Review
Author(s): Fredric L. Cheyette
Review by: Fredric L. Cheyette
Published by: The University of Chicago Press
Stable URL: http://www.jstor.org/stable/2865769
Accessed: 21-03-2016 16:34 UTC
Reviews


Most of the essays begin with a map identifying places discussed in the text and conclude with a bibliography. Most authors also provide an individual table of contents. These are especially valuable because of the length of some of the essays—five exceed forty pages—and also because of the authors’ differing approaches to organization. For example, Golielli contrasts traditional and new forms of hagiography; Sigal separates writings about noncontemporary and contemporary saints; Rener distinguishes functional groups such as monastic leaders, bishops, and lay saints; and Thiry-Stassin presents works according to their period of composition.

The contributions differ in other ways as well. Head summarizes themes from his recent book (Hagiography and the Cult of Saints: The Diocese of Orléans, 800–1200 [Cambridge, Eng., 1990]; reviewed in Speculum 67 [1992], 683–85), while Bledniak reports preliminary conclusions from a thèse in progress. Courcelles interprets her assignment narrowly, providing two pages of summary and twenty pages of lists; Görlach adopts a more comprehensive view and discusses the use of Latin and French as well as English in the composition of legends in Britain.

The goal of concentrating on authors and works clearly placed some contributors at a disadvantage. Faced with large numbers of vernacular texts mostly based on Latin originals, Williams-Krapp and Görlach focus much of their attention on legendaries rather than individual hagiographies. Viewed in terms of the goals of the project, the most successful essays are generally those addressing a limited corpus of texts that possess a thematic unity. Bastiaensen’s review of the works of a single author, Bonnassie’s emphasis on the Peace of God movement, and Sigal’s comparison of the treatment of ancient and contemporary saints exemplify those qualities. Head brings a similar unity and succinctness to a larger body of material by generalizing on the basis of his own detailed investigation.

Anyone who expects a reference work to provide parallel treatments and comparable information on the topics it covers will find Hagiographies disappointing. But the range of its subject matter and the stature of its authors assure that experienced researchers as well as novices will consult this collection for years to come.

JOHN M. MCCULLOH, Kansas State University


Susan Reynolds must have received with dismay or resigned disbelief the Medieval Academy’s announcement that by popular demand the next volume to appear in the MART series would be François Ganshof’s Feudalism, for this, her most recent book, already widely reviewed, is nearly five hundred pages of vigorously argued anti-Ganshof. Attacking Ganshof’s “classic” and clearly still-admired work is, of course, not new; it was a commonplace in conference hallway conversation when I was barely out of graduate school, and Elizabeth Brown (to whom Reynolds dedicates this work) led the way in print over twenty years ago in a wide-ranging and sharply pointed article in the American Historical
Review. Until Reynolds, however, no one has tried to grind systematically through the sources on which that conventional (non-Marxist) construction of "feudalism" has been based to see whether they will bear the weight. Nor has anyone in many years looked closely at the twelfth-century Libri feudorum, which Brown already pointed to as the fons et origo of the modern concept of feudalism. Now Reynolds has done both, missing (with regret) only the Hispanic peninsula in her sweep through the library stacks. Her conclusion is that the ism is built on sand, that the documents interpreted by generations of medievalists as representing "feudal" forms of landholding and "feudal" relations of vassals and lords will do so only if forced, only if one reads them with the assumption that that is what they must do. Without that assumption, she argues, they can be read to say very different things; and indeed many texts can be read through the lens of conventional "feudalism" only by doing them violence.

Because Reynolds covers such vast territories, even without the medieval Spains, this is a very complex book, and the relentlessly argumentative style in which she carries on her discussions with a century of medievalists will probably put off many readers. One often has the impression of listening to half of a heated telephone conversation, knowing you could discover the other half by following out the footnotes and learn what exactly is being argued about by reading the documents to which those footnotes lead, but wanting the time or ambition or even the access to the well-stocked research library that such further reading would require. If the tone and style get in the way of the contents it would be unfortunate, for Reynolds's ambition is nothing less than to change the way historians of medieval European society and politics (and to a certain extent of literature as well) do their work, the way they read their documents, the way they understand what the words that pass before their eyes imply. And her arguments must be taken seriously. If she is successful, certain assumptions will no longer hold easy sway, and post-Reynolds accounts of European society in the half millennium from Charlemagne to 1300 will differ in significant ways from the pre-Reynolds accounts that she criticizes.

Reynolds, following Ganshof (and many others), condenses "feudalism" to two concepts (distinguishing carefully, as she tries to do throughout, between words, concepts, and phenomena): vassalage and fiefs. The first she dismisses in one short chapter, concluding that "vassalage . . . is a term that no longer matches either the evidence we have available or the conceptual tools we need to use in analyzing it. It is both too diffuse and too narrow—not surprisingly, since it survives from a primitive stage of the study of social relations. . . . Vassalage is too vacuous a concept to be useful" (p. 47). The remainder of the book is then devoted to fiefs, whatever they may have been called in medieval Latin or in the various vernaculars, and far more generally to "the rights and obligations of property." This is the heart of her work. Piloted by the numerous historians who have written about medieval property or analyzed medieval charters, Reynolds has "trawled" (her metaphor) the farthest recesses of the British Library, using gill nets that stretch for miles; she has worked enough fishing banks to put mere archive anglers, such as myself, to shame. Going country by country and century by century to the printed sources, she asks, in effect, If I don't assume in advance that there was such a thing as feudalism and that the words fevum, beneficium, casamentum, and their equivalents have a specific technical meaning (such as the one given by Ganshof), that is, if I make no a priori assumptions about the words used in these documents, what do they tell me about the rights and obligations of property?

It will take years to unpack the holds Reynolds has filled with her catch to see how much is really fit to be served up and how much had best be thrown back in the sea. There are

more than enough individual hypotheses and particular readings here to keep a legion of seminars and dissertation writers going for a generation. No reviewer unwilling to spend years fishing these same banks can give more than a modest and very partial assessment of the individual source readings that make up the bulk of her work. Those she has found in collections from Occitania, my own narrow range of expertise, she reads in a limited and sometimes strange manner (I will discuss one example later), but, in my view, a more exact reading of those texts would strengthen rather than weaken her argument. Reynolds does not twist the texts to conform to her purpose; she has simply read them too rapidly. Given the vast reach of this book, one may suspect that this is more than occasionally the case; but whether that invalidates her argument only detailed and more exacting research will tell.

In the meantime, it is not such painstaking text-by-text readings that should command the attention of historians, but rather the larger arguments that run in the background while all the detail work occupies the screen. Three seem to me of particular importance. The first is methodological: it is about how the words in the texts we use as evidence are to be interpreted. This argument has the greatest potential to make us uneasy about our traditional manner of reading charter evidence. The second argument is historiographical: it asserts that we have been imprisoned by a centuries-old interpretive tradition and asks us by implication where else in our constructions of medieval society we have been so imprisoned. It also demands an alternative construction, whose elements Reynolds tries to supply (with but partial success, in my view). In particular, it focuses our attention on the changes that twelfth- and thirteenth-century rulers were bringing about in the way they exercised authority and the way that professional jurists helped shape those changes, and asks us to reconsider radically the relationship of what came after those changes to what came before. The third argument is anthropological and philosophical: it concerns the meaning and nature of law and government as features of social structure. I shall take these up in order.

The methodological issue is everywhere implicit in Reynolds's argument. Those who read medieval documents through the lenses of the conventional construction of “feudalism” make two fundamental assumptions: first that the documents, especially charters, are legal documents, drawing their meaning from a preexisting body of law, be it Roman (Theodosian, Visigothic, Justinianic), ecclesiastical, or customary; second—a corollary of the first—that particular words in those documents have technical meanings which they retain over long periods of time, meanings given to them by that body of law. For “feudalism” (and thus for Reynolds's argument) the critical words are nouns that refer to property, such as proprietas, alod, feudum, casamentum, beneficium, honor, and verbs, such as tenere, associated with them, as well as expressions such as auxilium et consilium, cavalcatum, and others that express obligations. The very structure of Ganshof’s little book depends on these assumptions; Annales social history in the wake of Georges Duby’s Mâconnais (at which Reynolds levels a stringent critique, pp. 166–67) would not exist without them; they have their most vocal contemporary defenders in such French historians as Elisabeth Magnou-Nortier, Jean-Pierre Poly, and Eric Bournazel. This suggests the stakes on the table.

To these assumptions Reynolds responds with a radical nominalism. Sometimes it takes an extreme form: “Abstract nouns like feo, fevum, feudum . . . cannot be assumed to have had consistent meanings outside their contexts. Even if one context suggests some content for a word, that content cannot be assumed to be inherent in the word itself in such a way as to be transferred to other contexts and other cases. Contexts, unfortunately, are often unhelpful in this period [900–1100]. . . . Scribes may have used apparently classificatory nouns to describe pieces of property without being concerned to distinguish anything we might call different and definable categories of property” (pp. 119–20). Taken at face value,
this turns eleventh-century scribes into the Humpty Dumpty of Alice through the Looking Glass, “When I use a word . . . it means just what I choose it to mean—neither more nor less.” Other occasions, happily, prove that this is just a (not uncommon) example of Reynolds’s loose talk. What she really means most of the time is what she asserts later in the same paragraph: “Even if [the scribes] were interested in distinctions, the words used in records . . . could not have had the technical senses they might acquire in later ages of professional law.” Sometimes, as in this particular argument, Reynolds asserts that meanings may have varied from monastery to monastery; at other moments the argument seems to be that they varied from region to region and even in the same community or region may have varied significantly over time.

Professionals (if not undergraduates) have long abandoned the quaint notion of “the medieval mind.” Reynolds seems to urge us to push that skeptical recognition of regional and temporal differences to its farthest extreme, to the point where it seems to undermine the very possibility of language’s acting as a vehicle for meaning. Yet she is convinced she knows what words do not mean, without apparently seeing the logical box she thus puts herself in. In the end, the theory seems less important to her than the practical result she is striving after: to undo our confidence in our ability to interpret early charters securely by merely referring to Du Cange or Niermeyer, or, far worse, by referring to legal history textbooks or reading the thirteenth or fourteenth century back into the eleventh or northern French or English practice into Italy or Germany.

One need not go to Reynolds’s nominalist extreme to recognize the importance of her critique. How many times does one see a historian using the simple word feudum in a text, or the presence of a lord approving an alienation, or the notation that a piece of land or a right was “held of” someone as a justification to extrapolate into the world of that text a complex of obligations and relationships of subordination, or—a leap of reasoning Reynolds does not canvass, but which is of the same order—using a charter that states A gives a piece of land or rights to B to assert that A somehow possessed what he or she was giving, the historian then spinning out some scenario to put that land in A’s hands in order for it to be given away. In an article published in this journal in 1988 I showed how mistaken that second move could be; in the eleventh century the common forms of conveyance—sale, gift, mortgage, etc.—did not necessarily carry the implications that they did in ancient Rome or do in the modern world; we impute such contents to them at our peril. If such simple and straightforward words as “sell,” “give,” and “grant” did not have technical meanings on which we can unfailingly depend, should we not also be wary of words like “alod” and “fief”?

Being wary, however, can be a road to paralyzing skepticism of the sort implied in Reynolds’s most extreme formulations. How can we save ourselves from being forced to renounce the possibility of knowledge? Traditional lexicography assumes a fixed meaning or set of meanings for words (and assuming words to be “legal” pins those meanings down even more exactly). Here, however, we seem to face not a plurality but a fluidity of possible meanings. How then can we interpret what we read? Beyond the general exhortation to “look at the context,” Reynolds is not much help. What context? What should we mean by “meaning”?

Traditional lexicography finds what it looks for by comparing texts; it assumes that words share common meanings across texts. This has been the method employed by Annales social history to interpret medieval charters; it is what gives many of those works their strikingly “institutional” air. An alternative (whose possibilities would have sent many of the arguments in this book in a very different direction) has been richly explored by Barbara Rosenwein and Stephen D. White when they have asked of their eleventh-century Burgundian and Loire valley charters, What are the particular ongoing social relationships
to which these documents testify? When they ask, that is, what people are doing rather than what is the nature of legal institutions or social classes. Perhaps this is what Reynolds means by “context,” but if so, she does not make it clear or draw the consequences, the most important of which would be to question her own premises in writing this book: that the critical issue is defining property rights and the connection of words to rights, rather than looking at the dynamics of human relations and asking to what extent and in what ways the documents that survive represent or structure those relations.

For Reynolds, the problem of what words like _feudum_, _beneficium_, and _alod_ mean is part of a larger problem: the source of “feudalism” itself as a historiographical concept. “Fiefs and vassalage are post-medieval constructs, though rather earlier than the construct of feudalism. . . . Even when historians follow the terminology of their documents . . . they tend to fit their findings into a framework of interpretation that was devised in the sixteenth century and elaborated in the seventeenth and eighteenth. . . . We cannot understand medieval society and its property relations if we see it through seventeenth- or eighteenth-century spectacles” (pp. 2–3). Reynolds goes on to argue that the academic law of fiefs was the creation of the later Middle Ages and of bureaucratic, professionalized governments and, as “expert law,” did not develop out of the customary law of noble property of an earlier age; whatever connections it had to practices of an earlier age, she adds, was to the relations of bishops and abbots to their tenants rather than of great secular lords to theirs.

Reynolds does not, to be sure, always stick with this chronology: at one moment she suggests that learned lawyers may have influenced practice in Montpellier at the beginning of the twelfth century; at others she is certain she has found such influences in the later twelfth or early thirteenth centuries, though the arguments she presents are largely hypothetical and her sentences liberally sprinkled with the auxiliary verb “may”—again presenting a rich lode for future seminar-paper writers.

The chronological issue, however, is only part of the problem.

Let us assume that Reynolds is correct in asserting that a “law of fiefs,” or more exactly “laws of fiefs,” developed in the later Middle Ages in various European venues, and that—outside of England—such laws were profoundly shaped by academic law, especially from the texts and commentaries on the _Libri feudorum_. This is not a priori an unlikely scenario. The _Libri_ quickly became part of the Roman law studied in the universities, and, from the fourteenth century on, the case law developed in the Parlement of Paris, for example, was profoundly influenced by academic laws, with pleaders regularly appealing to texts they had learned on school benches to support their arguments. It would be surprising if this was not the case elsewhere on the Continent as well. Suppose then that we find appeals to such academic laws of fiefs in the records. Would this not raise the question why such rules and practices were allowed to shape court decisions? Should we follow the school of judicial “Realism” so popular in America earlier in this century and say that, in the later Middle Ages as well, the law was whatever judges said it was, assume that they and the advocates who plied their trade in their courts could shape the law any way they saw fit? Would plaintiffs have chosen to litigate in those courts? Would royal lawyers have been able to impose a structure of property law that did not conform to some accepted notions of substantive right held, at the very least, by the propertied classes? We know what difficulties it caused when kings attempted to impose other obligations and restrictions on their subjects’ property through taxation, how lengthy and complex the negotiations had to be. In

contrast, there were no revolts, no demands for concessions or charters of privilege in response to a wholesale introduction of new legal rules. The argument that property law was radically reshaped under the influence of academic law seems on its face to be unlikely. Whose interests would it have served?

How, then, was what became the “law(s) of fiefs” in the age of academically trained lawyers related to what came before? This is not one question but a whole complex of questions, and on them Reynolds’s occasional brief comments shed little light. They are, however, by implication at the very center of her enterprise.

Traditional accounts of “feudalism”—Bloch, Ganshof—speak of “the joining of fief to vassalage” or “the reification of fidelity.” Reynolds has no truck with either the expressions or the various chronologies given for this linkage (see esp. pp. 18–19, 92–93, 118–19). Yet eleventh- and twelfth-century documents that explicitly tie fidelity to property are not all that hard to find. Indeed, Reynolds discusses one group at length, as an example of what she takes to be the intrusion of professional law into the relations of secular lords and their subjects at the very beginning of the twelfth century. The documents are contained in the Liber instrumentorum memorialium of the lords of Montpellier, and Reynolds (pp. 260–65) reads them as the original nineteenth-century editor of the cartulary and everyone since has done, as conversions of alods into fiefs—in French terminology as “fiefs de reprise.”

Strictly speaking these are groups of texts that all follow the same formula: one states that a donor gives his castle and village or other property “ad alodium” to William [V or VI] of Montpellier, usually getting money in return; a second states that William gives the same property to the donor “ad feudum”; a third records an oath of fidelity. (Sometimes the first two acts are rolled into one.) Reynolds has not noticed a strange property of a number of these donations, however: they include sometimes quite elaborate provisions regarding the succession to the lordship of Montpellier of the sort (to take the text involving the castle of Pignan in 1114) “if William [V] of Montpellier did not devise (dividebat) this castle [by will], then I give it to his eldest son; if his eldest son has died (moreretur) without heir, then I give it to his second son” (etc. with more detail following). What are we to make of this? It could be simply read as an attempt to assure the donor’s continuing loyalty to the dynasty, a complement to the notation in the same document that William’s wife has given forty solidi to the donor “for his love” (pro drudo). But another possibility behind this strange sequence of verbs (imperfect, present, imperfect, present, all clearly regarding future events, for William V did not die until 1121), aside from indicating the scribe’s limited Latinity (and thus seriously questioning “academic” influence on this document), is that it makes explicit the expectation that on William V’s death the donor would give the castle again to William’s sons in the order of succession that William had already determined. That is, the gift “ad alodium” was not a once-for-all-time event; it was not conceived of as permanently transferring a “bundle of rights” from one person to another. The later gifts “ad alodium” by other members of the original donor’s family, including his mother in 1139, were thus not gifts of different rights to Pignan but of the same rights as those contained in the first surviving donation, as different people took possession of them.

Evidence that this hypothesis is not purely fanciful can be found elsewhere in the same cartulary in the records for the castle of Poujet, for which a Girundes, daughter of Adivena, gave an oath of fidelity in the later eleventh century; in 1124 three of her descendants, including another Girundes, sold “ad alodium” to William VI and received back “ad feudum” exactly the same rights their ancestor Girundes had held. For their share in the same

---

castle, Adalais and her husband William Assalit gave oaths of fidelity in 1114 to William V and in 1127 to his second son, William of Aumelas; after her husband's death in 1132, Adalais sold her share “ad alodium” to William VI and received it back “ad feudum.”

Why, then, do we not have repeated donations or sales of the same castle joined to the repeated oaths of fidelity in this cartulary? Perhaps, in fact, we do but cannot recognize them because the genealogical connections among the donors or sellers are no longer recognizable. Another possibility is that the notary who assembled this cartulary early in the thirteenth century was not simply a mindless gatherer and copier of parchments lying around his office; he was already a historian, and his choice of what to include and what to omit was already an act of interpretation. He was a school-trained lawyer; from the point of view of Roman law, repeated gifts made no sense, so, we might hypothesize, he copied the earliest surviving example and put aside the others.

When read closely, these documents and others like them impel us to go well beyond Reynolds’s general critique of “feudalism.” The practice of repeatedly giving “ad alodium” from generation to generation what is then repeatedly granted “ad feudum” indeed calls into question the distinction commonly made between “full property” and “fief”; if the original donor has “full” rights, which he gives to his lord, who gives less than full rights back (for those rights are restricted at the very least by the oath of fidelity and the obligations it expresses), then there is something paradoxical in the original donor’s heirs making an equivalent donation at the time they take up their inheritance. But the practice raises far more questions than that: it suggests that an analysis of the rights one or another individual might hold in the property is here simply beside the point. What seems to be important for the participants is the entire ritual of donation, return grant, and oath of fidelity, a ritual that served to implant a personal relationship, what the document from Pignan refers to as “love,” into the landscape. The particular words that the scribe scratched on parchment were of less importance than the action and the words that were uttered. But Reynolds will have no part of this. While she admits that “interpersonal relations between powerful people ... mattered” (p. 46), her entire chapter on “vassalage” is a sustained argument against placing such relations anywhere but on the periphery of our analysis of power and its operations.

I would not argue for a moment that we should return to the conventional concept of vassalage to explain what is taking place in these documents from Montpellier, or to the conventional scenario of “the joining of fief to vassalage,” for such legalistic concepts seem to drain the relations involved of much of their richness and complexity, to draw a veil over a world where a word like “love” can find its place in charters as well as in lyric poetry. Reynolds, one indeed could argue, is not radical enough in her critique of “feudalism.” She has rejected the Ganshoian description of the rights and obligations associated with fiefs and vassalage, but she does not herself escape the analytical categories of rights and obligations associated with property. Yet the documents from Montpellier insistently push us outside of those categories, telling us that scribal words do not always correspond one-to-one with social processes.

What keeps Reynolds enclosed within these categories is the sharp distinction she insists upon between powers of government on the one hand and property on the other, the distinction at the heart of Enlightenment and nineteenth-century Liberal political theory. In her discussion of Eigenkirchenwesen, for example, she remarks, “There does not seem to be any reason to see control and interference as falling on the property side of the boundary between proprietary and governmental rights” (p. 61; see also pp. 418–19). Elsewhere she argues that the consent of lords to alienation of property implies rights of

---

"political or governmental" nature rather than anything about land law, that jurisdiction is "an attribute of government rather than a right of property" (pp. 61, 163). Often she argues that a particular exercise of lordship looks like "what we would call government." "What rulers at any level had to do," she says of the Carolingians (p. 131), "was to make their subjects . . . acknowledge, in effect, that their property was held under government."

By her account, then, what people did with property must be analyzed separately from the way they acted "governmentally"; it is only the anachronistic application of "feudalism" that has led us to confuse the two. At times Reynolds identifies the "governmental" or "political" with "bureaucratic" or "institutional" (e.g., p. 26) as when Carolingian counts are termed "government employees" (p. 87); at other times government is "coercive domination" (p. 34) and the state "an organization . . . that more or less successfully claims the control (not the monopoly) of the legitimate use of physical force" (p. 27).

Doesn't this modification of Max Weber's classic definition, however, beg all the interesting questions: who makes the claim? what are the means of control? does the absence of a claim to monopoly make an important difference? and, above all, who asserts and who decides what is "legitimate" and what is not and on what grounds? Stephen D. White in his review of the book comments extensively on Reynolds's use of "government" and "political" as an analytical category; there is no need for me to go over the same ground.

If we are asked to reject the essentially modern analytical categories of "feudalism," why should we be asked to accept without further discussion these other essentially modern categories? Reynolds seems to have forgotten that the distinction between property and government traveled a long and tortuous road in Western thought. One has only to read Charles I's speech from the scaffold in front of the Banqueting Hall to recall how tenacious the "confusion" of the two still was in the mid-seventeenth century.

That "confusion" goes far back into the Middle Ages. Even in a case law as academically informed as that of the fourteenth-century Parlement of Paris one would be hard put to find the "boundary" between government and property that Reynolds so easily discovers before the twelfth century. Jurisdictions, the right to tax and the right to be free of taxes, the right to summon men to military service and the right to be free of summons, even indeed the powers and jurisdictional rights of bishops—in short all the sorts of activities that "we would call governmental"—were litigated as property rights. In this both judges and litigators were only following the habits long entrenched in the practice of eleventh- and twelfth-century scribes and their employers, who made no distinction between vineyards, fields, castles, the right to hang thieves, and dominatio or districtus when they drew up charters: they were all property that could be sold, given, mortgaged, divided up in testaments, granted as dowries or marriage portions. It is exactly this "confusion" that makes the argument over the existence or nonexistence of a public/private distinction in the Middle Ages so difficult to resolve. For we are wont to place "governmental" activities in the public sphere and "property" in the private, and medieval practice did not.

In the creation of this distinction we hold to be so obvious, law and lawyers played a prominent part. It is one of the important contributions of Reynolds's volume to remind us of the great transformation they brought about in the thirteenth and fourteenth centuries, a transformation that has been somewhat lost from sight with the decline of traditional institutional history. Reynolds's work may serve as a wake-up call to renew the questions

we put to what in the pre-Dubian age was the central focus of much Anglo-American medieval history. At the same time, by implication, it reminds us how carefully we should consider what those questions ought to be. Just as she has projected "government" as an analytical category back into the centuries before 1200, so has she done with "customary law," even though that concept is also the creation of academic law. To Reynolds, the transformation is thus one from customary law to "professional, academic law" and bureaucracy.

This, of course, is of a piece with her concentration on the "rights and obligations" of property as distinct from "government." It seems worth asking, however, whether, or to what extent, pre-1200 society was ordered by abstract rules that we can call "customary law." Or do we find them there—as Reynolds argues for "feudalism"—only because we assume them to have existed? To the extent that they existed, to what extent were they thought about systematically? (Reynolds argues that they were not.) Were they rules that concerned property rights? Or did they concern such values as honor, family solidarity, status, fidelity, and love—values whose realization had to be continually renegotiated and repeated exemplified and confirmed in rituals of exchange? If we focus our attention primarily on the property caught up in these exchanges, are we perhaps mistaking the secondary for the primary? And are rights and obligations attached to property, or are they instead an outcome of the ritual of exchange itself? Should we look for institutions abstracted from individuals, or should our research prey be, first, the individuals in their particular, complex networks of relationships and, second, the systematic practices and transactions in which they engaged?

These are all, I believe, important questions, and a less powerful and less daring work than Reynolds's would not have raised them. Thanks to her thoroughgoing critique of the conventional concept of "feudalism," historians will have to reconsider some of their most fundamental questions about the social structure and political organization of the central Middle Ages. Reynolds joins Jean Durliat and Dominque Barthélemy, among others, in shaking the foundations of what was once our confidently accepted common wisdom. We are clearly in for an exciting ride. Her book is one that everyone concerned with the period should read, reread, and ponder.

Fredric L. Cheyette, Amherst College


This volume accompanied an exhibition of the same name at the Bibliothèque Nationale in Paris. However, instead of a straightforward exhibition catalogue, the authors produced an informative and extravagantly illustrated book that examines childhood in all its late-medieval manifestations. In addition to a wide variety of written sources, they utilized miniatures, paintings, sculptures, prints, and occasionally decorative arts; examples are culled from all over Europe, with an emphasis on France during the fourteenth to sixteenth centuries. These examples clearly demonstrate the presence and significance of children in home, at work, and in the church, from conception to adolescence, during this period. This subject has, of course, been much debated since the publication of Philippe Ariès's L'enfant et la vie familiale sous l'Ancien Régime (1960); in his conclusion that medieval children were often ignored and of little importance, Ariès misinterpreted or overlooked much of the critical artistic evidence. More recent studies have tried to dispel his theories, and especially valuable work has been done by these two scholars. Alexandre-Bidon's publications on the iconography of medieval childhood challenged Ariès, as did Riché's work.