Introduction to "Marriage and Elite Structure in Renaissance Florence, 1282-1500"

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Resumen

El siguiente texto, salido de una ponencia de conferencia y escrito en 1994 es el primer borrador de un artículo publicado en el 2010 en *Renaissance Quarterly* y llamado "Open Elite? Social Mobility, Marriage, and Family in Florence, 1282-1494." Esta introducción debate las justificaciones metodológicas para publicar un primer borrador, escrito seis años antes del artículo final más sofisticado. Al descubrir algunos de los pasos del proceso de investigación permite examinar la relación entre los dos estilos de "interpretación" en la historia y el "test de hipótesis" en ciencias sociales.

Palabras clave: Movilidad social, familia, historia, cualitativo-cuantitativo.

Abstract

The following conference paper, written in 1994, is the first draft of an article eventually published in 2010 in *Renaissance Quarterly*, entitled "Open Elite? Social Mobility, Marriage, and Family in Florence, 1282-1494." This introduction discusses the methodological justifications for publishing a first draft, written sixteen years before the final, more sophisticated article. By unveiling some steps of the actual process of research, it allows a discussion of the relationships between the two styles of "interpretation" in history and "testing hypotheses" in social science.

Key words: Social Mobility – Family – History – Qualitative/Quantitative.

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The following conference paper, written in 1994, is the first draft of an article eventually published in 2010 in *Renaissance Quarterly*, entitled "Open Elite? Social Mobility, Marriage, and Family in Florence, 1282-1494." (Padgett, 2010). What are the justifications for publishing a first draft, written sixteen years before the final, more sophisticated article? There are interesting findings in this first draft, which did not make it into the final version—in particular, the micro marriage dynamics of dyads and triads, which point to ebb and flow in Florentines' concern with hierarchical asymmetry, and macro comparisons of the evolution of Florentine elite structure, as represented in multi-dimensional scaling. The dramatic change in elite structure between Cosimo de' Medici and Lorenzo de' Medici—where Medici partisans married enemies under Cosimo, but only each other under Lorenzo—is of particular historical interest in the scaling results.

The primary justification for publishing this old conference paper, however, is methodological. When reading only finally polished pieces of historical or social scientific research, one can be misled about the actual process of research. Polished pieces are deeply embedded in the historiographical literature of previous research, because the objective is to advance a coherent (and hopefully at least somewhat novel) interpretation of some empirical phenomenon, in light of existing (and perhaps contending) alternative interpretations. Some regard the two styles of "interpretation" in history and "testing hypotheses" in social science to be contradictory. I am not of that view, believing instead that at a deep level both styles of research are trying to do the same thing—namely, to develop elegant explanations of complex phenomena, where "elegance" is some sort of ratio between the range and diversity of empirical evidence brought to bear, and the parsimony of principles used to comprehend this diverse evidence.[2] Whereas historians place their emphasis on "range and diversity of evidence" and social scientists place their emphasis on "parsimony of principles," going too far in either direction, without due attention to the other side, does not add up to "elegance" (or to scientific achievement) in my view.[3]

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[2] For elaboration on this epistemological position, see my contribution to Fowler et al. (2011), entitled "Triangulating on Causal Process."

[3] Note that by "parsimony of principles," I do not advocate "universality of principles." I am completely persuaded by historians and others that human behavior is time-and-place context specific. But contextualism is no argument for descending into either antiquarianism or extreme relativism, where no coherent conversations across multiple contexts are possible. I believe more in the promise of inductive science than of deductive science, although both have their role. In the inductive approach, structured

From this perspective, the problem with looking at only final products of research, of either genre, is that the preliminary "blocking out" process, where the problem becomes defined in the first place, is hidden from view. Good research is not just a matter of coming up with clever answers; good research is also a matter of coming up with informed and penetrating questions. There is an important (and usually not visible) phase of preliminary exploration involved in developing the research capacity to ask good questions.

The "blocking out" process of preliminary exploration was particularly important in a large-scale data collection project like this one. Following in the trail of David Herlihy and Christiane Klapisch-Zuber (1978, 1985), who together pioneered large-scale quantitative research in Florence (and indeed in history in general), this project of mine studies the *longue durée* of two centuries' worth of co-evolution or linked change in marriage networks, political networks, and economic networks, during the Florentine Renaissance from 1300 to 1500. Numerous publications have appeared from this project to date,[4] with more to come. But in 1994, a big chunk (particularly the marriage chunk) of the archival coding had just been completed, and the overall shape of this material was far from clear.

In 1993, I published an article on Florentine political networks, entitled *Robust Action and the Rise of the Medici* (Padgett and Ansell, 1993). This article, first written in 1990, was a reanalysis of data coded from secondary sources, especially the excellent prosopography of Dale Kent (1978). Marriage figured centrally in both Kent's and my analyses of political-party formation, and I was eager to see how these short-term political dynamics fit into the longer-term evolution of the Florentine elite—particularly because Kent, like most historians, had "sampled on the dependent variable," as social scientists say. That is, she had started with the phenomenon of interest—factional networks, in her case—and had entered the archives looking for ties among partisans.[5] This produced terrific research on that corner of the global Florentine social-network system, but it left almost completely

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dialogue between historical subjects and contemporary investigators, which reveals hidden assumptions on both sides, certainly is possible.

[4] On political organizations and networks, see Padgett and Ansell (1993), McLean (2007). On marriage and family networks, see Padgett (1994, published here), Padgett (2010), Padgett (2012a, 2012b). On economic organizations and networks, see McLean and Padgett (1997), Padgett (2001), Padgett and McLean (2006), Padgett and McLean (2011), Padgett (2012a, 2012b).

[5] Using different language, fellow historian Cohn (1980) also criticized Kent for this sample bias. In truth, the only sure way to avoid this difficult problem is to be exhaustive in data collection, like Herlihy

blank social networks to and among non-partisans. This sampling bias made for a superb fine-grained portrait of political organizational structure, but it compromised the study of the emergence of that organization out of more diffuse elites, who might or might not have joined various sides under slightly different circumstances.

Under the intellectual influence of the historian David Herlihy and the social scientist Harrison White, this originally narrow sample-bias motivation for enriching data collection of political networks broadened into a substantive interest in long-term change in the social, political, and economic networks of Renaissance Florence. Coded from over 100 archival sources (for details, see Padgett, 2010), and assembled together and cross-referenced into an ACCESS relational database, the data currently comprise 42,763 males and 12,875 females, arranged into 1,697 families of patrilineage descent, who lived during the study period of 1282 to 1500.[6] As of the 1994 date of this article's writing, 960 families were included. Because of this high rate of coverage, even sixteen years ago, all of the results reported in this early draft are confirmed in the 2010 *Renaissance Quarterly* final version. Only extensions—in data, in statistical sophistication, in historiography, and most importantly in orienting question[7]—are involved in the difference between first and last drafts.

Given that the 2010 *RQ* article clearly supersedes this 1994 draft, what remains useful about this 1994 draft? My answer is that this 1994 piece demonstrates one useful way of assembling a forest from the trees—not only that, but of seeing an evolving forest within a kaleidoscope of moving trees. What changes and what does not in kinship and marriage over two centuries? Historians and social scientists alike are sometimes more focused on the research task of identifying social patterns than they are on identifying social change. With Herlihy and Klapisch-Zuber being a prominent exception, almost all of the historiography on Florentine kinship and marriage is devoted to case studies of particular families, or sometimes to a

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and Klapisch-Zuber were. Practically speaking, almost everyone, historian or social scientist, delimits their topic in some way, and this drawing of research boundaries inevitably involves tradeoffs.

[6] The 1282 starting date is the beginning of the Florentine republic. The ending date is arbitrary.

[7] The orienting question of the 2010 *RQ* article was social mobility—in particular, how could demonstrably high rates of social mobility (an "open elite") be possible in an elitist and conservative society, demonstrably opposed to such mobility? The answer provided there was contradiction among three status orders, which had surprisingly low correlations (r = -0.1 minimum to +0.2 maximum) among themselves—namely, wealth, ancient lineage, and political power. Implications for the diffusion of elitist patriarchal values into middle sectors of the society, and for changes in the internal structure, on average, of Florentine lineages—namely, a shift from predominantly military to predominantly economic to predominantly political functioning—were also identified.

comparison of as many as four families.[8] When I started this research, with my intense interest in the Medici family, I proceeded the same way—coding up a family, then reading about it, family by family, before coding up another one. Proceeding this way is natural, because the Florentine archives (at least for prominent families) are largely organized by family. By conceptualizing Florentine history as a set of parallel family histories, one is thinking about history in the same way that elite Florentines thought. This organized the structure of their own written records, which is what we see.

At this point, all researchers face a choice. Do we "let the archives speak for themselves?," which means accepting uncritically the cognitive categories of natives, and possibly descending into antiquarianism.[9] Or do we impose our own cognitive categories, thereby misleading ourselves into thinking that we know what they were thinking? This paper shows, I believe, that it is possible to use statistics to find a middle way. Namely, to try out many, many different cognitive categories on the data—most of which derive from natives, but some of which can be derived from analysts, if measurement exists—in order to find out which of these cognitive categories are consistent with the actual behaviors of Florentines and which are not. Or even more interestingly, one can use measurement to uncover change over time, where cognitive categories shift in and out of consistency with behavior over time.

A fascinating example of the first type is the cognitive category of *popolani* in Florence. This social class term—meaning descendants of the founders of the Florentine republic—was central in the Florentine political discourse and conflict of the period, especially the Trecento, but also the Quattrocento. Yet intermarriage data never, at any period, show the relevance of this cognitive category to actual marriage behavior. Does this mean that Florentine elites were blind to social class? Hardly. But the data reveal that social class was an unexpectedly second-order construct for Florentines. The first-order cleavage that "social class" coded was between magnates and new men. The politically dominant *popolani* were like magnates in kinship, but like new men in business. They had a Janus-faced network

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[8] Many studies are discussed in the *RQ* article, with Goldthwaite (1968), Kent (1977), and Molho (1994) being singled out for particular attention. Molho is a methodological hybrid: aggregate statistics combined with a close-up case study of the Rinuccini family.

[9] Antiquarianism really means that the researcher is acting as the spokesman/propagandist for the deceased.

structure and identity, which was crucial both to their victory and to the legitimacy of their new republican institution. Such insight, I maintain, comes from imposing an "obvious" native linguistic category on the marriage data and watching it statistically fail.[10]

Not all findings, of course, are counterintuitive like this. The Florentines also talked a lot, in their marriage negotiations,[11] about dowry and political position. And low and behold, wealth and winning political faction turn out to be critical determinants of marriage choice in all periods of my data. The interesting twist, discussed more in the *RQ* piece than here, is that the tumultuous political history of Florence had major implications for social mobility and the evolution of family structure over two centuries.

A non-obvious example of the second type—namely, change in cognitive category over time—is neighborhood. Florentine historiography is full of discussion of the powerful influence that *gonfalone* had on structuring sociality of all forms in Renaissance Florence, including marriage. The marriage data examined here show that to be overwhelmingly true early in this period, but not true at the end. Whatever *gonfalone* meant in government administration, it faded in its influence on (at least one type of) social behavior.

My central methodological point with these examples is that using statistics does not mean that one is a positivist, at least in the slur sense of that term (i.e., insensitive to cultural categories) that humanists often imply by that label. All that use of statistics implies is that one insists on comparing cognitive categories with measurable behavior, to the illumination of both.

One feature of my research not revealed well in this 1994 conference paper is my preference for blending statistical with qualitative and textual materials.[12] Here the emphasis is all on the statistical side, not at all on the textual side. But this derives

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[10] Of course I also had to know enough about Florentine political discourse and brutal violence not to go to the other extreme and draw Pollyannish conclusions about "classless society" and other such hogwash.

[11] See for example the letters of Alessandra Strozzi (Gregory 1997), or the harder to interpret, because so idealized, discourses on the family by Leonbattista Alberti (Guarino 1971).

[12] For one particularly sustained attempt to compare statistical with textual evidence, on the same behaviors, see Padgett and McLean (2011). There, transactional data on commercial credit are compared with businessmen's letters to each other about those loans. These two types of evidence reveal complementary but not the same aspects of this economic activity. Discrepancies as well as consistencies across evidentiary types are revealing.

from the exploratory "blocking out" purpose of this paper, not from deep philosophical commitments. In the final analysis, statistical (behavioral) and textual (cultural) evidence need to engage with each other, not just to round out each other's individually revealed "truths", but to highlight inconsistencies and contradictions between them. Such inconsistencies and contradictions, in my priors, are fruitful places to look for uncovering frictional sources of change, not stasis.[13] But one can't do everything in a first draft. Blocking out historical facts and trends over two centuries, which were never known before, is a good enough achievement.[14]

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[13] Part of the reason that exclusively culturally oriented studies often are poor at describing and explaining change is that they sometimes do not give themselves the evidentiary space to find inconsistencies and contradictions between the cognitive categories and the behaviors of their subjects.

[14] A closing footnote perhaps is warranted to explain the hand-drawn character of the figures in this paper. Computers of course existed, even way back in 1994, to draw these in an automated way. But when I started this project, I resisted that. The self-imposed discipline (and tedium) of hand drawing figures forces one to look at the data very up close. On the one hand, this gives one the space and the time to reflect on what the data are saying. On the even more important other hand, this pico view of data is essential for data cleaning. Hand drawn figures do not cause data cleaning, so much as they reflect the data cleaning going on at that time. One is sometimes shocked at the data-quality sloppiness that is induced by the trance of beautiful computerized statistics and pictures. "Data quality" in this project does not mean transcription errors so much as it does inconsistency across sources, a very tedious and time consuming matter to try to clarify with 60,000 cases and 100+ sources. Fortunately the very volume of my sources gave me some leverage on this difficult task. Now that I am more confident in the quality of my cross-checked data, I am more accepting of computer assisted research.

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