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THE ITALIAN EXCEPTION: A DEBATE
ON RONALD WITT'S "TWO LATIN
CULTURES OF MEDIEVAL ITALY"

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An Early Medievalist's View on Ronald Witt's «Two Latin Cultures»: Original Questions and Tentative Conclusions

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*The article discusses the first half of Ronald Witt's *The Two Latin Cultures and the Foundation of Renaissance Humanism in Medieval Italy*, devoted to the period between the 9th and 11th century. One of the aims of the book is offering a general history of the Latin culture of the Kingdom of Italy in the Early and High Middle Ages. The review thus considers the heuristic value of the conceptual framework proposed by Witt as a means to reconstruct the cultural history of the Kingdom in those centuries and addresses specific features and limits of Witt's overview. Finally the article discusses how certain more recent studies may help integrate or nuance the reconstruction proposed in the book.*

Convinced as I am that the primary causes of progress in historical research lie in posing original questions, it is my hope to introduce with this book a new historical problem. My own responses to the issues it broaches are based on four decades of reading, conversation and thought. All the same, I regard my conclusions as tentative, and more as challenges to other historians to disprove or expand upon, rather than as final answers to the questions I raise [Witt 2012, 13 (2017, 30)].

In the spirit of these considerations advanced by Witt concerning his own work at the end of the introduction to *The Two Latin Cultures*, I wish to offer a critical reading of its first half, devoted to the period

between the 9th and 11th century¹. As mentioned in the introduction to this debate, I think that the greatest merit of the volume lies in the author's choice to embark for the first time on a parallel analysis of the history of documentary (and later legal) culture and ecclesiastical/traditional-book culture, with an original and fruitful perspective, which covers the whole range of literacy and brings into focus the interactions between the two cultures thus identified. Within the framework of this debate, Dario Internullo will be highlighting the merits of integrating documentary and juridical culture for the sake of historical reconstruction, whereas I will be focusing on the other aspect of the volume that is of general interest: the fact that it explicitly presents itself as a general history of the Latin culture (or, rather, cultures) of the Kingdom of Italy in the Early and High Middle Ages [Witt 2012, 1 (2017, 17)].

Such an extensive yet synthetic overview touching upon all aspects related to the written sources from a kingdom, across four and a half centuries of history, could only be accomplished by a great scholar at the end of his career, by drawing upon «four decades of reading, conversation and thought», to quote Witt's own words. But, then again, Witt's overview is built on a very strong conceptual framework because it fits within his broader research on the origins of humanism and his enquiry into the emergence of the figure of the lay intellectual in the Kingdom of Italy. Acknowledging this simple fact immediately cautions us against approaching the volume as a “neutral” overview of Italian Latin culture in the Early and High Middle Ages – as a university textbook, for example. The temptation to do so is certainly strong, not least given the absence until now of any synthetic work of this kind.

In the following pages I wish, first of all, to reflect on the heuristic value of the conceptual framework proposed by Witt as a means to reconstruct the cultural history of the Kingdom of Italy in the 8th–11th

¹ In particular: parts I and II, which comprise chapters 1-4: Witt 2012, 1-225 (2017, 1-267).

centuries. Secondly, I wish to highlight certain features and limits of his overview. Finally, I aim to discuss how certain more recent studies may help integrate or nuance the reconstruction proposed in *The Two Latin Cultures*.

1. The question of the origins of the lay intellectual that lies at the basis of Witt's work is a legitimate and very interesting one. However, in Witt's own research it is strictly subordinated to the question of the genesis of humanism, which, in turn, is understood and defined with very specific characteristics, and mainly as literary classicism [Witt 2000, 22 (2005, 28)]. Therefore, back projection in search of the «historical antecedents of humanism [Witt 2012, IX (2017, 13)]» entails the risk of teleological distortion. To paraphrase something that Robert Black said with regard to *In the Footsteps of the Ancients*, it might be argued that Witt's account can become less a history of Italian Latin cultures than a history of those figures that meet his criteria as the antecedents of humanists². Even when viewed in such terms, his research is still legitimate. However, the use of interpretative categories designed to shed light on humanism – in particular, the imitation of the classics in original works and the clear-cut contrast between lay scholars and clerics, as well as between their respective cultures – does not strike me as the best way of developing a «general history» of the Latin culture of the Kingdom of Italy in these centuries.

The general outline of Witt's reconstruction suggests that traditional ecclesiastical culture in the Kingdom of Italy, while being the heir to the grammatical study of the classics, was de facto unproductive compared to the rest of Carolingian Europe: according to Witt, the only really “creative” culture within the Kingdom was the juridical-rhetorical one shaped by laymen, in particular from the 11th century onwards, [Witt 2012, 70 (2017, 93)]. However, this special focus on “producti-

² Robert Black's observed: «(Witt's) account can become less a history of humanism than of those figures that meet his criteria as humanists» [Black 2000, 275].

veness” and hence the general downplaying of traditional ecclesiastical culture, which draws upon the observations of Gaspari [1885-1888] and Novati, Monteverdi [1926], largely depends on the author’s way of conceiving humanism: the litmus test of humanism is the stylistic imitation of the classics, which can only be found in creative production (and, within this context, in works of a particular kind)³. Already in his 2000 book Witt had therefore ruled out philological humanism from his field of enquiry, which is to say the kind of study and preservation of the classics which does not take the form of original (and imitative) writing [Witt 2000, 22 (2005, 28)]. Actually, this comparison between cultural productivity in the Kingdom and in the rest of Carolingian Europe ought to be reassessed, for a number of reasons: first of all, and quite obviously, because the early-medieval Kingdom of Italy cannot be compared with the modern nations of France and Germany; a more fitting comparison, from the point of view of geographical scale and political profile, might be made with a macro-region of similar dimensions, such as early-medieval Aquitania or Bavaria. Secondly, in this comparison many works are simply ruled out a priori, as in the case of hagiographical and exegetical texts⁴. Finally, there is the problem of the chance conservation of our sources (of which the author is aware [Witt, 2012 10-11 (2017, 27-28)]): on the one hand, it may depend on the less prominent role played by monastic libraries in the Kingdom compared to transalpine Europe (for these libraries are more conservative than the cathedral libraries so common in the Italian context); on the other hand, it may be due to the very features of literary production in Italy, as suggested by some evidence we shall soon be considering. This caution notwithstanding, Witt’s comparison remains a useful and stimulating one, insofar as it brings into focus a real difference *vis-à-vis*

³ See Vera Fravventura’s review in this debate.

⁴ On the relevance of hagiography: Vocino 2014. On exegesis, see Fravventura’s review.

transalpine Europe, which ought to be searched for on the qualitative rather than quantitative level. This constitutes a new question from which scholars must set out, as François Bougard has already suggested in his recent paper at the international conference *Was there a Carolingian Italy?*⁵

2. As in the case just mentioned, Witt's set of questions, while not particularly suitable for tracing a general history, provide some stimulating insights for future research, because the questions are posed from an original angle. A number of features of Witt's reconstruction are worth discussing in this respect, both with regard to their relevance to the conceptual framework structuring the book and independently of it.

Given the strong interpretive approach of the volume, the choice of the Carolingian conquest as the starting point of this long path does not seem completely coherent. It is an understandable choice, as it is justified by the increase in the number of available sources from the 9th century onwards [Witt 2012, 2 (2017, 18)], but it fails to grasp a particularly significant moment in the history of either culture: the peculiarities of both cultures – as regards the late-antique rhetorical-grammatical heritage and the legal importance assigned to written documents (particularly bolstered during Liutprand's reign)⁶ – were already distinguishing traits of the Lombard Kingdom *prior to* the Frankish conquest. Any transformation following the incorporation into the Frankish framework, through the application of “Carolingian programmes”, does not reveal any “Italian exceptionalism”; if anything, it brings the centre-north of Italy closer to transalpine Europe. In fact, in certain cases, such as that of documentary culture and of the presence

⁵ This was held in Vienna on the 25th and 26th of April 2016. The proceedings will be published by Cambridge University Press under the title of *After Charlemagne: Carolingian Italy and its rulers*.

⁶ See Lazzari 2017 and Everett 2003, 197–234. Cf. Dario Internullo's review.

of lay notaries, this incorporation led to a paradoxical development: in the medium term, the reforms promoted in this field by Charlemagne were only concretely implemented in the Kingdom of Italy, making it – in this respect – the most genuinely “Carolingian” part of the Empire, if only on account of its original peculiarity [Ghignoli 2004; Bougard 2009 and 2019].

A second problem with the structure of the general history presented in the volume is the author’s choice to have the various chapters coincide with the major stages in the Kingdom’s political history (partly identified according to obsolete schemes), rather than to follow the stages of development of the two cultures that are the focus of enquiry. Thus the chapter on the “Carolingian conquest” is immediately followed by one on the “Ottonian renaissance”, as two definitely separate phases. In such a way, the two chapters blur, so to speak, the “post-Carolingian” phase (888–962), which is *de facto* incorporated into the Ottonian chapter; but what is lost through this approach is something important for both cultures.

As far as traditional ecclesiastical culture is concerned, the author overlooks the fact that surviving Italian sources from this period, such as the *Gesta Berengarii imperatoris* or the works of Atto of Vercelli, stand in absolute continuity with what preceded them and arguably represent the most productive and original stage of a fully Carolingian culture in Italy. If literary production in this period seems different from that of the early 9th century, it is only because such culture reaped what previous generations of Carolingian masters had sown, and particularly with regard to the study of Classics. It seems to me that Witt’s approach is mainly due to the old interpretative frameworks of the “Century of Iron” and “second invasions”, with the accompanying idea of regionalisation and cultural decline: models already outdated at the time of his writing [Leonardi 1999].

The disappearance of a self-standing post-Carolingian phase might also seem questionable from the point of view of “lay documentary

culture”. In Witt’s reconstruction, it is precisely this phase that marks a first turning point, in particular through the transformation of judges into *iudices domni regis* and of local notaries into *notarii sacri palatii* at the hands of Hugh of Arles, as a means of imposing royal hegemony on the high aristocracy – according to a policy later adopted and further strengthened by the Ottonians in a different context [Witt 2012, 112 (2017, 140)]. This transformation is to be evaluated (and measured in its effectiveness) within the post-Carolingian context that produced it, rather than placed within a predetermined line of development. But in this case too, Witt is still offering us an original insight.

Witt defines the actual Ottonian phase on the basis of the new educational curriculum which in his view was promoted by the Ottonians, and which he sums up using the formula *litterae et mores*. This formula is borrowed from C. Stephen Jaeger’s study *The Envy of Angels. Cathedral Schools and Social Ideals in Medieval Europe, 950-1200*. Published in 1994, Jaeger’s study focused on the 12th-century transition from a “Charismatic Culture” to an “Intellectual Culture”, and derived the idea of a new educational curriculum from a rather rigid understanding of the Ottonian-Salian *Reichskirchensystem* – which was usual in those years. If this was the specific system of government in which clergymen were called to take part, it follows – according to Jaeger – that they must have received an ethical-professional education (i.e. one based on *litterae et mores*, even though this expression does not occur in our sources), as opposed to a “literary” one. This idea, which Witt fully embraces, once more leads to an expected outcome: if government was the purpose of this ecclesiastical transformation, this explains the low productivity of traditional culture even at such a stage.

Leaving aside the weight of this theory in the reconstruction offered by the volume, it may be seen as revealing of a more general aspect: with few exceptions, the reference bibliography does not go beyond the 1990s or even, as regards Italian historiography, the 1970s – the period in which Witt first outlined the plan of his ambitious research into

the origins of humanism. This is quite understandable for an overview produced after an extensive course of study and which touches upon such a wide range of topics – particularly when it comes to those centuries on which the author is not a specialist. However, the reader must be made aware of this, because it influences both the overall framework of Witt’s general history and specific aspects of his reconstruction. As far as the latter are concerned, the use of studies and editions from as far back as the early 20th century, often ones with a local focus, leads to the repetition of errors that have largely been overcome and of misunderstandings that could easily have been avoided⁷.

The historiographical framework adopted also accounts for the main limitation of Witt’s analysis of the great transformations of the 11th century. What emerges here is the author’s openly “Flichean” approach [Witt 2012, 184 (2017, 222)] of an *Eglise au pouvoir des laïcs* – which actually underlies all the previous chapters as well. For example, Rather of Verona and Atto of Vercelli are still seen as «pioneers of the Reform», thereby assigning them an ecclesiological perspective that was only to emerge one century after their death, in a very different context. Also connected to this approach is one of the underlying problems of the first half of the volume, namely whether it is really possible to clearly distinguish between “lay” and ecclesiastical culture prior to the 11th-century reforms – a problem that Witt is in any case aware of [Witt, 2012 5 (2017, 22)]. This is not the place to retrace the long historiographical season that, from the 1960s onwards, challenged the traditio-

⁷ To take just a few examples: Leodoin’s letter (which, owing to a typo, is erroneously dated one century later) does not deal with teaching [Witt 2012, 50 (2017, 70)]; Heraclius of Liège did not teach in Verona, but in Bonn [Witt 2012, 80 (2017, 105)]; *Israel grammaticus* was probably Breton, and certainly not from Italy [Witt 2012, 76 and 78 (2017, 101-103)]; the three *Ambrogius* identified as the same person must be at least two different figures with the same name, for chronological reasons [Witt 2012, 78 and 81 (2017, 103 and 106)]; and Drogo of Parma and Drogo of Liège were certainly two different figures [Witt 2012, 150-151 (2017, 184-185)]. These are minor errors: regrettably, however, in certain pages such slips are so frequent as to make the reconstruction unreliable.

nal interpretation of the “Gregorian reform”. However, it must be noted that the reconstruction offered by the volume would have benefited from greater attention to the complexity of the processes underway and of the range of actors involved, which are taken for granted in contemporary studies⁸. Phenomena that Witt quite rightly brings into focus – such as the growing importance of dialectic, the triumph of the juridical-rhetorical mentality, and the space gained by canon law studies in ecclesiastical culture at the turn of the 12th century, to the detriment of other traditional disciplines – are much better explained by the development of the above processes and, later, by the struggle for a new supremacy waged by the Church of Rome against episcopal churches, rather than within the framework of a simple contrast between papacy and Empire or, worse still, by the alleged anti-Classicism of someone like Pier Damiani or of the Reformers party [Witt 2012, 176–177 (2017, 213)]. The fine pages that the author devotes to *Libelli de lite* [Witt, 2012 191–201 (2017, 230–241)], by successively discussing the works of Pietro Crasso, Benzo of Alba, Guido of Ferrara, Bonizo and Anselm of Lucca, would no doubt profit from this perspective.

3. The recent historiographical developments that may nuance or enrich the framework proposed by Witt are numerous, and I will only briefly touch upon some of them, including both specific studies and general approaches.

As far as literary productiveness in the 9th century is concerned, recent findings suggest that we not just redefine the comparison drawn by Witt, but actually frame the whole question in a different way, as already noted. The recent rediscovery of a fragment with 53 hexameters from a panegyric to Louis II, probably composed between 872 and 875 [Orth 2017], provides an unexpected glimpse of the level of Carolingian culture in Italy (in terms of both its creativeness and relation with

⁸ Witt refers to Fliche 1924–37: Witt 2012, 184 (2017, 222). The debate dates back to Capitani 1965 and Miccoli 1966. For a general overview: Cantarella 2001; for general references and a bibliography: Sereno 2006.

the Classics), while at the same time giving us an idea of how much we may have lost, in particular when it comes to encomiastic and “occasional” literature of this sort, which probably characterised the rhetorical-grammatical culture of the Kingdom. Besides, texts of this kind are very likely to become lost, once the political conditions in which they were produced have changed.

The most recent historiography can also serve as a benchmark for the topic of the relation between the two cultures in these centuries: as far back as 2004, Antonella Ghignoli published a study that makes it possible to further define and articulate Witt’s suggestions with regard to the relation between ecclesiastical institutions and lay notaries, on the one hand, and between ecclesiastical “notaries” and royal documentary production, on the other [Ghignoli 2004]. This latter topic can now be set out more clearly thanks to Wolfgang Huschner’s works on royal chanceries [Huschner 2003] and, more generally, publications connected to the *Italia Regia* project.

One of the most promising fields of research in recent years for the history of early and high medieval culture is the study of glosses, commentaries and paratextual apparatuses produced from the mid-Carolingian period either to accompany the writings of ancient authors or as part of original works (e.g. ones written in the so-called hermeneutic or glossematic style)⁹. These texts rarely found any place in 19th and 20th-century editions, which treated them as mere school products. New editions and studies are progressively showing how through such texts (as well as lexicographical works)¹⁰ it is possible to highlight the

⁹ Some examples of recent editions are: Zetzel 2005; Scholz, Wiener, Schlegelmilch 2009; Hellmann, Ullrich, Wiener 2011; Grazzini 2011, 2012 and 2018; San Juan Manso 2015; Some overviews and individual studies on the topic may be found in: Teeuwen 2011 and 2015; Masselli, Sivo 2017; Teeuwen, van Renswoude 2017; Duplessis 2015 and 2017; Vignodelli 2017.

¹⁰ These include, among others, the new edition of the *Liber glossarum*: Grondeux, Cinato 2016.

interconnection between the various European centres, the spread of studies and succession of generations of teachers, with an unprecedented level of detail for these centuries. Some significant results are already available for the post-Carolingian phase: the integration of Italian authors and *scholastici* with transalpine ones during this stage of alleged regionalisation seems evident by now. This interconnection predates the Ottonian conquest of the Kingdom, suggesting a degree of late-Carolingian continuity, as in the case of the early (if not immediate) circulation in Italy of commentaries from the “School of Auxerre” and of the itinerant education received by the anonymous author of the *Gesta Berengarii imperatoris*, known today through the works of Frédéric Duplessis [2015 and 2017]. But the same close connections are also reflected by the role played both by well-known Italian masters such as Ambrose, the teacher of *Israel grammaticus*, and previously unknown ones, such as the exegete Lanfranc, brought to light through Hartmut Hoffmann’s analysis of the Paoline commentaries from *Würzburg* [Hoffmann 2009]. These findings, moreover, will allow us to consider the relations and possible differences between the two traditional “renaissances” – the Carolingian and the Ottonian – and to investigate the specific role played by Italy in passing down the title from the former to the latter.

To conclude, Witt’s volume offers many new questions and many insights for future research, which here I have only partly summed up, for the benefit of scholars investigating the history of the “two cultures” of the Kingdom of Italy between the Early and the High Middle Ages – even though those centuries do not really constitute the focus and purpose of Witt’s reconstruction. Much has been done since the years in which Witt first conceived his research, yet much more remains to be done: it is up to historians to accept Witt’s conclusions «as challenges to disprove or expand upon», thereby honouring his advice and work.

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