Commentary

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Broadening the Perspective on Military Cohesion? A Reply

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Abstract

In 2018, Ilmari Käihkö published a special collection in *Armed Forces & Society* on the debate about small unit cohesion. Later, in reaction to a response by Guy Siebold, he published a further intervention with Peter Haldén. Focusing on my 2006 article in the journal and my subsequent debate, Käihkö has claimed that the cohesion debate is too narrow. It ignores organizational factors in the armed forces and wider political factors, including nationalism and state policy. Consequently, it is incapable of analyzing non-Western state or irregular forces and is only relevant for the 20th and 21st centuries. This response shows that while Käihkö's extension of the empirical archive to non-Western armed groups is to be welcomed, none of his theoretical claims are sustainable.

Keywords

small units, cohesion, combat effectiveness, Eurocentricism

Why do soldiers fight? The fact that soldiers have willingly risked and sacrificed themselves for each other in battle is a remarkable phenomenon. The question of how the bonds between small groups of soldiers have motivated them to fight and die for each other has, therefore, been a pertinent topic not only in sociology but also in history, philosophy, and psychology (Aran, 1974; Arkin & Dobrosky, 1978;

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Ben-Shalom et al., 2005; Cockerham, 1978; Henderson, 1985; Marshall, 2000; Shils & Janowitz, 1948; Siebold, 2018; Stouffer, Lumsdaine, et al., 1949; Stouffer, Suchman, et al., 1949; MacCoun et al., 2006). Small unit cohesion has, naturally therefore, been a major theme in *Armed Forces & Society* more or less since its inception. However, in the light of the wars in Iraq and Afghanistan in the first two decades of the 21st century, the question of small unit cohesion has become a central concern to military scholars from across the disciplines. In stark contrast to the last quarter of the 20th century, when, outside the brief exceptions of the Falklands, Grenada, Panama, and the Gulf War, ground forces were primarily committed to deterrence and peacekeeping, American and Western troops were once again fighting in close combat. For over a decade after 2001, infantry squads, platoons, and companies were regularly engaged in battles and firefights. It was, therefore, more or less inevitable, that the question of combat motivation and performance—small unit cohesion—would become a major issue in the social sciences, not least because suddenly scholars had the benefit of a vast new archive of evidence. Ilmari Käihkö's interventions into this debate (Käihkö, 2018a, 2018b; Käihkö & Haldén, 2020) and his attempt to "broaden" the analysis of small unit cohesion is, therefore, to be welcomed. As he rightly notes, small unit cohesion is not only a fascinating topic in itself, but it also has profound implications for understanding the armed forces and civil–military relations much more generally. The question is whether his call for "full spectrum social science" is justified.

Focusing primarily on my 2006 article (King, 2006) and my 2007 debate with Guy Siebold (King, 2007; Siebold, 2007), Käihkö argues that it is now necessary to broaden "the view of military cohesion" (Käihkö & Haldén, 2020, p. 518). For Käihkö, my work—and the current debate—is compromised by a major failing; "The debate, however, assumed the existence of societies and states similar to Western states" (Käihkö, 2018a, p. 565). Thus, Käihkö asserts that "King's (2013) emphasis on training and tactical-level combat explicitly played down the importance of social influences on the performance of military professionals" (Käihkö, 2018b, p. 580). Consequently, my work and that of other scholars systematically ignored and marginalized non-Western and non-state armed groups, which we putatively presumed must accord with a Western model. He argues that it is time to consider "non-state and pre-twentieth century armed groups" especially from non-Western societies; "considering that the vast majority of armed groups belong to these categories, it is clear that the perspective on military cohesion needs to be broadened" (Käihkö, 2018b, p. 572).

For Käihkö, the military cohesion debate can be advanced only by expanding the field of analysis from the small combat unit itself to the wider institutional and political context. Military cohesion cannot be understood merely by studying the practices and interrelations between soldiers in a small unit; military organizations and the nation-state are themselves always implicated in the micro-dynamics of the squad. Accordingly, Käihkö insists that micro-level analysis, to which my work is limited, must be augmented by "meso" and "macro-level" research. In addition to the infantry platoon, scholars should explore the culture and structure of the armies and regiments, of which they are part, and the polities of which they are citizens, subjects, or employees. Käihkö is particularly influenced by Morris Janowitz and Roger Little's *Sociology and the Military Establishment* in which they state that "the empirical study of primary groups must extend beyond factors that contribute to the social cohesion in the smallest tactical units" (Janowitz & Little, 1974, p. 94; cited in Käihkö, 2018b, p. 579; Käihkö & Haldén, 2020, p. 518). By considering wider organizational and political factors, Käihkö believes that the analysis of small unit cohesion might be extended to non-Western state forces, to irregular armed groups and beyond the 20th and 21st centuries. Käihkö has then a conceptual and an empirical complaint about the contemporary analysis of military cohesion and my work, in particular; its analytical focus is too narrow and, consequently, it is ethnocentric. Let us consider the validity of these claims in turn.

Käihkö complains about the narrowness of the debate about cohesion in this journal. Yet, one of the most remarkable aspects of Käihkö's intervention is its very limited focus; his argument is almost entirely predicated on his interpretation of the "most recent debate about cohesion" (Käihkö, 2018b, p. 572) in *Armed Forces & Society* between myself and Guy Siebold. It is gratifying that both my paper and the response it engendered is still relevant to researchers today. However, published in 2006 and 2007 respectively, they can hardly be described as recent, still less, "most recent" even in this journal (see, for instance, Brownson, 2014); the debate occurred over a decade before he published his special section on cohesion in 2018. This misattribution is flattering, but it allows Käihkö to neglect the development of the debate and to ignore my own research on the topic (some of it published in *Armed Forces & Society*) since that time. Above all, it has allowed Käihkö to overlook my monograph on the topic of small unit cohesion, *The Combat Soldier*, even though, at over 400 pages, it is self-evidently my mature thoughts on the topic.

Perhaps this negligence is accidental. Yet, had Käihkö taken *The Combat Soldier* seriously, it would have become obvious to him that he could not sustain the claim that I or, indeed, the other researchers, on whom I drew, were only ever myopically interested in micro-level interactions. *The Combat Soldier* examines the question of small unit combat performance; its central research question was explaining why Western infantry squads and platoons were generally able to fight successfully in Iraq and Afghanistan. Cohesion, for me, did not refer to the interpersonal bonds between the troops (crucial though they were to motivation) but to combat performance itself. The book claimed that, in stark contrast to the 20th-century citizen armies which they replaced, the combat performance of Western all-volunteer, professional combat troops was to be explained primarily by reference to their training and drills. Infantry squads and platoons had become highly cohesive teams, capable of sophisticated choreographies in combat, because of extensive training.

It is absolutely true, therefore, as Käihkö suggests, that I prioritized training and battle preparation as the prime explanation of battlefield performance. Most of the book discusses combat techniques, actions, and training to, I hoped, an unusual level of detail.

However, it is simply not true that I was concerned only with squads and platoons, still less that I dismissed the significance of Käihkö's meso- and macro-levels. On the contrary, the entire work is organized around the historic transition from mass, citizen armies to a professional, all-volunteer forces. The abolition of conscription for Western forces was not just a change in the method of recruitment and conditions of service; it has profoundly altered military ethos and culture, not simply at the highest organizational levels but right down to the interactions between individuals in infantry squads. Soldiers and marines in an all-volunteer force were no longer merely "buddies," as citizen in arms had tended to be; they were professional comrades united by their training and expertise. Professionalization altered their individual and collective performances, their expectations of each other, their motivations, and their identities. The integration of non-white, female, and homosexual soldiers into small combat units has been possible (in conflicted ways) precisely because of this transformation.

I was also intensely aware of the political character of military professionalization. Professionalization involved a profound transformation in civil–military relations, societal–military and, indeed, state–societal relations. It reflected a changing relationship between the state and its citizens and, in most cases, the alteration and perhaps erosion of state authority. I discussed some of these changes in my book, *The Transformation of Europe's Armed Forces* (King, 2011), and in my work on commemoration (King, 2010). In addition, professionalization often politicized relations within the armed forces themselves, influencing small unit cohesion. For instance, in my article on the UK's Special Air Service (SAS), published in *Armed Forces & Society* (King, 2009) but ignored by Käihkö, I argued that professionalization had led to a concentration of resources on elite forces. This was not a natural, rational process, though. On the contrary, the rise of the SAS in the UK in the 1970s and 1980s was a contested and intensely political process in which this unit's commanders acted as skilled entrepreneurs promoting their regimental interests over other parts of the army (King, 2009, pp. 649–651). Consequently, the physical capabilities of SAS troops in combat could not just be explained by their drills (although it was crucial to understand these) but were themselves a manifestation of deep institutional processes, where the regiment had already earned political patronage at the highest levels. In a more recent paper (King, 2016), I showed how Special Operations Forces' urban battle techniques have been adopted by regular infantry units partly to increase their status and, therefore, funding. Against Käihkö's imputations, the armed forces as an organization—and its internal politics—was always already implicated in my analysis of the small unit. Indeed, Käihkö's colleague, Peter Haldén, himself accepted that my work on professionalization

> also deals (but not so much) with the wider social context. He [King] claims that combat performance rests on the skills of the platoon but also on being motivated to apply these skills in combat. However, he also opens for the importance of macro-factors by emphasizing that although soldiers are motivated more by "an ethos of professionalism" than by ideology. (Haldén, 2018, p. 609)

Does my work ignore the macro-level? It is true that *The Combat Soldier* concentrated on the analysis of small unit combat performance. I explicitly stated: “In a professional army, combat soldiers are motivated not in the first instance by their masculinity, nationality, or ethnicity but by an ethos of professionalism” (King, 2013, p. 424). Yet, as the phrase “in the first instance, shows,” I was well apprised of the importance of ethnopolitical motivation to combat performance in the small unit. For instance, while I focused on training and drills because, in Iraq and Afghanistan, I regarded them as the primary independent variables in explaining combat performance, I was not completely ignorant about macro-level factors pertaining to the state and the nation. On the contrary, in the conclusion of the book, I emphasized the palpable patriotism of U.S. troops in Iraq and Afghanistan. It was completely impossible to spend any time with them without feeling their national pride. I stated: “A sense of national mission is very prominent in the professional forces of the United States” (King, 2013, p. 426). American service personnel in Iraq and Afghanistan understood themselves to be at war, on behalf of their country. This fact was important to their combat performance and helps explain their generally superior combat performance in comparison with their North Atantic Treaty Organization allies.

Nevertheless, while I deliberately de-emphasized political motivation as a primary factor in small unit cohesion in the 21st century, it remained absolutely central to my explanation of combat performance in the 20th century. The book includes a complete chapter on combat motivation in the citizen army. There, by reference to material from German, British, American, Italian, French, and Australian armies, I argued that ethnopolitical motivation was always essential to the motivation of troops in primary groups between the First World War and Vietnam. Precisely, because they often lacked training, mass armies actively promoted the patriotism of their troops in order to unify them. Indeed, for most of the 20th century and, especially in the Second World War, this political motivation was inflected with a disturbing degree of racism, even among the Allied Forces. At the end of a long analysis of the material, I was able to conclude:

> one of the central means by which armies recurrently sought to overcome the Marshall effect was not ultimately military at all. The civil society, the army, and troops themselves resorted to appeals to masculine honor, nationalism, ethnicity, and patriotic duty in order to encourage participation on the field of battle. (King, 2013, p. 97)

Of course, as I fully acknowledged, my own argument here was anything but original. It drew upon the rich scholarship of cohesion, some of which Käihkö acknowledges (e.g., Moskos, 1970, 1975; Wessely, 2006) and others of which he seems to ignore (Segal & Kestnbaum, 2002; Watson, 2011; Wesbrook, 1980). Neither my own work nor that of other scholars has overlooked the institutional and political contexts—the meso and macro as Käihkö would have it.

Käihkö also complains about the ethnocentricism of existing work on cohesion. It is certainly true that my work and the recent debates about cohesion and small unit combat performance in *Armed Forces & Society* have concentrated on Western state forces. In any field of inquiry in the social or, indeed, natural sciences, it is necessary to limit the scope; it is impossible to study everything. The attempt is normally a recipe for disaster. It is understandable that Western scholars—like myself—would examine their own armed forces, in the first instance, especially since in the early 21st century, as in the 1940s, they had been involved in heavy fighting. Certainly, my research consciously focused on British, French, German, Canadian, and American armies and Marine Corps precisely because they had participated, to varying degrees, in combat operations in Iraq and Afghanistan. However, it is simply untrue that I ignored the empirical limitations of my study of cohesion, was uninterested in the implications of my findings for other state and non-state forces, or believed that cohesion would automatically look the same in other armed forces or armed groups. On the contrary, I fully recognized those differences:

> Clearly by focusing exclusively on the major western powers, the findings of this study are perforce limited. Indeed, Jeremy Black has highlighted the often unwitting ethnocentricism of military historians who assume the existence of a "Western Way of Warfare," ignoring developments in other parts of the world or presuming that western military practices are simply replicated elsewhere. He has rightly argued for the need for a genuinely global history of war and warfare which recognises the great differences which have historically pertained between western ways of warfare and those practised in other parts of the world. (King, 2013, pp. 20–21)

My hope was, however, that my findings would help others to investigate other non-Western, non-state cases—rather than obscure them. Indeed, although it was brief, in the concluding chapter of *The Combat Soldier*, I considered whether Chinese, Russian, and Brazilian armies were also professionalizing in a manner which, although distinctive, might be compatible with Western patterns (King, 2013, pp. 421–423); I discussed the Israeli Defence Force at various other points too. In a subsequent collection, I discussed the literature on cohesion in early modern Europe (King, 2015b, pp. 16–20); Tarak Barkawi's chapter in that volume (King 2015a) sought explicitly to overcome Eurocentricism, and his analysis of the Indian Army found there was elaborated in his subsequent monograph (Barkawi, 2015, 2017)—which I have reviewed. Finally, in the 2015 debate in *Armed Forces & Society*, I actively recommended widening the empirical investigation to other types of military forces across history, for which Käihkö now calls:

> The combination of thick descriptions of the military lifeworld and historical, international comparison enables new academic perspectives on the armed forces to be generated, in some cases, resolving long-standing debates that are substantial only because scholars have developed their arguments on the basis of overly narrow evidence. The archive of military history is dramatic, rich, and effectively infinite. Even, we, as social scientists, should make full use of it. (Siebold et al., 2016, p. 480).

Some subsequent work, in which I was involved, affirms the point. After *The Combat Soldier*, I moved on to study command. Thematically and methodologically, the subsequent monograph (King, 2019) was a direct extension of my analysis of cohesion, but the subject matter was quite different. Yet, I retained a close interest in the question of small unit cohesion and, specifically, in seeing whether my own analysis might be extended to test the thesis against other examples. Although they worked independently, two of my doctoral students subsequently published work on professionalization and cohesion which reflected this endeavor. Bury (2018) studied cohesion in the reserves in the British Army; he focused on a state army, albeit their part-time element. Yet, extrapolating explicitly from my work, Finnegan (2019) showed how the Provisional Irish Republican Army moved from a mass citizen insurgency in the early 1970s to a highly professional terrorist organization by the end of the decade. In other words, Finnegan explicitly sought to explore cohesion in a non-state force, as Käihkö advocated.

In the light of his complaints about the ethnocentricism of the literature, Kaiho's special issue obviously involves analysis of non-Western armed groups. I am delighted to see this work. Yet, the essays there do not always advance the debate as much as he might like. For instance, Haldén's (2018) article is an analysis of Max Weber's concept of domination; it seeks to categorize non-Western, non-state militaries on the basis of Weber's famous concept of traditional, rational, and charismatic authority. Consequently, it involves little empirical material and, while erudite and interesting, has only oblique relevance to the specific debate on small unit cohesion. Hansen's (2018) work on jihadists groups and territory is similarly pitched at a high level of abstraction and empirical generality; the small unit is not the focus.

Ironically, those papers in the collection, which do analyse non-Western forces, affirm the central theoretical arguments and concepts of the cohesion debates rather than deny them. For instance, Verweijen's (2018) work on the Federal Armed Forces in the Democratic Congo examines how patronage networks affect organizational cohesion. Unfortunately, Käihkö (2017) does not include his own excellent work on Charles Taylor's Government of Liberia Army, but, closely echoing Verweijen's work, it shows how patronage networks and its cash nexus undermined their battlefield performance. Because the incentive structures were so weak, combat performance in the Liberian Army was poor; excluded from political and economic privileges, there was no reason why soldiers should risk themselves (Käihkö, 2017, p.65-6). By contrast, Taylor's elite anti-terrorist units received such significant financial rewards that they were, therefore, ironically also unwilling to fight (Käihkö, 2017, p. 65). This research on patronage networks in sub-Saharan Africa is highly pertinent, but conceptually, it only affirms the military cohesion literature. Both Käihkö and Verweijen show that where political motivation, primary group solidarity, or training is lacking, money alone will rarely be sufficient to motivate troops in combat. It is a point that Wesbrook (1980) eloquently noted in 1980.

The same affirmation of existing research is evidenced in Nilsson's (2018) piece on cohesion in the Pershmerga and Hezbollah. This article includes some fascinating interviews with Kurdish and Lebanese fighters. Yet, at the theoretical level, it only confirms existing scholarship. In *The Combat Soldier*, I argued that in the 20th century, mass armies compensated for poor training by means of ethnopolitical motivation; they appealed to the patriotism of their troops to encourage and unite them. Käihkö implies that the mechanics of cohesion will be very different in irregular, non-Western armed groups. Yet, Nilsson's work suggests that the generation of combat motivation is actually comparable. In the case of the Pershmerga, training was inadequate and, consequently, although the context was quite different, like mass armies of the 20th century, the Kurdish forces actively sought to motivate their troops through appeals to nationalism. Käihkö's collection affirms my own work, rather than refuting it. It represents a felicitous empirical extension of the existing literature to new armed groups, not a theoretical rebuttal.

It is very pleasing that cohesion remains an important topic in this journal. It is a crucial topic for military sociology. The infantry platoon is a remarkable lifeworld in which the dynamics of the social group are uniquely legible. Consequently, the small unit offers a privileged empirical opportunity on which sociological theories and concepts can be investigated and tested. It is vital that as military scientists, we continue to extend our fields of empirical inquiry and to test and refine our concepts and theories. There is much to admire, then, in Käihkö's intervention; I am grateful for it. However, it is also rather disappointing that his program is founded on such an odd reading of the existing scholarship. Käihkö aims to broaden the analysis of social cohesion. Yet, in fact, he can advocate such an "extension" of the research program, only because he so consistently fails to acknowledge what it already involves.

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