
INTRODUCTION

Beyond the Tilly Thesis

*How States Did Not Make War and War
Did Not Make States*



Writing some twenty years ago, Gerald Helman and Steven Ratner decried “a disturbing new phenomenon”: “the failed nation-state” characterized by “civil strife, government breakdown, and economic privation” (Helman and Ratner 1992, 3). In truth, it is the “successful nation-state” that is really new, but the “failed states” concept caught on quickly. In the intervening years, literally dozens of studies have been published on “failed states” in Africa (Bates 2008), Latin America (Taylor and Center for Naval Warfare Studies [U.S.] 2004), central Europe (Sörensen 2009) and the Middle East (Mandaville 2007) in multiple languages (Bock, Bühler-Thierry, and Alexandre 2008; Weiss and Schmierer 2007) and by authors from across the political spectrum (Chomsky 2006). In 2005, *Foreign Affairs* even began publishing a “failed states index,” an annual ranking of all members of the United Nations sorted into five discrete categories: “critical,” “in danger,” “borderline,” “stable” and “most stable.” Interestingly, the last category is exclusively populated by small European states (e.g., Sweden and Belgium) and settler colonies (e.g., New Zealand and Australia). Meanwhile, no Western states appear in the bottom three categories.

Why are so many of the “strong states” European states or their offspring? Today, the canonical answer in historical sociology, comparative politics and international relations is what is colloquially known as the “Tilly thesis”: “war makes the state, and the state makes war” (Tilly 1990). On this reading, the strong states of modern Europe (Bean 1973; Downing 1992; Finer 1975; Hale 1985) and also of contemporary China (Hui 2005) are the evolutionary offspring of a Darwinian struggle between predatory rulers questing after

power and resources. Conversely, the weakness of the state in Latin America and Africa is mainly due to the historically low levels of geopolitical competition there (Centeno 2002; Herbst 1990, 2000). By now, this war-centric or bellicist account of state formation has migrated out of academic discourse and into the received wisdom of the chattering classes. So much so, in fact, that some commentators now argue that the best remedy for “state failure” is to “give war a chance,” to simply allow the Darwinian logic to play itself without any outside “interference” (Luttwak 1999).

Should hard-headed policymakers heed this tough-love line? They should only if a strong version of the Tilly thesis actually holds for western Europe. Unfortunately for Tilly—and fortunately for Africa—it does not. Or so I argue in the first six sections of this introductory chapter. In the first section, I retrace the prehistory of the Tilly thesis, from its initial formulation by the military historian Michael Roberts through its gradual unraveling in the course of the “military revolution debate.” In the second section, I examine its emigration into comparative social science via Perry Anderson’s (1974) seminal study of the “absolutist state,” where it was gradually qualified and refined beyond recognition. In the third section, I turn to its period of extra-European exile, where it finally assumes its current moniker—“the Tilly thesis”—but only after shedding all manner of qualification and nuance. In the fourth section, I argue that the central problem with the war-centric approach to state formation is that it rests on a wholly inadequate theory of war; I then propose a neo-Clausewitzian approach that reintegrates war into politics and politics with religion. In the fifth section, I apply this approach to the early modern period, showing how it resolves certain persistent conundrums in historical sociology, comparative politics, and international relations. Finally, in the sixth section, I argue that the bellicist model operates with an inadequate model of the state itself, which blinds us to other important effects that the reformation movements had on early modern polities. In the conclusion, finally, I seek to give war its due—but *only* its due.

THE “MILITARY REVOLUTION” THESIS: THE CAREER OF AN ARGUMENT

The Military Revolution Debate. Of course, it would be wrong to lay all of the blame for the Tilly thesis on Tilly himself, since his argument is really just a lightly modified, second-order derivative of Michael Roberts’s well-known “military revolution” thesis (Roberts 1967) and its various offshoots (Bean 1973; Downing 1992; Duffy 1980; Hale 1985; Mann 1986; Parker 1996).¹ In his much-cited inaugural address at Queen’s University in 1953, Roberts had argued that a combination of tactical and technological innovations—namely, the use of line infantry equipped with gunpowder weapons, sparked

a veritable “military revolution” that transformed early modern geopolitics, politics, and society. New tactics, he argued, led to an arms race and intensified warfare, which led in turn to a rapid expansion of the fiscal and administrative capacities of the early modern state. The central problem with this account is that the putative causes (i.e., gunpowder and infantry) antedated their supposed effects (i.e., intensified military conflict and sociopolitical change) by a century or more (Ayton and Price 1995; Black 1991; Rogers 1995) and, indeed, that the supposed effects (e.g., administrative rationalization) often failed to materialize (on this, more below).

Still, the Roberts thesis does identify an important correlation between military conflict and political change in early modern Europe. One of the central arguments of this chapter (and the red thread that runs through the chapters that follow) is that *this correlation is largely spurious*, that the observed relationship is, in fact, the product of a third, “unobserved factor”—namely, the “Protestant Reformation” or, more broadly, confessional conflict. The fracturing of Latin Christendom and the emergence of rival churches or confessions—Catholic, Lutheran, and Calvinist or Reformed—unleashed a long period of total war on the European continent, as had not been seen on such a scale since Roman times, even if more localized conflicts against revolutionaries and heretics were every bit as intense. At the same time, it brought about a fundamental transformation of state, church, and society and the relations between them, laying the groundwork for the contemporary nation-state, strong, sovereign, and secular. The “military revolution,” in short, was more the effect than the cause of these developments. But before developing this argument at greater length, we must first give the bellicists their due, beginning with Roberts himself.

In the Germanic lands, military history was always tightly integrated with social, political, and even religious history right from the outset (Clausewitz, Howard, and Paret 1984; Delbrück 1975; Hintze 1975; Sombart 1975). Despite the fact that Scottish Enlightenment writers were probably the first to think systematically about the impact of war and warfare on politics and society (Ferguson and Oz-Salzberger 1996; Smith, Skinner, and ebrary Inc. 1999), in the anglophone world, military history has been more closely aligned with the history of science and technology (Barton 1994). Michael Roberts was an English-born Swedish historian, and his military revolution thesis may be seen partly as an effort to bridge these two traditions: His argument, remember, was that technical innovations were the engine of sociopolitical transformations. Roberts’s style is not particularly systematic, and his essay on the military revolution is extremely wide ranging, but there is a central line of causal argument in his essay that runs essentially as follows: The military revolution was sparked by (1) *tactical and technical innovation*: the replacement of armored knights by line infantry equipped with firearms.

This greatly decreases the unit costs per man and leads to (2) *strategic innovation*: larger and bolder campaigns. This greatly increases the incidence and intensity of warfare and gradually stimulates (3) *military expansion and innovation*: larger units, more drilling and standing armies. This greatly increases the overall level of military expenditures driving (4) *state expansion and rationalization*: a larger, more centralized and more bureaucratized state. The time frame of the story is 1560–1660; its principal protagonists are Maurice of Orange—the tactical innovator—and Gustavus Adolphus—the strategic entrepreneur. In short, Roberts is a classical realist in the lineage of Carr and Morgenthau (Carr and Cox 2001; Morgenthau and Thompson 1985): He assumes that rulers are naturally power hungry and, consequently, that states are naturally bellicose.

Despite its immediate and enormous influence, it was nearly two decades before the Roberts thesis was first subjected to serious scrutiny. In another widely cited article, Geoffrey Parker, also an English military historian, asked whether the military revolution was a “myth” (Parker 1976). He ultimately concluded in the negative but greatly revised Roberts’s story in the process. Already the author of several well-received books and articles on the Eighty Years’ War and early modern war finance (Parker 1970, 1972a, 1972b, 1973), Parker was able to hold Roberts’s claims up against a broader and deeper canvas and so to spot several important errors and exaggerations. One concerns the time frame. Many of the innovations that Roberts identifies occurred long before the sixteenth century, in Spain, Italy, France, and even England. As Parker rightly notes, “Wherever a situation of permanent or semipermanent war existed, whether the Hundred Years War of the later Middle Ages or the Thirty Years War of the seventeenth century, one finds . . . standing armies, greater professionalism . . . , improvements in military organization, and certain tactical innovations” (Parker 1976, 201). In other words, *it is intensified conflict that causes technical innovation*, rather than the other way around. What is more, technical innovations were not diffused immediately or automatically. For example, the tactical superiority of ranked pikemen over mounted knights was quite clear by the mid- to late fifteenth century, yet mounted knights would continue to dominate many armies for another half century or more (Parker 1976, 207). So, the first link in Roberts’s causal chain was clearly defective. The last one proves weak as well. While it is quite true that military budgets ballooned during the early modern era (Bonney 1981, 1999; Bonney and European Science Foundation 1995), the fiscal demands of large-scale warfare were often as not met either with short-term expedients (e.g., the sale of Crown lands or state offices) (Desert 1984; Parrott 2001; Rowlands 2002; Thompson 1976; Wood 1996) or solved through non-state means (e.g., tax farming or private finance) (Caruthers 1996; Kiser 1994; Tracy 1985). Thus, Parker does not make much

of Roberts's claims about administrative rationalization. In his book-length treatment of the military revolution, first published thirteen years later, Parker also did severe damage to the second link as well. While it is true that the size of campaigns grew, at least in absolute terms, Parker contends, early modern warfare consisted mainly of sieges against urban centers, not pitched struggles on the battlefield (Parker 1996). All that really remained of Roberts's venerable thesis at this point was the third link: the expansion of force size and the creation of standing armies.

In light of these findings, one might expect that Parker would have turned toward a more fully contextualized argument in the German tradition—a neo-Clausewitzian argument concerning political stakes, say, or a neo-Hintzian argument concerning social barriers to military innovation. But he did not; instead, he doubled down on technical determinism. The real catalyst for the military revolution, he asserted, was the introduction of gunpowder artillery, which rendered vertical stone fortifications useless, leading in turn to an epidemic of predatory warfare and, later, to the construction of costly new earthen defenses (the so-called *trace italienne*). And the real effect of the military revolution, he continues, was not on the internal structures of European states so much as on the external balance of power with the rest of the world. “The key to the Westerners’ success in creating the first truly global empires between 1500 and 1750 depended upon precisely those improvements in the ability to wage war which have been termed ‘the military revolution’” (Parker 1996, 201). Thus, the real roots of the “rise of the West,” he claims, are not to be found in the democratic revolution of the eighteenth century or the industrial revolution of the nineteenth century, but rather in the military revolution of the sixteenth century (Cipolla 1965). In Parker’s reconstructed version of the military revolution thesis, to summarize, “gunpowder artillery” replaces “line infantry,” and “colonial expansion and domination” substitutes for “political expansion and innovation.” The time frame widened in both directions as well: Parker’s military revolution lasted from 1530 until 1710. Parker’s argument was soon taken up and elaborated by other historians as well (Duffy 1980; McNeill 1982).

Since the initial publication of *The Military Revolution* in 1988, Parker’s reconstructed version of the Roberts thesis has been subjected to a series of withering assaults on multiple fronts (Gunn, Grummitt, and Cools 2007, 2008). One attack has focused on Parker’s flanks. Thus, some historians have challenged his periodization of the military revolution, arguing that the early modern military revolution began earlier and/or ended later than even Parker had allowed (Black 1991, 1995; Prestwich 1996, 2003). Others have challenged the singularity of the military revolution, arguing instead that there have been two or more such revolutions (Ayton and Price 1995; Knox and Murray 2001). A second attack has concentrated on Parker’s center. Some

scholars have questioned and relativized Roberts's and Parker's claims about army expansion. For example, research by Parrott (1995) and Prestwich (1996) suggests that the actual (as opposed to claimed) size of field armies in the Hundred Years' War (1337–1453) and the Thirty Years' War (1618–1648) were roughly the same (i.e., around 30,000 men)—an extraordinary finding, given the two-fold demographic expansion that lay between the two wars. Others have challenged their claims about institutional rationalization. Thus, Colin Jones and Michael Prestwich have argued that the logistics of English supply in the latter decades of the Hundred Years' War was every bit the equal of French supply in the early decades of the seventeenth century (Jones 1995; Prestwich 1996). A third and final group has laid siege to the theoretical foundations of the Parker thesis—namely, technical determinism. For example, a quantitative analysis of the *longue durée* by Rasler and Thompson finds that periods of army expansion are associated with battles over geopolitical primacy rather than with instances of technical or tactical innovation and that the correlation between expansion and innovation is most likely spurious, with geopolitics probably driving both (Rasler and Thompson 1985; Thompson and Rasler 1999).

To say that the military revolution thesis does not hold is not to assert that no military change occurred. Gunpowder weapons and earthen fortifications became more common. So, of course, did siege warfare. Military drill for the infantry and military academies for officers took hold. In most places, the percentage of the population in arms rose, as did overall levels of military expenditure. The problem with the military revolution thesis is not the facts so much as the way they have been linked together into a causal chain in which technical and tactical changes drive the story. There is a much simpler and much more plausible way of explaining these changes. As Simon Adams puts it, “The emergence of the confessional issue meant that all major wars between 1559 and 1648 were to some extent religious civil wars. Political boundaries became of less relevance, popular participation (particularly in sieges of major cities) was far more extensive than in previous conflicts, and to a far greater degree the role of armies was that of crushing dissidents and controlling hostile populations” (Adams 1995, 262). In other words, the religious wars of the confessional era, like the Hundred Years' War before and the Napoleonic wars after them, were *total wars*, with significant involvement of civilians and a *Vernichtungsstrategie* toward the enemy. They were also *social wars*, not simply wars about the relative status of competing dynasties, but wars about the very basis of the social order. As Vivek Sharma puts it, they were wars about “rules,” not “rulers.” Confessional strife cannot be understood without attending to the *political stakes of religious reform*. Hardly a surprising claim, of course, and one that is addressed in more detail in the fourth section of this introduction, but a point that eluded military historians for half a century, nonetheless.

THE MILITARY REVOLUTION THESIS AND COMPARATIVE-HISTORICAL SOCIOLOGY

It would be difficult to overstate the influence of the military revolution thesis and of war-centric analysis more generally on contemporary historical sociology and comparative politics. Indeed, the reception of Roberts (and also of Hintze) during the mid-1970s marked a major turning point in these fields. Leading comparative-historical political sociologists of the 1960s, such as Reinhard Bendix (1964), Shmuel Eisenstadt (1963), and Barrington Moore (1966), did not even cite Roberts's famous essay in their well-known works on state development. Nor, for that matter did several early works of "second wave" historical sociology by Theda Skocpol (Skocpol 1979) and Charles Tilly (Tilly, Ardant, and Social Science Research Council [U.S.] Committee on Comparative Politics 1975) (though both did include appreciative, if highly selective, citations of Otto Hintze's writings). In fact, the first work to make systematic use of the Roberts thesis was Perry Anderson's *Lineages of the Absolutist State* (Anderson 1974), arguably the foundational work in the contemporary literature on state formation.

Anderson's argument is a neo-Marxist one. His central thesis is that the rise of absolutism in the polities of western Europe resulted from the "crisis of feudalism." In his view, this crisis had two underlying sources: the commutation of peasant dues and the rise of the urban bourgeoisie. These developments threatened the economic base and the political dominance of the landed nobility, respectively. In order to escape this deadly pincers movement, Anderson believes, Western aristocracies chose to throw themselves into the protective arms of the "new monarchs." For Anderson, then, Western absolutism was "a redeployed and recharged apparatus of feudal domination, designed to clamp the peasant masses back into their traditional social position" as well as a "coercive force capable of breaking or disciplining individuals *within* the nobility itself" (Anderson 1974, 18, 19–20). It was a political means for defending the *collective* interests of the noble class.

But what about eastern Europe, where there *was* no "crisis of feudalism," but more often a "manorial reaction" that reinstated peasant bondage? This is precisely the point where Anderson's argument leans on Roberts's. The "full emplacement of the Absolutist State" in western Europe, Anderson contends, went hand in hand with "the effects of the 'military revolution.' . . . Armies rapidly multiplied in size, becoming astronomically expensive in a series of ceaselessly expanding wars" (Anderson 1974, 51). And this ratcheted up the geopolitical pressure on the poorer states of eastern Europe. "The very modernization of troops and tactics brought about by the 'military revolution' in the West after 1560 rendered aggression into the vast spaces of the East more feasible than ever before. . . . Thus, at a time when infrastructural relations of production were diverging, there was a paradoxical conversion of super-

structures in the two zones” (Anderson 1974, 198). The immediate source of this pressure was Sweden, the “hammer” of Western absolutism. Still, there was a crucial difference in the political organization of the two absolutisms. Western absolutism was (supposedly) characterized by the sale of public offices, Eastern absolutism by the creation of state bureaucracies (Anderson 1974, 212). Why? Because the influence of the urban bourgeoisie in western Europe led to a creeping commercialization of public life, while the economic backwardness of eastern Europe forestalled such a development.

From the vantage point of contemporary scholarship, Anderson’s analysis now appears almost shockingly crude. The most obvious problem is that many early modern states were not absolute monarchies at all. Venice and Switzerland remained republics, and the Netherlands became one. England and Poland were constitutional monarchies and successfully fought off attempts to impose absolutist rule. Sweden, too, was a constitutional monarchy for much of the early modern period. Representative assemblies retained considerable power in many German principalities as well. Indeed, one could argue that there were really only a handful of absolute monarchies—Spain, France, Denmark, and Brandenburg-Prussia—and that even they did not become fully absolutist until the mid-seventeenth century at the very earliest. In short, the actual variations in regime type are not adequately captured by Anderson’s rough-and-ready distinction between East and West. Nor is this the only serious problem: The division between “venal” and “bureaucratic” forms of office holding does not line up with the division between western and eastern Europe or between developed and backward economies. Venality was much more pronounced in Spain and France than it was in England or Sweden, for example. And the bureaucratized administration of the Prussian state was more the exception than the rule in central Europe. Finally, one might add that the Swedish “hammer” that supposedly forged Eastern “absolutism” was not itself “absolutist” but “feudal” or, better, entrepreneurial.

Subsequent works on state formation would labor hard to theorize and account for these variations—even as they placed even greater weight on the impact of military mobilization. Consider Anderson’s colleague and compatriot, Michael Mann. In the first volume of his monumental study *The Sources of Social Power*, Mann opens his discussion of early modern state formation with a deft summary and synthesis of the Roberts and Parker theses (Mann 1986, 1: 453). He then goes on to argue that the divergence between “constitutionalist” and “absolutist” states was primarily the result of varying modes of war finance (Mann 1986, 1: 457). Constitutionalist states, such as England and Holland, “relied on taxation of both the landed and trading rich with their consent.” By contrast, “absolutist regimes relied on taxation of the landed poor with the consent and repressive help of the landed rich” (Mann 1986, 1: 479). Either way, the result was an “organic state” characterized by an “organic unity between state and dominant classes” (Mann 1986, 1: 515).

“The ‘modern state,’” Mann concluded, was “the product of the developments often called the Military Revolution” (Mann 1986, 1: 454).

But what were the underlying causes of the military revolution in this accounting? Mann explicitly eschews the “technological” and “military determinism” that underlies Roberts’s and Parker’s approaches, pointing instead—rather vaguely, it must be said—to “pressures emanating from the geopolitical and military spheres” (Mann 1986, 1: 454). Whence the pressure? Alas, Mann never really answers this question.² Ultimately, war remains an “exogenous” variable in his account of state formation.

In many regards, Tilly’s *Coercion, Capital and European States* simply restates Mann’s arguments in a slightly different language. Like Mann, Tilly argues that variations in early modern state structure were due mainly to different modes of war finance, while variations in war finance, he adds, were determined by the geographical distribution of material resources. In capital-rich polities, such as the Netherlands or northern Italy, for example, wars could be financed with a minimum of coercion by means of parliamentary levies and/or financial markets. In these cases, “rulers relied on compacts with capitalists . . . to rent or purchase military force, and thereby warred without building vast permanent state structures.” In capital-poor polities, such as Brandenburg-Prussia, by contrast, “rulers squeezed the means of war from their own populations and others they conquered, building massive structures of extraction in the process.” Finally, in other cases, such as Britain or France, where “capital” and “coercion” were both available, “rulers did some of each” (Tilly 1990, 30). Thus, where Mann distinguishes two main outcomes—constitutionalist and absolutist—Tilly inserts a third—“capitalized-coercion” or “national states”—that lies between them.

In at least one regard, however, Tilly’s argument diverges quite significantly from Mann’s and attributes an even greater weight to geopolitical competition. From 1500 onward, he argues, “the pressures of international competition (especially . . . war and preparation for war)” effected a gradual, pan-European convergence on the Anglo-French model of “capitalized-coercion” because it “proved more effective in war” (Tilly 1990, 31). In Tilly’s version of the fiscal-military model, then, war not only serves as the primary catalyst for state growth; it also functions as a *selection mechanism* that determines state form.³ How precisely does this selection occur? By means of predatory conquest, says Tilly: “The enormous majority of states that were around to bid for autonomy and strength in 1500 disappeared over the next few centuries, *smashed and absorbed by other states-in-the-making.*” And why do rulers engage in such conquests? Out of self-interest: “The central tragic fact is simple: coercion *works*; those who apply substantial force to their fellows get compliance, and from that compliance draw multiple advantages of money, goods, deference, access to pleasures denied to less powerful people” (Tilly 1990, 70).

There are two very serious problems with Tilly's theory of predatory conquest. First, as Vivek Sharma and I have shown elsewhere, the principal mechanisms of territorial expansion in medieval and early modern Europe were not military but *dynastic*. Polities grew mainly through marriage and inheritance and only rarely through outright conquest and subjugation. To be sure, Tilly is quite right that the total number of independent polities on the European continent declined considerably over time. But he enormously overstates the role that military predation played in this process. As a number of scholars have already pointed out, the "weak states" on the Continent were rarely eliminated by military means (Osiander 2001; Ruggie 1989, 1993). And even states that did "die" were typically resurrected later on (Fazal 2004, 2007). Further, it is not until Frederick the Great's invasion of Silesia and the Polish partitions (1772, 1793, 1795) that we encounter a clear-cut case of "predatory conquest," and the partitions, it should be noted, involved much lower levels of actual physical violence than the religious wars of the sixteenth and seventeenth centuries (Davies 1981, 2: 40ff.). This points to the second major difficulty in Tilly's account: Predatory conquest and total war do not always go together. Total wars are often civil wars, while expansionary wars often employ limited means. Why this should be becomes clearer in the next section.

Still, the bellicist wave had not yet crested. Two years later, the Roberts/Parker thesis was given marquee billing in Brian Downing's (1992) analysis *The Military Revolution and Political Change*. Much like Mann and Tilly, Downing contends that the structure of early modern states was primarily determined by the mode of fiscal mobilization which they adopted in response to the military revolution. Rulers' never-ending demands for resources led to increasingly heated confrontations with the "territorial estates," the representative assemblies that had emerged during the Middle Ages. In Brandenburg-Prussia and France, Downing contends, "domestic resource mobilization" culminated in the destruction not only of representative institutions but of "medieval constitutionalism" more generally, and in the advent of "military-bureaucratic absolutism." But not all early modern states were absolutist states. Downing argues that some states were able to avoid a knock-down, drag-out confrontation of this sort, either because they were (1) very wealthy (the Netherlands), (2) good plunderers (Sweden), or (3) geographically isolated (England). In these cases, the trials of "domestic resource mobilization" were avoided and the vestiges of "medieval constitutionalism" were preserved. The case of Poland confirms the model. Despite its geopolitical exposure and economic backwardness, the Polish nobility successfully resisted efforts at "domestic resource mobilization." The result was partition. Or so Downing argues. Note that Downing's model differs from Tilly in at least two important ways: First, following Otto Hintze's lead, Downing takes geopolitical exposure to be a crucial variable; second, Downing eschews Tilly's structural

determinism and insists on the “agency” of rulers (meaning the possibility of strategic mistakes).

Despite his careful attention to cross-national variations, Downing’s version of the fiscal-military argument generates as many questions as it answers. If domestic resource mobilization undermines medieval constitutionalism, why did the Hundred Years’ War fail to usher in absolutist rule in medieval England and France? If partition was the price that Poland had to pay for preserving its representative institutions, then why was Hungary spared this expense? If Gustavus’s plunder saved early modern Sweden from military-bureaucratic absolutism, then why did American gold not have the same effect in Spain and Portugal? If England’s geographical location saved it from the political consequences of domestic resource mobilization, then why was its populace the most heavily taxed in Europe and its navy the most powerful in the world? And if the structure of Downing’s argument is rickety, the foundations are crumbling. For, if there was no “military revolution,” then what is really behind the “fiscal-military mobilization” of the early modern era?

These and other vexing questions are explicitly raised and clearly addressed in what is by far the most erudite, ambitious, and sophisticated elaboration of the fiscal-military model: Thomas Ertman’s (1997) *Birth of the Leviathan*. Working in a dozen languages and ranging across the entire Continent, Ertman expounds a complex and nuanced model of early modern state formation that can (logically) account for all of the key variations identified in previous works and in (almost) all cases. Indeed, one of the signal contributions of Ertman’s book is a clearer conceptualization of the variations in question. Earlier versions of the fiscal-military model typically aligned “absolutism” with “bureaucracy” and tacitly opposed them to “constitutionalism” and “patrimonialism.” But as Ertman rightly points out, other configurations were possible. Louis XIV’s France was certainly “absolutist,” but was it fully “bureaucratic”? Not on Weber’s definition. After all, most royal administrators in Louis XIV’s France *owned* their offices and “separation of person and office” is a hallmark of bureaucracy. On Ertman’s coding, pre-revolutionary France was therefore a case of “patrimonial absolutism.” England was assuredly “constitutional” at this time. But was it “patrimonial” as traditional accounts have insisted? Not on Ertman’s reading. Drawing on the work of revisionist historians such as John Brewer (Brewer 1989; Brewer, Hellmuth, and German Historical Institute in London 1999), Ertman notes that venal office holding was quite absent from the civilian tax administration of Hanoverian England (though not from the officers’ corps). England, then, is a possible example of “bureaucratic constitutionalism.” While earlier versions of the fiscal-military model distinguished two or three outcomes, Ertman’s conceptualization yields four possible permutations: bureaucratic absolutism, bureaucratic constitutionalism, patrimonial absolutism, and patrimonial constitutionalism.