

Response to Thornton's response to my critique of *Reading History Sideways*

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At the November 2005 meeting of the Social Science History Association (SSHA), I participated in a panel discussion on Arland Thornton's new book, *Reading History Sideways: the Fallacy and Enduring Impact of the Developmental Paradigm on Family Life* (Chicago, 2005). In January 2006, Thornton posted a lengthy response to my comments on his website, along with five other working papers that address parts of my critique.¹ The present note is my response to Thornton's response, and attempts to clarify my critique of Thornton's argument. I do not summarize that argument; my discussion assumes that readers are familiar with it.

I do not dispute Thornton's argument that many scholars have used cross-cultural comparisons to infer relationships between economic conditions and social behavior. Scholars have also frequently used these comparisons to support theories of historical change in the European past. I further agree with Thornton that this technique can be problematic, and has the potential to yield misleading results.

I disagree with Thornton in several key respects, but perhaps most importantly with his interpretation that such comparative analysis led European scholars to grossly incorrect conclusions about the history of the family. Even if reading history sideways has the potential for error, not every statement supported by geographic comparison is wrong. I contend, in fact, that most of the generalizations Thornton cites about historical family change by these scholars have turned out to be true. In particular, the four key elements of Thornton's great family transition—an increase in nuclear families, an increase in marriage age, an increase in the status of women, and a rise of individualism—all probably occurred at various points in the European past.

Consider, for example, the case of family structure. Thornton's earliest scholar who explicitly discussed a change in living arrangements was Le Play, whose main work on the subject—not cited by Thornton—was first published in 1870. Le Play argued that at the time he was writing, a shift from stem families to unstable families was underway. In stem families, according to Le Play's definition, the father selected one child to remain near the parental homestead to work on the farm, and eventually inherit it, thus continuing the family line. All other children left the parental family to form their own nuclear households.² In unstable families, by contrast, all the children left home and formed their own households, and the family was extinguished upon the death of the parents. Le Play is not describing a transition from large extended families, as

¹ <http://developmentalidealism.org/pubs/rhs-ssha05.html>.

² "Un des enfants, marié près des parents, vit en communauté avec eux et perpétue, avec leur concours, la tradition des ancêtres. Les autres enfants s'établissent au dehors quand ils ne préfèrent pas garder le célibat au foyer paternel. Ces émigrants peuvent à leur gré rester indépendants l'un de l'autre ou tenter en commun des entreprises, rester fidèles à la tradition ou se placer dans des situations nouvelles créés par leur propre initiative" (LePlay 1883: 10).

Thornton describes them, “with children, parents, grandparents, and married aunts and uncles all living together (Thornton 2005a: 5).³

When he describes stem families, Le Play is not talking about a particular living arrangement at a given moment in time, but rather about a process whereby property and the family line was handed down from one generation to the next. As many scholars have pointed out, is hard to observe a very high frequency of stem households in cross-sectional sources under a preindustrial Northwest European demographic regime. High fertility meant that each stem line generated multiple new nuclear families, while late marriage and early death meant that there was often little or no overlap between the marriage of the heir and the death of the parents. Thus, it was impossible for most stem families to include a married child at any given moment (Berkner 1972; Ruggles, 1986, 1987, 1993, 1994, 1996a, 1996b, 2003; Ruggles and Brower 2003). Moreover, Le Play never claimed that all or even most families followed stem-family inheritance rules, just that a transition was underway from stem to unstable families.

We have strong evidence that the transition Le Play describes was actually taking place in the second half of the nineteenth century. The frequency of stem families varied widely across France in this period, but there can be little doubt that the custom was diminishing everywhere (see for example Bourdieu 1972; Collomp 1983; Darrow 1989; Fauve-Chamoux 1988, 1995, 1996; Fine-Sourac 1977; Shaffer 1982). There are no good national-level time-series data for France, but we do have good data for the United States, and they show the trend clearly: the percentage of persons aged 65 or older residing with both a child and a child-in-law dropped steadily, from 26.0 percent in 1860 to 18.8 percent in 1910 and 8.2 percent in 1960.⁴ The evidence from the French studies suggests that “among the working class populations subject to the new manufacturing system of Western Europe,” in Le Play’s phrase, the shift away from stem families began even earlier (Silver 1982: 260).

On marriage age, Thornton’s main theorist is Robert Malthus, who discussed the issue in the 1803 edition of his *Essay on the Principle of Population*. Malthus knew that age at marriage occurred at a late age in 18th century Northwestern Europe compared with both other regions and with the Ancient world. It was reasonable for him to conclude that Northwestern European marriage age must have increased at some point in the past. Thornton argues that Malthus was wrong, and that Northwest Europeans had married late from time immemorial. Although the issue remains somewhat controversial, most historical demographers do not accept this interpretation, even if confined to England; as John Hatcher puts it, “a rising chorus of critics in recent years have put forward contrary views” (Hatcher 2003:118; see also Seccombe 1992). If

³ Le Play does describe patriarchal families of nomads in which “all the married sons remain near the father” (Silver 1982: 259), but makes it clear that this pattern did not exist in Northwestern Europe. I am not aware of any speculation by Le Play about the existence of patriarchal families in Northwest Europe at some point in the distant past. If he did make such a speculation, however, it would be impossible to prove him wrong, since we have no representative data on living arrangements before 1599.

⁴ Some would argue, following Kertzer (1995), Hareven (1996), and others, that the high frequency of coresidence in the nineteenth century represented not stem families but old-age assistance. There is, however, overwhelming evidence that this was not the case; coresident elderly tended to be the household heads and property owners, and they were healthier and wealthier than those who did not reside with children; see Ruggles (2003, 2005).

we look at Northwest Europe outside of England, there is substantial evidence of an increase in marriage age in the early modern period (e.g., De Vries 1976: 9-10; Lynch 2003: 44-46).

Even if we ignore the sketchy and sometimes contradictory evidence of historical demographers, however, we can be sure that there must have been some time in the past when the Northwestern European marriage pattern became distinguished from the pattern of earlier marriage prevailing in the rest of the world. There are only two logical mechanisms by which this differential could have occurred: either marriage age in Northwest Europe went up, or that of the rest of the world went down. The former hypothesis seems much more plausible than the latter, and it is reasonable to conclude that Malthus got this one right; certainly, there is no evidence that he was wrong.

The evidence on both the rising status of women in early modern Europe and the rise of individualism is even more compelling. When Millar (1771) wrote of wives becoming “neither the slaves, nor the idols of the other sex, but the friends and companions,” he may have based his conclusion on comparative analysis, but virtually all historians of gender would agree—using purely historical sources—that a shift to companionate marriage was indeed beginning to occur at the time he wrote (e.g., Leacock 1984; Stafford 2002). Some British exceptionalists perceive signs of modern individualistic laws and practices even in late medieval England, but no scholar prior to Thornton—not even MacFarlane (1979)—has suggested that there was no increase of individualistic values and behavior between the thirteenth and the nineteenth centuries.

Thornton’s premise is that a large number of scholars writing from the 18th to the 20th centuries, by “reading history sideways,” came to incorrect conclusions about changes in the Northwest European family. I argue, however, that those conclusions were largely correct. If the historical generalizations made Thornton’s writers were for the most part true, that seriously undermines the premise of the *Reading History Sideways*.

The remainder of this comment will address the specific comments Thornton (2006) made in response to my critique at SSHA.

Mischaracterization of Thornton’s argument (pp. 6-7). Thornton writes that I demonstrate misunderstanding of his book because of two important inaccuracies in my characterization of his argument. First, Thornton points out that *Reading History Sideways* focuses on Northwest Europe, not just Europe as I expressed it in my PowerPoint slide 2. This was an oversight on my part (although on the following slide, I did specify “Northwest Europe”).

Second, Thornton disagrees with my characterization of his argument that a consensus of scholars reading history sideways adopted a coherent set of myths about historical change in the European family that occurred *before 1700*. Thornton objects that the book does not specify “before 1700.” At the SSHA conference session, I acknowledged that I must have misread this.

Now I am not so sure. One of Thornton’s new working papers opens as follows:

I argue elsewhere (Thornton 2005a) that many scholars of the 1700s and 1800s believed that family life in the Northwest European past had been very different from

what it was during their lifetimes. This suggested a great transition in family life *sometime before the 1700s or 1800s*. Among the important changes that were believed to have occurred were transitions from extended to nuclear families, from young and universal marriage to older marriage and extensive celibacy, from extensive family solidarity to individualism, from arranged marriage to love matches, and from high to low parental authority (Thornton 2005f: 1; italics mine).

In his newest work, then, Thornton writes that the beliefs of many scholars suggested a transition before the 1700s or 1800s.⁵ If my presentation overstated the case, it was not by much.

Dating the emergence of the theory of a great family transition (pp. 8-10). In my comments at SSHA, I expressed skepticism about Thornton's view of the early development of a consensus theory of change in the family. In particular, I expressed doubt that there was a coherent consensus among scholars who wrote before 1850 that Northwest Europe had been through a transition in the family which included a shift from extended to nuclear household composition, from familistic to individualistic orientation, from early to late marriage, and from low to high status for women. Although I acknowledged these ideas had all been expressed by the end of the nineteenth century, I also expressed doubt that these views should be characterized as a dominant and coherent theory before the twentieth century, and perhaps not even then.

Thornton argues that this coherent consensus actually emerged in the 18th century, writing in his response that "I document in Chapter 3 (especially pages 61-72) of the book discussions of specific elements of the supposed great family transition by 1803 in the works of such scholars as Alexander, Condorcet, Home, Malthus, Millar, and Smith" (Thornton 2006: 9). *Reading History Sideways*, however, does not actually provide this documentation.

To see if I missed something the first time, I went back and examined every citation of a pre-1850 source on pages 61-72 of *Reading History Sideways*. I provide a full list of those mentions in the appendix to this note, but the results are easily summarized. I found only two brief passages in which Thornton attributed a specific family transition to a specific author writing before 1850, one to Robert Malthus and the other to Adam Smith.⁶ For the most part, Thornton instead makes his case through broad assertions with no details and no citations, such as this one on p. 62:

[The family scholarship of the 1700s through the early 1900s] suggested that, in the Northwest European Past, societies were overwhelmingly organized around family

⁵ In addition, Thornton (2005a: 84), while trying to refute the "extended family myth," writes that there was no evidence for a decline in extended households "during the centuries preceding the 1700s and 1800s."

⁶ The first of these occurs on page 62; referring to Malthus, Thornton writes "On comparing these two groups of societies (the less advanced and the more advanced), he interpreted the differences in marriage patterns that he found as the natural product of societal development (Malthus 1986b/1803)." The other case in which Thornton attributes a specific family change to a specific pre-1850 author occurs on page 68: Thornton tells us that Adam Smith made a "brief comment" which "suggested that in the past, married children lived with their parents while, at the present time, they lived alone."

and kinship relations, that they were familistic rather than individualistic, that they were relatively undifferentiated (i.e., contained few nonfamily institutions), and that they were characterized by a household structure that was extended rather than nuclear.

And this one on page 67:

With one possible exception, all the family transitions in this chapter had been reported at least by 1803 in the work of such scholars as Millar, Smith, Alexander, Robertson, and Malthus . . .

Thornton writes that he documented that there was a consensus among scholars before 1850 on a theory of European family transition. I remain unpersuaded, however, that his brief references to Malthus and Smith are sufficient to prove that there was a significant scholarly consensus on this issue.⁷

Personal Observation Method versus reading history sideways (pp. 11-22). In my presentation at SSHA, I made the argument that many of the people who talked about changes in the family in the late nineteenth and early twentieth centuries were writing from personal experience. I specifically quoted three of the people responsible for the establishment of the U.S. Social Security program: Thomas Eliot, Nelson Cruickshank, and Ewan Clague. I argued that they knew about the shift in the living arrangements of the aged since they had witnessed it. When these men were children, they observed that virtually all the elderly people they knew resided with their adult children, but by the 1930s only a minority did so, and solitary elderly had become a social problem. I presented a graph to show the timing of this change, and pointed out that these people did not need to use “reading history sideways” to realize that change was taking place, since they could observe it directly through personal observation.

Thornton characterizes my comments as the “Personal Observation Method.” He writes that

⁷ I am not attempting to demonstrate that a consensus of scholars did not exist, just that its existence remains unproven. Thornton’s new working papers help to clarify who wrote what when. In these five papers, Thornton documents several generalizations about European family change from two authors writing before 1850: Robert Malthus and John Millar. Thornton (2005c) shows that Malthus observed that marriage age occurred later in Norway, England, and Switzerland than in other parts of the world or in Ancient Europe, and concluded that marriage age must have declined at some point in the past. Thornton (2005f) discusses Millar (1771), who argued that the position of women and children was better in commercial societies than in hunting, pastoral, or agricultural ones. Thornton does not mention any of the secondary literature on Millar, and his reading of Millar seems oversimplified compared with other interpretations (e.g. Bowles 1984, 1986; Olson 1998). Nevertheless, it is clear that Millar did feel that women must have had lower status in Europe at some time in the past, and that arranged marriages must have been more common. Thornton (2005f) also cites John Robertson, who in 1783 compared Native Americans and Europeans and, like Millar, pointed to arranged marriages and low status among the non-Europeans. But Thornton does not argue that Robertson had a theory of change over time in Northwestern Europe. It adds up, then, to some generalizations from Malthus about changes in marriage timing and some generalizations from Millar about changes in gender relations. Overall, although the working papers are far better documented than the book, they do provide insufficient evidence to demonstrate that there was a coherent consensus theory among scholars writing before 1850 that Northwest Europe had been through a great transition in the family.

The Personal Observation Method is an alternative method for studying family change, and it is different from reading history sideways in that it involves the investigator making his/her own personal observations about actual family change in at least one particular society. However, Ruggles provides no information about the method and how it was used. He also provides no information about how specific scholars such as Malthus, Le Play, and Westermarck used the Personal Observation Method rather than reading history sideways to describe long-term trends in family life (Thornton 2006: 11).

Thornton states that there although is no evidence that scholars of the 1700s and 1800s used the “Personal Observation Method,” there is ample evidence that they used the reading history sideways method. Then, for the next ten pages, Thornton describes the ways in which Malthus, Westermarck, and Le Play used cross-national and cross-regional comparisons to identify relationships between economic development and social behavior.

As I note in my opening comments above, I do not doubt that many scholars used comparisons across regions and cultures to analyze the relationship of economic conditions to family patterns, and in many instances, they used these cross-sectional difference to support theories of historical change. These writers, however, also observed changes going on around them. In the case of Le Play, this was quite self-conscious; he and his researchers visited peasant homes and systematically asked them questions about their youth and about their own family history and “the principal phases of its existence” (Silver 1982: 64).

Thornton’s representation of my work (pp. 22-29). In my SSHA presentation, I objected to several of Thornton’s quotations and citations of my work to prove points that were the opposite of what I intended. One of these occurred when Thornton is describing Laslett’s explosion of the myth of the extended family. Thornton begins by citing a dozen publications to support the thesis that “in Northwest Europe, households had been predominantly nuclear (or a weak stem form) for hundreds of years” (Thornton 2005a: 84). Thornton then states that Laslett found no marked changes in family arrangements for seven hundred years—an odd comment, given that Laslett’s earliest source on household structure dates from 1599. Then, he quotes me:

Steven Ruggles subsequently called the idea of extended households in the past *the extended family myth*, noting: ‘There are now few adherents to the myth that extended families predominated in the world we have lost’ (Ruggles 1987, 4).

The problem with this is not that Thornton misquoted me, but rather that he took my comment out of context. The point of my book was that Laslett and his followers found a superficial resemblance between families in the seventeenth and the twentieth centuries only because they used poor measurement strategies with poor data and ignored the confounding effects of demographic constraints on family structure. One of the book’s major conclusions was that most of the people in preindustrial England who could have lived in stem families were doing so.

In short, my book is a critique of Laslett and his fellow myth-bashers, and argues that there was a dramatic simplification of living arrangements in England and America beginning in the middle

years of the nineteenth century.⁸ Thornton, however, quotes me to support that very theory I am attacking, without even mentioning that I sharply disagree with it.

After quoting me and citing several additional actual supporters of the Laslett thesis, Thornton concludes that “there was, thus, apparently no evidence for the purported substantial decline in extended families in Northwest Europe during the centuries preceding the 1700s and 1800s” (Thornton 2005a: 84). There are two main problems with this statement. First, Thornton has not quoted anyone who purported a decline in extended families in the centuries preceding the 1700s and 1800s—Le Play was talking about a shift from stem families to unstable families in the late nineteenth century. Second, we simply do not know what happened in the “centuries before the 1700s and 1800s”—there are no household listings from before 1599, and just a scattering from the seventeenth century. I do not think it appropriate to quote my research in support of such undocumented generalizations.

Thornton goes on for several pages to defend his repeated use of the phrase “predominantly nuclear (or a weak stem) form” to describe the Northwest European family pattern in the past. I am not sure why he does this, since I did not express any objection to the phrase in my original critique of the book.

There are several other places in which Thornton misrepresents my arguments. To take one example, Thornton writes “Ruggles (1994) himself interpreted the strong correlation between education and living arrangements as representing the influence of ideational factors” (Thornton 2005a: 176). That paper, however, did not analyze education, and the only mention of education in the paper is my statement that “as life chances were increasingly determined by education instead of inheritance, the incentives for grown children to remain in their parents’ households would have diminished” (Ruggles 1994: 127). The meaning here is very close to the opposite of Thornton’s interpretation; I was stressing the role of *economic* incentives in the residence decisions of the younger generation.

⁸ One of the problems here is that there is some confusion about terminology. Thornton uses the term “extended family” to mean a very large household containing many married couples, including married aunts and uncles. This definition differs from the usual usage of historical demographers, and such households are exceedingly rare in both historical and contemporary populations around the world. Thornton argues that “scholars before the last half of the 1900s believed” that such households predominated in the past (Thornton 2006: 25), but I am highly skeptical about this, and Thornton provides no evidence.

Thornton defines stem households as units containing both married parents and a married child; under Laslett’s classification these would be a multiple family households, since they contain more than one conjugal family unit. Such households, as Thornton acknowledges, were necessarily fairly rare in pre-transition Northwestern Europe, since “high mortality and fertility and late marriage and childbearing combined to limit the number of families that were able to form them” (Thornton 2005a: 51).

It is vital to remember that just because the crude percentage of households containing both married parents and married children was low, that does not mean that stem families were rare. Rather, it means that stem families are difficult to observe in a census cross section, since stem families often went through just a brief phase as a multiple family household. Indeed, in many—probably most—stem families, at least one parent would be dead before the marriage of the heir. We must remember, as Berkner (1972) pointed out, that the stem family as defined by Le Play is not a particular household configuration but rather a process by which families and farms are perpetuated from generation to generation.

As I indicated in my presentation, Thornton's consistent misreading of my own work casts serious doubt on his ability to interpret sources. My meaning may sometimes be obscure owing to poor writing, but I doubt it is harder to understand than that of seventeenth and eighteenth-century moral philosophers. If Thornton cannot understand the argument of an article I wrote in 1994, should we believe him when he cites 18th-century authorities to support his revisionist intellectual history, without even providing quotations and page numbers to document his interpretation?

Daniel Scott Smith (p. 30). Daniel Scott Smith, perhaps the most respected historical demographer in the United States, has argued that the idea of a transition from extended to nuclear families was never a significant theme of scholars or social scientists before the mid-twentieth century, and even then, it was no more than a minor theme.

Thornton does not engage Smith's central contention. If the idea of a shift from extended to nuclear families was really as pervasive and powerful as Thornton claims, it would be a simple matter to refute Smith by providing a few clear quotations of eighteenth or nineteenth century theorists talking about this famous transition. Thornton, however, does not have any quotations, and he does not have any citations on this issue except for those of Adam Smith and Le Play discussed above.

In his response to my critique, Thornton does not address the issue any more thoroughly than he did in *Reading History Sideways*; he merely states that "Smith showed no awareness of the use of reading history sideways methods in the past" (Thornton 2006: 30), the same point Thornton made in the book. This, however, is a red herring; Smith was not writing about how theorists derived their theories, and there was no reason for him to have discussed the comparative method. If Smith is right—and I believe he is—and the shift from extended to nuclear families was not a significant theme before the twentieth century, then reading history sideways is irrelevant to his argument.

Thornton also points out that Smith disagrees with me on the issue of neolocal marriage. This is sad but true. Smith believes neolocal marriage was universal in some parts of Europe, whereas I argue that it was only universal among non-heirs. One day, I hope to persuade Smith of the wisdom of my interpretation. I do not see how this is relevant, however, to the issue I raised in my critique of the *Reading History Sideways*; just because Smith is slightly misguided on one issue does not mean that he is wrong to say that the myth of the extended family was a straw man invented by the myth-busters themselves.

Influence of developmental thinking on family change (pp. 30-31). For many years, Thornton has persuasively argued for an independent role for culture in demographic and family change. But Thornton now goes much farther: he is also arguing that an incorrect social theory developed hundreds of years ago using flawed methods is now a key influence on social behavior throughout the world. In my SSHA presentation, I expressed skepticism about this. In his response, Thornton reiterates his confidence in the extraordinary power and influence of social theorists, but I remain unconvinced.

Citations. (pp. 32-33). There was one point upon which all the panelists at the SSHA session agreed: Thornton's documentation of his research is inadequate. Etienne van de Walle, for example, commented that Thornton's approach will "set the field back by decades" because graduate students will think that Thornton's approach is an acceptable way to document primary sources.

I pointed to two specific problems. First, Thornton almost never quotes his sources, and rarely even attributes a specific idea to a specific author. Second, he almost never gives page numbers, but instead cites entire volumes or even multivolume works.

In many cases, Thornton makes large generalizations about the history of thought and then cites a dozen books spanning several centuries in one large footnote. On pages 61-62, Thornton states that scholars "transformed geographic differences that they observed into a series of developmental sequences of changes in family patterns," and then tells the reader to "see" a list of sixteen books by fourteen eighteenth and nineteenth century authorities, with no page numbers and no indication of which source said what. Did each of these authors explicitly use the reading history sideways methodology to infer a sequence of changes in family patterns? Or did some of them merely contribute to the reading history sideways method? Not only does this make it impossible to check Thornton's interpretation of specific passages, this approach makes it impossible to determine who is alleged to have contributed what idea. This casual approach to the evidence undermines the credibility of the argument.

Each of the panelists spent considerable time discussing the problem, but we must all have been unclear: Thornton completely missed our point. In his response, Thornton explains at great length the social science system of citation and his method for indicating the original publication date, as if that were the source of our complaint. Let me be clear: the particular citation style is irrelevant. What we need is sufficient information to locate the specific passage used to support the specific point.

Conclusion. I first encountered Thornton's theory listening to his 2001 presidential address to the Population Association of America (PAA). In the speech, Thornton expressed the view that there was universal acceptance among family historians that Laslett was correct, and that there had been little change in the Western family for hundreds of years. This finding, Thornton maintained, had overturned a powerful myth of transition from extended to nuclear families. At the reception following the speech, I buttonholed Thornton and explained that not all historians of the family agree with Laslett, and that many of us argue that his findings were misleading. I also told Thornton about Daniel Scott Smith, and his argument that the theory of a shift from extended to nuclear family structure was insignificant before the twentieth century.

Subsequently, Thornton and I corresponded by email, and we met to discuss the issue at the 2002 PAA meeting. I expressed my view that Smith was highly persuasive, and that to disprove his interpretation it would be necessary to have overwhelming documentation. Thornton did modify his argument slightly, but he did not change the central tenets; nor did he provide any evidence that would cast doubt on Smith's view of theories of family change.

Thornton has contributed enormously to the study of the family, and I greatly admire his work. He has persistently reminded us that ideational factors can affect behavior, and this work has been an essential corrective to the field. He is not, however, expert in the methods and approaches of intellectual history, and I doubt that his foray into this field will be counted among his great successes.

Appendix:
References to pre-1850 sources on pages 61-72 of
Reading History Sideways

On pages 61-62, Thornton states that scholars “transformed geographic differences that they observed into a series of developmental sequences of changes in family patterns,” and then tells the reader to “see” a list of sixteen books by fourteen eighteenth and nineteenth century authorities, with no page numbers and no indication of which source said what.

On page 62, Thornton makes the generalization that

[The family scholarship of the 1700s through the early 1900s] suggested that, in the Northwest European Past, societies were overwhelmingly organized around family and kinship relations, that they were familistic rather than individualistic, that they were relatively undifferentiated (i.e., contained few nonfamily institutions), and that they were characterized by a household structure that was extended rather than nuclear.

There is no footnote here, so we cannot tell which of the scholars from the 18th century felt this way.

On page 63, Thornton cites eight authors who wrote before 1850, but only in support of his generalization that they offered developmental interpretations of political institutions.

On page 64 Thornton makes his first comment that ties a specific family change to a specific pre-1850 author. Referring to Malthus, he writes “On comparing these two groups of societies (the less advanced and the more advanced), he interpreted the differences in marriage patterns that he found as the natural product of societal development (Malthus 1986b/1803).

On page 65, Thornton cites Malthus (along with LePlay and Westermarck), saying that he used historical evidence as well as comparative evidence. He also cites four pre-1850 authors that apparently mentioned increases in nonfamily institutions, “such as schools, businesses, and government agencies,” but evidently did not talk about families.

On page 67, Thornton writes “With one possible exception, all the family transitions in this chapter had been reported at least by 1803 in the work of such scholars as Millar, Smith, Alexander, Robertson, and Malthus . . .” (no footnote or citations).

The exception is the transition from extended to nuclear families, for which Thornton could not find “definitive” evidence before LePlay in 1855.

On p. 68. the second specific attribution of a specific family change to a specific pre-1850 author: Adam Smith made a “brief comment” which “suggested that in the past,

married children lived with their parents while, at the present time, they lived alone.” There is no quotation or page number, so the reader interested in evaluating the comment is out of luck. Thornton goes on to say that Smith also stated that in pastoral societies, distant relatives live in the same neighborhood, and that this was also true among the Scottish highlanders in the past. Finally, Smith “went on to indicate that adult siblings usually establish separate households.” Thornton concludes that because Smith’s suggestion that married siblings and cousins seldom live together was “consistent with the implication that the transition to nuclear families had occurred by the 1700s.”

On p. 68, Thornton writes that

the Northwest European Marriage pattern documented by Malthus (1986b/1803) is intricately linked with a nuclear- or (at most a stem-family household system. This suggests that Malthus understood that, at the time of his writing, Northwest Europe had a nuclear (or stem) household system.

The Northwest European marriage pattern is only “intricately linked” with a nuclear or stem family household system in the theories of some late-twentieth century social historians. The logic seems to go like this:

1. Malthus said Europeans married late.
2. Hajnal (or MacFarlane or Laslett or whoever) said European late marriage was tied to nuclear family structure.
3. Therefore, Malthus must have had a theory of a transition from extended to nuclear families.

On page 69, he notes that Montaigne suggests that parents should live apart from children, and Locke assumed that many children grow up and live separately from their parents. Thornton makes no suggestion, however, that they were talking about any change over time.

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