

GRZEGORZ PAC

*Instytut Historyczny Uniwersytetu
Warszawskiego*

LET'S WRITE A BETTER BOOK ABOUT MIESZKO I!*

Abstract: The paper is a critical review of Przemysław Urbańczyk's monograph on Mieszko I, but rather than being a systematic discussion of the whole work, it focuses on those issues discussed in the book, which concern the relations between archaeology and history. Fragments basing exclusively on archaeological research are considered the most valuable by the author, whereas he is more critical about those parts which are based on the on interpretation of written sources. He postulates that such works, relating to both archaeology and history, should be created in close cooperation between representatives of both disciplines.

Key words: archaeology, history, Mieszko I, Christianization, Thietmar from Merseburg.

One cannot deny that in the research dedicated to the oldest period of the history of Poland archaeology has recently been playing a growing role. The discovery of new knowledge thanks to the intensive penetration of not only old but also new sites and the application of recent research methods have broadened archaeology, allowing us to go beyond the information available in the very limited existing written sources. At the same time, interpretation of the latter is also changing, which results not only from accumulation of knowledge of subsequent generations of historians, but also from material methodological changes within the area represented by them. Consequently, it seems that that progress in research on the period of the first historical ruler of Poland, Mieszko I, being, obviously, the point of interest of both archaeologists and historians, depends to a high extent on the ability to cooperate and good communication between representatives of these two disciplines.

Przemysław Urbańczyk, in his work devoted to Mieszko I, made an attempt to look at this period using knowledge resulting from an analysis of

* Referring to: Przemysław Urbańczyk, *Mieszko Pierwszy Tajemniczy*, Toruń: Fundacja na Rzecz Nauki Polskiej; Wydawnictwo Naukowe Uniwersytetu Mikołaja Kopernika, 2012, 509 pp.

archaeological material and historical sources. At the same time, one of the most important problems discussed in this monograph is the very issue of cooperation between history and archaeology, especially with respect to the beginnings of Polish history. And I want to treat this issue as the main one in the discussion related to Urbańczyk's work and to consider what conditions this cooperation should meet in order to be fruitful. Obviously, expectations presented here will be the expectations of a historian and, at the same time, a dilettante in the area of archaeology, but I am sure that our colleagues in archaeology could approach us with similar expectations. This area of focus made me decide against a detailed discussion of all plots presented in the book or polemics with all the author's theses which could require such polemics (as far as this is concerned, I was relieved by Dariusz A. Sikorski, who presented an exhaustive, critical review of *Mieszko Pierwszy Tajemniczy*).¹

Defining the rules adopted by me in this text I would like to begin with a quotation from the reviewed book addressed to me as a reviewer. The author writes: 'It is very likely that different specialists in narrow areas will once again formulate painful accusations of my ignorance regarding "all" literature, lack of knowledge of "very important" details or "erroneous" reading of classic sources. My reply to all of them is as follows: WRITE A BETTER BOOK ABOUT MIESZKO I!'² (underlined by the Author) (p. 16).

I probably will not write any book about Mieszko I, but I would like to consider the conditions that a book about Mieszko should meet in order to be better. I will try to avoid all things, regarding which the Author replies — in advance — to reviewers. Therefore, I will not focus on details but on the things which the Author himself considered material in this work. I will not expostulate ignorance regarding 'all' literature, and I will refer to — as far as possible — only those works which the Author cites. The only thing I cannot omit is reviewing the erroneous reading of sources, since it constitutes the basis of a historian's work, which, without constant evaluation of correctness of source reading and without discussions about interpretation thereof is useless. I write this article as a historian and medievalist, who expects that publications regarding my area of research fulfil the specified methodological rules. Does this give me the label of 'a specialist in a narrow area'? Let others decide.

¹ Dariusz A. Sikorski, 'Mieszko Pierwszy Tajemniczy i jeszcze bardziej tajemnicza metoda historiograficzna', *RH*, 79, 2013, pp. 183–203. After submitting my article for publication Urbańczyk's reply to Sikorski's review appeared together with a response from the latter: Przemysław Urbańczyk, 'Furor Sikorscianus, czyli pryncypialny strażnik ortodoksji', *RH*, 80, 2014, pp. 267–77; Dariusz A. Sikorski, 'Mieszko I mniej już tajemniczy (odpowieź Przemysławowi Urbańczykowi)', *RH*, 80, 2014, pp. 277–85.

² 'Zapewne różni specjaliści od wąskich zagadnień znów będą mnie boleśnie smażyć zarzutami nieznamośności "całej" literatury, braku orientacji w "bardzo ważnych" szczegółach czy też "błędnego" odczytania klasycznych źródeł. Wszystkim im od razu odpowiadam: NAPISZCIE LEPSZĄ KSIĄŻKĘ O MIESZKU I!'.

1. Urbańczyk's work is divided into ten chapters, which are to in different degrees related to its main hero, Mieszko I. The major part of the first chapter is dedicated to theoretical considerations of the way of understanding such terms as state or dominion (German: *Herrschaft*) in the early medieval period, which, however, is supposed to constitute, first of all, a broad background for the question of how to qualify the political creation ruled by the first historical member of Piast dynasty. But the connection of the issues constituting the majority of the second chapter with the subject of publication is unclear. I mean the lecture on the ethnic picture of Central Europe in the Arab sources (the figure of Ibrahim ibn Jakub, a Jewish merchant from Spain describing Mieszko's dominion, which is important in further parts of the work, here appears rather incidentally), and, mainly, the otherwise interesting considerations on the meaning of the Vistula River as the identifier of the area in which the Polish state was later formed.³ In the third chapter the Author focuses on the pre-Piast period and gets involved in polemics with the concept of 'tribes' present in historical and archaeological studies. He also analyses mechanisms of development of territorial infrastructure by the Piasts, and finally formulates a thesis on the foreign origin of Piasts, indicating a possibility that they stemmed from the Moravian Mojmir dynasty.

In the subsequent chapter the Author analyses Mieszko's name; he considers it to be a special construct, 'the inspiration for which should be probably searched for in biblical connotations' (p. 189). In chapter five the Author analyses — in the broader context of expansion to the northeast parts of Europe — the problem of the presence of Christianity in the areas of the later Polish state before Mieszko's christening, dated usually at 966, and the beginning of the Christianization of his dominion. The Author proposes here to consider the possibility that Mieszko (as — according to his thesis — the successor of the Mojmir dynasty) was at that time already a Christian, which did not have to result in his attempts to carry out a conversion of the people subordinate to him.

The next three chapters — the sixth, seventh and eighth — deal with three aspects of the ruler's activity (Christianizing ruler — builder — politician).

In the first, the Author analyses the organization of the Church in the times of Mieszko's rule and the progress in the Christianization process. In the chapter on Mieszko's activity as a builder, the Author discusses the

³ Sikorski provided similar comments on these parts of the publication, 'Mieszko Pierwszy Tajemniczy i', p. 184. In response Urbańczyk (*Furor Sikorscianus*, p. 269) explained their connection with the subject of monograph, admitting at the same time: 'maybe indeed I should have added one paragraph to each of these chapters that would explain the issues which, due to my naivety, I considered as obvious for an averagely intelligent reader?' ('może faktycznie powinienem był dodać do tych rozdziałów po jednym akapicie wyjaśniającym sprawy, które w swojej naiwności uznałem za oczywiste dla przeciętnie inteligentnego czytelnika?'). I can only agree with him that there is no such explanation in the book and to uphold the opinion that a reader (also an averagely intelligent reader) may not notice this connection, unless he/she reads explanations contained in the Author's response to Sikorski.

problem of residential and sacral architecture and gords (burgwalls) from the period of his rule. In chapter eight, the Author follows the political history of Mieszko's 'state', which he reconstructs chronologically, with division into three periods, in a style which is nowadays rarely used in historiography and which is reminiscent of the tradition of nineteenth-century positivist political historiography. In chapter nine, we read about the territory ruled by Mieszko I. The Author analyses it based on archaeological sources and the *Dagome Iudex* document, that is the donation of the area described as 'civitas Schinesghe' by Mieszko, his wife Oda and their two sons to Saint Peter and papacy, preserved only in the form of a register (summary) from the second half of the eleventh century. In his interpretation the Author returns to the earlier thesis, according to which 'Schinesghe' from this register covers Szczecin, not — as most scholars think — Gniezno. The last chapter tells us about Mieszko I's burial. Here we have a thesis that graves no. 186 and 187, located in Poznań cathedral, which are often described in literature as the graves of Mieszko I and his son Bolesław Chrobry, are in fact two graves of the first Polish bishop, Jordan. The one marked by archaeologists as no. 186 was supposedly built in the initial phase of the construction of the cathedral, and 'haste caused that the gravestone was not put precisely on the axis of the central nave [...]. Thus, it was decided that this mistake would be repaired' (p. 419) and the body was moved to the grave known as grave no. 187.⁴

2. In my opinion, the most interesting and the most valuable are those fragments of the work in which the Author relies fully on archeological material. I mean, first of all, extensive fragments, in which he considers archeological proofs of progress in the Christianization process during the rule of the first historical Piast. The perspective presented here is very interesting from a historian's point of view and makes one review certain widely accepted convictions. The first of them relates to the burial custom, which — as Urbańczyk shows — is a complex reality, dependant on numerous factors, and in any case, is not necessarily directly dependant on the conversion to a new religion; 'looking for a single reason of the archeologically noticed change in the burial ritual may be misleading' — he states (p. 275).⁵ So the reviewed work contains examples of inhumation in pagan areas (sometimes under the influence of Christian neighbours) and its concurrent occurrence with cremation. On the other hand, in the areas subjected to Christianization the burial ritual usually changed very slowly, sometimes over centuries, and the Church was not always equally consistent in combating the elements of the old ritual (pp. 262–75). And how did it look in Mieszko I's 'state'? 'According to the current state of knowledge, regular burying of the dead in accordance with

⁴ 'pośpiech sprawił, że grobowiec nie leżał dokładnie na osi nawy głównej [...]. Postanowiono więc naprawić błąd'.

⁵ 'poszukiwanie jednej przyczyny zaobserwowanej archeologicznie zmiany obřądku grzebalnego może być zwodnicze'.

Christian ritual was systematically organized over half a century after the official conversion, and the oldest [probably dated (as the Author explains in another fragment of the book) – G.P.] skeletal burials are from the post-Mieszko period (p. 280).⁶

This lack of early burials of a clearly Christian character corresponds with another archeological observation – that ‘currently we do not have any cogent evidence of extensive investments by Mieszko I in the material church infrastructure’ (p. 303).⁷ In other words, besides Poznań, it is difficult to point out a religious building with a certain tenth-century certificate (pp. 301–03). Of course this puzzle may be partly solved by the supposition that the earliest objects of worship were made of wood, and, therefore, they are more difficult to describe from an archeological point of view (pp. 255, 302). But there may also be another reason. As the Author indicates, referring to foreign analogies, sometimes Christian rulers accepted the fact of ruling pagan people or even treated this religious difference as an element emphasizing their own separateness and elite character (pp. 255–56, 286–90). The same may have applied to the Piast ‘state’. Anyway

archaeology does not [...] currently provide any proof to support a hypothesis that Mieszko’s christening resulted in an intensive Christianization of his people. On the contrary – the lack of expected findings (numerous sacral buildings, clearly Christian burials or even minor symptoms of the new religion in the form of pendant crosses) makes one suspect that the conversion of the prince and his closest circle did not initiate extensive changes in the religious sphere (p. 286).⁸

Certainly, this conclusion resulting from archeological research may not be ignored by historians dealing with the earliest period of Polish history in their discussions on the Christianization of the Polish lands.

3. It is these fragments of the book, in which the Author analyses archeological material not so well known to historians, that may be important for them. Nevertheless, here we also have a problem, since being dilettantes in the area of archaeology, historians are usually unable independently to evaluate the results of research and they must rely on specialists’ findings and conclusions. However, in order to trust them they must be convinced that these

⁶ ‘według aktualnej wiedzy regularne chowanie zmarłych zgodnie z rytuałem chrześcijańskim zostało systematycznie zorganizowane ponad pół wieku po oficjalnej konwersji, a najstarsze pochówki szkieletowe pochodzą z czasów pomieszkowych’.

⁷ ‘nie mamy dzisiaj przekonujących dowodów ekstensywnego inwestowania przez Mieszka I w materialną infrastrukturę kościelną’.

⁸ ‘archeologia nie dostarcza [...] dzisiaj dowodów na poparcie hipotezy, że skutkiem chrztu Mieszka była intensywna chrystianizacja podległej mu ludności. Wręcz przeciwnie – brak oczekiwanych znalezisk (licznych budowli sakralnych, jednoznacznie chrześcijańskich pochówków, czy nawet drobnych oznak nowej religii w postaci krzyżyków) każe podejrzewać, że konwersja księcia i jego najbliższego otoczenia nie zapoczątkowała szerokich zmian w sferze religijnej’.

conclusions are internally consistent, logically correct, and that they report the latest state of knowledge. This puts before archaeologists, who want the results of their research to be acquired by historians, a special challenge of presenting their findings in such a way that they can – at least on the general level – be understood and used by historians.

Speaking of consistency I mean using univocal criteria in the interpretation of archeological material, which seems to be sometimes missing in the reviewed work. So the Author gets involved in the justified – as it seems – polemics with ‘Norman theory’, showing that the presence in the Polish territory of Scandinavian imports or graves known in the North is not proof of the actual presence of Vikings (pp. 139–42). But on the following pages, gathering arguments supporting his thesis that the future Piast dynasty stemmed from Moravia, he enumerates specimens from the area of the later Poland interpreted as connected with Great Moravia (pp. 144–56). He mentions products coming from the south, Moravia-type graves, and he gives examples of embankments built in a manner known from Great Moravia. He is, of course, aware that he uses the same type of argumentation which he contested a while earlier. Therefore, he stresses that these finds may not support the thesis on Moravians’ presence (p. 152), but it is not clear why, in such case, he is so scrupulous in enumerating them.

Continuing this subject in the context of southern Poland he adds:

Analogies of the complex organizational structures are more convincing. A good example is a large (7.5 ha), multi-element gord in Gilow near Niemcza, where not only the archeological finds of Great Moravian origin were discovered (weapons, tools, ornaments and ceramics) and a similar construction of embankments (stone cladding) was determined, but also a close structural similarity was noticed to the probably Great Moravian gord in Křenov-Mařina near Morawska Třebovia (p. 152).⁹

The Author does not explain what this structural similarity consists of but we learn it from Krzysztof Jaworski’s work,¹⁰ to which he refers. It mainly concerns the multi-element character of defence construction consisting of the main gord in the form similar to the one in Mařina, and of two *suburbia*; Jaworski, nevertheless, mentions that Gilow is not the only Silesian multi-element gord. Here one

⁹ ‘Bardziej przekonują analogie kompleksowych struktur organizacyjnych. Dobrym przykładem jest wielkie (7.5 ha), wielocłonowe grodzisko w Gilowie koło Niemczy, gdzie nie tylko odkryto zabytki proweniencji wielkomorawskiej (broń, narzędzia, ozdoby i ceramikę) i stwierdzono zblizoną konstrukcję wałów (licowanie kamieniami), ale też dostrzeżono bliskie podobieństwo strukturalne do prawdopodobnie wielkomorawskiego grodziska w Křenovie-Mařinie koło Morawskiej Třebovi’.

¹⁰ Krzysztof Jaworski, ‘Czy aby tylko “pogański książę silny wielce, siedzący na Wislech”?’, in *Swoi i obcy w kulturze średniowiecza. Wykłady popularno-naukowe zorganizowane w ramach VII Festiwalu Kultury Słowiańskiej i Cysterskiej w Łądzie nad Wartą w dniach 18–19 czerwca 2011 roku*, ed. Michał Brzostowicz, Maciej Przybył and Jacek Wrzesiński, Poznań and Łądy, 2011, pp. 35–45.

may ask, why the Author considers these similarities as arguments that may denote that this gord was built by Moravians, if he does not consider as such the use of embankment construction technique known from Great Moravia. Does this mean that he is of the opinion that the use of one type of gord construction is always proof that their creators belonged to one ethnic group or represented one 'state' organism? I leave the problem of determining who the builders of Gilow gord were to archaeologists. But I would like the Author involved in this discussion, who is so sceptical about such arguments in other cases, to explain in detail why he seems to support it in this particular case.

Another thesis, connected with the one about the Moravian origin of the Piast dynasty, which is obviously a subject of interest for historians, is Urbańczyk's supposition that Mieszko could have been a Christian before year 966. Archaeology is supposed to deliver evidence here, but material relating to burials and religious buildings from Mieszko's times, denoting a slow progress of Christianization in the period referred to above, does not strengthen this thesis (although in the interpretation proposed by the Author it does not oppose it either). Therefore, just one argument remains, that is remains, namely that the date on the chapel in the Poznań gord, which is based on dendrochronological dating of the board at the entrance to the Poznań palatium — the Author sets it (after Hanna Kočka-Krenz who discovered the object) at 'after the year 941' (pp. 219, 297). In his opinion, this dating 'would indicate that the construction of the Poznań complex was ordered by Mieszko I's father' (pp. 297–98),¹¹ which would mean, in turn, that Piasts had been Christians before the year 966.

Taking into account that this board becomes in a way a cornerstone of the theory which has the potential to revolutionize historiography of the earliest period of Polish history, one might expect that the Author will take care to present his way of thinking in a more precise way, especially for the sake of historians, as they do not have to be specialists in dendrochronology, but they do know very well what *terminus post quem* means. And from the logical point of view, information presented by the Author that wood felled after the year 941 was used in this construction does not so much make one conclude that the construction was funded by Mieszko's father, but that it was surely not funded by his grandfather. Similarly, the Author does not explain why he is convinced that the palatium and the chapel were built at the same time, although this statement is of a key significance for this reasoning. Lack of explanation with the concurrent stressing of the physical separation of both objects (pp. 221, 301) misguides the reader. Finally, it is surprising that the Author, presenting such a daring thesis, does not express his opinion on the doubts raised by his professional colleague Michał Kara in a work known to the Author. Referring to the proposal of Hanna Kočka-Krenz to date the object to the mid-tenth century, he states that 'one cannot agree with such early

¹¹ [dating] 'wskazywałoby na to, że budowę kompleksu poznańskiego zarządził ojciec Mieszka I'.

dating of relicts, at least in the light of the published archaeological sources'.¹² I cannot evaluate to what extent Kara's doubts are justified, but I cannot accept the conclusions of the reasoning presented in the reviewed work either, since the Author does not take a stance (even negative) on the earlier counter-proposals.

4. In this situation a historian can do nothing but wait for the more univocal and reliable interpretation proposals from archaeologists, and for the time being believe in the clear source messages, which state that Mieszko I was christened about the year 966. Of course, the Author is right in his statement that the fact that the oldest of these messages, Chronicle of Thietmar from Merseburg, was created half a century later, does not make the case easier. However, counter to Urbańczyk's intent, this text is fully univocal in its message relating to the very fact of the act of christening. Yet the Author states: 'After all, Thietmar (IV, 55) did not call Mieszko directly a pagan; he only wrote that he was "plunged in various errors of paganism"' (p. 225,¹³ in the original version: 'vario gentilitatis errore implicatus esse'). But in other places, Thietmar speaks directly about Mieszko's paganism. He writes that the prince 'got rid of the venom of the innate paganism/disbelief' (IV, 56: 'innatae infidelitatis toxicum evomuit'), and a description of the customs prevailing in his country is accompanied by the more precise information that they were applicable 'when he was still a pagan' (VIII, 3: 'cum adhuc ille gentilis esset'). The Author should be aware of this, since he cites the first of the quoted fragments just one page earlier (p. 224), and he cites the other one a bit later (p. 276).

Let us also notice that the Author presents two different, mutually contradictory, ways of understanding the second fragment. Once he cites it properly, to illustrate what Thietmar said about burial customs in Mieszko's 'state' in the pagan period (p. 276), but eleven pages later he describes it in the following way 'cruel penalties (cutting heads off and mutilating harlots and adulterers) with which Mieszko induced observance of the rules of the new religion (Thietmar, VIII, 2 [*recte*: 3 – G.P.])' (p. 287).¹⁴ However, it clearly follows from the text that this applies to the pagan period and that heads were cut off not as a penalty – this was the way of killing wives during their husbands' burials.

I mention the interpretation of Thietmar's message not only due to the fact that it can hardly be considered a detailed issue, and I promised not to write about details. It also gives the opportunity to explain a certain misunderstanding connected – as it seems to me – with the understanding by the Author of certain problems related with the interpretation of sources. Urbańczyk presents

¹² 'z tak wczesnym datowaniem reliktyw nie sposób się zgodzić, przynajmniej w świetle opublikowanych źródeł archeologicznych', Michał Kara, *Najstarsze państwo Piastów – rezultat przełomu czy kontynuacji? Studium archeologiczne*, Poznań, 2009, p. 356, n. 1882.

¹³ 'Przecież Thietmar (IV, 55) nie nazwał Mieszka wprost poganinem, lecz napisał tylko, że był "pogrążony w wielorakich błędach pogaństwa"'.
¹⁴ 'okrutne kary (obcinanie głów oraz okaleczanie nierządnic i cudzołożników), jakimi Mieszko wymuszał przestrzeganie zasad nowej religii (Thietmar, VIII, 2)'.

himself as an opponent of the opinion that written messages can be read only in one way — he speaks about it in the sentence quoted at the beginning of this text, when he responds in advance to polemicists criticizing him for ‘erroneous’ (in inverted commas) reading of sources.¹⁵ And one should applaud him for this! — different interpretations of sources are possible and necessary. The misunderstanding lies in the fact (this relates to rudimentary issues but it seems worthwhile to explain them), that there is no contradiction between the need for different interpretations, and, in this sense, different readings of sources, and the possibility of evaluating certain ways of reading as erroneous (without inverted commas). Thus, one may differently interpret Thietmar’s message about cutting off wives’ heads, but one cannot think that there is no contradiction in understanding it both as a penalty connected with the introduction of Christianity and an element of the ordinary pagan burial custom.

Referring once again to the example from Thietmar: the bishop of Merseburg writes clearly enough that Mieszko was a pagan, and as long as it is not proved that all scholars to date have understood him in a wrong way, negating this fact (that is, that Thietmar wrote this) is simply a wrong way of reading the source (not an understanding, omission or misunderstanding). Nevertheless, this fragment may be legitimately interpreted in different ways, sometimes depending on the accepted theoretical or methodological assumptions.

Thus, one may argue that Thietmar how it really was, or that he did not know, or one may refrain from a judgment on it. One may think that the goal of the tale is simply a presentation of history of the neighbouring country, but one may also believe that the actual or fictitious paganism of Mieszko is for the bishop a way to achieve another goal, such as to ‘incriminate’ his son, Bolesław Chrobry. Finally, one may be of the opinion — I am inventing it *ad hoc* — that the issue of christening is of a secondary meaning for him, and by describing the paganism of Mieszko and his subjects, Thietmar is making an allusion to the Longobards and their king, Arduin, whom he considers crypto-pagans and whom he describes in the previous paragraph. Or that the chronicle writer made up Mieszko’s paganism, which is only a pretext for presenting a moralizing tale about Dobrawa, who decided to break the Lenten fast to proselytize her husband; the goal is a kind of relativizing moral evaluations of such acts. Although some of the above proposals, created *ad hoc*, sound improbable and surely would not withstand any discussion, they should not be considered totally unjustified. And such different interpretations (although probably of a better quality) are what history needs and it is worthwhile to discuss them.

At the most basic level — the Author cannot oppose this — there occurs the correct or wrong (no inverted commas) reading of sources on the philological

¹⁵ Similarly, in his response to Sikorski Urbańczyk (*Furor Sikorscianus*, p. 270) writes: ‘Unlike D.A.S. I do not think that there is only one authorized way of reading early medieval texts’ (*‘W przeciwieństwie do D.A.S. nie uważam, że istnieje tylko jeden uprawniony sposób czytania wczesnośredniowiecznych tekstów’*).

level. Let us refer to one example (I must admit – it relates to a detail) from the discussed monograph: considering the circumstances of Mieszko and Dobrawa's marriage the Author cites the following words of Thietmar: 'In the region of Bohemia region he took a wife'.¹⁶ In Urbańczyk's opinion, these words indicate the place of marriage and are a basis for polemics with the comment of Kočka-Krenz, who states that this wedding took place in Poznań (p. 160). Therefore, one may have the impression that the scholar – presenting her hypothesis – did not notice a basic source or wrongly read it, that is, she did not understand its message. But it was her opponent who did not understand it, since the act of marriage took place in Bohemia only in the Polish translation of the Chronicle written by Marian Z. Jedlicki, and in the critical edition of the Latin text this fragment goes as follows: 'a Boemia regione [...] sibi uxorem [...] duxerat',¹⁷ so – quoting the English translation of the Chronicle by David A. Warner – 'He took a [...] wife from the region of Bohemia'.¹⁸ Thus, the issue of the place of marriage remains, in the light of Thietmar's record, open.

These source-related misunderstandings on the part of an archaeologist illustrate well the key point at issue for me, that is, relations between the field he represents and history. Just like historians, who sometimes get involved – without the necessary qualifications – in the independent interpretation of archeological materials, the same – as the Author puts it – 'archaeologists often give in to the desire to use them [written sources – G.P.] in their interpretations' (p. 310).¹⁹ It seems that they assume that 'a text is a text is a text' (to paraphrase Benedykt Chmielowski, an eighteenth-century Polish encyclopaedist, who – assuming that a horse is a generally known animal – defined it briefly: 'a horse is a horse is a horse, of course'). I do not deny their right to use written sources, but when doing so they should remember that this interpretation of texts constitutes the *clou* of the method of a separate scientific field, is governed by certain rules and requires specific knowledge and skills.

5. Here I approach another issue, namely the problem of the nature of discussion or even polemics between these two fields. It seems that in order for it to be fruitful it is advisable to formulate opinions in a more careful way, and, above all, carefully listen to the partner in the discussion, whereas the discussed work leaves the reader with the impression that its Author sometimes is in polemics

¹⁶ 'W czeskiej pojął on za żonę' (p. 160). The Author cites here the Polish translation of *Kronika Thietmara* by Marcin Z. Jedlicki, Poznań, 1953, p. 218.

¹⁷ *Die Chronik des Bischofs Thietmar von Merseburg und ihre Korveier Überarbeitung*, ed. Robert Holtzmann, Berlin, 1935, pp. 194–95 (Book IV, Cap. 55), MGH SrG n.s., vol. 9. Fragment with the same wording also in Latin text accompanying the aforementioned translation of the Chronicle, based on Holtzmann's edition.

¹⁸ *Ottonian Germany. The 'Chronicon' of Thietmar of Merseburg*, transl. David A. Warner, Manchester and New York, 2001, p. 191 (Book IV, Chapter 55).

¹⁹ 'archeolodzy często nie mogą oprzeć się pokusie ich [that is written sources – G.P.] wykorzystania w swych interpretacjach'.

with his own image of Polish historiography. Urbańczyk reconstructs several times — in a critical tone — opinions which are, according to him, of a general nature, but these comments are not accompanied by references indicating examples of these 'general' convictions, and when such references are provided, they usually refer to the works of two authors: Henryk Łowmiański and Gerard Labuda (for example pp. 62–63; 66, 104). Both these scholars are no longer with us and they both represented the generation, the students of which are now gradually reaching scholarly retirement, so the question arises if they can really be representative for Polish historiography AD 2012 (the year of publication of the book). The fact that Labuda, who was active in his scholarly work for a long time, published a biography of Mieszko I as late as 2004 does not change much, since it is obvious that the author, even so outstanding, whose oldest article cited in the discussed work goes back to 1947 (!), in the final period of his life evaluated and practised historical research differently from his younger colleagues, whose opinions prevail in the scholarly debate.

Of course, it may also be the case that opinions of the deceased scholars are still dominant and contemporary historians just thoughtlessly repeat them or omit subjects dealt with by their masters. If this were the case, Urbańczyk's criticism should be considered not only pertinent but also valuable, and historians should accept it with gratitude. But the obligation to prove such a state of facts is on the part of the Author, who does not mention any new publications. He himself seems not to be sure to what extent the opinions that he discusses are accepted and prevail in the contemporary debate. For instance, on page 66 he writes that in the research on the beginnings of statehood 'the dominant perspective is still optimistic evolutionism', and on page 62 he states that 'in Polish historiography [...] the conviction still dominates that "with respect to the Piast state the evolution theory is fully applicable" (Labuda 2002:50)'.²⁰ Urbańczyk illustrates this perspective with citations from Labuda and Łowmiański, and only once refers to archaeologist Andrzej Buka (pp. 60–62). Still, on pages 100–01 'the evolutionist concept of the beginnings of the Polish state inherited from the millennium research programme', illustrated with opinions of Łowmiański from the sixties, is already described as 'applicable until recently'.²¹ What does, therefore, the Author polemicize with — the contemporary historiographic vision or with the concept which rather belongs to the history of historiography?

6. And, last but not least, if one is to polemicize with historiography as such, one must get to know it well to be sure that the shortcomings that it is criticized for are real shortcomings and not just gaps in one's reading list. And the defensive words of the Author about reviewers, who 'will hit me painfully with accusations of not

²⁰ 'w polskiej historiografii wciąż [...] dominuje przekonanie, że "w odniesieniu do państwa piastowskiego pełne zastosowanie ma teoria ewolucji" (Labuda 2002:50)'.

²¹ 'odziedziczona po programie badań milenijnych, ewolucjonistyczna koncepcja początków państwa polskiego [...] do niedawna obowiązująca'.

knowing “the whole” literature’ are useless here — starting a discussion with the whole historiography (or archaeology), one sets the bar high. Let us illustrate this with an example: in *Mieszko Pierwszy Tajemniczy* there appears a statement that with respect to the times of Mieszko I the terms ‘state’ and ‘tribe’ are generally and rather thoughtlessly used, and ‘Polish medieval studies were deprived of a broader theoretical discussion on this subject’. ‘A glorious exception’ — as Urbańczyk writes — is the book *Geneza i funkcjonowanie wczesnych form państwowości* edited by Michał Tymowski and Mariusz Ziółkowski from the year 1992 (p. 30). Polemic passion made Urbańczyk use overly strong statements, since he himself refers on the following page (p. 31) to the session which took place (with the participation of the Author) at the German Historical Institute in Warsaw in 2010, and was entitled ‘What was the Piast *regnum*, or: did states exist in the early medieval period?’, which — as he claims — was ‘an attempt to revive similar reasoning’. Of course, the Author may be critical about the discussion taking place in the Polish medieval studies or consider it as too modest, but it should not be stated that it does not occur at all. Another proof of it is the conference with the Author’s participation, which took place in Poznań in 2012, entitled ‘Institution “of an early state” in the perspective of the multitude and versatility of cultures’, although I understand that it was probably held too late to be mentioned in the reviewed book published in the same year. But one cannot use such justification for the total omission by the Author of two articles, by Tymowski from 2008²² and by Piotr Boroń from 2001.²³ It does not matter if — in the Author’s opinion — the aforementioned texts give a satisfactory answer to the research problem, which is discussed in the reviewed book. If he has a different opinion, he could mention them as insufficient, not deep enough, or engage in the polemics with them. But he cannot write that the subject which they deal with is not discussed at all and not notice two texts from recent years. It is not accidental that I mention these two texts — the researcher could easily become acquainted with them, since they were published in the volumes in which his texts are also contained.

Summing up: criticizing the current state of the whole scientific fields is not an easy task, and surely formulating an opinion containing general quantifiers is not justified, if one or two names are referred to as evidence. So maybe it would be more useful (although less impressive), instead of polemicizing with ‘Polish historiography’ (or ‘Polish archaeology’) as such, to initiate serious polemics with specific representatives of a given field of science.

²² Michał Tymowski, ‘Organizacje plemienne na obszarze Polski w IX–X w. w świetle antropologicznych teorii systemu segmentarnego i wodzostwa (*chiefdom*)’, in *Europa barbarica, Europa christiana. Studia mediævalia Carolo Modzelewski dedicata*, ed. Roman Michałowski et al., Warsaw, 2008, pp. 263–83.

²³ Piotr Boroń, ‘Słowiańskie plemię. O pojęciu i jego rozumieniu w polskiej historiografii’, in *Viae historicae. Księga jubileuszowa dedykowana Profesorowi Lechowi A. Tyszkiewiczowi w siedemdziesiątą rocznicę urodzin*, ed. Mateusz Goliński and Stanisław Rosik, Wrocław, 2001, pp. 189–207.

7. It is obvious that such polemics between archaeologists and historians will take place. Nevertheless, one can expect that representatives of both disciplines, entering their respective research fields, will carefully follow the findings stated so far. In order to avoid errors it is not even always necessary to get acquainted with the 'whole' literature on a given subject — sometimes it would be enough to read more carefully those works, which are used anyway. Therefore, although Sikorski is not the only historian writing about the practice of combining the positions of abbot and bishop in the Imperial Church (German: *Reichskirche*), it would suffice for the Author to get acquainted with the paragraph of the monography on Bishop Unger of Poznań, which is known to him,²⁴ to make substantive parts of his work look differently, since he would know that the fact that Unger appears in the document from the year 991 as the abbot of Memleben does not mean that he did not stay in Poland as a bishop, because he could combine these two functions. Urbańczyk rejects such a thought but he does not justify this rejection; he only writes: 'it is difficult to suppose that a bishop residing in Poland and subordinated to Mieszko I²⁵ could be allowed to concurrently hold a profitable position of the abbot of the emperor's convent foundation' (p. 251). He does not refer to Sikorski's comments or to the fact described by the latter and occurring in literature regarding combining these two positions. One would expect him to do so since — as I have mentioned — this is not a marginal issue from the point of view of the reasoning. The conviction that Unger did not stay in Poland before the year 991 constitutes a basis for several proposals: the one related to the circumstances of the creation of the *Dagome iudex* (pp. 251–53, 374), the conditions of the alleged construction of Poznań cathedral (pp. 258–59); it is also a partial explanation of the absence of Christian burials from the Mieszko period (pp. 355–56), and is supposed to explain why Mieszko did not initiate the programme of construction of churches (p. 254). The alleged absence of the bishop of Poznań in Mieszko's country in the first years after his consecration is supposed to strengthen the Author's concept for identifying the grave in Poznań cathedral as Jordan's grave (as the justification of burial with a small portable altar — p. 413), is taken into account in considerations on the character of the two first marriages of Bolesław Chrobry (p. 352), and, last but not least, is an important or maybe even a key element of reconstructing the political activities of Mieszko and his relations with the Empire in the period 985–92 (pp. 351–60). I find Sikorski's statement that there are no premises to exclude the presence of bishop Unger in Poznań as early as the year 983 justified. Therefore, since the Author did not invalidate this opinion, all I can do is to consider large fragments of the reviewed work based on the conviction that Unger came to Poland as late as in 991, as misguided or simply untrue.

²⁴ Dariusz A. Sikorski, *Kościół w Polsce za Mieszka I i Bolesława Chrobrego*, Poznań, 2011, pp. 186–91.

²⁵ Urbańczyk seems not to take into account the possibility that Unger, as a bishop, was still the emperor's subject, although there are reasons to suppose that this was the case. Cf. Sikorski, *Kościół w Polsce*, p. 193.

8. As we see from the above discussion, although the reviewed work considers cooperation of medieval history and archaeology as one of the main problems, it cannot serve as an example of this kind of successful cooperation. Although in the parts interpreting archaeological material the book is cognitively interesting for the historian (even though not always univocal), in the fragments containing an analysis of written texts it has noticeable methodological shortcomings. Shortcomings, for which — let us stress — one should not blame a non-historian, just like one should not be surprised that a non-archaeologist is not perfectly acquainted with archaeological methods. But this should all the more make the Author entering historians' research field carefully check the results of their research, especially on the issues on which he intends to polemicize with them; alas, in this aspect there are also numerous cases of negligence in the analysed work.

After these comments let us return to the key question: what conditions should be met by a book about Mieszko I, or rather about Piast dominion in the tenth century to be a better book? Well, such a work should take into account the comment of the Author, who cites Sikorski's statement that a historian 'using archaeological material usually commits drastic errors of interpretation, adding: 'an archaeologist also faces technique problems when attempting an independent reading of written sources' (p. 20).²⁶ The Author's response is a postulate to establish multidisciplinary studies, 'which would educate young researchers understanding equally well the methodological nuances of history and archaeology, but also other related disciplines (such as history of art and numismatics)' (p. 24).²⁷ One must agree with him and support such a postulate — medievalists often do not have even a basic knowledge on the methodological bases of disciplines other than their own, and our doctoral studies definitely do not offer it. However, on the other hand, one should realize that this postulate is rather utopian — the time and energy needed to properly deal with one's own field results in the fact that in other fields one can usually acquire basic knowledge at the most, and one cannot count on 'equally good understanding of nuances'. And even having general knowledge in the fields not being our specialty, we are still dilettantes limited to reporting the results of research achieved by the others, since we live in times of narrow specializations; whether we like it or not, the times of 'all-embracing scholars' have irretrievably passed.

But maybe I am wrong (and I would like to be) and such multidisciplinary studies offering 'equally good understanding of nuances' of different disciplines in medieval studies are possible. Does it mean that we have to wait for the work which would present a broad look at the beginnings of Polish statehood until we

²⁶ [the historian] 'wykorzystując materiał archeologiczny, zazwyczaj popełnia karygodne błędy interpretacyjne', 'tak i archeolog napotyka problemy warsztatowe, próbując samodzielnej lektury źródeł pisanych'.

²⁷ [studies] 'które wykształciłyby młodych badaczy rozumiejących równie dobrze niuanse metodologiczne historii i archeologii, ale także innych dyscyplin pokrewnych (np. historii sztuki i numizmatyki)'.

have the graduates of such multidisciplinary studies, so (if such studies were to be established) at least several if not more than a dozen years? Not necessarily. My answer to the question of how to write a better book about Mieszko is more simple. Such a work should have at least closely cooperating authors, a historian and an archaeologist, each of them well acquainted with the methodology of his field, and at the same time aware of his limitations.

(Translated by Elżbieta Petrajtis-O'Neill)

Summary

The article is a critical review of a monographic study by Przemysław Urbańczyk: *Mieszko Pierwszy Tajemniczy* (Toruń, 2012); at the same time, it concentrates more on relations between archaeology and history than on a systematic presentation of the reviewed publication. The fragments of the based exclusively on archaeological research are regarded as of greatest value even though the article's author simultaneously draws attention to certain inconsistencies hampering the use of the presented material by historians. The reviewer is more critical of the book's fragments dealing with an interpretation of written sources and in certain instances notices an erroneous approach. The very character of the polemic conducted by Przemysław Urbańczyk with historians also gives rise to doubts in view of his arbitrary and selective citations of their works and the sometimes not quite comprehensible focus on older studies. An acquaintance with some of the findings made by scholars, and often contained in works mentioned in Urbańczyk's book, would certainly make it possible to avoid dubious interpretations or to better justify his views. Nevertheless, it appears that the indicated problems can be regarded as typical for attempts at entering an unfamiliar domain — after all, similar mistakes appear also in the works of historians trying to interpret archaeological material on their own and without suitable training. This observation suggests the following thesis: the writing of much-needed works combining these two disciplines should be accompanied by a closer cooperation between representatives of both fields, particularly in the case of studies about the beginnings of Polish history.

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Biography: Grzegorz Pac — Dr, Institute of History, University of Warsaw. Research interests: Early and High Middle Ages in Poland and Europe, social role of women, queenship, ideology of power, hagiography and the cult of saints, the cult of the Virgin Mary, monasticism. E-mail: gl.pac@uw.edu.pl.