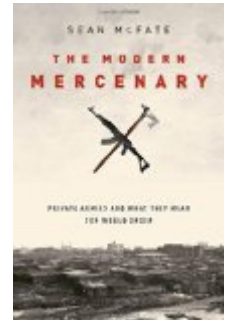


Sean McFate. *The Modern Mercenary: Private Armies and What They Mean for World Order.* New York: Oxford University Press, 2015. 272 pp. \$29.95, cloth, ISBN 978-0-19-936010-9.



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The last fifteen years have seen an explosion of debate and literature on the topic of privatized military force. Peter Singer's landmark book *Corporate Warriors: The Rise of Privatized Military Industry* (2003), excited both scholarly and popular interest in the issue, and many works have followed. Some have been autobiographical, some exercises in self-justification, some polemic, and some have attempted to apply scholarly rigor to a particularly opaque world. It has been difficult to advance our general understanding of the phenomenon of private military force, however, largely because neither the US government nor academics have access to data, such as the names and backgrounds of people employed by private military companies (PMCs), whether or not they misbehave, and how many of them are killed or injured. As Sean McFate points out, even Congress is generally not allowed to see the contracts the government makes with PMCs.

Sean McFate's ambitious book, *The Modern Mercenary*, aims to both contextualize and explain the phenomenon of the private market for

force, and to do so he reaches back to the late medieval period. The book thus attempts to cover about a thousand years of history, and ostensibly to produce a new theory of international relations that will more accurately describe what McFate sees as a post-Westphalian world. The chapters on modern PMCs are valuable both as a synthesis of current debates and as a detailed look at how the private market for force functions. The quasi-historical and theoretical chapters are weaker.

The book combines the author's insights from his personal experience in the industry with his ideas about why, historically, we are seeing the rapid expansion of the private market for force, and what its implications for international security might be. Because of this somewhat reflective nature, there is no particular methodology followed. For that reason, it may not appeal to social scientists looking for an empirical test of the author's theory. The book is interesting and well written, but should be read along with other works in order to get the most accurate historical picture. For example, David Parrott's *The Business*

of War (2012), Frank Tallett and D. J. B. Trim's *European Warfare, 1350-1750* (2010), Peter Reid's *Medieval Warfare* (2007), and Sally Paine and Bruce Elleman's chapters on the Opium Wars in *Modern China: Continuity and Change 1644 to the Present* (2009) would all be valuable historical context.

In the chapters that draw on his own personal experience working for DynCorp--particularly his experience in Liberia--McFate's discussions are detailed, interesting, and well sourced. In the more historical and theoretical chapters, there is a deficiency of sourcing and citation. While the sources used are appropriate, there is a great deal of assertion that demands more citation, and there are some inaccuracies. For example, at one point McFate refers to Carl von Clausewitz as "the great seventeenth-century Prussian war theorist" (p. 65--Clausewitz was born in the late eighteenth century and wrote his masterpiece following the Napoleonic Wars), although this error is not repeated. In addition, McFate frequently refers to the pope as a nonstate actor (see, for example, pp. 67, 72), despite the fact that from the late 700s to the late 1800s, many popes were temporal rulers with armies and territories appropriately named the Papal States.

Much of the discussion of history is simplified and truncated to fit into the narrative. For example, the main argument McFate makes about the international system is that the current system looks a great deal like the medieval system, both of which look very different from the intervening "Westphalian" system. However, while there is a significant amount of text devoted to discussing the purported similarities between the current system and the medieval system, there is almost no discussion of the intervening period, which McFate alleges functioned according to the ideal-type of state supremacy, sovereignty, and monopoly of force. This is a difficulty for McFate's argument, as the period between 1648 and 1945 most certainly did not function that way (see, for exam-

ple, the books cited above, and the examples below).

The argument that the Westphalian order eliminated the phenomenon of overlapping sovereignty or authority is easily undermined by a brief glance through the various empires and confederations characterizing Europe during that period, not to mention the rest of the world, where states were neither ubiquitous nor necessarily the most important actors. Indeed, the entire concept of federalism is one of overlapping sovereignty. War between states was certainly an important issue, but nonstate warfare did not disappear. For example, there were the Indian wars in the Americas, the wars of eastward expansion in Eurasia, the wars of rebellion under the British by the Irish, the Zulu, the Sepoy, the Boer, and others (some of which the British Empire lost), as well as the various civil wars in well-established states. States were certainly important actors, but they were not the only important actors: the Opium Wars in China were started not by states, but by merchants ignoring the sovereignty of the Chinese emperor; Sir James Brooke, a British adventurer, simply established himself as king of Borneo, with no connection to the British Empire; and the British and Dutch East and West India Companies operated for decades, if not centuries, essentially independently of their governments' control. Not all rebellious or terrorist actors aspired to statehood, either: nineteenth-century terrorism in Europe--particularly Russia--was mainly anarchist in character and did not aim at establishing or taking over a state. To characterize the Westphalian period as having cleaned up and solved all the messiness of the medieval period is thus problematic, to say the least.

McFate is also inconsistent in identifying which period constitutes the "Westphalian" era--he sometimes refers to the entire period since 1648 (see, for example, p. 167), and other times suggests that only the nineteenth and twentieth centuries represent the Westphalian ideal (see, for

example, pp. 102, 136, 166). While there are of course important differences in all these periods, there are also continuities, and while McFate is doing a service in drawing out the parallels between the modern and medieval markets for force, it is misleading to imply that there was ever a time when states were the only important actors and had a monopoly on the use of force (see, for example, pp. 6, 28f., 65, 68ff., 82f., 91f.).

McFate draws a distinction between what he calls “mercenaries,” or those private military/security companies that provide actual trigger-pullers, and “military enterprisers,” or those companies that recruit, organize, and train military or police forces for another actor (p. 14 and chapters 10-11). This is, however, a difficult distinction to maintain. As McFate himself points out, any company that can do the latter can probably do the former, with many companies doing both. Most companies (and their employers) also claim a significant distinction between trigger-pullers who are doing guard or security work, and trigger-pullers who are engaging in combat. Even the historical example McFate uses to illustrate the distinction he wants to make is not particularly helpful: he refers to “the tradition of military enterprisers such as [Albrecht von] Wallenstein, who built military forces rather than using them” (p. 158). But this is not accurate; Wallenstein did indeed recruit and organize massive forces (up to around 58,000 men at one point—see Parrott, *The Business of War*, p. 117), but he also commanded them in the field. He was not hired to recruit and train imperial troops; he was hired to recruit and train his own army, and then command it in the service of his employer. This is far closer to what McFate calls “mercenaries” than to what he is calling “military enterprisers.” Indeed, it appears that an important component for McFate’s definition of “military enterprisers” in the twentieth and twenty-first centuries is that they are raising police and military forces *for a state*.

McFate distinguishes “mercenaries” and “military enterprisers” because his prescriptive argument is that it is important to shape the market for force to encourage the latter and discourage the former. He argues that neither states nor the international community have been successful in producing regulations to rein in the damage that a free market for force can create, so the best alternative is to manipulate market incentives to maintain a “mediated” market for force. By “mediated,” he appears to mean a market in which states and other responsible nonstate actors such as the United Nations (UN) hire vetted, responsible PMCs to raise and train military and police forces *for state actors*, which can then enforce order. This, at least, is the implication of the fascinating chapters on Liberia and Somalia. However, he also makes clear that he expects both state and nonstate actors to hire PMCs to conduct operations: he argues that the UN could make very good use of PMCs in humanitarian disaster or peace-keeping situations by, for example, having them create humanitarian safe zones for a period of weeks or months while the UN puts together a more permanent solution. How this differs from using “mercenaries” is not entirely clear, although McFate contends that the UN could and should develop a vetting and licensing regime that would simultaneously set standards for the industry and encourage good behavior in order to be eligible for contracts.

What McFate does not discuss is the problem that, regardless of whether large consumers such as the United States and potentially the UN create licensing regimes, without significant, enforceable, and enforced regulation, the market for the less savory firms engaged in less savory behavior will not go away. The market will simply differentiate, as it already does, into those firms that want to go for the big contracts with the big, legitimate employers, and those firms that are perfectly happy to serve whoever wants to pay them, be it warlords in Somalia or Afghanistan, or shipping companies, or smaller countries trying to resist bigger

countries, etc. So long as there is both a demand for applied force and a supply of skilled labor, the market will bring those things together. Indeed, if one does not find McFate's argument about the distinction between enterprisers and mercenaries convincing, the book's message becomes even bleaker. It then implies that the market will create its own demand, allowing low-level violence to undermine weaker state actors, and increasing costs for trade and travel.

While the arguments about neomedievalism or the options for shaping the market may not be convincing, the chapters on contracting, Liberia, and Somalia are interesting as primary source accounts. Not many of the books currently available go into such detail on the contracting process or provide such finely grained accounts of how various contractor missions have played out. These chapters are valuable to anyone interested in either military and security contracting in particular, or issues of national defense policy and international security more broadly, and would be appropriate for advanced undergraduate- and graduate-level classes on those topics. McFate's point that the United States missed an historic opportunity to shape the industry is certainly on target, and serves as a sobering contribution to the ongoing debates about the costs and benefits of privatized military force.

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