New Approaches to Roman Military History

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When it came to military technique, the Romans of the Republic were great borrowers. As the Roman Kaiso says to the Carthaginians on the eve of the first Punic War in that anonymous fragment, the *Ineditum Vaticanum*, "We got hoplite arms and phalanges from the Etruscans, maniples from the Samnites, and siege know-how from the Greek—and we beat them all at their own games. Don't go challenging us at sea or we'll take up seafaring and make you sorry," as the Carthaginians in fact were. So when it comes to military history, it's no surprise to find that Roman historians have proven themselves equally ready borrowers, and I'd like to think that if we haven't gone our colleagues in other fields one better, at least we've not shown ourselves a whit inferior. This is clearly the case in what in my view is the most novel recent approach to the venerable study of Roman warfare, a genre that might be called "face of battle" studies. As you've already heard from my fellow panelist Peter Hunt, this approach made its first appearance in ancient studies in Victor Davis Hanson's highly influential—and increasingly controversial—study of battle in Classical Greece, *The Western Way of War*, which itself was heavily indebted to John Keegan's pioneering work, *The Face of Battle*. This sort of military history represents a radical departure from earlier works, which pass lightly over the actual combat and killing that form the *raison d'être* of any army. "Face of battle" studies on the contrary are all about combat and killing. Not surprisingly, given how large warfare bulks in our sources, a number of modern Roman historians have adopted the approach, notably Adrian Goldsworthy in *The Roman Army at War*; and Gregory Daly in *Cannae. The Experience of Battle in the Second Punic War*; as well as Philip Sabin, A.D. Lee, and A. Zhmodikov in article-length studies. This type of military history focuses principally on the mechanics of battle: how soldiers were arrayed for combat, the capabilities of

their weapons and defensive armor, the characteristics of the opposition, command and control in the course of the fighting. But at their heart these sorts of studies are about how and why men fight: what happens in and around the so-called "killing zone" of an ancient battle and what motivated soldiers to enter it and stay there. Ted Lendon's recent *Soldiers and Ghosts*, although less interested in the mechanics of battle, shares this intense focus on the psychology of the ancient warrior.

Scholars in the "face of battle" tradition have not lacked for detractors, who focus much of their critique on these scholars' use of theories and analogies from better documented eras to supplement the meager ancient evidence on the experience of combat in antiquity. And to some extent such criticisms are justified, particularly those that take issue with the mechanistic application of ideas developed by S.L.A. Marshall from his studies of U.S. soldiers who had undergone combat during the Second World War. Marshall held that fear among combatants was so intense that only about a quarter of them ever fired their weapons, while those who did so were not motivated primarily by courage or discipline but by a desire not to let their comrades in their platoon or squad down. Unfortunately, professional officers and military theorists now view Marshall's work with considerable skepticism, and that repudiation has provided critics a stick with which to beat studies that purport to reveal the "face of battle." But the use of Marshall's ideas or any other modern construct is simply part of a much larger trend not simply within Roman military history but in ancient history generally, and that is an increasing willingness to look to disciplines outside of ancient history for ways to answer questions that a paucity of evidence would otherwise stymie. To understand how ancient battles worked at the level of an individual combatant and to appreciate what kept him fighting when instinct ought to have dictated flight are inherently worthy goals, and yet we have nothing like the trove of first-hand accounts from members of the rank and file or contemporary observers that have enabled scholars of warfare in other periods to come much closer to understanding the "face of battle." But much the same could

be said about any number of other problems in Roman or Greek history, and the temptation to supplement our meager ancient evidence with materials drawn from better reported periods or theories and models developed by other disciplines is understandable and essential. What we cannot do is return to a kind of age of innocence in which we restrict ourselves solely to whatever scraps we can glean from the ancient sources. Rather, the challenge is to use such aids judiciously, to help enlarge the range of possible solutions and to suggest alternatives that need to be tested against our limited evidence. Roman armies might be conceived of as organized agglomerations of small groups of comrades determined not to let one another down, or as Phyllis Culham suggested thirty years ago in what in my view is still one of the most provocative articles on ancient warfare, they might be thought of as intricate human systems that in battle undergo intense pressure and thereby become prey to sudden transformations best studied through the lens of chaos theory. Was it discipline that kept a legionary in place, as it did the soldiers of Frederick the Great, or loyalty to a group of buddies fighting along side him like that which may have animated G.Is in Europe, or a competitive drive to display *virtus* inspired by memories of a long-ago world of single combat, as Lendon has suggested, or simply greed for the booty that victory would bring? Or should we simply abandon the search for monocausal explanations and just admit that the more possibilities we can think of the richer our understanding will become of an extraordinarily complex phenomenon like the behavior of men who found themselves facing a life or death struggle in the crucible of ancient combat?

Another area in the study of Roman warfare in which theories and models from other disciplines have had a significant impact is in the study of Roman imperialism. Art Eckstein in his recent *Mediterranean Anarchy, Interstate War, and the Rise of Rome* has sought to bring neo-realist theories of interstate relations developed by political scientists to bear on our understanding of why Rome went to war during the middle Republic. To simplify greatly, neo-realists hold that interstate relations, particularly under conditions like those in which Rome found itself in these

years, were as Eckstein's title suggests anarchic and, worse, highly dangerous. Knowledge of the aims and actions of other powers was limited and the consequences of military defeat potentially so horrible that states had no choice but to go to extremes in defending themselves. The tendency was to strike first, before a threat could fully materialize or, better yet, cultivate a military reputation so formidable as to deter potential challengers from aggression in the first place. Eckstein is quite self-conscious in his application of these ideas to the Roman case, and in making his argument he has also explicitly engages the theoretical underpinnings of rival interpretations, notably William Harris' in *Roman Imperialism* and Erich Gruen's in *The Hellenistic World and the Coming of Rome*, which tend to go unspoken. In doing so, Eckstein has raised the level of the debate. For the issue can no longer be simply what does the evidence say. Instead, we have also to ask ourselves what sort of interpretative model does it fit best into, and what are its implications. Was Rome a "warrior state" driven to continual aggression by internal pressures (an analysis that draws on unit attribute theory), or was its bellicosity really no different in kind from that of its rivals, as neo-realist would argue? Was the Republic drawn into conflicts that it would have preferred to stay out of by the actions of second-rank powers (as center-periphery theory would have it), or does it make more sense to see, with the neo-realists, Rome forced to over-respond in order to protect its military credibility and keep potential rivals from thinking that it was not as tough as they had imagined and that an attack might just succeed? And if we go that route, are we in effect on our way back to good old Maurice Holleaux and his theories of "defensive imperialism?" Finally, should our analysis remain at the level of a single political entity, in this case Rome, or should we think in terms of a system of powers with its own characteristics and dynamics? Again, no matter what answer we finally settle on, bringing theory and models to bear enlarges our range of potential interpretations and forces us to confront their implications. This can only enrich the study of war at Rome.

Modern Roman historians have also borrowed from their Greek colleagues in the study of

military operations. Operational histories are the most traditional sorts of accounts of war. They focus on the movements of armies and navies to and upon the battlefield and the decisions, strategies, and tactics that bring them into conflict and that have long been thought to determine the outcomes of battles. This approach might be thought of as the "general's-eye view" of war, a way of understanding battles heavily criticized by Keegan in *The Face of Battle* and quite contrary to his mode of analysis, but one ably defended recently by Kimberly Kagan in *The Eye of Command*. But little is novel here. Scholars ever since the beginnings of modern historical research on Rome have been fleshing out the ancient accounts of particular wars, and while they are still enormously popular with the general public—there have been at least two full-scale histories of Rome's wars with Carthage in English within the last ten years that I am aware of—they involve little in the way of innovative methodology or subject matter. Indeed while their insights on this or that aspect of a campaign may be valuable, to my mind they differ little in their approach from Polybius' or Caesar's narrative accounts of the wars they chronicled.

Innovation instead has come in the study what might be termed the "tail" of ancient armies that supported the "teeth" that engaged in combat. Shifting the focus here represents a novel departure in operational studies, and again, an historian of Greek warfare led the way. This was Donald Engles in his genuinely groundbreaking *Alexander the Great and the Logistics of the Macedonian Army*, which examined the problems Alexander faced in supplying his army with food during his campaigns in Asia and the ways in which these challenges constrained his operations. In good Roman fashion, scholars of the Republic and Empire have borrowed freely from the Greeks and made themselves masters of the genre. Paul Erdkamp's *Hunger and the Sword,* Jonathan Roth's *The Logistics of the Roman Army at War*, and M. Junkelmann's *Panis Militaris* all appeared within the space of a couple of years in the late 1990s, and all draw their evidence mainly from the surprisingly abundant, if highly scattered, references to the supply of Roman armies in our literary sources. But they also attempt to make sense of those references

through judicious comparisons with military logistics in better reported periods and by attempting to model the nutritional requirements of soldiers and the supply and transport problems that they would have posed when aggregated up to the scale of entire armies.

Finally, modeling and comparative evidence have made important contributions to my own study of the effects of warfare on Roman and Italian society during the middle Republic. In order to challenge the long-standing belief that the conscription of farmers for lengthy tours of duty overseas caused many of their farms to fail for want of adequate manpower, which in turn constituted a major cause of agrarian change in second century Italy, I attempted to model the labor demands and nutritional requirements of some hypothetical farm families in order to test the effects that conscripting one or more family member would have had on a family's ability to grow the food it needed to feed itself. I found that the critical variable was not so much the loss of labor conscription entailed but rather when in the family's life-cycle it occurred, a concept I freely admit I borrowed from Tom Gallant's very stimulating *Risk and Survival in Ancient Greece.* I concluded based on these models that because Rome tended to draft men at times when their farms had a surplus of labor and to avoid taking them when they were in the early stages of their marriages, conscription would have caused few farms to fail. In those instances where a son represented the sole source of adult male labor, I drew on comparative evidence to suggest that most women would not have been incapable of doing the heavy farm work they needed to do in order to survive. To assess the effects of military mortality on the Roman population, I looked to "face of battle" studies to assess the effectiveness of legionaries armor and the weapons of their opponents, and to medical literature and comparative evidence to understand the mortality rate for the types of wounds legionaries might sustain and the types and deadliness of the diseases that might have affected Roman armies. And relying on techniques introduced by Keith Hopkins and others, I sought to estimate an order of magnitude for the number of men who might have died as a result of military service above ordinary mortality based on the above considerations and an estimate of the

percentage of soldiers who might have been killed in a typical Roman battle. The numbers turned out to be surprisingly large and led me to consider the demographic effects that followed mortality crises in other periods, especially following the Black Death, might have been in play in Italy during the second century. This allowed me to offer a rather different interpretation of Roman warfare's effects on the population of ordinary Romans and Italians, one that I think makes better sense out of the census figures we possess for the second century.

My approach to the problem of understanding the impact of Roman warfare in this period on Roman and Italian society has not failed to draw skeptics. Apart from a certain discomfort with what I admit is a considerable degree of speculation in some parts of the study, criticism has focused on the fact that conclusions I reach seem to fly in the face of the testimony of our principal literary sources, which I have to admit they do. A very different picture of developments emerges if we privilege the evidence of Plutarch and Appian and Sallust, as traditional accounts for the most part do. Should we then reject the use of models, theories, and comparative data when the conflict with the more usual sorts of evidence? As I suggested a moment ago, the way forward is not to return to the methods of the past. The challenge is to use new methods judiciously, weighing their indications against older sorts of sources. And when they do not agree, we need to remember that the latter rarely offer an unbiased and unmediated picture of the past. They and their sources are as much artifacts of their time and place as our models and theories are of ours. And sometimes, when the scales tip strongly—or even somewhat—to the side of modern constructs, we must remember that the ancients were just as capable of getting it wrong as we are.

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