Uszkalo's focus on the possessed rarely allows much insight into the impact on the accused witches. She does mention how the sources attribute supernatural causation to possession, such as refusing charity to an alleged witch or an imagined encounter with the devil in the shape of man or magical beast. Yet she does not really distinguish between "bewitchment" and "bedevilment." The former often sought a human to blame, while the latter could vaguely hold "evil" responsible. Instead, she interchangeably concentrates on those performative behaviors which those kinds of possessions shared in common.

The possessed do not remain in the past. Uszkalo's conclusion offers a final example of relevancy concerning a relatively recent group event in upstate New York, although her one-page description does not adequately connect it to her subject. As for possessions in early modern England, however, Uszkalo presents some theoretical suggestions for understanding what was really happening. She has drawn on many good accounts of possession sources, including a few unpublished narratives. Uszkalo's models of performance and neuroscience offer intriguing viewpoints grounded in physical reality to understand encounters with supposed witches and demons.

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Stephen Broadberry, Bruce M. S. Campbell, Alexander Klein, Mark Overton, and Bas van Leeuwen. *British Economic Growth, 1270–1870*. Cambridge: Cambridge University Press, 2015. Pp. 461. $39.99 (cloth).
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British Economic Growth, 1270–1870 is the culmination of an outstanding effort by Stephen Broadberry, Bruce M. S. Campbell, Alexander Klein, Mark Overton, and Bas van Leeuwen to reconstruct British historical national accounts over the very long run. The real per capita gross domestic product estimates that are the main outcome of the first half of the book illustrate the enormous improvement, over time, in real income and consequently standards of living. The most original insight is that the emergence of sustained growth was much more gradual than it was previously thought, and that it started around the mid-seventeenth century. The data also emphasize the historical nonlinearity of the long-term growth process. The second half of the book goes beyond this task and provides a magisterial overview of what we currently know about consumption practices, distribution, labor productivity, and comparative income levels relative to other countries in this period. (It is truly difficult to do justice to this landmark publication in a short review; for a more in-depth discussion, see the forthcoming working paper: Nuno Palma, *Book review of "British Economic Growth, 1270–1870*.")

In addition to their own new data, the authors rely on a tremendous wealth of secondary source information produced by generations of economic historians, the equivalent of which is simply not available for other continental countries, with the notable exception of the Netherlands. What are the main new conclusions that result from this impressive exercise? The authors argue that the economy grew substantially during the early modern period, especially after 1650. This position stands in sharp contrast to that of Gregory Clark, *A Farewell to Alms: A Brief Economic History of the World: A Brief Economic History of the World* (2007), who argues that the English economy was trapped at an approximately constant (nonphysiological) "subsistence" level until it finally broke away during the nineteenth century.

Two aspects of their findings are especially striking. First, real income per capita approximately quadrupled between 1270 and 1870, but this growth was far from uniform over time: "not much" happened from approximately 1380 to 1650. This finding is perhaps a

	England/GB	Holland	Germany	France	Italy	Spain	Sweden	Portugal
1500	39	37	49	50	68	50	-	58
1550	39	37	-	-	64	54	35	30
1600	37	68	34	50	60	53	36	44
1650	34	69	-	-	62	41	-	51
1700	55	54	40	54	65	48	53	45
1750	61	60	45	55	68	46	41	59
1800	75	67	42	56	60	54	40	50
1850	100	79	61	78	66	64	52	46

Figure 1—Output per capita in Europe (GB 1850 = 100), using the 1850 benchmarks of Prados de la Escosura (page 24), and assuming Italy's relative level was constant 1850–1860. For details and the country-specific sources, see Table 1 in the above referenced forthcoming discussion from Palma.

little surprising in light of the fact that the authors also argue for significant levels of structural change between 1520 and 1650. (Perhaps as long as surplus labor was still available in the countryside, incomes did not need to rise in the cities?) For the following period, the authors show that premodern growth was fastest in the period after the Civil War, but preceding the Glorious Revolution. Indeed, real per capita growth was faster then than during the "classical" period of the industrial revolution, 1760–1820. These new findings, if confirmed, present a considerably new picture of the early modern period.

In one of the last chapters, the authors provide a set of international comparisons in Geary-Khamis "international" dollars of 1990 (374–75). (For a critical discussion concerning the Geary-Khamis construction method, see pages 3–4 of Leandro Prados de la Escosura, "International Comparisons of Real Product, 1820–1990: An Alternative Data Set, *Explorations in Economic History* 37, no. 1 [January 2000]: 1–41; and Angus Deaton and Alan Heston, "Understanding PPPs and PPP-Based National Accounts, *American Economic Journal: Macroeconomics* 2, no. 4 [October 2010]: 1–35.)"

Unlike the use of market exchange rates, in principle, using international dollars of a constant year allows for comparison of income levels across space and time. In practice, however, the devil is in the details. No proper purchasing power parities (PPPs) exist before the twentieth century, which means that backward projection using volume indices could easily lead to greatly compounded errors. As we move back in time, modern categories become less representative, leading to severe index number problems that the use of hedonic indexes may be able to mitigate but cannot credibly solve.

It would be unfair to the authors to aim much criticism at their usage of the Geary-Khamis international 1990 dollars method, which is standard in the literature and needed to be included. However, I would have preferred a more frank and thorough discussion of its limits, and in particular, it would have been useful to also show the results under the presently available alternative, the shortcut or indirect method of Prados de la Escosura cited above, especially in light of the fact that this method leads to a considerably different picture of comparative income levels for early modern Europe. Specifically, if this method is used, the early modern "little divergence" in incomes defended by Broadberry (*Accounting for the Great Divergence*, 2015) and others, largely disappears (Figure 1).

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No work is perfect, but this book—together with related research of Broadberry and others—represents the synthesis and culmination of the work of generations of economic historians of Britain and a significant research effort of the authors in their own right. It is the most important contribution made in the last quarter of a century toward establishing the facts that may one day permit answering that which is perhaps the most important question of all social science: Why are the societies we live in so much richer than it was the case in the past?

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ANDREW SNEDDON. *Witchcraft and Magic in Ireland*. Palgrave Historical Studies in Witchcraft and Magic. Basingstoke: Palgrave Macmillan, 2015. Pp. 221. $90.41 (cloth).
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Despite the last half century's vast growth of academic interest in early modern European witchcraft, the subject has attracted few commentators on aspects of the phenomenon in Ireland. Indeed, the last in-depth treatment of the subject is now more than a century old. Therefore Andrew Sneddon's *Witchcraft and Magic in Ireland*, a synopsis of recent work on Irish witchcraft, alongside his own original insights, is very timely. As he remarks in his opening comments, it is not difficult to see why so many have been reluctant to engage with the subject, the destruction of so many official records in 1922 making the historian's task hugely problematic. The lack of sources is, of course, a constant source of frustration for early modern historians of Ireland, but as eminent specialists like Toby Barnard and Raymond Gillespie have demonstrated, it is not an insuperable one. Sneddon's work is written in the same spirit, and he is to be congratulated on uncovering a whole range of new materials, many hitherto disregarded, with which to shed light on the nature of Irish witchcraft.

Sneddon's broad approach is to attempt to place Irish witchcraft within the context of developments within Europe generally at this time, particularly in as much as they impinged upon the formation of elite thinking on the subject. Clearly, a key factor here is religion. Early modern Ireland was deeply divided along religious lines, and Sneddon sensibly traces how the different denominations—Roman Catholic, Anglican Protestant, and Presbyterian—developed their own unique approaches and attitudes to the sin and crime of witchcraft. In doing so, he also acknowledges the importance of developments in England and Scotland, as settlers from both were increasingly influential in shaping the political, socioeconomic and cultural settlement of Ireland in the early modern period. Nowhere was this more important than in the establishment of a legal framework for the examination and punishment of crimes such as witchcraft, which, as Sneddon shows, were largely in line with English precedents. However, there is little doubt that the lack of surviving legal records makes the task of analyzing the impact of such developments upon official and popular attitudes to witchcraft almost impossible. Indeed, we only have reliable, in-depth information for two Irish witchcraft trials—at Youghal in 1661 and Islandmagee in 1711 (itself the subject of a monograph by Sneddon)—with only the scantest of references to other trials.

It is nonetheless fair to conclude, as Sneddon does here, that despite the fragmentary nature of the surviving evidence, Ireland did not experience sustained witch-hunting or witch trials in the early modern period. Why so? This is perhaps the single most important question to emerge from the book, and not surprisingly various explanations have been proposed. None fully convince, though I have a personal preference for Elwyn Lapoint's thesis, first propounded in 1992, that the Roman Catholic majority were largely unwilling to entrust the prosecution of witches in their midst to Protestant courts, which they saw as inimical to justice. Such