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Response to Bruce Venarde's review of Constance Berman, *The Cistercian Evolution: The Invention of a Religious Order in Twelfth-Century Europe*.

By Constance Berman, University of Iowa.

Venarde opens his discussion of *The Cistercian Evolution* with a careful summary of its findings on how, when, and how much the Cistercians contributed to that process of institution-building in the twelfth century that I have called the "Invention of a Religious Order." He seems to understand that although I do not question traditional assertions that the Cistercians invented this new institution, mine is nonetheless a very different picture of the Cistercians from standard ones to date. As I show, the process of inventing the new institution was slower than traditionally thought, involving trial and error, and probably borrowing innovations from other twelfth-century groups. I thus deny the precocity and exclusivity of the Cistercians in creating the religious Order, suggesting that it dates to considerably later in the twelfth century than the year 1120.

Venarde is correct that this is not the long-awaited book on Cistercian nuns. Writing *The Cistercian Evolution* intervened in my work on religious women. As I began to investigate the early history of Cistercian nuns, I found more and more discrepancies between traditional Cistercian narratives and the evidence I was collecting: on Cistercian women, on southern-French affiliations of houses by the Cistercians, on the types of land that Cistercians accumulated. I found myself dismantling a traditional model of twelfth-century monastic history that appropriated for the Cistercians a very early creation of the religious Order as a new institution and at the same time denied that women were part of that reform movement. If attempting to establish an objective early history for the Order meant rewriting much of its twelfth-century history for the sake of eventually restoring women to their roles in that development, that has been just part of the job. It is to be hoped that *The Cistercian Evolution* will clear the ground for my own continued writing on Cistercian nuns, and that my "time-out" from writing on women will benefit the work of others as well. [1]

In *The Cistercian Evolution*, I use three independent strands of argument interwoven to support two related revisions to our understanding of the traditional narrative of early Cistercian history. *One finding is* that evidence for the religious Order as an institution, including for the Cistercian Order as a specific institution, appears only in the late 1140s; moreover, there is considerable evidence that monastic communities did not operate under the oversight of such an institution until that decade or later. *A second and related finding is* that such an incremental development of this new institution during the twelfth century has been obscured by treatment of the anonymous, in-house Cistercian narratives such as the *Exordium Parvum* as if they were reliable, eyewitness accounts of early-twelfth century events: in fact, they only appear in manuscripts which can be dated to the 1160s or later and their reliability should always have been suspect. These two revisions of our conclusions support a *new picture* of Order-building in *The Cistercian Evolution*. My work allows us to describe the emergence of an Order possibly beginning with the creation of a special group of houses associated with Clairvaux in the 1140s. Indeed, for a brief moment in the 1140s it was a Clarevallian Order that almost emerged. But after the deaths in 1153 of Bernard of Clairvaux and Eugenius III (whose association with Clairvaux means that he might

as well be called a Clarevallian as a Cistercian pope), new forces within the reform movement associated with Cîteaux and Clairvaux came into play. New leaders began to construct the Cistercian Order as we tend to know it in its full-blown, thirteenth-century form. They had begun to do so primarily in the decade of the 1160s when the Order came under considerable criticism from Pope Alexander III. I suggest that the creation of an Order in which some adherence to standards would be expected was in response to that papal censure.

It is regarding that criticism of the Cistercians by Alexander III in the 1160s that I would contend that it is not I who has, as Venarde complains, “tossed-out” documents that have gotten in the way of my story. Instead, traditional Cistercian historians have ignored inconvenient documents, such as the contents of a papal bull by Alexander III in which Cistercians are criticized, by omitting the full text from standard collections and ignoring the attempts to restore that text by Jean Leclercq.[2] I have, it is true, as Venarde contends, questioned the authenticity and dates of certain documents, but I have used even the contents of redated documents to tell my very different story of Cistercian origins.

Given the complexity of the story told in *The Cistercian Evolution*, the reader interested in how I weigh the reliability of various types of documents, or my methods and justification for using the evidence of charters (even in cartulary copies) in large numbers, may find it useful to look at it in reverse order. My discussion of charters in chapter four may be usefully followed by that in chapter three, where I discuss the appearance of the word “*ordo*” in large numbers of such diplomatic sources, and then by the contents of chapters one and two. It is in that order that I discuss *three independent strands of evidence*, all three of which stand virtually independent of one another, which support my conclusions in *The Cistercian Evolution*.

I begin with a first strand of evidence, that of charters and the use of charters in large numbers. In chapter four I discuss the reliability of evidence of charters and the viewpoint of those monastic scribes who made them, making the point that in general they tend to deflect attention away from the monastic community as an active agent acquiring endowment and towards the donor seeking heavenly benefits.[3] Despite that bias, the peripheral evidence from such charters, when used in large numbers, can be useful.[4] In chapter three I turn to the evidence for changes in word usage derived from large numbers of such charters, sometimes available only in cartulary copies, to establish trends about the changing use of the word “*ordo*,” both in the charters and in other types of evidence.[5] This evidence from large numbers of charters produced by monastic houses in southern France and elsewhere shows that the word “*ordo*” continued to mean a way of life—and not a religious institution—until the last years of the 1140s.[6] Here there was no need for concern about whether the documents being used were copies or originals, but only about the dating of the earliest ones in which “*ordo*” appeared, those of the 1140s. These charters showed no use before the late 1140s of “*ordo*” to mean, “our group,” and no use of the words “*ordo cisterciensis*” until somewhat later. Yet the words “*ordo cisterciensis*” would be commonplace in every charter by the thirteenth century simply to identify a religious house. The shift between “*ordo*” as a way of life, for instance in “*ordo monasticus*,” and “*ordo*” meaning our particular group, as eventually in “*ordo cisterciensis*,” appears to be confirmed more generally, for instance in letter and charter collections for Cistercians and non-Cistercians alike.[7]

Others have suggested that in at least one of *The Letters of Bernard of Clairvaux* translated by Bruno Scott James, Bernard uses the word Order to mean “our group.”[8] While this meaning seems very possible for the 1140s, the assertion that this and associated letters date from the 1120s is less likely. Letters admonishing Arnald, abbot of Morimond, and his companions not to abandon their monastery to go on Crusade have only quite recently been asserted to date to the 1120s on the argument that the Pope C addressed in one of these was Calixtus II (1119-1124) rather than Celestine II (1143-44), despite Mabillon’s assertions that it was the latter. It is likely that this is an error arising from too easy dependence on Canivez; the early abbots of Morimond are in fact obscure, and the assertion that there even was an abbot Arnaldus of Morimond in the 1120s to whom Bernard could have written is based on

nothing but a reference to Canivez, according to Constance B. Bouchard's list of abbots.[9] Canivez cites the letter, thus making a fully circular argument by the editors of the letters. In all likelihood, Bernard's letters concerning Morimond's abbot and monks deciding to join the Crusade date to after the fall of Edessa in 1143 and Bernard's preaching of the Second Crusade. Moreover, the addressee found in the collection of Bernard's letters, abbot Arnaldus, most likely reflects a copying error, Arnaldus being the misheard homonym for R'nald or Raynaldus, the abbot of Morimond in the 1140s confirmed by more evidence.

While the issues concerning the use of charters in large numbers may be specific to medievalists who must invent, as scholars such as David Herlihy did, creative ways to use intractable medieval sources, *the second and third strands of evidence* for my argument are developed by the use of more standard evaluation of sources, by asking the commonsensical questions that all historians should ask of all sources. Such methods that we all use, often without being explicit about them, are set out in detail in the recent volume by Martha Howell and Walter Prevenier, *From Reliable Sources*.[10] This is standard source-criticism at its best, of the sort associated with Mabillon and nineteenth-century positivism, applied not only to medieval and early-modern problems but to issues from the recent past. It is worth outlining here the necessary skepticism of any historian in face of her sources with queries provided by Howell and Prevenier, which I restate only slightly:

1. What does the source say?
2. What are the facts of its composition? That is, when was it composed? Where? In what social setting or under what legal circumstances? By what individual or individuals?
3. If these questions about its composition are not provided by the source itself, how can we determine such facts as a probable date of composition, where it was composed?
4. Is it what it appears to be? Is it authentic?
5. What is the competence of the author? Did he or she have first-hand knowledge or depend on earlier written or oral reports? Was he apt to interpret life in a negative way? Was he satisfied with his circumstances or casting envious glances elsewhere? Is there reason to believe that he could have known what he was talking about?
6. Is the author selective in his reporting? What parts can we tell he or she leaves out?
7. What prejudices inform the account? Is this a work that is self-congratulatory? Self-justifying? Under what outside influences was it written? In what ways did the author suppress or shade knowledge to conform to particular models, party-lines, or opinions? Was this account written by the winners or the losers? Were issues of politics, reputation, or vanity at play?

These are powerful questions to ask, although as Howell and Prevenier conclude, "Even when all the sources available agree, even when there are a lot of sources concerning an event, even then historians cannot be sure about the facts because it is often the case that the sources available represent the opinions, or the versions of the events, of the winning party in a contest or the dominant powers in a system." [11] These are the kinds of concerns I brought to my sources in *The Cistercian Evolution*, for we must certainly treat the Cistercians as the winners in twelfth-century monasticism.

These standard ways in which historians since the nineteenth century and earlier have weighed sources had not been applied previously to the Cistercian sources that I treat in this study. Such questions, when asked of those "primitive documents," quickly reveal a wealth of problems about their past handling. For example, one of my early findings in this regard was that the first volume of a standard much-cited collection of Cistercian *Statuta*, edited by Canivez in 1933, does not distinguish in its entries for the twelfth century between narrative accounts written by early-modern Cistercian abbots and authentic twelfth-century documents.[12] The fact that such source criticism has never been applied to early Cistercian documentation goes a long way towards answering Venarde's question about why my book is full of findings of forgeries and misdated documents. No self-respecting nineteenth- or twentieth-century historian of France or elsewhere would take such evidence written by a group in support of its

own interests at face value in quite the ways that historians of medieval religion have taken these Cistercian in-house accounts. But much twelfth-century monastic history is derived from undated, anonymous in-house justifications written by such winners.[13] In hindsight, I can see that in writing *The Cistercian Evolution* I was probably too cautious in that I did not attempt to impugn sources such as the *Exordium Parvum* on the basis of such built-in biases alone. Instead, I adhered to the established terms of discussion about Cistercian history, which have rejected any suggestion of biased reporting by in-house sources. Thus in order to maintain any discussion with the large group of non-professional historians who work on Cistercian history, I was forced to establish the reliability or unreliability of the early Cistercian narratives on more basic terms, by evaluating their authenticity and date. This is what is done in the second and third strands of my argument described next.

The second strand of the argument in *The Cistercian Evolution* concerns the narrative accounts for the creation of the Cistercian Order, often called the “primitive” or early Cistercian documents. They are found in a series of manuscripts which contain the earliest versions of the Cistercian customary of liturgical and other practices, the *Ecclesiastica Officia*. [14] The Dijon 114 manuscript, which contains the *Ecclesiastica Officia* (but not the *Exordium Parvum*) provides a *terminus ad quem* for dating the series of manuscripts to the 1180s. [15] But critical to establishing the earliest possible date for this series is my discussion of two manuscripts earlier than Dijon 114: Trent 1711 (in which the *Exordium Cistercii* is not dated to 1135 by the liturgical text but is in a quire attached later) and Montpellier H322 (which contains statutes dated to the late 1150s but has as yet no *Exordium Parvum*). My discussion of these manuscripts establishes that the earliest extant manuscript evidence for these narrative accounts of the foundation at Cîteaux comes from the 1160s and that the composition of any versions that have subsequently disappeared must have been in the third, not the first, quarter of the twelfth century. This means that neither the *Exordium Cistercii* nor the *Exordium Parvum* is an eyewitness account and that both were written a half century after the events, a half century during which considerable change in monastic institutions occurred. Their accounts of the foundation at Cîteaux are unreliable if for no other reason than because they are not from the date to which they have been attributed.

But in fact, this is to kill an ant with a sledgehammer. Even if it were impossible to demonstrate (as I have) that these accounts come from the later dates, they are nonetheless suspect as soon as one begins asking of them the questions listed above. In particular, because we have a simple account of the foundation of Cîteaux, the *Exordium Cistercii*, which contains none of the charters which I might call pseudo-charters found in the *Exordium Parvum*, the latter would be pronounced by the principles of source criticism established by Mabillon as an expansion on the earlier text into which additional materials, even further from the events of the *Exordium Cistercii*, were incorporated. Such an anonymous and undated “in-house” account should be treated with suspicion. The fact that it contains “documents” can in no way be used to argue for its authenticity, as has been claimed, particularly since none of the documents embedded in the *Exordium Parvum* can be shown to exist outside the pages of *Exordium Parvum* manuscripts. The same argument goes for the purported papal bull of Calixtus II *Ad hoc in Apostolicae*, which also appears for the first time only in the *Exordium Parvum* manuscripts. Robert’s *Bullaire* for Calixtus II, which collects the language of the authentic originals for this pope, shows that *Ad Hoc in Apostolicae* does not conform to that language. [16] Instead, *Ad Hoc in Apostolicae*, attributed to Calixtus II, is a forgery, one of the group of forgeries confected to add meat to the bare bones of the *Exordium Cistercii*. It cannot be used to support arguments for an issuance of the Cistercian *Charter of Charity* in 1119. Similarly, although the *Charter of Charity* might be dated to perhaps a decade earlier than the first extant manuscripts in which it appears, it cannot be dated to forty-five years earlier than the first written trace in manuscripts that I date to the 1160s for reasons having to do with the series of customaries mentioned above. Related documents, such as the earliest statutes, come from the late 1150s, and the collection traditionally dated to 1134 must be asserted to be even later than the 1150s.

A third strand of argument in *The Cistercian Evolution* is the very fact to which Venarde calls attention: that when you begin to push at the documents held up as proofs for an early foundation of a Cistercian

documents, such “proof texts” turn out to be either forgeries or misdated or not clearly applicable to the specific case they are being used to uphold. It is interesting that Ludwig Falkenstein had noted only slightly earlier the problem of such precocity of Cistercian documents and in particular of *Ad Hoc in Apostolicae*.<sup>[17]</sup> That such proof texts turn out to be dated too early is particularly clear with regard to another purported papal bull, Innocent II’s *Habitantes in Domo* of 1132 addressed to Cîteaux, which I reject as a forgery, although accepting the parallel bull of tithe exemption granted to Clairvaux. That there was a problem about the 1132 *Habitantes in Domo* for Cîteaux has been known since the late middle ages because notices from that period in the same folders in Dijon as some of the copies of papal tithe privileges note a problem. The Clairvaux bull purportedly written within the same week, on the other hand, is likely authentic. It is different in its language and does not mention an Order of any kind but only abbeys subject to Bernard. There are technical issues about the text of *Habitantes in Domo* and about the earliest extant copies where the rota inscription is one from the 1180s, not an 1140s one. But it is the fact that *Habitantes in Domo* as a tithe privilege for the entire Order also does not make sense in the larger situation that is most telling. It can only hold if we assume a very early creation of an Order (which is not shown by the other strands of evidence), and if it is reliable, why a separate and totally differently worded grant to Clairvaux?

In contrast, if it is the Clairvaux privilege that is authentic, it can fit more easily into the general story of twelfth-century events, as follows: This is a grant of tithe privilege to Bernard and abbeys subject to him because of, as Innocent II says, “the firmness, constancy, and perseverance of Bernard in defense of the cause of the blessed Peter and your holy mother the Roman church in the conflagration of the schism of the Pierleoni.” Bernard, having supported Innocent, now receives a lavish concession. What remains somewhat surprising to me is that when Cîteaux’s monks discovered (in the 1400s?) that there was no 1132 privilege for Cîteaux, they did not simply copy that for Clairvaux; it was easier just to copy or transform that issued to Cîteaux in the 1180s.

In terms of my larger argument, the fact that I find no reason to doubt Innocent II’s grant to Clairvaux can be added to two smaller points about the dating of documents and events to 1142 and 1147 which confirm my argument that if any religious Order did develop before 1153, it was a Clarevallian, rather than a Cistercian one.<sup>[18]</sup> The tithe privilege for Clairvaux and abbeys subject to it of 1132 is part of a larger pattern in which we see houses and individuals flocking to attach themselves to Clairvaux, the most famous of those individuals, of course, being the future Eugenius III. Possibly a first break in the sequence of Clarevallian, rather than Cistercian, documents might be seen in 1142. The charter published by Canivez is a copy from the archives of a Praemonstratensian house now found in Chaumont; it is not the original that Canivez says it is, but a copy that once had a seal, as can be seen in my figure 23, page 84. But there is good evidence in a letter from Bernard of Clairvaux to Hugh of Fosses, dated by Leclercq to circa 1150, that a Peace Charter may have originally been established between Praemonstratensians and Clarevallians circa 1150, rather than with all Cistercians in 1142. Certainly the letter from Bernard to Hugh of Fosses concerns issues as yet unresolved between Prémontré and Clairvaux.<sup>[19]</sup>

The same could be said of the early development of a description of what I contend was an entirely fictional meeting of the General Chapter at Cîteaux in 1147 with Pope Eugenius III in attendance. I do not doubt that Eugenius III was in France or that he visited Clairvaux. There is what appears to be an authentic agreement to “something” between Eugenius III and the abbot of Savigny at Saint-Seine at the end of September 1147, and there is no reason to disbelieve that particular papal bull. (A series of later copies of papal bulls dated to Cîteaux and to the mid-September traditional general chapter dates seem to be forgeries; if there had been a General Chapter the Pope would certainly not have been issuing bulls to non-Cistercian houses at the time.) Whatever happened with the abbot of Savigny in 1147 was not in a General Chapter at Cîteaux and was in a Clarevallian, not a Cistercian, context. We might surmise an agreement on forming a prayer society or sharing liturgical practices, the first step towards a later “merger” between Savigny and Clairvaux. There is no other contemporary evidence for

what later came to be described as a General Chapter at Cîteaux in 1147 with Eugenius III in attendance, a meeting at which Savigny and Obazine were incorporated and Sempringham refused. This makes a good story, found in the *Vita Prima* of Bernard of Clairvaux (revised after 1160, although some parts of it were written before his death, and another source that needs to be treated with some of the questions discussed above), but the evidence of other documents does not support it. Indeed, historians have to tie themselves into knots to make this account fit the archival evidence for those congregations, particularly for the Savigniacs in England, whose archives show that they were not affiliated so early with the Cistercians. Such Savigniac evidence fits better with mergers in the 1160s than the 1140s. These considerations of how the various documents used to support the story fits better with the creation of an Order in the 1160s comprise the third independent strand of evidence that I have woven together in *The Cistercian Evolution*.

Finally, Venarde expresses unease about how little I have provided to replace the traditional narrative. I have given approximate dates for the first moves towards an Order in the late 1140s (when it was probably a Clarevallian order that was at issue). I have also pointed to the date for a first articulation of an Order with supporting texts in the 1160s at a time when the Cistercians are reacting to papal denunciations and to an initial papal refusal to canonize Bernard of Clairvaux. I have not named by whom and where the *Exordium Parvum*, for instance, was written. I do not know, although there are some individuals associated with Grandselve, the great abbey near Toulouse attached by Bernard in the mid-1140s, who might be good candidates. The earliest reliable statutes suggest that the Order's major practices were finally ironed out in the 1180s at the earliest dated meetings of the General Chapter.

The scenario I can provide is that thing got urgent sometime after Bernard of Clairvaux's death in 1153, and probably after the first unsuccessful attempt at getting him canonized circa 1160, and possibly even after the papal denunciations of the Cistercians by Alexander III in 1165. At that point a gathering of abbots occurred that might be envisioned as something like an Iowa political caucus, but with monastic leaders instead of party members splitting themselves into interest groups and congregating in different corners of the room. Those concentrating on rewriting the *Vita Prima* of Bernard went to one corner and those interested in responding to denunciations of the original foundation of Cîteaux and its secession from Molesme into another. The split into two groups would be along lines associated with the later filiations or branches of filiations. Those associated with Clarevallian and Savigniac houses were primarily concerned to rewrite the *Vita Prima*. Those concerned about criticism of recent practice and Cîteaux's origins (but also about the Clarevallian assumption that it was more equal than others even after Bernard's death) would work on a document that became the *Exordium Parvum*, which is remarkable in its leaving out of any mention of Bernard at all. This latter group would have included members of houses that would be considered daughters of Cîteaux, such as Bonnevaux (diocese of Vienne) and probably those houses associated with la Ferté and Pontigny; it is likely that Morimond took no part. It was this second group that started with an early draft which was the *Exordium Cistercii*, but made it more forceful by confecting a series of documents to insert or attach to it in the form that is known as the *Exordium Parvum*. It was they who borrowed language from a Bonnevaux papal bull of 1120 from which to confect an 1119 papal confirmation by Calixtus II of Cistercian practices, which has come to be known as *Ad hoc in apostolicae*. (Indeed, the point of my Appendix three was to suggest the Bonnevaux archives as a source from which that forgery could be made. Venarde quite rightly notes that the Bonnevaux bull is also a copy, although its language conforms to the rules for usage that Robert established in his *Bullaire*. There is also much stronger evidence for Calixtus II, who had founded the abbey before being elevated to the Papacy, to have confirmed a list of property for Bonnevaux than there is for his issuing something like *Ad hoc in Apostolicae*.)

Whether or not its readers find it correct in every detail, *The Cistercian Evolution* should have repercussions for our understanding of the twelfth century. For those yet to embark on the many new dissertations on the religion of the high middle ages still needing to be written, these remarks should clarify some of the traditional source-criticism that I have incorporated into my work.[20] Venarde's

sympathetic reading and careful raising of points needing clarification have made it possible to look back myself with fresh eyes at what I was attempting and what I accomplished in *The Cistercian Evolution*. I look forward to future reviews and further discussion when I have completed *The White Nuns*.

## NOTES

[1] See Constance H. Berman, *Women and Monasticism in Medieval Europe: Sisters and Patrons of the Cistercian Order* (Kalamazoo, Mich.: The Medieval Institute, 2002), a collection of translated documents for teaching; a monograph on Cistercian women, *The White Nuns*, is close to completion.

[2] See Appendix V of *The Cistercian Evolution* where I reproduce the entire Latin text of a papal letter of Alexander III addressed to the assembled Chapter at Cîteaux in 1170. Its opening paragraphs of praise are included in the pages of Canivez's *Statuta* published in 1933 and cited by Cistercian historians; so is its final paragraph. Central parts of the letter were omitted by Canivez; Jean Leclercq's restoration of the rest of the text, in "Passage supprimé dans une épître d'Alexandre III," *Revue bénédictine* 62 (1952): 149-51, changes the 1170 papal letter from one of wholehearted praise to one in which a polite opening is followed by censure.

[3] The major problematic charters are "foundation charters" which, as V.H. Galbraith noted in "Monastic Foundation Charters of the Eleventh and Twelfth Centuries," *The Cambridge Historical Journal* 4 (1934): 205-222, 296-298, are often written retrospectively, collapsing into a single act events that had happened over some time. We must beware of foundation charters taken out of the context of the medieval charters, for instance those found in the *Gallia Christiana* or other such texts, for not all such charters extracted from other contexts are equally reliable.

[4] Using large numbers of charters to determine trends was done brilliantly by David Herlihy in "Land, Family, and Women in Continental Europe, 701-1200," *Traditio* 1(1962): 89-120, where he used their peripheral references and inadvertent comments to extract information from the interstices of the documents, for instance by looking at who was described as owning an adjoining holding rather than who was involved in a charter transaction itself.

[5] On this see the work of Michael Gervers and associates in *Dating Undated Medieval Charters*, ed. Michael Gervers (Woodbridge: Boydell, 2000).

[6] I started with the exhaustive collection of charters (about 8000) from southern France that I had collected for my first book, *Medieval Agriculture, the Southern French Countryside, and the Early Cistercians* (Philadelphia, Penn.: American Philosophical Society, 1986), but added several thousand more charters by consulting all the published cartularies I could find for Burgundy, Normandy, and England (excluding, however, many of those associated with Savigniac houses that only later became Cistercian).

[7] Hasty readers have asserted that I denied Bernard's use of the word "ordo" altogether, but see page 66 of *The Cistercian Evolution*, where I state, "I have found only a single place in Bernard of Clairvaux's collected writings where *ordo* and any form of the place name Cîteaux are even found in proximity." What I was looking for there was language such as is found in a letter from Peter, abbot of Montier-la-Celle to Archbishop Eskil of Lund (dated late 1157-early 1158 x autumn 1161), "*Iam non solum in herbam sed et in spicas Cisterciensis sive Clarivallis ordo ibi multiplicatione fratrum excrevit, nichilominus et religio Premonstratensis.*" See *The Letters of Peter of Celle*, edited and translated by Julian Haseldine (Oxford: Clarendon, 2001), letter 12, pp. 28-33. Note the confusion by the author about whether a Cistercian or Clarevallian Order, and the parallel use of *religio* to describe the Praemonstratensians.

[8] *The Letters of Bernard of Clairvaux*, trans. Bruno Scott James (London: Burns and Oates, 1953); reprinted with an introduction and tables by Beverly Mayne Kienzle (Phoenix Mill, Gloucestershire: Sutton, 1998), letter 4, page 21.

[9] Constance B. Bouchard, *Holy Entrepreneurs: Cistercians, Knights, and Economic Exchange in Twelfth-Century Burgundy* (Ithaca, NY: Cornell University Press, 1991), pp. 217-19.

[10] Martha Howell and Walter Prevenier, *From Reliable Sources. An Introduction to Historical Methods* (Ithaca, NY: Cornell, 2001), esp. pp. 43, 66, 68.

[11] Howell and Prevenier, *From Reliable Sources* p. 82.

[12] *Statuta capitulorum generalium ordinis Cisterciensis ab anno 1116 ad annum 1786*, ed. J.M. Canivez, 8 vols. (Louvain, 1933), especially volume one.

[13] See in particular the Cistercian *Exordium Magnum* of Conrad of Eberbach (of circa 1200), which repeats the *Exordium Parvum* account unchanged, but then goes on to describe the failures of earlier monasticism; I discussed the rhetoric of this text in "Disentangling Cluniacs from Cistercian Fictions," in a session on Cluniacs organized by Barbara Rosenwein at the Fifth Annual Medieval Studies Conference, University of Leeds, Leeds, England, July, 1998.

[14] *Les "Ecclesiastica Officia" cisterciens du XIIe siècle: texte latin selon les manuscrits édités de Trente 1711, Ljubljana 31 et Dijon 114. Version française, annexe liturgique, notes, index et tables*, ed. Danièle Choisselet and Placide Vernet (Reiningue, France: La documentation cistercienne, 1989); the slight variations from one to the next in a series of such texts (or those containing the earliest statutes) in surviving twelfth-century manuscripts made it possible to establish a series for such manuscripts for the mid twelfth century.

[15] Regarding Dijon 114, although my notes are correct and the treatment in my text of *The Cistercian Evolution* of this manuscript is based on those notes, there is an erroneous statement in my published Appendix Two (a discussion of relevant texts which established dating, never intended as a complete description of manuscripts); under Dijon 114, page 244, the sentence "In comparison...in that order" is an inadvertent accretion which escaped my proofreading.

[16] *Bullaire du Pape Calixte II, 1119-1124: essai de restitution*, ed. Ulysse Robert (Paris: 1891), reprinted New York: George Olms Verlag, 1979.

[17] Ludwig Falkenstein, *La Pauvreté et les abbayes françaises aux XIe et XIIe siècles* (Paris: Champion, 1997).

[18] See remarks by Marcel Pacaut, "La Filiation claravallienne dans la genèse et l'essor de l'Ordre cistercien," *Histoire de Clairvaux. Actes du Colloque de Bar-sur-Aube/Clairvaux, 22-23 juin, 1990*, pp. 135-47 (Bar-sur-Aube: Némont, 1991), about the very different articulation of the filiation of Clairvaux from that of the rest of the daughter-houses.

[19] *Letters of Bernard of Clairvaux*, trans. James, number 328 (dated by Leclercq to circa 1150).

[20] Most secular historians recognize that in the work ahead we must have a healthy skepticism about traditional narratives and about the reliability of documents. For instance, see Janet L. Nelson's recent "check-list" on how to approach monastic history, which includes the warnings: "Beware of exaggerating the novelty of the twelfth century in the history of monasticism. Take Cistercian polemic

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with a pinch of salt." See Janet L. Nelson, "Medieval Monasticism," Chapter Thirty-three in Peter Linehan and Janet L. Nelson, *The Medieval World* (London: Routledge, 2001), pp. 576-604, at p. 586.

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