Provincial Families of the Renaissance

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INTRODUCTION

IN THIS BOOK I investigate a cluster of unremarkable people who lived in two midsized Italian cities five hundred years ago. This study is based on texts that they produced and records that others generated about them, and seeks to uncover their priorities and strategies: what engaged them sufficiently to write, how they arranged their lives within the possibilities (and limitations) accorded them, the choices they made and the choices made for them.

Three features shaped the project. The first was controlled serendipity, when in the course of thorough archival searches a colleague and I discovered rich documents of an unexpected type. The second was my growing conviction that these texts might serve as the basis for an examination of experience in settings that the historical literature has seldom considered. Third, the writing of the book has been conditioned by a newfound sense of freedom to apply an eclectic, primarily empirical methodology.

The Sources

Some years back I found the memoir of the Arnaldi family of Vicenza. I was surprised by the work's existence, because my professional training had never mentioned domestic chronicles in any region except Tuscany. On a subsequent visit, I was informed that the Vicentine state archive had acquired a massive multifamily cache of documents that was thought to contain Arnaldi material. The staff's intuition proved accurate, and officials graciously permitted me to photograph the 1,500 parchments and several account books in the Arnaldi trove, along with two thousand notarial documents relating to the family. Later I determined that another memoir from Vicenza, by Manfredo Repeta, was in the original an equally revealing source for private life; a poor and partial nineteenth-century edition had obscured many of its best
features. Further search uncovered Bartolomeo dal Bovo’s family chronicle in Verona, Ruggiero Cortusi’s annotated account book in Padua, and the Freschi memoir in Venice, as well as brief or fragmentary entries by the Trento, Guastaverza, da Romagna, and Feramosca families.

Meanwhile, Gian Maria Varanini found comparable manuscripts from the Verità, Fracastoro, and Stoppi families of Verona, and drew my attention to the miscellany of Bartolomeo Muronovo, which had been cited for other reasons but never examined for its copious domestic notices. The Veronese archivist Gloria Maroso showed me the uncatalogued memoriale of Bonaventura Bovi. Varanini and I are preparing a critical edition of the Veronese and Vicentine memoirs. These memoirs, supported by evidence from tax rolls, testaments, notarial registers, and family archives, provide the documentary basis for this study.

It has been remarked, accurately, that the genre of the memoir does not exist in the Veneto. The term itself, as well as its synonyms “family chronicle,” ricordanza, and “domestic chronicle,” is a catchall label rather than a precise descriptive. Heterogenous in format and content, the family texts of Verona and Vicenza share only what has been called an anagraphic feature: the recording of births, marriages, and deaths within domestic units. Bovi’s memoriale alone is freestanding, dedicated solely to the writer’s immediate family. The rest fall into two main groupings. Some began as property account books with inventories and summaries of transactions, into which authors inserted anagraphic material: this was the case in the Stoppi, Cortusi, and (perhaps) Fracastoro manuscripts. Others began as zibaldoni, miscellanies of classical, spiritual, poetic, prophetic, political, medical, and astrological writings, into which authors entered domestic notices: this was the case in the Verità, Muronovo, and dal Bovo manuscripts. The catasto of Manfredo Repeta is a hybrid, originally a land account book that Repeta gradually turned into a zibaldone, recording the careers of his children along the way.

Because the genre does not exist in terms of uniformity or autonomy, can these texts sustain a monograph? I think so, on three grounds. First, the anagraphic material is remarkably constant in format throughout, and provides a common core of commensurate demographic data. Second, the heterogenous quality of the nonanagraphic texts enhances rather than throws up an obstacle to the study of family life: the variety of information allows commentary on a wide range of topics. Third, the memoirs of Verona and Vicenza are no more miscellaneous than the Florentine ricordanze that have supported several recent studies. As I shall argue in the introduction to the
critical edition of these works, virtually all that *libri di famiglie* have in common is their rich personal and domestic record.

The Subjects

It is meaningless, as Lucien Febvre and Thomas Kuehn have charged,\(^5\) to justify the writing of a book on the grounds that its documentary base is interesting or novel. Interesting to whom, besides the author? And what greater purpose can new information serve? Equally dubious is the premise that a book fills a gap: some topics have been overlooked deliberately and justly, as Girolamo Arnaldi has said, because they are of little importance.\(^6\) The very notion of filling gaps smacks of the positivist program of a Great Wall of History that will someday be completed as historians multiply sub-studies, a scheme that has lost currency.

Bringing long-forgotten families from two semi-important cities to light, then, requires justification. It could hardly be argued that we know little about domestic life in the fifteenth century: the bibliography is now too vast for anyone to master. The geographic basis for that bibliography, however, remains slight indeed, as the field rests on case studies from a very few metropolitan centers. The family history of Italy is the history of great houses in great cities. At a rough guess Florence has claimed about 85 percent of scholars' attentions, the rest of Tuscany a further 5 percent, Venice another 5 percent, and Milan and Genoa combined the remainder. Of late there has appeared considerable work on lesser centers—collaborative histories of Brescia, Prato, Vicenza, and Vigevano, and monographs on Bergamo, Pescia, and Brescia—but these retain a traditional focus on politics and administration, economic structures, and political elites, with little regard paid to the domestic and the ordinary.

This imbalance has understandable historiographic roots. The preference for major urban units is very old: as the fourteenth-century jurist Bartolus of Sassoferrato commented, "It is better to be a middling citizen of a noble and honorable city than to be a leading citizen of a moderate city."\(^7\) When the field of Renaissance studies was founded in the nineteenth century, few found cause to alter that judgment. Partisans of the Risorgimento and the nation-state located appropriate historical models for unified, sovereign poli-ties in the free city-states of the past, and left fragmented, unfree little cities to local antiquarians. Evolutionary historians of the left and right located advanced social and economic change in metropolitan centers, and thought provincial cities backward. Cultural historians looked to centers of innova-
tion and production, and saw no reason to examine backwaters. That legacy has persisted to a surprising degree in the twentieth century.

Too, it can hardly be denied that archives of the capitals are incomparably richer than those of the provinces: Florence, above all, provides copious evidence for questions that cannot even be posed for second-tier cities. There is, as well, the inescapable fact that a few centers set the tone for the rest. In the exercise of power, or high culture, or economics, capitals never eradicated localism but continued to provide overall direction; local societies resisted the hegemony of their masters only at the cost of remaining marginal. It has been reasonable, then, to look at the sites that gave cues to others.

The metropolis's near-monopoly on historical writing is, however, a risky proposition for social history, however that field is defined. Recovery of the quotidian and nonelite cannot be achieved with confidence if only a few major locales provide models and evidence. The overriding assumption for a metropolitan focus must be either that Florence or Venice constitute worlds unto themselves, such that there is no need to look beyond, or that we can safely extrapolate from the center to the periphery. Both propositions make those of us who work in the provinces nervous. One of my mischievous graduate students likened the situation to studying American society by looking at Manhattan alone: the one great borough might indeed offer a representative sample, but we would only be sure if we looked at Jacksonville, Dayton, Tulsa, Tucson, and Seattle as well.

Verona and Vicenza were fair- to good-sized cities: the former counted about 35,000 residents by the late fifteenth century, the latter about 21,000, compared with Venice's 100,000, Florence's 55,000, Genoa's 60,000, Milan's 100,000 and Naples' 120,000. Both ranked with cities such as Ferrara, Bologna, Mantua, and Perugia, and both were considerably larger than the second tier of Lombard and Tuscan cities. Neither was independent, having been incorporated into the Venetian dominion in the first decade of the century. Neither was a center of extraregional exchange, though Verona in particular enjoyed prosperity on a local scale and sent merchants abroad. Neither exercised much political or cultural influence beyond its borders.

These provincial settings were, then, rather different in their formal characteristics from metropolitan centers. They are considered here because they offer perspective on the Florentine and Venetian experiences. It is not claimed that they were somehow more typical of Renaissance Italian societies than were the big cities: as nuanced case studies proliferate, it becomes difficult, even futile to claim that any one experience is representative or unrepresentative. Nor does this book intend to mount a frontal assault on Floren-
tine- and Venetian-based historiography, most of which is cited here with approval. Provincial towns are not good bases for thorough revisionism. Instead, the experiences of Verona and Vicenza provide an additional, alternative viewpoint. At some junctures they replicate metropolitan patterns, and so help universalize hypotheses generated from major centers. At some junctures they do not, and so suggest that the social history of the peninsula is more variegated than we would otherwise suppose.

None of the people who emerge from the Veneto memoirs accomplished anything significant, good or bad, which might warrant fame on more than the local level. This book is not an attempt to rehabilitate the unjustly overlooked: its subjects are indeed obscure. They attract our attention only because, unlike their fellow citizens and for reasons that remain unclear, they chose to write about themselves and to record texts that mattered to them.

Here, too, I hope to provide a novel perspective, that of the solid but un conspicuous citizen of the lesser center. This figure has not received much attention. Nineteenth-century bourgeois did not wish to study the middling sort critically because to do so might call themselves into question; the left viewed them as unprogressive, Romantics viewed them as dull, and the right viewed them as ignoble. When social history detached from traditional political-administrative-diplomatic concerns, scholars moved in new directions, but these, too, have seldom been concerned with the established but pedestrian. Many have looked at elites, to discern mechanisms by which the upper ranks of Florence, Venice, and Genoa acquired, consolidated, and perpetuated their prestige, power, and wealth. Other historians swung to the other extreme, examining artisans, urban underclasses, and peasants. Lately, many have studied outgroups: the miserable and the abused, criminals and rebels, and those classified in their time as deviants. Partly these categories have received attention for their own sake, as unfairly neglected in traditional history, and partly because marginality can furnish clues to the boundaries of the conventional.

This book, once again, does not even implicitly condemn those concentrations: the experiences of the mighty, the low-ranking, and the dispossessed should indeed be made known. Instead, the Veneto memoirs can contribute to social recovery by examining a stratum that remains nearly unknown: the middling sort, those who lived their lives in relative quiet and passed quickly into obscurity, neither powerful nor powerless, well fed but not out-
standingly rich, neither victimizers (to any major extent, at least) nor vic­
timized, literate but not cultured. The people who appear in these pages were
modest in their victories and local in their ambitions. If the subjects of this
book fall into the “abyss between the rich and poor, the well-connected and
the isolated,” it is also the case that to scorn and/or ignore them is to miss
a significant portion of the historical landscape.

I do not claim here that these families of Verona and Vicenza were in any
way more representative of fifteenth-century experience than were metro­
politan elites, the poor, or marginals. Indeed, as the many people without his­
tory are increasingly recovered and put at the center of the historical stage,
any effort to establish typical experience seems futile and even undesirable.
Rather, I make a more modest claim: that the middling sort, too, had a voice,
and that if we undertake the task of hearing all voices we should listen to
theirs as well.

Methodology

The Veneto memoirs cannot by themselves support a thorough history of
the provincial family in the early Renaissance. On many issues that presently
engage historians they are, alas, silent. While that silence at least indicates
that the priorities of these authors were not those of contemporary histori­
ography, it cannot be used as the basis for substantive comment. For other
topics they provide brief and tantalizing bits of information, suggestive but
insufficiently dense to permit confident conclusions. Furthermore, the Veneto
offers little corroborating literature such as treatises, sermons, moral pre­
scriptions, or spiritual guides, and the secondary literature on family history
in the region is scant.

How, then, to put the data of the memoirs into a larger context? The strat­
egy adopted here is that of the gloss, a point-by-point exegesis of the texts
using comparison with and explanation from other sources. Some of these
sources are commensurate (e.g., the memoirs, notarial instruments, and tes­
taments of other cities). Some are not: to establish the prescriptive and the­
oretical field for memoir entries, I have drawn upon works of law, theology,
pedagogy, demographics, spirituality, and ethics that were produced else­
where. For example, memorialists in the Veneto demonstrate an under­
standing of an intricate theory of nobility but did not articulate it. They wrote
for descendants who shared their culture and did not require elaboration. We,
on the other hand, have not inherited that culture and require explanation
for what seem murky but critical issues. Because the texts are laconic, and
fellow citizens provided little commentary, the only available guides are treatises of jurists and humanists, most of whom wrote elsewhere.

The nature of the gloss requires a leap from the discrete case to a much larger setting. The leap may at times seem ungainly and will invite challenge, but it should be kept in mind what the gloss tries to do. There is no suggestion here that the modestly educated writers of the Veneto actually knew and used the works of Alberti, San Bernardino, Bartolus, or Francesco Barbaro. Still, ideas and models circulated widely in Quattrocento Italy, detached from the works in which they were most famously articulated: humble memorialists in provincial cities shared some elements of a cultural patrimony with learned authors. For example, memoirs and testaments frequently mention orders for postmortem Masses, which responded to a belief in purgatory. Veneto citizens probably knew little of that highly technical and contentious doctrine firsthand, but they addressed generalized notions about easing the afterlife; so the high texts discussed in Le Goff's magisterial Birth of Purgatory can shed light on their actions.

Sometimes, on the other hand, the gap between the treatise and provincial experience is very great. Yet that fact alone measures the distance between high and middling culture. It is frequently the case that observed behavior in these outlying cities did not correspond to the dictates of prescriptive literature. People in everyday life did not always do what theologians, canon lawyers, and moralists thought they should do. Learned writers and preachers, for example, urged audiences to put penitential and Marian themes at the forefront of devotions; memorialists paid little heed to contrition and the Holy Family. Here the normative literature, precisely because it is remote from praxis, reveals the extent to which people in the Veneto went their own way, resisted authority, and so exercised considerable freedom in shaping their lives.

This book is decidedly empirical. I have gathered available data, drawn conclusions that I judge supportable and plausible, and borrowed historians' insights to provide reinforcement for or contrast with those conclusions. Both sources and techniques are borrowed in eclectic, "ecumenical" fashion: whatever sheds light on the core evidence has been deemed useful. Notes are copious, so that those who wish to follow up leads or query interpretations will have the resources to do so. The language is as plain and straightforward as possible, shorn of highly theoretical phrasing, so that the book can be understood by specialists in other fields and even by that perhaps mythic figure, the general reader. The overall approach is thus rather traditional. At any rate,
the task at hand requires both conservatism and eclecticism: to provide comparatives with and perspective on secondary studies from other regions, I must at least in part replicate their methods.

I only hope that this book is not naively empirical. Many criticisms of "dig and muddle through" history are entirely justified. I am aware, too, of the profound contributions of allied disciplines that have enriched the writing of family and social history. Indeed, many of the questions posed here are drawn from allied disciplines. But I have chosen not to take the lead of multidisciplinarians, and little is based on firsthand reading in other fields. Partly this is due to personal inclination. Partly, too, this is due to the nature of the sources. The accounts of Veneto families are never thick enough for social anthropology, nor sufficiently compact and linear for microhistory or the "new narrative"; the serial data cannot support more than rudimentary quantification. The texts are not sufficiently revealing to be closely deconstructed, and the web of signification is patchy rather than dense.

Empiricism is also possible because the triumph of pluralism in the historical community has cancelled any notion of a single inherently superior approach. The field of family and social history began with a two-part program: an attack on traditional interests and methods, and a willingness to borrow alternatives from neighboring fields. Both were largely successful, but there has been a price to pay. Appeals to allied methodologies have become so numerous as to be cacaphonous. With traditional history pushed to the background, champions of new histories justify their appeals partially on the grounds of merit but largely through criticism of competitors. Interdisciplinarians have fallen out among themselves.

The present internal self-examination in social history resembles the scene in the Chinese folktale of the War of the Five Armies, in which equally matched factions march down from mountain strongholds to join in chaotic and inconclusive battle on the plain below. Much criticism has indeed been telling: that sociology and structural anthropology impose rigid, functionalist categories; that quantification and collectivist social science are reductive and deterministic, denying variation and choice, and force aggregate analysis upon evidence that was never intended for such purposes; that non-quantification can only fall back upon the anecdotal; that most subfields ignore the technicalities and conventions of the texts that carry (but also condition) information; that overconfident social anthropology is blind to the degree to which texts are made to answer presentist questions (as ethnographers hear only responses to questions they themselves ask) and cannot speak for their own ages, hence becoming mere vehicles for historians' pro-
jections. Many defenses are equally convincing, and most historians would acknowledge that there has been masterful work in all fields. In consequence, no approach is beyond reproach, but no approach is eliminated altogether. If all techniques distort the past in some manner, no single methodology can claim inherent preference and none—including the empirical—is inherently without promise.

Granted, empiricism can only rejoin the ranks of the acceptable by assuming a degree of reserve seldom found in earlier generations, which were blithely confident that the data spoke in a straightforward manner. Current thinking rightly teaches that words are unstable and often defy understanding; that the formulas and literary conventions of texts construct (and so partly conceal) rather than directly reveal events; and that the historian is inevitably active transformer rather than passive reporter. There must always be kept in mind the question posed by Anthony Molho: can we, at five hundred years' distance, understand what people in the fifteenth century meant by their words? An unequivocal yes is indeed naive. An unequivocal no, however, allows only two unpleasant alternatives: the despair and paralysis of those who see only the radical otherness of the past and thus find historical reconstruction impossible and ultimately pointless, or else the cheerful anarchism of those who, likewise denying the enterprise of seeking to understand the past on its own terms, justify its use as raw material for purely topical inquiries.

Staking out a middle ground among the fruitless varietals of blind optimism, nihilism, and abandon, insisting that foreign cultures can become somewhat familiar and that partial recovery of the past is indeed within our reach, demands extreme care in reading documents and limits expectations that we can establish their meaning. Occupying that middle ground, a cautious empiricism that tries to remain close and faithful to its sources may at least avoid the more obvious distortions.