Registers: Kalisz Voivodeship in the 16th Century', *Atlas of Sources and Materials to the History of Old Poland*, ii (2015), www.atlasfontium.pl [Accessed: Dec. 7, 2015].

[5] For more on the topic, see Anna Rutkowska-Płachcińska (ed.), *Historia kultury materialnej Polski w zarysie*, ii (Wrocław, 1978), 115–18.

[6] Tomasz Jurek, 'Kilkanaście niedrukowanych dokumentów wielkopolskich z XIII i XIV w.', in *idem* and Izabela Skierska (eds.), *Fontes et historia. Prace dedykowane Antoniemu Gąsiorowskiemu* (Poznań, 2007), 108–10, no. 10: *molendinum dictum Wazny Mlyn de duabus rotis vel si plures commode fieri poterint in fluvio Wartha intermedio villae Curowo et molendinum Jaschkonis molendinatoris in Czarnkowo in districtu Coninensi*.

[7] Elżbieta Kowalczyk-Heyman, '"Młyny ważne", "młyny na wagach". Przyczynek do genezy nazwy i konstrukcji', *Studia Geohistorica*, iii (2015), 61–71.

[8] *Ibidem*, 67–8. Sochacka erroneously qualifies *Hengelmühle* as a type of ownership (p. 172).

('mallet', 'hammer') only in west-northern parts of the country (former Voivodeships of Poznań and Kalisz).[9] The other regions generally welcomed the name *kuźnica* or *ruda* (*minera ferraria*, *minera alias ruda*[10], *fabrica*). This onomastic diversity certainly has to do with the social and economic effects of fourteenth- and fifteenth-century colonisations done under the German law. There is no coincidence in the fact that the names of specialised tools used in forges or smithies had oftentimes no Polish-language equivalents.[11]

The ownership and location of watermills is another issue calling for discussion. Grouping mill facilities based only on their names, without minute source analysis, may again result in significant simplifications. Various onomastic intricacies are clearly seen, for instance, with the mills situated in modern towns. The density of settlement had a bearing on how the names were shaped: names of watermills were formed differently in urban areas compared to rural areas – a fact that has escaped the attention of the aforesaid three authors. Classification of mills by type of ownership ought, in the first place, to depend upon tax- or inventory-related written sources.[12] In the urban

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[9] Benedykt Zientara, *Dzieje małopolskiego hutnictwa żelaznego – XIV–XVII wiek* (Warszawa, 1954), 64; Barbara Czopek-Kopciuch, *Adaptacje niemieckich nazw miejscowych w języku polskim* (Kraków, 1995), 88, 218; *Corona Regni Poloniae. Central Part in the End of 16th Century*, sheet D, scale 1:250,000, in Marek Słoń (ed.), *Historical Atlas of Poland in the 2nd Half of the 16th Century. Voivodeships of Cracow, Sandomierz, Lublin, Sieradz, Łęczyca, Rawa, Płock and Mazovia*, i: *Maps* (Geschichte – Erinnerung – Politik. Posener Studien zur Geschichts-, Kultur- und Politikwissenschaft, 6, Frankfurt am Main, 2013). Cf. Jacek Laberschek, 'Kuźnice żelazne w ziemi krakowskiej w średniowieczu', *Teki Krakowskie*, iii (1996), 97–115.

[10] In this case, the Polish word *ruda* corresponds with the English *bog iron*.

[11] Adam Mańkowski (ed.), *Inwentarz dóbr biskupstwa chełmińskiego z r. 1614* (Toruń, 1927), 3: *kuźnia, w niey śięń y jzdebka, w niey piec prosty, okno szklane, drzwi dwoie na zawiassach. U iednych zamek, u drugich klamka. Statki kowalskie. Kamień do Toczęnia 1, Miech 1, Kowadło 1, Pasiękiel 1, slachhamer 1, Klochhamer 1, Helzmeizen 1, Nagieleizen wielki 1, Sroteizen w klocu 1, Cąg wielkich y małych 5, Seteizen 1, warclakhamer 1, werhamer 1, sroteizen 1, dorszlakow 2, Nagieleizen małe 1, Herspis 1, szparynk 4, sztempel 1*. Cf. Henryk Samsonowicz, *Rzemiosło wiejskie w Polsce XIV–XVI w.* (Warszawa, 1954), 84–5.

[12] In Polish settlement studies, only four basic categories of ownership are used for the period of the Middle Ages to the end of Early Modern Period (eighteenth century): king's, noble's, ecclesiastical, and urban. Co-ownership sometimes appeared, with a half of the mill owned by a noble and the other half by the king, but these cases are quite unique in source materials. Cf. Piotr Plisiecki, *Młyny wodne w województwie lubelskim (do schyłku XVI w.)* (Lublin, 2015), where the attached map, entitled *Własność miejscowości z młynami wodnymi na terenie województwa lubelskiego* [The ownership of localities with watermills in the Voivodeship of Lublin], shows some questionable types of ownership.

tissue, the names of such objects were often related to their topographic location. The amassment of watermills tended to be significant, to the extent that it posed serious problems even to the contemporary people – as can be very well seen with sixteenth-century extraordinary tax registers.

To give an example, the tax collectors doing their job in the entire period under examination described watermills connected with the Poznań agglomeration as located *circa Posnaniam*, the only chance to identify each of them was to give the name and an approximate location.[13] Not only the compilation of treasury inventories was thus facilitated: the method was also of high economic importance. Any error in noting down the amounts of tax paid might have resulted in the watermill's proprietor getting entered in a register of debtors, leading, in consequence, to collection of a higher tax in the following year, due interest inclusive. Investigating the names of the shredding facility poses yet another challenge. The names of watermills were often derived from the names of persons (or families) running them and working for them.[14] Reservations of this sort are many; research into onomastic content without prior or parallel research is not correct methodologically.

In appraising the publication in question, attention should be paid to some second-rank but still important things. Apart from one exception (p. 40), the reader would look for English summaries of the articles in vain. Lack of such abstracts severely limits the book's reception outside German-speaking territory, primarily in Western Europe, where research of mills is the most advanced. Also the illustrations are laid out rather uncomfortably: black-and-white pictures are included in the respective essays, colour illustrations can be found at the book's end. While it is clear that limited printing funds have enforced such a solution, no references present between the colour pictures and the related studies renders the reception of the text somewhat difficult. For instance, the reader interested in the study by Winfired Schich is referred to its accompanying illustrations at the end of the text (pp. 92–6) as well as several dozen pages further on (pp. 325–6).

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[13] The Central Archives of Historical Records, Warsaw (AGAD), Archive of the Crown's Treasury (*Archiwum Skarbu Koronnego*) [hereinafter ASK], I 5, f. 733: *molendinum novum in Cibyna* [Cybina was a river by which many watermills were located, owned by the Bishopric of Poznań]; ASK I 5, f. 540: *molendinum ex opposito Colegii* [i.e., the Jesuit College] … *molendinum Regium ante portam Wratislaviensem* or *molendinum post claustrum Corporis Christi*.

[14] For more on members of the mill-related families working at the watermills over generations, see Tomasz Związek, 'Testament młynarza z Koła. Przyczynek do badań nad późnośredniowiecznym młynarstwem na ziemiach polskich', *Średniowiecze Polskie i Powszechne*, v (2013), 154–69; cf. Jerzy Wiśniewski, 'Nazwy młynów w Polsce', *Kwartalnik Historii Kultury Materialnej*, xviii (1970), 449–55.

A separate commentary is owed to the collection of maps attached. I am definitely fully aware of the fact that it is a collection of conference-related papers; still, no cartographical editing policy is unacceptable. While the graphic attachments in Jens Berthold's text form a cohesive whole, the maps in the section authored by Gerson H. Jeute leave much to be desired; one of them appears to be a slightly processed reprint from another publication.[15] The way this map is made is far from the standards of historical cartography (misguided selection of signatures, no typology of mill facilities, individual icons overlapping, the base-map wrongly chosen, and no historical context with respect to the settlement environment of the phenomena under discussion). Needless to add that the map on page 276 (*Ersterwähnungen hochmittelalterlicher Mühlen in Europa*), showing the number of watermills in several time ranges: twelfth century and before; thirteenth century; fourteenth century; and, fifteenth century, portrays Poland and Teutonic Prussia as a territory with almost no watermills: such a picture is completely spoiled, the authors thus proving completely ignorant of Polish historical literature.[16]

In my opinion, a definitely, perhaps even prevalently, strong point of this book is the essay by Jens Berthold, collecting information on the major archaeological excavations with respect to watermills in Western European area. Such study has been much in demand, be it for the sake of considerably scattered historical as well as archaeological publications of this sort. Berthold provides information on the major recent discoveries and quotes a rich iconographic material (types of waterwheels, construction of gutters feeding water to the overshot wheel, sketches of mill buildings, and so on).

The book under review is, in my opinion, hard to evaluate in unambiguous terms – not only because of the wide array of topics it tackles but also due to the way in which the outcomes of research are described. The publication is underproduced in editorial terms. Some of the studies partly repeat the existing research dispersed across other publications; some others are based on a weak methodological foundation. The notion of *Ostmitteleuropa*, limited

[15] Map no. 2: *Ersterwähnungen hochmittelalterlicher Mühlen in Brandenburg*, 277. Cf. Gerson H. Jeute, 'Social and Ethnic Aspects of Rural Non-Agrarian Production in Brandenburg (East Germany)', in Jan Klápště and Petr Sommer (eds.), *Arts and Crafts in Medieval Rural Environment. Ruralia VI. 22nd–29th September 2005, Szentendre-Dobogókö, Hungary* (Turnhout, 2007), 366.

[16] Of the studies published before 2013 (incl.), let me mention the following: Zofia Podwińska, 'Rozmieszczenie wodnych młynów zbożowych w Małopolsce w XV wieku', *Kwartalnik Historii Kultury Materialnej*, xviii (1970), 373–402; Rafał Kubicki, *Młynarstwo w państwie zakonu krzyżackiego w Prusach w XIII–XV wieku (do 1454 r.)* (Gdańsk, 2012); Tomasz Związek, 'Kształtowanie sieci młynów wodnych na przykładzie powiatu konińskiego (ok. 1300–1550)', *Studia Geohistorica*, ii (2013), 118–42; Rafał Kubicki, 'Sieć młynów wodnych w dobrach klasztornych na Pomorzu Wschodnim w XIII–XVI w.', *Hereditas Monasteriorum*, ii (2013), 35–56.

in the concept of the authors and editors by the line of the Oder River, leaves a lot to be desired: such a restriction has completely excluded, most unfairly, the achievements of Polish, Lithuanian, or Hungarian historiographers.[17]

trans. Tristan Korecki *Tomasz Związek*

Wanda Wyporska, *Witchcraft in Early Modern Poland, 1500–1800*, Palgrave Macmillan, Basingstoke, 2013, 244 pp.

As the author states in the introduction to her study, the key underlying issues were to: (i) determine who is responsible for having defined the notion of witch (*czarownica*) in Poland; (ii) find what attributes were normally ascribed to witch; and, (iii) see into whether witchcraft trials in Poland were correlated with the intellectual discussion on witchcraft. Another important – or, in fact, seemingly basic – objective was to investigate and analyse the witchcraft trials held at municipal courts in Greater Poland (Wielkopolska) – an aspect that would make the study region-focused. The author moreover resolved to rectify several erroneous conclusions with regards to persecutions of witches in Poland, which, to her mind, have been present in the Polish reference literature. The ultimate goal was to provide a preliminary introduction, of use for the English-speaking reader, to Polish demonology and witchcraft crimes in Greater Poland.

To begin with, doubt arouses around the framework chronology assumed, with the extreme dates 1500 and 1800: we find no explanation from the author why so. In light of the existing research, these dates are of no relevance with respect to witchcraft trials, in Poland or Greater Poland. Nowhere in this book could I find that it was in 1500 that some municipal court would have considered a relevant case, the first such in Wielkopolska. What is known based on the present research is that accusations of witchery and the related court cases first appeared in Poland (and in Greater Poland) in the late Middle Ages. In the latter half of the fifteenth century, witchcraft trials were held within the Kingdom of Poland before ecclesiastical as well as secular courts.[1] Although verdicts of burning at stake were passed, the Church courts-of-law did not enforce them. A capital punishment on account of witchcraft is first mentioned for, indeed, the land of Wielkopolska, when the Church court of Poznań sentenced a certain Dorota of Zakrzewo. No execution was finally

[17] Cf. Grzegorz Myśliwski, 'Utilisation of Water in Central Europe (12th–16th Cents.)', in Simonetta Cavaciocchi (ed.), *Economia e energia secc. XIII–XVIII* (Firenze, 2003), 321–33.

[1] Karol Koranyi, 'Czary i gusła przed sądami kościelnymi w Polsce w XV i w pierwszej połowie XVI wieku', *Lud*, xxvi (1927), 3.

carried out: with a third-party surety and a plea for revocation of her errors, the condemned was eventually released. An old woman of unknown name was, in turn, burnt at the stake in 1511 in Chwaliszewo, Greater Poland (a Magdeburg Law-founded town between 1444 and 1800, presently within the city of Poznań), on a sentence passed by a secular court: the victim was charged with dealing with malefic magic.[2]

The end date, 1800, also triggers doubt: to my mind, it does not, again, specifically refer to witchcraft suits, whether in Wielkopolska or Poland as a whole. Neither do these caesuras mark any political events or territorial changes. Greater Poland was incorporated in Prussia in January 1793; in 1795, resulting from the third (and last) partition, Poland was erased from the map of Europe for over 120 years.

Furthermore, I do not find the territorial coverage particularly clear, since the author refers, interchangeably, to Poland and Greater Poland. An unambiguous delineation of the territorial scope of Wielkopolska would be much of relevance as it is not quite strictly definable – with the notions of 'Wielkopolska', historically versus geographically, or the one known from administrative praxis, being divergent. Map no. 2 included in the opening section does not clarify the issue as, rather than showing the province's territorial confines, it shows local towns or cities (whether all of them, we are not certain) where the courts dealt with witchcraft charges – primarily, by assizes, visiting the villages where the charges or slanders were put forth. Thence, one cannot be certain whether Wanda Wyporska's book concerns the 'Wielkopolska proper', encompassing the Voivodeships of Poznań and Kalisz, or Greater Poland as a broader concept: the aforesaid map shows Sieradz and Turek, the towns of the Sieradz Voivodeship. Has the author included any other localities of the latter voivodeship in Greater Poland as well?

The study is composed of an introduction, seven chapters, end footnotes (whose placement severely hinders the reading experience), a bibliography, and a combined index of personal and geographical names plus subjects. Let us add that the chapters have subchapters bearing separate titles, which is (regrettably) not reflected in the table of contents. The book also features three maps (in the opening section) displaying (i) contemporary Poland, (ii) Greater-Poland cities and towns where secular courts dealt with witchcraft cases in the early modern period, and (iii) the Polish-

[2] Joanna Adamczyk, 'Czary i magia w praktyce sądów kościelnych na ziemiach polskich w późnym średniowieczu (XV – połowa XVI wieku)', in Maria Koczerska (ed.), *Karolińscy pokutnicy i polskie średniowieczne czarownice. Konfrontacja doktryny chrześcijańskiej z życiem społeczeństwa średniowiecznego* (Warszawa, 2007), 205; Małgorzata Pilaszek, *Procesy o czary w Polsce w wiekach XV–XVIII* (Kraków, 2008), 152.

Lithuanian Commonwealth and its neighbouring countries in the sixteenth to seventeenth century.

Since the book under review is targeted primarily at English readers, whose acquaintance with the history of Poland is rather moderate, its first chapter, justifiably, offers basic information on the sixteenth and seventeenth-century Poland. The first subsection in Chapter 1 is a very brief introduction to the early modern history of Poland; the subsequent one briefs the reader on Greater Poland. The following (three-page) section discusses the history of Polish research on the transgression of witchery, until the publication (in 1952) of Bohdan Baranowski's study *Procesy czarownic w Polsce w XVII i XVIII wieku*, also summarising this scholar's influence on the subsequent research.

The point of Chapter 2 was to present the statistics of witchcraft trials in Wielkopolska, in a chronological and geographical arrangement, and the evolution of the notion of 'witchery transgression'; also, to determine the statistics of witchcraft trials. This is meant to finally enable to determine the Polish paradigm of witch and its constituent elements. Based on her own research, Wyporska has found that between 1500 and 1800 a total of 225 cases were taken to secular (municipal) courts in Greater Poland, with more than 460 accused – this making two accused per less-than-one trial per year, on average. The number of lawsuits in the area concerned increased in the 1580s and in the first two decades of the sixteenth century, peaking between 1660 and 1740, and plunging in 1700–10. With respect to the number of charged/accused, the figures soared in the period 1670 to 1700 as well as in 1710–30. That witch-hunting in Poland peaked in the years 1670–1740, with the largest-ever number of court cases across Poland, including in Wielkopolska, was noted years ago by Stanisław Salmonowicz[3]; unfortunately, there is no mention of his article in the Wyporska's book.

One of the goals the author set for herself was to revise the erroneous information on witchcraft trials in Poland. It is a pity, then, that she has not unambiguously verified the information related to the 1775 litigation in Doruchów. Polish authors have for a long time assumed that this was the date and location of the last witchcraft suit in Poland-Lithuania. As many as fourteen women charged of witchery were reportedly burnt at the stake, and the case allegedly played a prevalent part in the abolishment of torture and the ban on conducting witchcraft lawsuits. Although Janusz Tazbir demonstrated years ago that there was no such trial at all[4] and the 'eyewitness report',

[3] Stanisław Salmonowicz, 'Procesy o czary w Polsce. Próba rozważań modelowych', in *Prawo wczoraj i dziś. Studia dedykowane profesor Katarzynie Sójce-Zielińskiej* (Warszawa, 2000), 315–16.

[4] Janusz Tazbir, 'Z dziejów fałszerstw historycznych w Polsce w pierwszej połowie XIX wieku', *Przegląd Historyczny*, lvii (1966), 590.

probably penned by Konstanty Majeranowski, is a forgery[5], the Doruchów trial has not disappeared from studies authored by historians[6] – the book under discussion being, sadly, a case in point.

The chapter in question also mentions a trial that took place in Kalisz in 1580, stating that the defendants, named Zofia of Łękno and Barbara of Radom, were subjected to torture and to a water test (p. 32). The date on which such a test was first carried out in the early modern Commonwealth relative to a witchery charge is unknown to us; certainly, though, it was not 1580 (the place not being Kalisz). The mentioned women were not ducked, contrary to what Wyporska maintains; they were, instead, sentenced to death by drowning for the crime of thievery as well as for indecent assault. The execution was carried out on 18th July 1580; the executioner threw both women into the water: Zofia got drowned[7], whereas Barbara did not go under, apparently owing to some *incantationum magicarum*; this led to her being interrogated once again. Asked how on earth did she 'come out of that water', she first replied she had requested a devil to help her out, and he promised her she would be saved. Subjected to a torture again, she reported she was saved by a herb called *niedośpiał* (of the genus Centunculus).[8] This clearly leads to the conclusion that those women were not ducked in view of proving that they were guilty of the offence of wizardry.

The third chapter deals with the role of gender in witchcraft trials. Female accused accounted for a clear majority (96 per cent) as far as witchery litigations in Greater Poland are concerned. The author has found, based on the statistics she collated, that among the charged whose profession has been identified, 23 per cent were maidservants (many of them employed by their accusers), 19 per cent bondservant peasants, 19 per cent shepherdesses, 7 per cent millers, 13 per cent innkeepers (alewives), the remaining 28 per cent being of unknown jobs.

Chapter 4 focuses on the judiciary. Since the author considered the Polish judicial system abuse-prone, she has deemed it usable to juxtapose and mutually compare two images of a legal system: one featuring a theoretical framework as prescribed in various codices of law, the other based on what has been found in court-filed documents. Subsequently, the crime of witchery, as it functioned in the ecclesiastical and civil law, is discussed. The structure of Polish courts is described, along with the judiciary in Greater Poland, the

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[5] Janusz Tazbir, *Cudzym piórem… Falsyfikaty historyczno-literackie* (Poznań, 2002), 103.

[6] For instance, Stanisław Grodziski, *Z dziejów staropolskiej kultury prawnej* (Kraków, 2004), 196.

[7] Bohdan Baranowski, *Najdawniejsze procesy o czary w Kaliszu* (Lublin and Łódź, 1951), 18.

[8] *Ibidem*, 23.

theoretical penal procedure applicable with municipal courts and, finally, the real-life witchcraft suits (pp. 83–7). The role of nobleman, as a lord or master, proprietor of villages and small towns, whose influence on the lives of their inhabitants was decisive, is justifiably emphasised.

The final section of the chapter discusses the final days of witchcraft lawsuits in Greater Poland. As Wyporska notices, the second half of the eighteenth century still saw as many as five such litigations in the region alone – to be specific, in Kalisz (1750), Kiszkowo (1761), Pyzdry (1761), Kopnica (1775), and Dobra (1781). It has to be remarked, however, that investigations conducted by the local courts in these towns, rather than litigations as such, is the case here. For instance, the town court of Kiszkowo held an away session in the village of Gorzuchowo. Also, the author has ignored at least one instance of proceeding conducted in Greater Poland in the latter half of the eighteenth century: namely, the 1753 suit of which we have been advised by Stanisław Karwowski's monograph of Gniezno.[9]

The fifth chapter discusses the image of the witch as perceived by the Church. As the author remarks at the outset of this section, her analysis, founded on a broad array of literature, demonstrates that the prevalent view whereby the witch-hunt in Poland merely followed up the German phenomenon can be debunked (p. 95). Among other things, the influence of Slavic paganism on Christianity and Protestant contributions to demonology are discussed, along with the related Calvinist *Wykład katechizmu* [Exposition of Catechism] by Paweł Gilowski, from 1579. Examples of demonological literature are introduced: a work by a Catholic author, Marcin Nowakowski, entitled *Kolęda duchowna* (1753) and Benedykt Chmielowski's encyclopaedia *Nowe Ateny*, also dating to the mid-eighteenth century.

I am not quite certain whether *Nowe Ateny* should be listed as a demonological literature item, unless the reason why the author has so decided was the fact that Chmielowski was a clergyman. It would have seemingly been more legitimate to take into account the catechisms touching upon the issue of witchery and the related superstitions, just to name Jan-Stanisław Wuykowski's *Chleb duchowny wszystkim Chrześcianom na posiłek w drodze do nieba idącym wystawiony*, whose first edition came out in Kalisz in 1733.[10]

Chapter 6 is entitled 'Beyond Demonology: Blame the Witches'. Having got acquainted with a broad choice of works of various literary genres, Wyporska comes to the conclusion that not only did their authors alter, to a varying extent, the ecclesiastical stereotype of witch: she finds that it "stood completely on its head" (p. 127). As a matter of fact, devil and witch often were the objects of irony and parody; and this is what the chapter was

[9] Stanisław Karwowski, *Gniezno* (Poznań, 1892), 126–7.

[10] Jan Z. Słowiński, *Katechizmy katolickie w języku polskim od XVI do XVIII wieku* (Lublin, 2005), 184.

meant to show. This ascertainment, let us add, is not a new one: of Polish authors, Julian Krzyżanowski, Michał Rożek[11], and others too, wrote about it. The author's statement that her "analysis of a broader sample of printed sources is one of the key contributions of this study, which challenges the presumed dominance of the largely ecclesiastical stereotype of the maleficent witch" (p. 128) is slightly exaggerated.

Chapter 7 – 'Sceptical Voices: Ending the Era' deals with the sceptical voices raised with regards to witchcraft trials in Poland, beginning with the seventeenth century, and the end put to the witch-hunt in Greater Poland and Poland as a whole. Emphasised is the role of the Poznań milieu in bringing about the finalisation of persecutions of alleged witches, with Poznań possibly being the main hub of the debate concerning witchery in Poland (p. 175). The author fairly extensively discusses the literary works of relevance produced by the Poznań scholars, including the anonymous *Czarownica powołana abo krotka nauka y przestroga z strony czarownic*, 1639, and Daniel Wisner's treatise *Tractatus brevis de extramagi lamii, veneticis* issued in the same year. Yet, not a word is said about the important contribution to the eradication of witch-hunt practices, and of the belief in evil acts the 'witches' allegedly committed, from the periodicals circulating in Poland in the latter half of the eighteenth century: *Monitor* and *Zabawy Przyjemne i Pożyteczne*. Both disseminated the Enlightenment thought, popularised literary pieces and the ideas of state reform. Franciszek Bohomolec, editor of *Monitor*, noted acerbically (on 3rd October 1767) that, whilst among his readers are such who do not believe in witches, those who find believing in magic and sorceresses still prevail.[12] An article published in this magazine somewhat later severely criticised the mentality of the nation, pointing out that for instance in Germany no witchcraft trials were held anymore since almost no-one there believed in witchery; moreover, they "pretend that all the witches have moved to Poland", owing to low educational standards in this country.[13]

The last and closing chapter finds, among other things, that witch persecution in Greater Poland and the intellectual discourse in Poland were "very similar to their counterparts in the rest of Europe, and not, as some historians would have it, 'backward'" (p. 177). Wyporska would not tell us, though who are these historians she is referring to, nor does she quote any specific title whatsoever. As she believes, witchcraft lawsuits in Greater Poland were basically akin to those taking part elsewhere in Europe (p. 188), whilst the

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[11] Julian Krzyżanowski, 'Boruta', a dictionary entry in *idem* (ed.), *Słownik folkloru polskiego* (Warszawa, 1965), 49–55; Michał Rożek, *Diabeł w kulturze polskiej. Szkice z dziejów motywu i postaci* (Warszawa and Kraków, 1993), *passim*.

[12] *Monitor*, 1767, no. 79, p. 684.

[13] Barbara Woźniak, 'Walka z przesądami na łamach "Monitora"', *Studia Historyczne*, xlvii, fasc. 2 (2004), 161.

witches in this region were in most cases servant maids by profession. The latter statement may raise a concern, though.

As the author has found, the dropping numbers of witchcraft suits might have been due to the progress of the Enlightenment in Poland and, partly, to the 1745 edict of the assessors' court (p. 186). While I would rather refrain at this point from debating whether the Enlightenment actually progressed in Poland at all, I do have to make the point that no edict was passed by the assessors' court in 1745 for witchery cases. Instead, for the Bishoprics (Dioceses) of Chełmno and Pomesania, the local bishop Andrzej-Stanisław Załuski (1739–46) procured a special royal rescript for witchcraft lawsuits, which was issued on 9th January 1745 in Warsaw by King Augustus III.[14]

The book under review is hard to appraise in unambiguous terms. While reading, my perception was that the author could not decide whether she was to write a history of witch-hunting in Poland in the sixteenth to eighteenth century, or a case study for Wielkopolska. As a result, the study is neither of these – which is a pity, for Greater Poland, as a province, is an interesting case in point. Wyporska has, in my opinion, missed an opportunity to analyse in more detail the material available from the extant dossiers of witchcraft suits executed by the region's municipal courts. We can virtually learn nothing from this book about a typology of the conflicts which led to charges of witchery, about how the accused and their relatives actually behaved, the importance of honour/good name in the daily life of the rural and small-town inhabitants, and so on.

To sum up, one finds that Ms. Wyporska's study will probably be of interest to English-speaking readers finding witchery in early modern Europe close to what they seek: this book will certainly provide information not to be found in other English-language literature. The Polish reader, though, would find it much less interesting or useful, since most of the information therein contained has already been covered in Polish-language literature.

trans. Tristan Korecki *Jacek Wijaczka*

Anne-Simone Rous and Martin Mulsow (eds.), *Geheime Post. Kryptologie und Steganographie der diplomatischen Korrespondenz europäischer Höfe während der Frühen Neuzeit*, Duncker & Humblot, Berlin, 2015, 294 pp.; series: Historische Forschungen, 106

Despite the extremely well-developed research on diplomatic history, a relatively small number of studies have dealt with secret intelligence, cryptography and steganography in the early modern period. The volume edited by

[14] Jakub Czechowicz, *Praktyka kryminalna…* (Chełmno, 1769), 196.

two German researchers Anne-Simone Rous and Martin Mulsow is one of the most recent publications in this area. It presents the substantial output produced by a conference at the Forschungszentrum Gotha of the University of Erfurt in 2013. As Martin Muslow explains in the short introduction opening the volume, the project on secret diplomacy fits squarely into the Forschungszentrum's research area, which focuses on the grey spaces and underground processes in the early modern period.

The book is divided into two parts. The first consists of four papers and serves as a theoretical introduction to the subsequent part. The second presents us with fourteen articles examining well-defined areas of study. The authors are researchers, mostly from German-speaking countries, specializing in the history of diplomacy and literature. Although studies on the Habsburg monarchy and the Holy Roman Empire outnumber those dealing with other European states, the reader finds articles focusing on the activities of French, English and even Swedish, Swiss and Polish high-ranking officials, envoys and spies.

A study by Anne-Simone Rous on the means used to protect diplomatic correspondence from unwanted disclosure in the early modern period opens the first part of the book. The sketch on methodological problems and research perspectives is followed by a general description of early modern cryptology and steganography. The author underlines the importance of a variety of elements such as trustworthy and well-organized "black chambers" with competent "information masters", safe communication resources and the use of steganography to protect state secrets. The subsequent article by Klaus Schmeh provides general information on the use of cryptology in historical research. He discusses which types of writings were most often encrypted, the varieties of early modern codes and the methods of breaking them used by contemporary researchers. The next author, Gerhard F. Strasser, a renowned specialist on sixteenth–seventeenth-century literature, emphasizes that attempts to construct an unbreakable code were intertwined with efforts to create a universal language. He sketches relations between cryptology and linguistic studies from the first printed book on cryptography published by Johannes Trithemius in 1518, through the works of Athanasius Kircher, a seventeenth-century German Jesuit scholar, to the language devised by François Sudre at the beginning of the nineteenth century. The article by Martin Espenhorst touches upon the fascinating issue of the secret clauses included in peace agreements. Looking at numerous peace instruments from the seventeenth and eighteenth centuries, he describes the role of the undisclosed points of the international agreements in early modern diplomacy. The introductory part of the volume closes with a study by Karl de Leeuw of the rise of "black chambers" and the exchange of knowledge between these institutions. He makes some brief remarks regarding three very interesting questions: (i) what can cryptologic literature tell us about their development?; (ii) to what degree and in what way was cryptoanalytic knowledge exchanged

between the "black chambers"?; (iii) did science contribute to the continuing rise of these institutions in the eighteenth century? De Leeuw's preliminary research leads him to conclude that "the emergence of the Black Chambers was due to a mix of espionage, or perhaps it is better to say, an involuntary exchange of information, and the rise of scientific thinking" (p. 97). If one could find any shortcomings in the introductory part, it would be the lack of an explanation of some crucial terms. Initially, the hermetic language of cryptology is hard to understand. Perhaps a glossary modeled on that written by David Kahn[1] would serve as a useful reference for less specialized readers.

In the second section, the texts are grouped geographically. This part contains five sections concerning Spain, the Habsburg monarchy, England, Saxony-Poland-Lithuania and France. The first article by Diego Navarro Bonilla and Julio Hernandez-Castro is devoted to one of the most reputed cryptologists at the service of the Spanish monarchy, namely Luis Valle de la Cerda. His activity as the cipher master of Philip II is described in the context of creating an early modern bureaucratic state. The authors claim that the recognition and honors given to de la Cerda clearly demonstrate that the "black chambers" became necessary, if not indispensable, institutions in sixteenth-century Europe. Four further articles are linked by the geographical scope of the matters under discussion in that they cover the Habsburg monarchy in the sixteenth–eighteenth centuries. The first study by Anton Walder focuses on the encrypted letter sent to Maximilian I by Alberto Pio, Graf de Carpi, the Emperor's envoy to Leo X in 1513. This successfully decoded letter provided by the author in its plaintext Latin version constitutes a starting point for a brief description of the role of cryptology in the Habsburg Monarchy at the beginning of the sixteenth century. The following article by Carolin Pecho asks us to look at the early modern codes from an entirely different perspective. The study deals with the correspondence between two Habsburg archdukes, Ferdinand and Leopold, from 1609–10. The author claims that Ferdinand used the code invented by the two brothers in their childhood in order to remind Leopold of their close family connections. He sought his sibling's support in the turbulent years preceding the death of the two childless emperors Rudolf II and Mathias II. This case study shows that the cipher was not only a tool to hide information but was also a message itself. The next article by Leopold Auer returns to the subject of the role of codes in the Habsburg chancellery in the seventeenth–eighteenth centuries. Gerhard Kay Birkner, the author of the final study, asks to what extent and in what ways diplomatic correspondence was protected from unwanted disclosure during delivery using as an example the letters exchanged between the emperor's envoys in Istanbul and the highest officials in Vienna. He draws

[1] David Kahn, *The Codebreakers: The Comprehensive History of Secret Communication from Ancient Times to the Internet* (New York, 1996), XV–XVIII.

an outline of the development of the postal system operating between the two capitals between the mid-seventeenth and mid-eighteenth centuries.

The next three articles are devoted to cryptology in England. The first by Ekaterina Domnina focuses on Tommaso Spinelli, one of the first English resident ambassadors in Europe. Firstly, the author briefly sketches out the political backdrop against which Spinelli operated. Then, she focuses on his private correspondence with his younger brother Leonardo, who was a chamberlain to Leo X. Domnina proves that these letters deserve our attention as they "shed additional light on political, cultural and economic relations between early Tudor England and the continent" as well as the rise of the "black chambers" during this period in Europe (p. 182). The following two articles concern the use of cryptology in the periods in which the English rulers limited the personal freedom of their subjects. The first by Martin Skoeries is devoted to the exchange of information between Protestants imprisoned by "Bloody Mary" and non-Catholics in England and Europe. The author makes an attempt to understand why the former sent so few encrypted messages. He points to two possible reasons. Firstly, many letters were designed to keep up the spirit of the persecuted Protestants. Thus, the text had to be written in a form which was easy for a broader audience to copy and read. Secondly, the authors, proud to be Protestants, would not have remained true to their conscience if they had hidden their religious beliefs under a code.[2] The second article authored by Andreas Önnerfors deals with encrypted letters sent by a Swedish diplomat in England to Charles, Duke of Södermanland after the publication of the "Unlawful Societies Act" in 1799. The future Swedish king, who was a Freemason himself, requested information on the English lodges in a period in which they were undergoing considerable changes as a result of the new law.

Michael Kreise, Mariusz W. Kaczka and Holger Kürbis devoted their articles to Saxon cryptology in the seventeenth–eighteenth centuries. Perhaps Kaczka's contribution is the most interesting for a Polish reader. He focuses on the encoded letters sent by the Saxon-Polish-Lithuanian envoys in the Ottoman Empire to Polish or Saxon high-ranking officials in the years 1712–14. The author draws attention to the double-track foreign policy of Saxony-Poland-Lithuania. He also suggests that the diffusion of cryptologic knowledge to the Polish chancellery was crucial to the success of the policy conducted by Augustus the Strong, who used the Polish and Saxon chancelleries interchangeably.

The use and protection of the grand chiffre in France is described in the following article by Jörg Ulbert. The secret diplomacy conducted by France, which is seen as a model centralized state, can be easily compared to the

[2] For the most recent studies on the Protestant resistance during the reign of Mary I, see Evelyn Evenden and Vivienne Westbrook (eds.), *Catholic Renewal and Protestant Resistance in Marian England* (Farnham, Surrey and Burlington, VT, 2015).

Swiss policy thanks to the final article of the volume written by Andreas Affolter. It explores the activity of Claude-Théophile de Bésiade, a French ambassador to Switzerland in the first quarter of the eighteenth century. In a picturesque way, the author describes the difficulties encountered by the ambassador, who had to collect information in a federal, decentralized state.

To sum up, the volume under discussion contains valuable articles based on unpublished archival materials. Many of the studies offer the first decoding of highly interesting and previously undeciphered texts. The authors' observations are well argued and are evidence of their considerable erudition. The focused scope of the studies under review allows the authors to offer a reliable summary of the established knowledge supplemented by their own findings and to point the way to further research; they do not try to settle the matters under discussion once and for all. The studies in this volume add significantly to our knowledge of the secret diplomacy of the early modern period. The book will be also useful for anyone researching the rise of the modern state in Europe, and it will be required reading for scholars focusing on early modern science. Lastly, the volume contributes to comparative studies by offering some valuable remarks on the similarities and differences in the organization of numerous European states in the early modern period.

proofread Christopher Gilley *Natalia Królikowska-Jedlińska*

Discourses of Collective Identity in Central and Southeast Europe (1770–1945). Texts and Commentaries, Central European University Press, Budapest and New York; i: Balázs Trencsényi and Michal Kopeček (eds.), *Late Enlightenment – Emergence of the Modern "National Idea"*, 2006, 351 pp.; ii: Balázs Trencsényi and Michal Kopeček (eds.), *National Romanticism – The Formation of National Movements*, 2007, 498 pp.; iii/1: Ahmet Ersoy, Maciej Górny, and Vangelis Kechriotis (eds.), *Modernism – The Creation of Nation-States*, 2010, 486 pp.; iii/2: Ahmet Ersoy, Maciej Górny, and Vangelis Kechriotis (eds.), *Modernism – Representations of National Culture*, 2010, 392 pp.; iv: Diana Mishkova, Marius Turda, and Balázs Trencsényi (eds.), *Anti-Modernism – Radical Revisions of Collective Identity*, 2014, 442 pp.; bibliog., glossary of key terms

To state that language barriers can seriously hinder historical research sounds pretty banal: probably every historian has come across this problem, to a varying extent. The linguistic diversity of Europe (to stay within the limits

of our continent) makes it culturally rich; yet, how hard-to-attain this cultural wealth sometimes is! This becomes particularly relevant when it comes to a content that is complicated and related to things of personal importance to people. And such is the case with problems of collective identity – particularly, national questions – that have been arousing strong emotion from time immemorial, attracting interest of numerous researchers in several last decades. The research in this field gets patterned into an easily explainable model. On the one hand, there are source-based studies, implying a well-established command of the language in which the investigated sources were originally produced; on the other, there are attempts at elaborating interpretative models (or even, theories) whose reach would be at least pan-European. How many languages can one master to a high degree of excellence, or proficiency? Except for the rare cases of extremely talented individuals, the answer is, substantially, not many. Individual scholars mostly tend to write dissertations and tracts on the countries of their birth, breeding, and education, whereas the exploratory forays outside their native territory are restricted by the merciless barrier of language skills. Teamwork is not a solution, for, with all its benefits, original scientific concepts are the domain of individuals. Research that aspires for originality, with pretence to generalising concepts and theory-building, acutely suffer from language limitations: their conceptual swing may be impressive and nominal geographical reach extensive, whilst in reality, they actually refer to a material from a few countries. Now, if this holds true for research scholars, what sort of trouble is the case with those academics who want their students to get acquainted with sources of foreign origin?

While these problems cannot possibly be completely helped, they can be alleviated. Entering into international – that is, English-language – scientific circulation at least a selection (even if just samples) of original reference texts, not quite accessible due to the language barrier (among other factors), is one possible method.

A task of this sort was undertaken a dozen years ago by a multinational team of young researchers who enjoyed institutional support from the Centre for Advanced Study in Sofia, Bulgaria, and the Central European University of Budapest, Hungary. The project's prime movers first met in 1999; the team established for the purpose was eventually joined by scholars from fourteen countries – that is, Albania, Austria, Bosnia and Herzegovina, Bulgaria, Croatia, Czech Republic, Greece, Hungary, Macedonia, Poland, Romania, Serbia, Slovakia, and Turkey. The team focused on 'Central and Southeast Europe'. As is well known, geographical demarcation of an area having been deemed a 'region' is a frustrating exercise since any suggestion made to this end may, not without a basis, be challenged. The authors of the volumes in question have endeavoured to avoid such trouble, distancing themselves from 'essentialism'. However, they have specified that the territories of their interest roughly correspond with those of the Habsburg and Ottoman Empire.

Thus, their publications describe the shaping of collective identities under imperial and post-imperial conditions – 'collective' actually meaning, in this particular case, 'national'. The reasons behind this terminological procedure seem rather clear: as we can learn from the introduction to the first volume, where the assumptions for the whole series are presented, the authors' core ambition is to "contribute to the emergence of a non-nationalistic vision that refutes the restraints of national grand narratives" (vol. i, p. 5). The nationalist narratives whereby nation is a 'natural' and 'perennial' entity, are deeply rooted in the central-eastern part of our continent. The authors endeavour to demonstrate the ways along which such discourses and complexes of ideas or concepts developed. Awareness of these processes would expectedly lead to a fading, or at least modification, of certain simplifications usually present in opinions on Central and Southeast Europe voiced by authors outside the region.

The series consists of four volumes (the third being two-part) showing the phases of the formation of modern 'collective identity discourses'. The first deals with the appearance of the 'national idea' in the late Enlightenment period: from new takes on 'symbolic geography' and period's utterances regarding identity, through to the emergence of national public opinion and the issues of 'reform and revolution'. 'National romanticism' and the shaping of national movements is the topic in vol. ii: a 'historicisation' of nation, various concepts of 'national spirit', 'nationalisation' of space, coexistence with adjacent nations/countries, and, finally, 'revolution and counterrevolution'. The first section in vol. iii focuses on modernism and emergence of nation-states. The primary issues include: creation of modern states in multinational societies; self-determination, democratisation, and homogenising activity of the state; 'national projects' in a regional context; federalism and the collapse of the empires; socialism and the national question. The volume's second part associates modernism with representations of national culture: what we find there is institutionalisation of 'national sciences' as a manifestation of cultural modernisation; a critique of romanticism; literary presentations of 'national character'; associations of modernist aesthetics with collective identities; regionalism and identity-related narratives of minorities. Vol. iv discusses aspects of anti-modernism and radical revisions of collective identity, the central issues being: integral nationalism, crisis of European consciousness, considerations of 'national ontology', conservative redefinitions of tradition and modernity, and – in the closing section – anti-modernist revolution.

Each of these issues are illustrated with a few, or a dozen, fragments of original texts authored by publicists, men-of-letters, scholars, and politicians. In total, there are 268 such texts (specifically, 44, 67, 111, and 46 in each of the volumes, respectively). Each such source has a note attached specifying the title, date and place of publication of the original, the language of the original publication, and the edition from which the presently published fragments have been extracted. A brief biographical information on the source

text's author follows, itself followed by a sketchy presentation of the cultural and political context behind the text. The original texts have been edited elaborately and competently (with errors scarcely occurring; for instance, the Polish League, which is mentioned in the biographic entry of Roman Dmowski, was not quite a conservative organisation; cf. vol. iv, p. 61).

The first volume opens with an extensive and searching introduction ('Inter-Texts of Identity'), telling us the story of the project's origins and summarising the methodological assumptions behind the venture. Discussed are also the political, ideological, and historiographical aspects of the effort undertaken. Succinct and reliable studies introducing the issues connected with the sources are included in each of the volumes. In the first, these essays are penned by László Kontler ('The Enlightenment in Central Europe?') and Paschalis M. Kitromilides ('The Enlightenment in Southeast Europe: Introductory considerations'); the other author being Miroslav Hroch ('National Romanticism' in vol. ii), Maria Todorova ('Modernism', vol. iii), Sorin Antohi and Balázs Trencsényi ('Approaching Anti-modernism', vol. iv).

The crucial problem encountered by the editorial team and the authors was the selection of the original sources. It is a difficult task now to assess how apt their decisions have turned out to be. First, the assumption would have to be made that the reviewer is competent enough to express his opinion with respect to all the countries concerned – a conviction that would be overoptimistic, at least in my case. Second, even an extremely capable polyglot reviewer would come across a snag when having to choose but one or two items per issue out of the material available for the given country, given the inevitable limitation of the volume's size (with over 2,000 pages in total, the series being quite extensive anyway). Such selection is somewhat easier when dealing with early sources as they are relatively scarce and the decisions with respect to them may, in some cases, be obvious; yet, the task becomes more complex as one moves toward our day. Altogether, I personally do accept the choice made – stressing, once again, that it is hard to offer an authoritative appraisal in this respect.

It is not easy, either, to evaluate the introductions to the issues tackled in each of the series' volumes. Prepared by outstanding experts, these essays interpret the research's state-of-play and offer their authors' points of view (still, let us emphasise, the authors do not propose their views uncompromisingly, sometimes just posing questions). To dispute against these viewpoints would mean to start a debate regarding the elementary issues which, needless to say, would remarkably exceed the confines of this review, probably leading to no final conclusion. Let me refer, instead, to the essay written by Sorin Antohi and Balázs Trencsényi, as certain doubts stem from it with respect to the methodological assumptions behind the project and publication.

One of these assumptions becomes apparent through the titles of the volumes, linking certain cultural formations with the consecutive phases of

emergence of collective identities. Among them are arguments well established in the research tradition and arousing no controversy whatsoever: it would be hard to deny that the shaping of a 'national idea' is strictly connected with the late Enlightenment age; similarly, the association of national movements with the romanticist current arouses no concerns. Problems come out with the appearance of 'modernism', on which Maria Todorova ponders penetratingly. This author excellently demonstrates the notion's fluidity and ambiguity; however, her essay would not tell us strictly in what (a) way(s) 'modernism' is to be understood throughout the publication. Todorova presents her own views ("… I was asked and agreed to provide a preface to a volume I did not help conceive", she declares; cf. vol. iii/1, p. 4); as to the stance assumed by the authors/editors, she confines herself to stating that the series chronologically situates modernism between the 1860s and the first decade after World War I, as – to the team's minds – an expression of civilizational optimism (vol. iii/1, p. 6). While I can accept that striving for a precise definition would not be much fruitful at this point, a more to-the-point explanation would have been welcome all the same. All the more that in their introduction to vol. iv, Antohi and Trencsényi do not resume the way(s) in which modernism would be comprehended but instead readily pass on to a broad discussion on 'anti-modernism'. While 'modernism', with all the ambiguity of the notion, seems of use to me in categorisation of the material processed for the project's purpose, 'anti-modernism' does trigger some concerns.

The authors of the introduction build an extensive conceptual instrumentation, trying to persuade the reader that the model of 'anti-modernism' they have elaborated does exist; in parallel, they emphasise that it is often hard to discern against 'modernism'. Would it not perhaps be better had they quit this distinction at all? "If it looks like a duck, swims like a duck, and quacks like a duck, then it probably is a duck": would not an approach like this – somewhat ingenuous, perhaps – prove reasonable, instead? Let me simplify: I guess that the project makers and publication editors wanted to separate the morally 'good' from the 'evil', which is not a unique attitude. Seen in this way, 'anti-modernism' would be the 'dark side' of what is modern. I do not challenge the need to discern between the different, positive and negative, aspects of modernity: in situations like this one it is difficult to shun valuation; yet, I do not opine that discernments of this kind ought to form the basis for the construction of such a publication, particularly if they are ascribed a chronological meaning, with some 'good' versus 'evil' epochs being created. Is whatever is called 'anti-modernism' to be recognised as a product of 'decayed modernity' – or, at least, of a 'modernity in crisis'? The answer is: yes, to an extent. Still, modernity, let us remark, remains under permanent crisis, all throughout its history (whenever it once began): this being standard with modernity, if not, bluntly, the essence of it. Singling out periods of 'crisis' in the history of culture is, to my mind, a dubious undertaking. What

inclines one to such marking off is, as I believe, a confidence that intensifying social and political conflicts, dissemination of violence, and mass crime all have stemmed from some mental, intellectual and moral turbulences and breakdowns. While this opinion is apparently not completely erroneous, it could also be said that the rebarbative and, at times, terrible phenomena occurring in periods deemed 'evil' have in many a case appeared as a consistent follow-up of things occurring in 'good' times – the things that enjoy good reputation in our day too. This makes me doubt whether the title of vol. iv, evoking 'radical revisions' of collective identity, is apt enough. There certainly have been revisions, but 'stories continued' have occurred alongside them. In brief, I would prefer that, based on the three last volumes, the reader may gain the conviction that s/he has encountered diversified manifestations of modernity, rather than two separate cultural formations and eras: a 'better' and a 'worse' one.

This would do for my objections, which are not at all meant to diminish my appreciation of this publication (it would be awkward should a publication like this, designed with a breadth, trigger no objection). I am confident that the series will satisfy its projected objective, facilitating to readers from a number of different countries access to knowledge on the central and south-eastern part of Europe.

trans. Tristan Korecki *Tomasz Kizwalter*

Andrzej Chwalba, *Samobójstwo Europy. Wielka Wojna 1914–18* (*Europe's Suicide: The Great War 1914–18)*, Wydawnictwo Literackie, Kraków, 2014, 642 pp., bibliog., index of names

One could see a book like *Europe's Suicide* coming. Thirty years had passed since the last Polish synthesis of World War I, written by Janusz Pajewski, saw print[1]; new research has emerged and new ways of writing history have been forged. As the author rightly points out in his introduction, in Poland the conflict was always overshadowed by World War II, consistently robbed of its 'greatness' and attracting only limited interest from Polish historians until the twenty-first century. Clearly, this state of affairs differed markedly from the situation in the west of Europe, where every decade saw hundreds of new monographs on World War I published, attracting growing numbers of readers to the subject.

Without a doubt, this state of affairs was caused for the most part by the marginal position of World War I in a vision of history focussed on the

[1] Janusz Pajewski, *Pierwsza wojna światowa 1914–18* (Warszawa, 1991) (the author finished the book in 1987).

Polish nation. It seemed almost like a piece of exotica of scant significance for Poles up until 1918, when the so-called Polish question – the question of the rebirth of the Polish state – demanded an answer. Furthermore, until 1989 World War I could only be seen through the fog of Communist propaganda, for which the conflict served as little more than an overture to the Russian Revolution. The fall of Communism, on the other hand, shifted the spotlight onto the Polish-Soviet War of 1920, previously a proscribed subject. This situation altered only about a decade ago: the rising interest of Polish historians in the conflict of 1914–18 finds reflection in a series of translations of popular foreign syntheses and an explosion of local histories.[2] The latter is credited with the restoration of dozens of war cemeteries, monuments, and plaques at the battlefields (mostly those of 1915). This wave presumably peaked in 2014 with events commemorating the hundredth anniversary of the outbreak of the war (though 2018 is still ahead). The book by Andrzej Chwalba appears to belong to the same trend, and its author, a professor at the Jagiellonian University, is particularly equipped to put forward a synthesis addressed to an audience extending well beyond the specialist readers who have long enjoyed easy access to translations of numerous foreign works. The author's pursuit of a synthetic perspective is even more apparent in an abridged version of the book that saw print in English in an elegant translation by Teresa Bałuk-Ulewiczowa under the title *1914–1918: An Anatomy of Global Conflict*.

Rather than altering the image of the conflict painted by contemporary research, Chwalba seeks to organise and sum up its findings – still a gargantuan task given the enormous breadth of both popular and specialist publications on the subject. Specialist readers are thus more likely to nod in assent than shake their heads in surprise while reading. The prime virtues of *Samobójstwo Europy …* seem to be common sense, a wealth of anecdotes, and a wariness of extravagant interpretations or controversial questions. If there is anything one could hold against the work, it would probably be the insistent focus on the progress of the military conflict, while social issues – though addressed, as the dominant tendencies of historiography of the past few decades require – seem to have been given merely a cursory glance. This is particularly visible in the Polish edition of the work, where the impact of war on civilian life is discussed in only a single chapter (little over 10 per cent of the entire text). Still, this may be a consequence of a conscious effort to restore the proper balance: Chwalba's book puts military action and the

[2] Among others: Ian Beckett, *Pierwsza wojna światowa 1914–18*, trans. Rafał Dymek (Warszawa, 2009) [orig. *The Great War* (London, 2001)]; Paul Ham, *1914. Rok końca* świata, trans. Adam Tuz (Warszawa, 2014) [orig. *1914: The Year the World Ended* (London, 2013)]; Jean-Baptiste Duroselle, *Wielka Wojna Francuzów 1914–18*, trans. Agata Ciastek (Warszawa, 2006) [orig. *La Grande Guerre des Français, 1914–1918* (Paris, 1994)].

fate of soldiers in the foreground, and the lives of civilians on the margins precisely because such was the nature of the conflict. In contrast to World War II, the victims of the years 1914–18 were predominantly soldiers, while politicians bowed to generals instead of shaping the contours of the conflict.

The history of politics and diplomacy, a fairly dominant feature of Polish research on World War I during the Communist era, is relegated to a tertiary role here. Aside from reasons of methodology and conditions of research, this interest clearly derived from a certain determinist streak: in the Polish optic of the period, the Great War figured primarily as a piece of the Great Play that led inevitably to Polish independence and the October Revolution. Chwalba successfully avoids this pitfall – his main interest is war itself. He devotes much of his attention to the technical and organisational side of the conflict: new types of weaponry, new means of war-making, the morale of soldiers and civilians alike, logistics, supplies, the scientific and technical capabilities of the warring nations. What may be found wanting is the short shrift given to questions of long-term effects of the conflict: the birth of Communism and Fascism and of nation-states in Central Europe, the Great Depression, the other World War, the impact on the 'lost generation' and European culture. The author merely hints at these issues in Chapter Eight, where he addresses the changing attitudes and status of the 'minor' nations – from Ireland to Finland, Poland, and the Balkans.

On the other hand, Chwalba devotes an entire sub-chapter to the ferociously disputed question whether war could have been averted – a problem he resolves by recalling the unequalled extent and vigour of preparations for war across the world, or at least in Europe. Among the book's virtues, not only from a Polish perspective, is clearly the author's consistent interest in the events on the Eastern Front, and particularly in the Kingdom of Poland and in Galicia. While it may seem to be a calculated effort to gratify the Polish reader, one might also see it as a symptom of a broader tendency: the growing realisation that the war of position in the trenches of northern France and Belgium was hardly the only – and not always the major – theatre of war in 1914–18.[3]

Samobójstwo Europy … is evidently designed for a broad audience – a plausible move given that a ground-breaking synthesis of World War I is an almost inconceivable notion today, and also because the numerous readers of historical books enjoy the subject of war as none other (even though no military conflict can surpass the popularity of the one in which the Nazis were involved). These assumptions put the question of style in the foreground, and here lies undeniably the greatest virtue of the work, as well as a certain conundrum for the inquisitive reader. Andrzej Chwalba writes in a vivid

[3] On this subject, see the first volume of: Włodzimierz Borodziej and Maciej Górny, *Nasza wojna*, i: *Imperia 1912–1916* [*Our War: Empires, 1912–16*] (Warszawa, 2014).

and colourful tone, outclassing the vast majority of his Polish colleagues. Short, clear sentences and impressive codas remind one of English-language best-sellers. Nonetheless, a careful reading proves that the author's penchant for clear and powerful claims often leads him astray into self-contradiction or explaining the obvious. While informing of the growing demographic advantage of Germany over the Allies in the decades leading up to the war, the author explains that it resulted from the fact that "more children were born in Germany than in Great Britain, and particularly France" (p. 70). The nomination of Joseph Joffre as the Commander-in-Chief receives the following commentary: "The choice was most fortunate since 'papa Joffre' counted among the finest generals of the French army" (p. 99). When describing the Schlieffen Plan (p. 64), he talks in the same paragraph of its minute attention to detail "cut to fit the mentality of the German officer and soldier", who were expected to conduct specific operations "with strict time-keeping", and that its virtue was to give the leaders on all levels "a significant amount of freedom". Since the book is devoid of notes, as is the norm with such broad syntheses today, it is impossible to identify the source of some of the thousands of bits of data mentioned by the author. Yet, certain errors must have been committed (e.g., it is hardly conceivable that German warships sunk 65 Spanish vessels of a combined tonnage of one million tons during the war, as is claimed on p. 483, since that would make the vessels in question the largest to have sailed on the high seas at the time). Nevertheless, the responsibility for such inconsistencies lays largely with the editors who also missed a number of grammatical errors – sadly a standard even at the biggest and supposedly best Polish publishing houses today.

All this does not alter the fact that *Samobójstwo Europy* … fulfils all requirements of the genre, is typified by a colourful and vivid narration seldom found in Polish historiography, and will undoubtedly long remain a standard Polish synthesis of the history of World War I among students and amateurs alike.

trans. *Antoni Górny* *Adam Kożuchowski*

Benjamin Conrad, *Umkämpfte Grenzen, umkämpfte Bevölkerung. Die Entstehung der Staatsgrenzen der Zweiten Polnischen Republik 1918–1923*, Franz Steiner Verlag, Stuttgart, 2014, 382 pp., maps, indexes; series: Quellen und Studien zur Geschichte des östlichen Europa, 84

Benjamin Conrad, a young German historian, set an ambitious goal for himself: recapitulate, complement, and update the present knowledge on the process of diplomatic determination of the frontiers of the reborn Polish

state. Abundant literature, in multiple languages, has accrued around the topic in question and has recently been complemented with new publications of relevant sources.[1] As a result, even without in-depth archival queries there is material available based upon which a synthetic study can be written. Still, the reading of *Umkämpfte Grenzen* … makes the reader clearly positive that the task was not an easy one. Rather than stemming from scarcity of information, the difficulties involved were based on excessive information and lack of objectivism. The question of post-war borders of East Central Europe became the subject of fervent propaganda and para-scientific activity still before they were finally established. Loyal to this tradition of 'committed history' were Polish and German historians (as well as their Soviet, Czechoslovak, Lithuanian and Ukrainian-emigrant peers) in the decades that followed. In the cacophony of various opinions, often contradicting (with nearly all of them categorical), clear-headed and well-balanced evaluation is not an easy thing to develop. It is with considerable self-assuredness, supported by his versatility in the literature in several languages of the region concerned, that the Mainz-based scholar strives for such an appraisal.

His book has seven chapters, a summary and a few annexes facilitating orientation in the rather complicated matter of territorial and political contests: a list of Prime Ministers of the Republic of Poland and maps of various frontier sections. The narration initially runs chronologically. The introductory section, discussing the basic notions, research state-of-play and, in a nutshell, the nineteenth-century history of Polish lands, is followed by two chapters on the borders of Poland as conceived and debated during and immediately after World War One. The subsequent sections, arranged by topic, discuss the circumstances of settling and setting the post-war frontiers – first, in the west and south-west of Poland and afterwards, in the country's east and north-east. While the disputes and bargaining involving diplomats is the focal area, well-informed descriptions on hostilities and the local determinants of the plebiscites are included as well.

The author has sought to verify some historiographical 'set claims' and to set the subject-matter in a broader regional perspective – as proves fairly successful in both. A strong point of Conrad's study is undoubtedly the clarity with which complicated occurrences are presented. The author's consistent carefulness in balancing the reasons and arguments of the parties engaged is particularly respect-worthy. The stances assumed by the national movements and Polish political representations in the course of the war are usually seen within the context of the home policies of the conflicting powers and the aspirations of other national movements (Ukrainian and Lithuanian in particular).

[1] The recent source publications worth mentioning include the very large series entitled *O niepodległą i granice*, prepared by a team of historians associated with the Aleksander Gieysztor Academy of Humanities in Pułtusk.

The Jewish question, otherwise long tackled by German historiographers[2], now appears missing in the picture: albeit the frontier problem was not quite high on the agenda in this particular case, Polish-Jewish relations formed a rather serious argument, which in most cases spoke (to be honest) against the territorial aspirations of the Republic getting rebuilt.

Consideration of the broader context of border resolutions produces the best effects in the sections on plebiscites in the contentious territories involving Germany and Czechoslovakia. Conrad very convincingly argues that the Polish authorities, involved in the armed conflict against Bolshevik Russia, deliberately sabotaged the plebiscites. These actions were based, in fact, on reasonable calculation. As the author remarks (somewhat exaggeratingly, perhaps – at least with respect to the Cieszyn/Teschen Silesia), the Wilno/Vilnius Land formed the only potential plebiscite area as to which the Polish party could be certain of their victory. In the other cases, the chance was small or quite uncertain, with the instability of the young state diminishing it considerably. Conrad has put special emphasis on the plebiscite in Upper Silesia. In reference to his earlier findings, he points out that the argument whereby the Silesians arriving from other regions of Germany exerted the decisive influence on the outcome of the voting, which can still be encountered in Polish historiography, is not veracious.[3] Similarly, Conrad rejects several other theories that charge off the unpopularity of the idea of affiliation to Poland in the contentious areas to the rules of voting being more advantageous to Germany.

The other instances where the author faces up to the polemics with some Polish researchers are of a lesser gravity. A sceptical attitude toward Józef Piłsudski's federalist idea is close to certain Polish historians (to name Andrzej Nowak, for instance) as well. More interesting seems to be the polemic against the opinion, predominant among Polish historiographers, about the stance presented by the British delegates at the Paris conference. While Conrad admits that Lloyd George himself was oriented against Poland, he remarks that the British team basically identified Polish-speaking people as Poles – a view which in many aspects was close to the position of the Polish negotiators. There is nothing left but to agree with the author's suggestion that the harsh appraisal of the Britons as prevalent among Polish historiographers stems, to a considerable extent, from juxtaposing their assessments of the ethnic situation, particularly in the Polish-German borderland, with the maximalist postulates advocated by Roman Dmowski.

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[2] I mainly refer here to the old, largely disputed and incessantly useful study by Egmont Zechlin, *Die deutsche Politik und die Juden im Ersten Weltkrieg* (Göttingen, 1969).

[3] Benjamin Conrad, 'Die Fälschung einer Niederlage. Zur Rolle der Heimkehrer in der Oberschlesien-Abstimmung', *Inter Finitimos* ix (2011), 103–18.

With the territorial postulates of the other parties to the conflicts in East Central Europe taken into account (rather than solely Polish aspirations), the attitude of the British delegates may appear accommodating, rather than confrontational.

The strong points of the study under review are the best visible in the moments where the author's knowledge and common sense have allowed balanced and logical conclusions to develop with regards to certain politically sensitive issues. Attempts at general conclusions founded upon specific details are more problematic in this particular study. It is hard to ignore, for instance, the author's somewhat inconsistent attitude toward the issue of ethnic identities. For one thing, he emphasises how complex and hard-to-appraise the matter is, particularly with respect to ethnic borderland; he realises the inadequacy of plebiscite questions, incongruent with the complicated matter of identity. On the other hand, however, especially with respect to East Prussia and the other territories contentious between Poland and Germany, Conrad attaches great attention to the official ethnic (or, linguistic, in this case) statistics. As it seems, the author's assumption (apt, to my mind) that the connection between language and identity was, at most, loose should have inclined him to a more critical stance towards the cognitive value of the official censuses. Even if the latter strictly reflected the facts (which is always dependent on how the questions are worded and in what ways the circumstances affect the polled), their relevance for considerations on ethnicity or nationality is infinitesimal.

The study's critique of the earlier Polish historiographers who ascribed Polish identity to virtually any ethnic group using any of the varieties of Polish does not seem to be legitimate in all aspects. Conrad observes that the outcomes of the plebiscites became the most explicit counterargument against the maximalist territorial postulates. This is certainly true, except that – as this same study repeatedly emphasises – these results should not at all be completely equated with the ethnic option of the voters. The author ignores the influence made on individual decisions at the voting booth by the actual political situation. I am not so much concerned, at this point, about the insistence exerted directly on the polled to declare the German nationality: the disputable issue of whether the plebiscites' outcome may at all (and, if so, to what an extent) be regarded as a declaration of a specified national awareness. I should think that there were other incentives too which influenced the voters' decisions, depending on the current occurrences and individual worldview. Some might have chosen not between the nation-states but between the known and fairly stable reality of the Reich versus a completely uncertain future offered to them by what was to become Poland – a country only getting delivered in pain. Clearly, as is the case with the aforementioned linguistic statistics, the author has again not followed his own assumptions.

This inconsistency possibly stems from Conrad's superficial approach to the nation-forming processes. The one and only footnote mention of the compulsory classic reading (Benedict Anderson, Ernest Gellner, Miroslav Hroch), made in the introductory section, could have sufficed, had the frontier been the sole topic of this study. Since, however, the inhabitants living within the borderlines is immanent to the problem, an in-depth afterthought on how these people's identities got shaped would have well served the argument proposed. By the way, another classical author, Józef Chlebowczyk, has employed such a perspective with respect to a similar issue. This author of a study on 'the right of small and young nations to exist' focused in his last published remarkable work on the shaping of the borders in East-Central Europe.[4] It can be regretted that *Umkämpfte Grenzen…* has not taken advantage of it.

A corresponding charge can be phrased with respect to the scientists' involvement in the political propaganda associated with the diplomatic struggle for the borders (a motif secondary to the narrative). The statement claiming that a coincidence between the territorial programme of the outstanding geographer Eugeniusz Romer and the ideas of Roman Dmowski means that the former rejected the principles of scientific objectivism is apt, to an extent. Yet, it oversimplifies the involvement – commented on by Conrad – of the period's scientists, especially geographers, in the political wrestling for ethnically-delineated territories during and immediately after World War One. Romer was not an exceptional case-in-point: conversely, he was a typical exponent of a very large international group of scholars who deftly combined formal scientific rectitude with political bias. Romer excelled not so much in terms of political involvement but, primarily, with his scientific professionalism.[5] The alternative 'science or politics' is simply irrelevant with the time and the problem in question.

Similarly to ethnic, national (or, regional) identities in the contentious territories, the phenomenon of political involvement of science is hard to describe in unambiguous terms, based upon numerical data or archival material. Both of these issues exceed the subject-matter of the study under discussion which aims – let us repeat – at verifying the facts related to the shaping of the borders of the Second Republic of Poland. This particular assumption has mostly been very well satisfied by the author. The inaccuracies, scarce and rather formal than substantive as they are, include a dozen-or-so linguistic errors, repetitions or misprints spread across the

[4] Józef Chlebowczyk, *Między dyktatem, realiami a prawem do samostanowienia. Prawo do samookreślenia i problem granic we wschodniej Europie Środkowej w pierwszej wojnie światowej oraz po jej zakończeniu* (Warszawa, 1988).

[5] Cf. Glenda Sluga, *The Nation, Psychology, and International Politics, 1870–1919* (Houndmills, 2006).

study. These minor defects do not diminish the value of this quite reliable and well-ordered report on the few years that became key to the shaping of the political frontiers in East Central Europe.

trans. Tristan Korecki *Maciej Górny*

Michal Pullmann, *Konec experimentu. Přestavba a pád komunismu v Československu* [The End of an Experiment. Perestroika and the Fall of Communism in Czechoslovakia], Scriptorium, Praha, 2011, 243 pp.

This study by the Prague-based historian specialising in the twentieth century is important not only due to its scientific and scholarly values but also with respect to the impact it has made on the public debate in the Czech Republic, the author's home country. Right after it came out in 2011, the book became gravely criticised, mainly by right-wing publicists. A fairly large group of Czech historians and sociologists sprang to the author's defence, which triggered, again, critical counteraction and a response to it, all this ensuring this well-documented (and not quite easy-to-read) book a rather unexpected publicity.

For an external observer, the reasons behind the controversy do not seem quite clear at first glance. Following the track of anthropological research on the language and everyday realities of the 'real socialism' (Alexei Yurchak's brilliant analysis of the language of the Brezhnev era in the Soviet Union having been an important source of inspiration[1]), Pullmann regards the process of transferring the vocabulary of the Perestroika from Gorbachev's Soviet Union to the 'normalised' Czechoslovakia. He starts from describing the discussions pursued by the reform-inclined economic counsellors of Andropov. Squashed for a short time during Chernenko's tenure, these debates revived once Gorbachev came to power. As Pullmann believes, they contributed to 'unfreezing' the thinking about the ruling system. Its deficiencies were no more chalked up to 'temporary difficulties' or sabotaging actions of some demoralised individuals. The debate proposed, instead, an insight into the system itself. The idea, absolutely inadmissible until right before then, that certain deficits might possibly be intrinsic to the real socialism was originally considered within teams of experts, and brought then on to the public forum. The effects of such reasoning proved revolutionary: once the limitations were found to be essential about the system, it was necessary to alter the system, if they were to be removed.

[1] Alexei Yurchak, *Everything Was Forever, Until It Was No More: The Last Soviet Generation* (Princeton, 2006).

The subsequent chapters analyse the Czechoslovak reception of the Soviet discussions: they came across an extremely modest interest, also in comparison with East Germany or Bulgaria, the regimes known for their reluctance to reform. This continence in deriving benefits from the Soviet model was due to the communist party leadership team's concern that mentioning a 'reform' (not even suggesting that a change might be carried out) would perforce evoke the memory of the Prague Spring. For Gustáv Husák's regime, such a course of things would imply a loss of legitimacy which had originally been based on the rejection of Alexander Dubček's idea of 'socialism with a human face'. The influence of the Perestroika on Czechoslovakia was moreover lessened by the manning decisions of the Soviet leaders: instead of placing a bet on the fairly open-minded group of communist activists grouped around the Lubomír Štrougal (Prime Minister till 1988), Gorbachev thought it right to have the party hardliners, with Miloš Jakeš as their epitome, represent his team in this 'fraternal country'. As a result, the interests of the Soviet team often appeared contrary to those of its Czechoslovak counterpart. While the pressure exerted by Moscow on Czechoslovakia to launch an agenda of economic and political reform was not overtly resisted in Prague or Bratislava, the Czechoslovak Communist Party (KSČ) spared no effort to prevent any actual reform from occurring.

As it soon after turned out, the conservative attitude of the Czechoslovak communists was based on fairly reasonable premises – as is proved by the discussions accompanying the first, modest attempt at reforming the system: the bill on State-owned enterprise drafted in 1987. Pullmann cleverly picks up on the deeper meaning behind the contributions to the debate made by managers, employees, economists, and party activists. Although the focus was limited to crew management, the debate quickly turned into a worldview contention revisiting the fundamental questions of equality, competition, or complete employment coverage. Ideas appeared in the official circulation hardly reconcilable with the previously predominant 'real socialist' tenets. In spite of the party leaders' attempts to countermine the trend, the sphere of open discussion became expanding – from economy to culture and social issues.

The legitimation crisis coincided with the developments that upset the foundations of the local regime. Firstly, as Pullmann shows, the late eighties saw a clear suppression of the sense of class identity and prestige of workers. The language of liberalism, which explicitly penetrated the official debate on the need for economic reform and paved its way to the worker milieu, was useless in a discourse on social justice or workers' rights. Secondly, these very years witnessed the growing in strength of the social groups whose attitude toward the socialist state was mostly detractive or, at best, indifferent: youth subcultures, ecological movements and the dissident movement (the 'democratic opposition'). All these phenomena, along with the (well-informed) sense of abandonment by the 'Big Brother', who was

preoccupied with himself, gravely affected the confidence of the communist personnel. A collapse of the hitherto-prevalent standard language attests to the insecurity of those in power at the time: Pullmann quotes a dozen racy examples of the capitulation of the 'language of the normalisation' in face of a pluralistic parlance ('pluralistic' also being an instance of breach) which went as far as using elements of the dissident vocabulary. Finally, almost every step the authorities made started turning into a prestige failure (primarily, in symbolic terms). The attempt at including some aspects of the heritage of the interwar Czechoslovakia in a 'progressive tradition', epitomised by the official celebration of the Independence Day on 28th October 1988, aroused – instead of acclaim – common complaint about the shops being closed due to the bank holiday (whose date fell right before a weekend). The resumption of strict discipline and force crackdowns on the opposition increased the popularity of those described as 'enemies of the socialist system', instead of pacifying the society. The personal experiences of members of the country's leadership team – almost all of whom joined the 'hard-hat' faction in 1968 – implicated for them disturbing associations with the Prague Spring. As Pullmann observes, this analogy triggered panic attacks, making Jakeš in his talks with Gorbachev in April 1989 ready to jeopardise the vested economic interests of his country, in exchange for a guarantee that the USSR would not alter its official interpretation of the 1968 events.

Yet, the ever more desperate endeavours to preserve the status quo could not save the regime anymore. As soon as the summer holiday of 1989 ended, a recording of Jakeš's compromising speech to local party activists of the city and region of Plzeň ran round the country. Incoherent and unclear manner of expression, intellectual shallows and vulgarity of the leader had a serious negative impact on the image of the ruling party as a whole. With the 'emperor' suddenly appearing to 'have no clothes', the system lost its gravity and dignity. The belated resort to force came as the final nail in its coffin: the clubbing of the students in Prague by the militia forces in mid-November 1989 came across widespread outrage among the 'ordinary' citizens, indignant with the illegitimate act of violence.

The story of the senile decay of the real socialism in Czechoslovakia, interestingly and competently recounted as it is, would probably arouse no deep emotion if not for the three key arguments proposed. The first is about the character of the communist system, with a focus on the reasons behind its long-lasting stability. Following the researchers of the history of the GDR[2], Pullmann identifies the foundation for the real socialism not as

[2] The major point of reference for Pullmann is Martin Sabrow, the author who uses an ambiguous notion of 'consensus dictatorship' with respect to the former East Germany (GDR). The term is applied with respect to a portion of the local society taking active part in the shaping of the communist rulers' policies as well

bare violence but as a consensus about the language. As long as both 'parties to the contract', the authorities and the society, accepted certain rules with respect to the (lack of) meaning and 'appropriate' use of words and phrases, the scheme remained stable. The Perestroika upset the balance and opened the way for discussion on the basic notions and ideas. The Czechoslovak specificity consisted in the fact that the 'linguistic tabooism' was stronger there compared to the other Eastern Bloc countries. Critical consideration of notions of socialism, society, or democracy implied the risk of recurrence of a Prague Spring, whereas the local communist party elite have built their legitimacy to rule upon rejection and negation of the ideals associated with that brief episode of liberalisation and reforms.

Logically, such a regard of the Czechoslovak normalisation implied a resolute rejection of the so-called totalitarian paradigm. While this stance would not seem much revolutionary in international historiography of communism, it became one of the reasons for the controversy over Pullmann's book in the Czech Republic. A picture of the system which, albeit superimposed, has become sustainable owing to the collaboration and contributions from the subjects, rather than solely through oppression and control, is no doubt more disturbing than a clear-cut division into the rulers and the ruled. Pullmann goes further on to draw more conclusions based on his ascertainment. He namely proposes a finding concerning the role of the democratic opposition. Since the system was based upon a sort of a contract (which, by definition, calls for action on both sides), the society could not possibly have been 'liberated' from the communist yoke. Instead, as testified by the above-summarised analysis of the semantics of the late real socialist era, the consensus eventually broke down and, consequently, the authority was delegitimised. Rather than having propelled the process, the dissidents have benefited from it, Pullmann remarks. The moment the system collapsed, they formed the optimum alternative elite at hand, capable of filling the emptied space of power.

However, the undersigned finds the most interesting yet another statement proposed by the Czech author in the conclusive section. Upon closing the history of the fall of the real socialism in Czechoslovakia, Pullmann takes the reader for a short ride outside the actual framework of his study, pondering what namely has come in the place of the former linguistic consensus of the normalisation. In the first years after the breakthrough, he notices, a number of ideas and ideological options appeared in the public space, indeed. Moral issues were disputed; human rights formed the foundation

as to several milieus taking over the controlling function with respect to themselves. Sabrow has most completely illustrated his concept in his description of the milieu of East-German historians in his book *Das Diktat des Konsenses. Geschichtswissenschaft in der DDR 1949–1969* (München, 2001).

of thinking about politics; ecological movements were reanimated; interest in spirituality and popularity of Churches increased – the latter never being patent in the specific Czechoslovak circumstances. All this, however, gave way in the former half of the 1990s to a new consensus which operated quite similarly to its former counterpart; this time, Pullmann remarks, the consensus was a neoliberal one. Compared to the communist analogue, its scope of emphasis was somewhat different, with new slogans – 'market', 'hard work', 'individual development' and others – replacing the old ones. The ritual function of words, used not to describe but to conjure the reality, to mark novel social divisions, hierarchies and borderlines, has remained unbroken.

Someone who watches from the outside the disputes presently involving Czech historians and publicists or commentators will probably find this particular statement inspiring – primarily because its importance exceeds the particular case of Czechoslovakia. The continuity between the last years of the communist regimes and the political transition period is the trail more and more frequently followed by scholars specialising in the recent history of East Central Europe – one most recent example being Philipp Ther's book (discussed in latest volume of *Acta Poloniae Historica*).[3] In simplified terms, combining the stances of the different authors into a logical sequence, one may say that the political system transformation turned as radically neoliberal as the social resistance against the change allowed; the more atomised the society was, the weaker the resistance. This 'individualisation' has been part of the real socialism's heritage. In Poland, it was historically caused by the destruction of the first 'Solidarity' movement and the martial law. In Czechoslovakia, where the workers' rights were better protected and the unemployment appeared less annoying, the phenomenon was deeper rooted, reaching back to the traumatic events of the summer of 1968, when the enormous explosion of public activity was quelled by the armed intervention of the five Warsaw Pact countries. Not only does *Konec experimentu* … record the process of decomposition of a consensus but it also provides the material to illustrate the rules that governed the individualised society of the declining real socialism.

Given the gravity and seriousness of the questions raised in this book, the cautiousness shown by the author when it comes to formulating general conclusions comes as a surprise. First, I do not find the reservation that his analysis is limited to the Czech lands, rather than extending to Slovakia (where the last years of the communist regime were different in nature), fully convincing. Apart from the fact that Michal Pullmann is a Slovak-born historian and does not shun quoting Slovak sources, such territorial limitation of his discourse seems overcautious. It is a pity, too, that an author so well versed in the history of the late Soviet Union as well as of the other Eastern

[3] Philipp Ther, *Die neue Ordnung auf dem alten Kontinent. Eine Geschichte des neoliberalen Europa* (Berlin, 2014).

Bloc countries did not embark on more frequent excursions outside Czechoslovakia. The book virtually lacks any mention of some issues of paramount importance for the delegitimization of the system, such as the Chernobyl catastrophe. The reader would not find out what discussions, if any at all, took place in Czechoslovakia about the occurrences taking place in Poland, where a partly free parliamentary election, with the triumphant democratic opposition, took place in June 1989. Absence of these, and other, topics may be owed simply to the content or condition of the sources Pullmann has used, thus indirectly testifying to Czechoslovak public opinion having been cut off from the outside world, in the reality of a peculiar 'petty-bourgeois communism' shared by Czechs and Slovaks. But even though, a note of this fact ought to have been made.

With these objections voiced, the reader should bear in mind that *Konec experimentu …* is, in fact, a case study. While the conclusions stemming from this particular analysis could be used in broader conceptualisations of the history of the recent transition in East Central Europe, a synthesised take of this sort is certainly not compulsory for the author. In reward, Michal Pullmann has offered the reader quite a lot – an insightful study of the powerfulness and decay of 'Czechoslovak-style' newspeak.

trans. Tristan Korecki *Maciej Górny*

Andrzej Brzeziecki, *Tadeusz Mazowiecki: biografia naszego premiera* [Tadeusz Mazowiecki: Biography of Our Prime Minister], Społeczny Instytut Wydawniczy Znak, Kraków, 2015, 608 pp.; Roman Graczyk, *Od uwikłania do autentyczności. Biografia polityczna Tadeusza Mazowieckiego* [From Involvement to Authenticity: Political Biography of Tadeusz Mazowiecki], Zysk i S-ka, Poznań, 2015, 472 pp.

Can biography and biographical exploration be an effective tool for historical inquiry? Biography as a genre is still often considered as a secondary and somewhat lower medium of history writing. By tracking and engaging in individual life stories, the argument goes, biography is intrinsically constrained in its scope, methods and analytical depth. From this perspective, the political or historical study of peoples' lives has the aura of a less scientific form of history, written by less skilled historians, reaching for a wider audience beyond professional historians. The supposed reduction of historical complexity and the lack of professional historical analysis and skills tied to the genre of biography is thus seen as glossing over the broader historical context in which the lives in question unfold.

Despite making some valid points, this critique of biographical history was confronted with some formidable challenges in the course of the 1970s and a growing interest in biographical practice ever since. This shift, in turn, was associated with the emergence of feminist history and women's history in 'the West'. The growing interest in the lives, experiences and self-understanding of prominent women was driven both by the need to retrieve forgotten life histories and the need to correct the epistemic bias of traditionally male-centred historiography.[1] Adding to this trend, the 1990s witnessed the development of an intellectual tendency among academic historians which came to be known as 'the new biography'. This approach applies an analytical framework to the study of life stories informed by postmodern sensitivity to detail and micro-narratives, as well as intellectual scepticism of traditional and institution-focused viewpoints. The biographical practice related to 'the new biography' is openly and clearly influenced by other disciplines such as literary criticism, for which culture and 'the self' are never coherent or essential but context-relative and embedded in linguistic practices (such as speech acts, discursive formations, etc.). For the historian involved in 'the new biography' this re-evaluation of basic assumptions has direct consequences for his or her research methods and, inevitably, their outcomes. Since lives and identities are always multiple and non-monolithic, historical inquiry into individual or group biography should emphasize, or at least take into account, this constantly shifting dimension of life. 'The new biography,' thus, prompts a re-conceptualization of biography as historical narrative, engaging readers with the subject by weaving the private, intimate and personal together with the public and political.[2] Put differently, whether as a part of 'the new biography trend' or not, the study of an individual life can illuminate other, hitherto unknown aspects of 'known' and well-researched historical events and processes.

Yet, are these developments in mainly English-speaking academia to be easily applied to contemporary Polish historiography and history writing? One might think so, given the growing significance of biography in current Polish

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[1] On feminist biography and the history of feminism in Great Britain, see Barbara Caine, 'Feminist Biography and Feminist History', *Women's History Review*, iii, 2 (1994), 247–61. For a prominent example of growing interest in the personal and intellectual lives of prominent women, see Janet M. Todd, 'The Biographies of Mary Wollstonecraft', *Signs*, i, 3 (1976), 721–34.

[2] For this view, see Jo Burr Margadant (ed.), *The New Biography: Performing Femininity in Nineteenth Century France* (Berkeley, CA, 2000); for the history of biography as a genre and its links to 'the new biography', see Laura Marcus, 'The Newness of "The New Biography": Biographical Theory and Practice in the Early Twentieth Century', in Peter France and William St Clair (eds.), *Mapping Lives: The Uses of Biography* (New York, 2002), 193–218.

discourse where it proves successful in bridging complexities of the social and political lives of post-1989 public figures, in the People's Republic of Poland.

In the pantheon of the struggle for freedom in the People's Republic of Poland Tadeusz Mazowiecki (1927–2013) occupies a special place. Thanks to two recent biographies of the first post-1989 prime minister of Poland readers fluent in Polish can now learn more about the life of this exceptional post-war intellectual and politician. While both Andrzej Brzeziecki's *Tadeusz Mazowiecki: biografia naszego premiera* and Roman Graczyk's *Od uwikłania do autentyczności. Biografia polityczna Tadeusza Mazowieckiego* share many similarities, e.g., being more or less chronologically ordered, readers will also find clear differences in the ways in which they narrate and contextualize this eventful life.

Brzeziecki's book is an illuminating biography moving across different phases and dimensions of Mazowiecki's life. In so doing, it allows us to examine the pattern of changes in Mazowiecki's biography but also to follow the formation of his political identity and style. Mazowiecki was born on 17th April 1927 in Płock, central Poland, as the third and youngest child of an established and patriotic noble family. His father had built up an excellent reputation as a doctor and, more importantly for Mazowiecki's own later views, exhibited a positive attitude towards Polish Jews far from any anti-Semitism. Mazowiecki's parents were members of an engaged local Catholic intelligentsia well-known for their community work. For instance, Mazowiecki's father initiated the founding of the House of the People with Disabilities in the city of Płock.

Given the private education, holidays at the family's summer house, a loving, socially engaged and respected family, Mazowiecki's early childhood could be seen as idyllic in comparison with the standard living conditions of that time. Acknowledging the role of family tradition in the formation of Mazowiecki's attitudes and ideals allows us to trace the emergence, trajectory and sustainability of certain core values in his adult life. Brzeziecki identifies these values, which became an inseparable part of Mazowiecki's way of thinking and acting, as "Catholicism, patriotism and the need to participate in public life".[3]

Mazowiecki's early idyllic life was interrupted by the death of his father in June 1938 when Tadeusz was eleven. After his father's passing the family ran into financial troubles, and with the beginning of WWII, young Tadeusz' peaceful world collapsed. During the war Mazowiecki took up a great variety of jobs to support his family, including as assistant to an administrator at a farm nearby Płock that was owned by a German-Briton. As the war and occupation carried on, another tragic event occurred. In 1944 Mazowiecki's

[3] Andrzej Brzeziecki, *Tadeusz Mazowiecki: biografia naszego premiera* (Kraków, 2015), 14.

older brother Wojciech was arrested for his activity in the underground Home Army [*Armia Krajowa*] and sent to the camp in Stutthof where he most likely died (the exact place, date and circumstances of Wojciech's death still remain unknown).

After the war, Tadeusz continued his education at the local high school and eventually enrolled in the law faculty at the University of Warsaw. During his studies he established a closer relationship with his friend Krystyna Kuleszanka who was working in the local library in Płock. In the war Kuleszanka had been imprisoned in the concentration camp in Ravensbrück for her political activity in the Home Army. As a well-read intellectual, she had a profound impact on Mazowiecki's intellectual growth at that time. The couple married in 1950 in his home town and embarked on a happy, if brief, period in their life. Krystyna had been suffering from tuberculosis, and as the post-war medical service and living conditions were still relatively poor, her health dramatically deteriorated and she passed away only a year after their marriage. The death of his wife and friend was a traumatic event in Mazowiecki's early life, to be followed by the death of his mother only two years later, in 1953.

While the new post-war order was characterized by a radical break with the past, it also offered many people new possibilities. Given his intellectual sensitivity and knack for the written word, Mazowiecki turned to journalism. One of the most controversial periods in his life is linked to his increasing proximity to the publisher of the Catholic weekly *Dziś i Jutro*, Bolesław Piasecki (1915–79), and his publishing empire. As a pre-war fascist, anti-Semite, anti-capitalist, nationalist Catholic involved in communism and a somewhat clever politician, Piasecki remains an intriguing and highly problematic figure in Polish twentieth-century history.[4] Seeking an opportunity for himself and hoping to build a bridge between the Party and the Catholic part of society Piasecki was an active supporter of the emerging political regime. As a result of his loyalty to the Party, he was allowed to found PAX, a relatively large, well-managed and financially successful organization with ownership of one of the biggest private companies and openly Catholic publishing houses in the People's Republic of Poland.

It is worth pausing here to consider Brzeziecki's account of Piasecki's reputation. It seems as if for Brzeziecki the greatest controversy surrounding Piasecki's career and life achievements concerns the latter's support for the communist authorities during Stalinism. Whether consciously or not, this emphasis in Brzeziecki's interpretation (which is similar to

[4] For a more comprehensive study of Piasecki, see Mikołaj Kunicki, 'The Red and the Brown: Bolesław Piasecki, the Polish Communists, and the Anti-Zionist Campaign in Poland, 1967–68', *East European Politics and Societies*, xix, 2 (2005), 185–225.

Graczyk's) encapsulates how in twenty-first-century popular discourse in Poland an association with Stalinism triggers more opprobrium than links to fascism.

Mazowiecki's first publication in the weekly *Dziś i Jutro* appeared in 1949 and his position within Piasecki's publishing empire continuously grew stronger until 1955. His close links with Piasecki's weekly could be explained by the fact that as a young post-war intellectual and journalist in search of his intellectual and political path Mazowiecki was clearly under the spell of social and economic promises of communism. Furthermore, in the context of the ever more intense process of the secularization in the public sphere marked by the difficult relations between the Catholic church and the state, Piasecki's milieu was seen by some as a social space connecting socialism with Catholicism; a space open to young Catholics who did not necessarily object to communist rule. Viewed in this light, the socially sensitive and even socialist dimension of Mazowiecki's Catholicism, which fed his enthusiasm for socialist Christianity, was partly in line with Piasecki and PAX. It seemed as if nothing could jeopardize Mazowiecki's sympathy for the organization: not the problematic and controversial past of the PAX founder, or the striking similarity between PAX publications and official communist propaganda discourse, nor the fact that PAX never managed to establish good relations with the Polish Catholic church.

However, around 1955 a generational and ideological conflict between young journalists – Mazowiecki and Janusz Zabłocki – and Piasecki broke out. The young collaborators demanded an internal democratization of PAX, turning against the authoritative figure of Piasecki and accusing him of 'limiting socialist solidarity to solidarity with the state apparatus'[5]. As a result of this major disagreement, on 4th October 1955 Mazowiecki, with a group of other rebellious workers, was fired from PAX. This parting of ways with Piasecki produced a strong private and professional friendship with Zabłocki that, however, was wrecked a few years later by conflicts that turned old friends into rivals in a process that Brzeziecki traces with much precision.

Following the wave of 'Polish October 1956' with Gomułka's renewed interest and openness toward Catholic milieus, and after unsuccessful attempts to get involved with the acclaimed group behind the *Tygodnik Powszechny* weekly, Mazowiecki and Zabłocki obtained permission to engage in their own public Catholic activity. In 1957 Mazowiecki co-founded the *Klub Inteligencji Katolickiej* [Club of Catholic Intelligentsia or KIK] and an intellectual Catholic monthly. Inspired by the twentieth-century French philosophical school known as 'personalism', as developed by Étienne Gilson and Jacques Maritain, Mazowiecki as the first editor-in-chief named the monthly, *Więź*. Its first issue appeared in 1958 and it gradually became one of the leading

[5] Brzeziecki, *Tadeusz Mazowiecki*, 73.

intellectual publications in Poland.[6] In this capacity, Mazowiecki came to occupy a position demanding advanced communicative skills and diplomatic finesse in negotiations with the party-related state institutions and other Catholic public figures. Interestingly, the then more prominent group of intellectuals working with the somewhat elitist *Tygodnik Powszechny*, such as Jerzy Turowicz, Stefan Zawieyski, Stanisław Stomma, and Stefan Kisielewski, had not always been enthusiastic about *Więź* accusing it of naively promoting 'utopian socialism'[7] despite its break with PAX.

Moreover, it was not easy for Mazowiecki to establish harmonious relations with Cardinal Stefan Wyszyński, as current political events repeatedly led these relations to cool down. After a beginning already lacking in trust, in 1961 Mazowiecki became a member of parliament joining the small group of Catholic MPs (until he left in 1972). This made it even more difficult for the Cardinal to exert influence on him. Without any doubt, this was an important period in Mazowiecki's political life as it was his first and somewhat formative experience as a professional politician directly involved with the state.

In the meantime, Mazowiecki had started a relationship with his secretary Ewa Proć, a young, smart and outspoken woman whom he married in the spring of 1955. Thanks to this relationship, Taduesz was able to rebuild his personal life. Ewa and Tadeusz had three sons: Wojciech, Adam and Michał. But Mazowiecki's family was to suffer from another sudden tragedy. In 1969 Ewa fell ill, and with her health dramatically worsening, she passed away in the beginning of 1970. In touching detail, Brzeziecki describes how, while sitting at Ewa's deathbed, Mazowiecki was reading to her from the book *The Little Prince*. Now widower and a single father of three sons, the youngest of them only three years old, Mazowiecki's life underwent major changes. He fell into depression but the help of his closest friends and a sense of parental responsibility towards his sons seem to have driven him forward and out of the crisis.

The deaths of both of Mazowiecki's wives – but especially Ewa's death – cast a long shadow over the remainder of his and his family's lives. What is remarkable about Brzeziecki's account is that it manages to explore Mazowiecki's politics and public life through the experience of everyday life. For instance, the biography skilfully captures how Mazowiecki was regarded and remembered by his sons as a principled, somewhat distant and at the same time very caring father consulting them on his political decisions once they grew older.[8] In this way, Brzeziecki manages to bring Mazowiecki closer to his readers as a respected figure but also as a human being caught among a commitment to politics, traumatic personal events and his struggle with

[6] *Ibidem*, 126.
[7] *Ibidem*, 137.
[8] *Ibidem*, 244–7, 266.

depression. By elegantly weaving the public into the personal in his narrative, Brzeziecki elucidates how Mazowiecki's personal context shaped his political and ethical development.

Marked by the deaths of his wives, Brzeziecki further argues, with time Mazowiecki's political commitment and Catholic faith became more mature over and he himself became more cautious, modest and calm. In the course of the 1970s Mazowiecki became involved in rebuilding German-Polish relations. Together with other Catholic intellectuals such as Władysław Bartoszewski and Stanisław Stomma he had been traveling to the GDR and to West Germany to deliver lectures on the political situation in Poland. These trips offered a valuable opportunity to meet foreign politicians, such as Richard von Weizsäcker, allowing Mazowiecki to obtain experience in mastering the political and diplomatic know-how for which he later became well known. Meanwhile, *Więź* was becoming more oppositional and critical towards the official party line. It is on the pages of *Więź*, which served as a platform for exchanges among Catholic intellectuals, that Mazowiecki was sharing with a broader audience his moral reflections on issues that were central to him, such as the development of a liberal and open Catholicism. As Brzeziecki convincingly shows, reading *Więź* allows us to follow the debates Mazowiecki had with others on crucial issues and his concerns as an engaged Catholic. This is even more remarkable if one takes into account that Mazowiecki's own struggle over the validity of Catholic traditions and values – both on the institutional and the personal level – took place within and often against hostile post-1948 political order in Poland. Around 1975 the relations between Wyszyński, who became a Primate of Poland, and the milieu around *Więź* become more friendly. During that time, Mazowiecki also started to embrace the discourse of human rights; simultaneously *Więź* was opening itself up to different oppositional groups.

With a series of highly visible protests and the founding of the Workers' Defense Committee (KOR) the year 1976 is often regarded as one of the pivotal points in the formation of the political opposition in Poland. Despite the lack of an explicit alignment with the KOR milieu, a year later Mazowiecki became a representative of the protesters during the hunger strike organized in the St Martin's Church in Warsaw. The hunger strike was provoked by the incarceration of the organizers of a ceremony of public mourning after the murder of a young KOR associate in May 1977 Stanisław Pyjas who was a student from Cracow. As Barbara Toruńczyk and Henryk Wujec recall, Mazowiecki risked jeopardizing the precarious relations between the State and the Church in putting every effort into protecting the protestors and getting across their message.[9]

[9] *Ibidem*, 267–8.

When the strike began in the Gdańsk Shipyard in August 1980[10] Mazowiecki together with Bronisław Geremek visited the protesters in Gdańsk. They delivered a letter, known in Poland as *List 64*, asking the protesters and the state to enter into a negotiation process in order to avoid the risk of violent clashes. The letter was written by intellectuals in solidarity with the striking workers. Again, Mazowiecki's reservation towards KOR became visible as none of the KOR associates were allowed to sign the letter. This tension was manifested itself most visibly in the difficult relation Mazowiecki had with one of the KOR leaders, Jacek Kuroń.[11] Following their support for the strikers, Geremek and Mazowiecki together with Lech Wałęsa decided to create a Committee of Experts consisting of intellectuals dedicated to supporting and advising the striking workers. After the legalization of the Solidarność Trade Union, in December 1980, there was a vacancy for the position of editor-in-chief of the Union's official weekly *Tygodnik Solidarność*. Eventually, Wałęsa offered this position to Mazowiecki. As a consequence, the weekly's first editor-in-chief, Mazowiecki performed a historic role in shaping the semi-autonomous public debate about the purposes, problems and future of the newly found union.

With the declaration of martial law in December 1981, like many other Solidarność associates, Mazowiecki was arrested and first sent to Strzebielinek, then transferred to Jaworze and eventually held in Darłówko. In January 1982 he saw his sons for the first time after his detention and a month later, he met his oldest son Adam's girlfriend Violetta in Jaworze. During that visit Adam informed his father that they planned to get married. In a letter to them Mazowiecki raised the issue of friendship by elegantly declaring it to be an inseparable dimension of love and marriage. As he himself put it, the letter was meant to be a continuation of the conversation they had in Jaworze.[12] The couple married in March 1982 in Jaworze making it possible for Mazowiecki to attend the ceremony. During his detention Mazowiecki's sons managed to smuggle their father's notes out of the prison, turning them into a book describing the experience of imprisonment. After a little over a year of detention, Mazowiecki was finally released in December 1982.

In describing the period of incarceration as experienced by Mazowiecki, Brzeziecki sheds light on an important aspect of imprisonment. Under the political and social conditions of the People's Republic of Poland rather than being essentially an individual enterprise political activism demanded the involvement of a whole family unit or a group of close friends. By reconstructing the visits of Mazowiecki's children Brzeziecki allows us to see that even

[10] For more on the strike, see Anna Machcewicz, *Bunt. Strajki w Trójmieście. Sierpień 1980* (Gdańsk, 2015).

[11] Brzeziecki, *Tadeusz Mazowiecki*, 286–7.

[12] *Ibidem*, 329.

the very restricted and precarious communication they were able to have was a source of invaluable support symbolically transgressing the confinement of Mazowiecki's solitude. In addition, this highlights a fact often omitted in contemporary Polish history writing: that alongside and behind the narrative of political heroism (still often embodied by men) stands a complementary story of care work performed by friends, families and partners.[13]

After a period of depression that started in the 1980s and after participating in the Round Table Talks Mazowiecki became one of Poland's major politicians. Out of three candidates for the position of prime minister – Bronisław Geremek, Jacek Kuroń and Tadeusz Mazowiecki – Wałęsa chose the last one as Solidarity's candidate. After being elected and giving his famous first parliamentary speech as prime minister on 12 September 1989 and as the first Prime minister of post-communist Poland Mazowiecki was immediately confronted with a whole series of daunting tasks. Apart from forming the government that had to include some post-communist politicians, his main duty was to initiate a programme for economic, political and social reforms that would allow Poland to integrate with Western Europe. The policy of economic liberalization included fighting inflation, reducing the national debt and privatizing state-owned companies. The architect of these economic reforms, Leszek Balcerowicz, then the Deputy Prime Minister and Finance Minister, managed to remain in the government and continue the reforms even after Mazowiecki stepped down as a prime minister. Interestingly, seeing Jacek Kuroń as a counter-balance to Balcerowicz, Mazowiecki offered the left-leaning Kuroń position of Minister of Labour and Social Policy. With regard to Mazowiecki's political legacy, his politics towards the post-communists have also been much debated, as it was aimed at trying to avoid a full-scale confrontation with ex-members of the communist party (known as the politics of the 'thick line', seeking to divide the past from the present).

Mazowiecki's working style notably involved long discussions into the late night. As Aleksander Hall recalls, Mazowiecki the prime minister was similar to Mazowiecki the editor-in-chief. He was open to different viewpoints and arguments and highly valued the process of collective deliberation. As Brzeziecki points out, Mazowiecki opposed political privileges and avoided enjoying them.[14]

The post-election euphoria did not last for too long within the post-Solidarność camp with clashes between Wałęsa and Mazowiecki becoming more intense and eventually leading to a confrontation. While Wałęsa felt bypassed by Mazowiecki in the decision-making process, the latter tried to distance himself from Wałęsa's influence given his strong leadership tendencies.

[13] For a brilliant example of such a story, see Danuta Wałęsa, *Danuta Wałęsa. Marzenia i tajemnice* (Kraków, 2011).

[14] Brzeziecki, *Tadeusz Mazowiecki*, 428–9.

Attempts at a mediation between the two camps centred around the two leaders did not bring about reconciliation. The growing atmosphere of mistrust, tension and coldness between the two men and their camps culminated in their rivalry in the presidential campaign of 1990. Interestingly, the campaign launched by Wałęsa's team succeeded in capturing his own style as a politician, being dynamic and at times aggressive with mocking hints at Mazowiecki's slowness and at times problematic anti-Semitic undertones.[15] Mazowiecki's campaign also reflected the candidate's somewhat non-charismatic and pensive style. One of the most catchy slogans, which fit Mazowiecki particularly well, was 'The Strength of Peace'. Most importantly, as Brzeziecki observes, the competition between old collaborators resulted in old friendships and bonds coming apart. As he points out in a generally positive evaluation of Mazowiecki's government – among whose achievements was the securing of Poland's Western borders – the issue was not the speed of reforms but rather the lack of political communication and information regarding the changes.[16]

Mazowiecki's participation in post-1989 politics also involved leading the party – the Christian-liberal Democratic Union (from 1994 on: Freedom Union) – which he founded in 1990. Furthermore, in 1992 he was appointed a Special Representative of the United Nations Commission on Human Rights for the Yugoslav conflict, a role to which he was very devoted. Shocked by the Srebrenica genocide and the UN's failure to protect civilians, Mazowiecki eventually resigned in 1995.[17] Both on a national and an international level his post-1989 political career underlines his continued strong moral commitment to principles such as human rights and pluralism.

Well-grounded in archival research and Polish secondary literature, Brzeziecki's book explores a range of topics that will help bring Mazowiecki's life and oeuvre closer to professional historians, undergraduate students and all those interested in Polish twentieth-century history. Thanks to the sincere narrative told by Mazowiecki's sons the reader gets a vivid, intimate and human account of Mazowiecki as a Catholic intellectual, strategist and mature politician but also as a single father of three sons.

As might be expected from an excellent biography, Brzeziecki's study traces the impact of changes in political systems on Mazowiecki's public identity as a Catholic. For instance, the continuous negotiations with Wyszyński amounted to a sophisticated process which shaped Mazowiecki's political style and way of thinking. Despite changing conditions and alliances, however, some things remained unchanged, especially Mazowiecki's commitment to an open and intellectual Catholicism, and to the pluralism of ideas.

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[15] *Ibidem*, 457.
[16] *Ibidem*, 462.
[17] *Ibidem*, 513.

Mazowiecki's political legacy has been the subject of intense polemic. Roman Graczyk's book seems to be conceived in this spirit – more so than Brzeziecki's account which is reconstructive and all-encompassing in its focus. As becomes already clear in its introduction, Graczyk's book is concerned with analysing Mazowiecki's legacy. Throughout his account, but especially in the second part of the book, one can get the impression that the author is entering into a critical conversation with Mazowiecki's decisions and choices. Moreover, the author allows himself to pass judgments on events and decisions made by others.[18] After having read Brzeziecki's well-balanced narrative, Graczyk's polemic and interventionist interpretations, that at points might come across as too blunt, strike one as a work of a historically oriented political commentator rather than a historian. For instance, Graczyk's discussion of the difficult relations between Mazowiecki and the KOR milieu during the Round Table Talks and, in particular, during the process of deciding who Wałęsa should chose as the candidate for the position of Prime Minister is at the same time informative and curiously suggestive.[19]

Graczyk's book is at its best when approached as a well-informed political essay on the genealogy and history of contemporary Polish politics. However, openly passing judgments on historical events and deploying a contemporary perspective in order to polemically engage with the history of an individual life (in this case Mazowiecki's post-1989 official political activity) always involves the risk of neglecting the complexities of the historical situation as lived and experienced by the agents themselves.

There is no doubt that a thorough biographical account of Mazowiecki's life was much needed, especially since his adult life sets an impressive example of a day-to-day, deep involvement with faith, politics and the society he lived in. Both authors manage to prove that studying the life of an individual is not exclusively about uncovering life stories and narrating specific events but can also be a way of reaching a better and deeper understanding of political and social developments and institutions, such as the relationship between the Catholic Church and the PZPR. Brzeziecki elegantly and indirectly exemplifies that narrowly understood positivist models of research do not necessarily provide the perfect tools for historical inquiry – precisely because individual context matters greatly. With his polemic style Graczyk, in turn, shows that knowledge always has a profoundly personal dimension and involves a commitment to impartiality as well as to political passion.

By delving into the post-1948 history of the Catholic Church in Poland, intellectual history and post-1989 political history both authors cross subfields in history that tend to be (sadly) studied in isolation. Reading

[18] Roman Graczyk, *Od uwikłania do autentyczności. Biografia polityczna* (Poznań, 2015), 157.

[19] *Ibidem*, 297, 305.

Brzeziecki's book we learn about interlocking life histories that serve as a window into the professional and personal networks of engaged Catholics who were Mazowiecki's friends and associates, and almost in passing we learn about the history of semi-independent journalism in post-war Poland. Even if it is at moments sad or depressing reading, these two biographies are ultimately inspiring and somewhat uplifting, not least in showing how an individual can overcome a myriad of obstacles of a personal and political kind and influence history.

The scholarly significance of these two studies – and especially of Brzeziecki's work – would, however, have been significantly increased if the available research on Polish post-war history in the English language (such as Mikołaj Kunicki's biography of Bolesław Piasecki referred to above) was consulted. In addition, more sustained historiographical reflections on the relevance of historical biography, along the lines indicated at the beginning of this review, would certainly strengthen the appeal of biographical writing in the context of Polish history beyond its popularity with non-academic audiences, an appeal both books could very well contribute to.

Nguyen Vu Thuc Linh