

exactly did the information nexus translate to modern economic growth? How important was information relative to other possible factors in the great divergence of the west? Another matter concerns the possible tension between open public access to information and private property rights in it. The book deals with patents only briefly. There is a provocative conclusion, however, on the future of capitalism in which Marks mulls over whether private interests are corrupting the information nexus. He calls for better state regulations. Each and every thorny issue in this sweeping and accessible book is worthy of future study.

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Walter Scheidel, *The great leveler. Violence and the history of inequality from the Stone Age to the twenty-first century* (Princeton: Princeton University Press, 2017. Pp. 528. 45 illus. ISBN 9781400884605 Hbk. £27.95/\$35)

This book forms an important and thought-provoking contribution to the academic and societal debate on inequality. Its author, Walter Scheidel, is originally a scholar of the economy and demography of the ancient world, but he now surveys the whole of history to track and explain the development of inequality. In doing so, he links up with a current trend of monographs devoted to specific economic issues, which are investigated by using history in its entirety, from the ancient past to the present, as a *Fundgrube* for empirical data, to illustrate a narrative or even as a kind of laboratory, to investigate causal relationships and test the related hypotheses (as with the works by Douglass North, Daron Acemoglu and James Robinson, and recently Branko Milanovic). This trend may reinvigorate interest in economic history outside the field itself. In particular, it may spark interest in the economic history of earlier periods, since this literature does not recognize hard barriers between modern and premodern periods, thus making premodern history equally relevant to understand present issues.

Scheidel's book is a great example of this trend and has the additional advantage that the author, trained as a historian, is able to give his account a profound historical depth and richness. A disadvantage of the book, however, is that it is organized in chapters that each discuss one possible mechanism of levelling, and each uses the whole of history, which makes the author jump from ancient Greece to the Spanish Civil War of 1936 to 1939 and causes some analyses to look too short and a little superficial. Also, there is a certain imbalance, as relatively ample text is devoted to classical antiquity on the one hand and to the twentieth century on the other, while the millennium-spanning middle ages are less discussed and often treated only in passing, which may have led to a certain bias in the conclusions, as will be discussed further below.

The thesis of *The great leveler* is rather straightforward. High levels of inequality are the norm in history and can be reduced only as a result of catastrophic events, including state collapse, massive plague, violent revolutions, and, most particularly, mass-mobilization warfare. Scheidel claims there are no peaceful means to reduce inequality, and that this holds for all historical periods, including the present. At first, a historian may feel a certain unease about this claim, as this would then hold for very different societal and institutional contexts, which seems unlikely. Still, Scheidel makes a convincing case, first, by showing that he is aware of the role institutional arrangements can play: a slight one in his account, but at least it is not a blind spot. Second, and more importantly, he convinces the reader by adducing ample and compelling evidence of catastrophic events from all of history that reduce inequality, and of peaceful attempts that are unable to do so.

A few methodological improvements could have lent the book more sharpness in its analyses. Distinguishing more clearly and systematically between wealth inequality and income inequality, for instance, could have made the analysis more rigorous, as the effects of catastrophes on each type of inequality may have been different. Likewise, I noted the lack of a clear distinction between economic and political means of accumulation and how catastrophes affected these in different ways. Also, I would have welcomed an approach starting from the opposite direction: finding situations of relatively low wealth or income inequality and then seeing whether these were all the result of catastrophes. Perhaps this would have pointed to other mechanisms than catastrophes alone and would have yielded a slightly more optimistic story. These three methodological omissions, however, do not substantially harm the book and its conclusions.

There are two other, more fundamental issues where I hesitate. One of them concerns the almost exclusive focus of the book on the state level, and on the wars, revolts, and collapses taking place at this level. The book thus largely leaves out the mechanisms at the local, non-state level and the periods in which central states were weak and did not carry much weight at the local level, such as most particularly western Europe in the high and late middle ages. This was the period in which town communities, villages, guilds, and commons were largely responsible for organizing economic life at the local level and often acted in order to reduce inequality. They did not do so by way of taxation—which often was regressive—but through restrictions on the transfer and accumulation of land and capital goods, imposing maximums on production, periodic redistribution of usage rights, and so on. That they were able to do so was the result of a lengthy process of self-organization of ordinary people, a process that included strikes and revolts, but also more peaceful forms of collective action. In the thirteenth to fifteenth centuries, they thus contained inequality at a relatively low level; the statements on high inequality in this period (p. 90) are largely unfounded, not representative of the general situation, and mainly based on one unpublished paper. A fuller assessment would have remedied the omission of not considering the possibility that indirect, largely non-violent actions at the local level were indeed capable of reducing inequality.

My other reservation concerns the treatment of the twentieth century. The relatively low levels of income and wealth inequality during most of this century are, again, largely explained by the mass-mobilization warfare of the two world wars and their indirect effects. Although there is a clear link between the reduction of inequality and the war efforts, the argument is perhaps carried a little too far. First, the reduction of inequality and the direct and indirect measures to this end seem to have started, albeit hesitantly, before 1914, that is, before the First World War. Second, as sometimes acknowledged by Scheidel, the effect of these wars took place in a specific context characterized by the rise of workers' organizations and left-wing political movements that had already started in the last decades of the nineteenth century. Also, the reduction in inequality was sustained for some decades after the Second World War, particularly as measures were embedded in legislation and the build-up of social welfare states. Again, more peaceful factors and their institutional effects probably played a larger role than acknowledged in the book.

It is possible that both of my reservations relate to differences in emphasis than fundamental issues. The book is engaging, thought-provoking, and largely convincing in constructing a link between catastrophes and the reduction of inequality. The book also points to the possibility that the low levels of inequality of the twentieth century were an exception and a situation that will not be easy to preserve or to regain.

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