raged disbelief ended with delight at such a dazzling display of interpretative skill, richness, and generosity. There is much to admire here, as also in Dinshaw's careful erudition and her scrupulous politeness and fairness to all the many other scholars she refers to.

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JOSEPH J. DUGGAN, *The "Cantar de mio Cid": Poetic Creation in Its Economic and Social Contexts.* (Cambridge Studies in Medieval Literature, 5.) Cambridge, Eng.: Cambridge University Press, 1989. Pp. xi, 178; 2 maps. $44.50.

The bloody battles that punctuate the story of the Cid, narrated in the singularly important epic of medieval Spain, are as nothing compared to the twentieth-century battles that have been waged over what we make of this *Cantar.* Even what is at stake in the Cid's battles in this poem — what it is "about" and what anyone in it is fighting for — cannot be roughly agreed on without first establishing where we stand in the contemporary ideological conflict over the very nature of poetic creation, particularly poetic creation in the medieval period. And behind a self-effacing appearance (the less than two hundred pages, the straight title, the dull, ocher dust jacket) Professor Duggan's book, ten years in the making and a cunning Trojan horse emerging from Cambridge itself, will — should — make history of the major wars. We have the rare work of scholarship here: strongly synthetic and radically innovative at once, and, while it is devoted to a cogent and global redefinition of many of the major theoretical components of a literary work, it is at the same time alerting us to the most minute but telling of details, gone largely unnoticed for centuries. History and literature, in other words, lovingly married.

Although there are a first chapter entitled "Historical and Theoretical Framework" and a penultimate one called "Mode of Composition," the complex and compelling model of poetic production and reception is in fact elaborated in every one of the nine chapters, as much the premise of the study as its very subject. Duggan's central preoccupation is to sort out the multiple levels of interpretation involved in dealing with "the Cid": principally to unravel and identify the nature and function of the Cid story as it was most likely (re)cast around the year 1200, under quite specific and powerful circumstances, from those of later critics/retellers.

The *Cantar* that has survived, in other words, should be understood as the "story" of the political tensions, economic forces, and ideological agendas of the time and place in which it was created, and the little nugget of a story about Rodrigo Diaz de Vivar that served as the poet's raw material was just that, the grain of anecdote and history and good story that was shaped by an artist — one, it is to be clearly understood, necessarily operating within all the historical contingencies of his own time. The historical "truth" of a literary text (at least of one, like the epic, which deals with an earlier historical moment) lies primarily in the moment and circumstances of its creation or performance, of its making. (One realizes, of course, the analogy with the historical novel, particularly of the nineteenth century, knowing full well, for example, that Manzoni's *Promessi sposi* is far more about the Risorgimento than about the older wars, against other foreign occupiers, that are being retold.)

It would be easy to underestimate the powerful reversal at the heart of this model of the relationship between history and literature: the regnant model (although it is often left unarticulated) is that the poet is an agent of literary devices and a rhetorical heritage and that the story being retold is the "history" at hand. But Duggan argues, in so many words, that this model, inexplicably elevated to canonical status in the last

decades and one within which the artist is really much more a scholar than a storyteller and within which writing (rather than narrating or singing) takes place outside vulgar political circumstances, is one that too suspiciously echoes of the constructs that medieval scholarship imagines for itself. And although Duggan is inspiringly skeptical about the possibility of any "objectivity," he does believe he can construct a more compelling vision — a better hypothesis, as he dubs it — of the contingencies shaping the composition of the single version of the Cid story that the ravages of history have left us. He argues, in fact, that it is precisely in those ravages, in the interstices, and in the details that make no sense within our own frameworks that the story is to be found. And it is a powerful story indeed.

As with so many compelling stories that suffer forms of sanctification and thus purification (and the Cid's elevation to role of national epic hero did that and more) the forgotten details and whitewashed themes are shockingly apparent once they are pointed out: the most persistent issue in the *Cantar*, never more than a handful of verses away, is the acquisition and circulation of wealth; and the dramatic finale is provoked by a slur on the Cid's parentage. The critical reactions to these two central readings of Duggan's are predictable (and already voiced in the *TLS* review by Richard Fletcher, whose fascinating *Quest for El Cid* appeared virtually simultaneously with this book): everyone already knew about the money issue, and the legitimacy question is too oblique and allusive. But, in fact, the money question has never been explored — and its centrality scarcely imagined. Duggan's discussion of it (in chapters 2 and 3) specifies its detail and relentlessness and reveals the Cid as a remarkably wise (and thus powerful) leader in an economic universe rooted in gift giving as the major mechanism for redistribution of wealth and thus acquisition of power. At the same time, of course, the economic motivations and rewards for the Reconquista are laid bare, and in Duggan's always subtle and complex readings it becomes clear that this ideological field and its struggles are in fact far more interesting and complicated, far more human and dramatic than the old "Christians versus Moors" reading in any of its various critical guises. This, in turn, provides the crucial setting for the denouement (itself revealed in dramatic fashion in Duggan's detective-novel-like chapter 4): the hero's legitimacy is called into question — and the accusation of bastardy is proven wrong in combat. Again, what Duggan reveals is not an oddity or an obscure line: the artifacts he discovers allow us to decipher very broadly, and now we see that, indeed, the question of legitimacy, like that of the acquisition of wealth and thus power to which it is inextricably linked, is everywhere.

Duggan applies his remarkable detective skills to the other great mysteries surrounding the only surviving epic poem of medieval Spain (who "wrote" it? who "saved" it?), and in the most difficult two chapters of the book, 5 and 6, he gives us answers that are convincing, if not so much for the detail, necessarily, as for the methods and the basic assumptions (and these are underscored in the chapter titles, "The Poet's Milieu" and "Geography and History"). In fact, I would argue that these are "variants" of the subsequent and final three chapters — and that the net effect of the whole is the definitive establishment of a mode of understanding epic poetry as a vital part of its historical milieu, both taking from it and giving to it, shaped and shaping. Moreover, and perhaps most crucially, at least for some of us, Duggan has shown, by marvelous example, that the most disputed, and often ridiculed, of premises, that epic poetry (as well as other artistic forms in the medieval period) is popular and performed, formulaic and traditional is actually the most complex and refined theoretical model and can lead to the most profound and engaging of literary readings. Nails have been sunk into coffins, and some battles, I think, have been fought for the last time. For once, I would say that the blurb on a book's dust jacket ("This ground-

breaking monograph provides a major contribution to medieval Hispanic studies") is overly modest. Only the most solipsistically cloistered are likely to continue reading the *Cid* outside these creative, economic, and social contexts — outside the context of this seminal study.

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Kaspar Elm, *Mittelalterliches Ordensleben in Westfalen und am Niederrhein.* (Studien und Quellen zur westfälischen Geschichte, 27.) Paderborn: Bonifatius, 1989. Pp. 258; black-and-white illustrations.

Readers who are looking for a comprehensive history of Westphalian monasticism will be disappointed. This is a collection of twelve articles written between 1965 and 1985 by Kaspar Elm, founder of the Arbeitskreis für "Vergleichende Ordensforschung" at the Free University of Berlin. The first piece is Elm's assessment of the significance, personality, and subsequent cult of St. Norbert, which appeared in the collection of articles Elm edited in 1984, *Norbert von Xanten: Adliger–Ordensstifter–Kirchenfürst,* and which I reviewed elsewhere (*Catholic Historical Review* 73 [1987], 299–300). The others can be grouped into three categories: studies of the Cistercians, the Austin friars, and the *Devotio moderna.*

The articles are not as disparate as this listing may suggest. First of all, they have a common methodology. Elm not only places the history of specific orders and houses in their broader social and religious context in the way that Herbert Grundmann pioneered, but he also stresses how religious orders were transformed over the centuries and how local conditions shaped the history of individual foundations. For example, he assesses the interplay between local conditions and the impact of external influences emanating from Bohemia, the Netherlands, and the Bursfeld congregation on the reform of the late-medieval Westphalian Cistercians. The articles are thus models for how the history of individual orders and houses should be written.

The articles are also connected, moreover, by their subject matter. The key link is the Williamite abbey of Gross-Burlo. Bishop Ludolf of Münster, who had been involved in its foundation in 1245, recommended the Hermits of St. William to his kinsmen, the lords of Holte, who established another Williamite house at Holte. Holte was incorporated into the Austin friars when Pope Alexander IV combined various Italian orders into a new mendicant order; and the brothers had moved by 1287 to Osnabrück, one of the three convents of Austin friars in Westphalia. The impetus for the incorporation of Gross-Burlo into the Cistercian order in 1448 came, not from the three male houses of Westphalian Cistercians, but from Dutch abbeys that had been influenced by the *Devotio moderna.*

The articles are repetitious at times, but they do contain some intriguing suggestions, for example, that the Austin friars who served as permanent *terminarii* in various late-medieval Westphalian towns were the functional precursors of the Lutheran pastors. Indeed, some of them became precisely that. On the other hand, I was less convinced by Elm's contention that the Austin friars, whom he characterizes as the Jesuits of the Middle Ages, became the enthusiastic promoters of the Reformation because it gave an increasingly socially isolated order a way to regain its traditional leadership role. The brothers' personal ties to Wittenberg and the order's traditional Augustinian theology were certainly more important factors, as Elm suggests else-