

**A Critique of**  
**"In Pursuit of Military Excellence" (Frank Cass, London 1997)**  
**by**  
**Shimon Naveh**

This book tries to trace the development of operational art up to the US Air Land Battle doctrine and the Gulf War. Naveh explores the roots from the end of 19th Century and place strong emphasis on the Soviet operational theories developed between 1920 and 1937. His work also relies heavily on general systems theory. Somewhat surprisingly, he has included a chapter on the German blitzkrieg. This has more of a polemic character than being a link in his main argument.

Why he has included the critique of the Blitzkrieg is not entirely clear. His books contain several parts. An attempt at linking operational theory to general systems theory, a discussion of Soviet operational theory, a critique of the Blitzkrieg and a claim that the success in the Gulf War was due to application of Soviet operational theory. It seems that his main thesis is that the Soviet operational theory is a very good concept and that it is supported by general systems theory. If one is to claim that Soviet operational theory is very good – not just as an idea, but also as a practically useful concept – this can either be corroborated by showing that it is a logical consequence of another theory that is well founded, or by empirical support. However, the virtues – or lack of virtues – of another concept does not provide any support or refutation, unless one can prove that there is a situation where only one of two existing concepts can be good. The latter is obviously impossible in warfare, since it has not been proved that there is a limited number of good concepts. Thus it is somewhat irrelevant to his main thesis what qualities the Blitzkrieg had or lacked. Despite this, we will devote considerable space to his coverage of the Blitzkrieg, because it can be shown that his methodology is hopelessly corrupt when he is dealing with this subject.

In fact, Naveh almost completely declines to give any empirical support at all for his thesis. He devotes a chapter to the Gulf War, but his description of it is almost more euphoric than anything that has come out of the United States after February 1991. Also it contains several factual errors, which will be dealt with below. But even if his description of the Gulf War were not subject to objections, one example is poor support for a theory. It would be better support if it could be shown that the side that prevailed had only one advantage, its operational theory, but this was hardly the case in the Gulf War. Rather the coalition had significant advantages in virtually every field affecting the outcome of military operations. How the factor "operational theory" is singled out is simply not discussed by Naveh.

Naveh could have looked at Soviet operations to corroborate his thesis, but he has only stated (p. 237f):

*"The series of frightening disasters and appalling defeats suffered by the Red Army during the first year of the war revived the theory of Deep Operations and similar conceptual achievements created by Tukhachevskii and his colleagues. In the course of a year of strategic transformation, which was highlighted by the publication of Stavka order No. 325 of 16 October 1942, the operations at Stalingrad and Kursk, the Red Army reapplied Tukhachevskii's main ideas, indoctrinated its command cadres with the essentials of the Deep Operation theory, restructured its forces, and regained the strategic initiative. The last two years of the war saw a succession of strategic-operational manoeuvres, which have no equal in scope, intensity and creativity in the history of modern war. In the course of operations, such as*

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*Korsun-Shevchenkivskii, Vitebsk-Orsha, Lvov-Sandomierz, Iasi-Kishinev, Vistula-Oder and Khingan-Mukden, expressing accurately the application of Tukhachevskii's ideas, the Wehrmacht and the Japanese Kwantung Army were defeated and the myth of Blitzkrieg was broken.*

*Unlike the Germans - who almost at the same time were developing their own conceptual version of armoured manoeuvre but failed to produce a coherent theory of operational manoeuvre - the Russians, following the application of the system approach, turn their collection of embryonic ideas into an institutionalized theory and uniform nomenclature. This achievement, which derived from Tukhachevskii's grasp of the need for a universal theory for the proper functioning of a massive and dynamic system, was a major factor in crushing the Wehrmacht, despite the superior tactical training and background of the German officers."*

Unfortunately he gives no further details or any reference to support this. There are further problems with this section, but that will be discussed later.

## General System Theory

In the end, the minimal empirical support provided by Naveh does not at all support his contention. Thus his statements depend on the support lent by general systems theory. As said, his book is an ambitious attempt at linking general systems theory with operational theory. Consequently, Naveh devotes his first chapter to general systems theory. He does provide a wide coverage of general systems theory; virtually the only source appearing in his footnotes is Ludwig von Bertalanffy's book "General System Theory".<sup>1</sup> However, it seems that Naveh has spent little time studying the subject. That he calls Bertalanffy a Hungarian scientist is just the first of many mistakes.<sup>2</sup> But worse is that he does not realize that Bertalanffy does in fact give no evidence for anything useful pertaining to social systems. Bertalanffy's book is a kind of appeal, rather than an argument or a presentation of evidence. Bertalanffy provides very little scientific rigour. The sole exception is found on pages 55-70. Here Bertalanffy defines a set of differential equations and from this he derives a number of equations. While this is principally correct, it has no generality, as von Bertalanffy himself concedes.<sup>3</sup> A closer look at Bertalanffy's work confirms this. He makes a number of assumptions for his set of equations and the mathematical manipulations he does with them. First he assumes there is a condition of stationary state, something that many systems exhibit, but not necessarily all. Second, he assumes that the system can be developed into a Taylor series. This is an important simplification, because subsequently he only use the linear term, which means he assumes that the system is at least approximately linear, which may quite often be true, but far from general.\* In the end, Bertalanffy uses his mathematics only to arrive at a few equations that have long before been known. While this may possibly show that some equations used in various fields have some similarities, it certainly does not show that there are general characteristics shared by systems. It is even less support for any notion that general systems theory can tell us how social systems, like military organizations, ought to be organized.

On page 5, Naveh makes a number of statements that are both unsupported or unjustified. The first is that he cites Bertalanffy to show the difference between a closed and open system. While it is correct that a closed system is isolated from the environment, this is hardly useful for any analysis of military systems as

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\* On page 19 in "General Systems Theory", Bertalanffy states that the equations are non-linear in the general case. However, in his subsequent treatment of the equations, he only use the linear terms of the Taylor series of the equations, thus his conclusions actually depend on linearity.

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Naveh claims.\* Virtually all social systems and organizations are more or less open systems.† Thus it can hardly help us distinguish between military organizations working more or less well. Rather, it is a truism.

Naveh goes on with the following:

*Since his basic argument presented the system's interaction as fundamental for its subsistence, Bertalanffy is correct in concluding that amplifying the dynamism of this interaction inevitably leads to an increase in the system's consequential product. However, according to Bertalanffy, attaining such dynamism prescribes that the nature of the interaction be non-linear.<sup>4</sup>*

If we try to look beyond the turgid language, the central point is non-linear interaction. However, Bertalanffy does not at all show that the interaction has to be non-linear. In fact he does not even try it on the pages Naveh indicates, and I have not found any other pages in his book where Bertalanffy tries to show what Naveh states. Thus, it is actually quite fraudulent by Naveh to footnote Bertalanffy for the passage given above.

But it does not end with this, because Naveh's next passage is:

*Applying this notion in a military situation requires a deep setting, hierarchical structure and a columnar mode of relation between the system's components, or between the sub-systems within the overall system. Thus, succession and echelonment constitute the first of the interaction's characteristics.<sup>5</sup>*

Here Naveh does not include any footnote, so we can probably conclude that this is his own thinking. However, he does in no way make it clear that non-linear interaction requires a "deep setting, hierarchical structure", nor that it requires a "columnar mode". In fact this is entirely wrong, it is fully conceivable that a system exhibit non-linear interaction without a hierarchical structure.‡ But even if it had been correct, it would hardly have been of any use, since all the military organizations studied by Naveh in his book are more or less hierarchical. The statement does not say anything useful about the extent of "hierarchical structure" or in what sense. To call it "deep setting" does hardly clarify matters. It is not entirely clear what is meant by "columnar mode of relation between the system's components", but presumably this means that there must be an interaction or some form of relation between the levels in the hierarchy. However, this is also a truism, since were there no interaction between levels, the system could hardly be called hierarchical. In the end, the passage contains only errors, truisms and an unnecessarily convoluted language.

But the problems do not end there, the following passages reads:

*The second characteristic is the absolute dominance of the system's aim. The initial assertion of the aim by the system's brain or directing authority predetermines the comprehensive whole, i.e. the all-embracing accomplishment of its future destined action.<sup>6</sup>*

Here Naveh refers to Bertalanffy, but again, Bertalanffy does not even faintly outline what Naveh claims, far less prove or show it. Naveh continues the passage with:

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\* A closed system is very rare in real life. Usually closed systems occur in experiments or by definition, to enable the scientist to focus on those interactions that are studied, or most relevant.

† In particular, any military organization engaged in war must certainly be considered an open system.

‡ For example, the system made up by the sun and the planets is a good example. The gravitational forces that cause the planet to interact as a system is dependent on the square of the inverse of the distance between the planets, thus a non-linear interaction. In fact the planetary system is actually chaotic, but it has been in a stable mode for a very long period, but this can change. The fact that the sun, by virtue of its very large mass, is centrally located does not make the system hierarchical. In fact, had the masses been more evenly distributed, this would not have caused the non-linear interaction to disappear.

*It also provides the focus of the system's performance since it creates the framework for the interrelations between its various elements and defines the orbit for the system's relations with its environment. In other words, the definition of the aim is the cognitive force that generates the system and determines the directions and patterns of the action.<sup>7</sup>*

Once again Naveh refers to Bertalanffy and Bertalanffy does not even remotely support Naveh's statements this time either. We could go on like this, because Naveh piles sentence upon sentence with unsupported statements on how systems should work or their characteristics, but in the end he gives no support whatsoever for these statements. Furthermore they appear quite illogical and unrealistic. For, example, above he claims the importance of the absolute dominance of the systems aim. This may be his opinion, but it probably says little about how real organizations consisting of human beings work.

But the most fundamental error made by Naveh is his assumption that systems theory can be a foundation for an operational theory. Systems theory can certainly contribute, but mainly as an inspiration or a source of terminology, concepts and ideas. To use it as a kind of normative application constitutes a misuse. Furthermore, if systems theory is to be used, it would be better to use a more modern source than Bertalanffy's book written in 1968. Also, this book actually deals with ideas he conceived in the 1930-ies. There have been advances in systems theory since then.

We can conclude that systems theory does not provide an useful support for Naveh's work. He also falsely refers to a source that does not give him the support he implies with his footnotes. Furthermore, he does not seem to understand the theory he writes about. The latter is repeated in other fields to. One example is his short discussion of the Lanchester equations.<sup>8</sup> Naveh claims that Lanchester developed a mathematical model which determined a scientific relation between quality and quantity. This is very wrong. First of all, a model can not determine a relation, whether scientific or not. A model can more or less well describe a relation. However, the Lanchester equations have not been validated, thus the degree to which they are scientific is very much open to debate. Furthermore they do not describe the relation between quality and quantity, nor do they deal with chances of success as Naveh claims. Rather they give the casualty rates of two sides involved in combat.<sup>9</sup> Naveh's description of them is totally wrong.

We will proceed later with Naveh's chapter on the Blitzkrieg, but before we turn to that part, it is necessary to look more at Naveh's terminology.

## **Terminology**

Two words used frequently by Naveh is linear and non-linear. Unfortunately he gives no definition of these words. In mathematics linear denotes a specific kind of functions or a specific kind of set. However, the word linear is also used in other fields, for example tactics during the 18th century has often been called linear tactics. However, the similarity of words does not mean that conclusions from one field are applicable in another. Naveh seems to use the word linear in almost any way.

For example, he writes that "of all existing forms the linear is the greatest consumer of space", which he says is rooted in Euclidian geometry. This is sheer nonsense. A form in mathematics is a class of functions; the linear is not a form. In fact there is no specific space consumption connected to "linear".

Another example is:

*The frontal or rather linear (horizontal) dimension expresses energy, whether by delivering it against the rival's system or by absorbing the rival's energy, hence it is essentially static and oriented towards holding actions. The depth (vertical) dimension expresses movement and response, and is, therefore, oriented towards delivering a shock.*<sup>10</sup>

Aside from being factually wrong, this is illogical, because it does not follow from the first clause that the rest of the sentence is true. Naveh gives no sources that could corroborate his statement. Moreover, this use of the word linear has nothing to do with the use of the word linear (and its counterpart non-linear) in Bertalanffy's treatment of system theory. Neither has it anything to do with the mathematical use of the word.

Another example is provided on page 16, where Naveh speaks of "a linear battle". What this means is not at all self-evident. Naveh uses it to describe the "Battle of Destruction" (Vernichtungsschlacht), but he does not make it clear in what sense it is linear. Furthermore, it can well be that a successful encirclement battle, which results in the destruction of a major enemy force actually take advantage of a linear deployment on behalf of the enemy. In that case, it is rather the opponent that may be criticized for using "linear methods".

In fact, it seems that to Naveh, linear is a word with negative value, while non-linear is a word with a positive value. This is not something Naveh is alone in doing. Alan Sokal and Jean Bricmont have discussed this.<sup>11</sup> To some people, the word linear seems to represent old-fashioned thinking, mechanistic (particularly for Newtonian mechanics) world-view, etc. Non-linear seems however to stand for more modern and advanced modes of thought, for example manifested by quantum mechanics and chaos theory. However, this is a complete misunderstanding, since Newton used equations that are non-linear for his mechanics, while for example the basic equation in quantum mechanics, the Schrödinger equation, is linear. Furthermore, many of the problems treated by chaos theory concern mechanical devices or systems.<sup>12</sup>

In fact, non-linear mathematics is not a particularly new field. Already the ancient Greeks used non-linear mathematics. These problems have often been difficult to solve and the advent of computers have provided new possibilities for dealing with them, but that is an advance in tools, not in principles. Neither is the linear vs. non-linear dichotomy particularly useful outside mathematics.\* That Naveh mixes linear in the sense Bertalanffy used it<sup>†</sup> with linear tactics or deployment only adds further confusion.

Statements like the following illustrate this:

*We have already seen that the non-linear nature of the interaction between the system's components determines the columnar form of the operational manoeuvre and the patterns of cooperation between the operational echelons.*<sup>13</sup>

This is reminiscent of his statement on page 5 that non-linear interaction requires a hierarchical structure. Naveh seems to have confused linear with horizontal and non-linear with vertical or depth. However, whether the interactions between a system's components are linear or non-linear have nothing to do with vertical or horizontal connections within an organization (hierarchical or not), nor does it have any relation military echelons.

\* In mathematics it is useful mainly because some methods that are only applicable to linear equations have been developed.

† Which admittedly is somewhat confused too, only when he uses it in his mathematical calculations, does he make it relatively clear.

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Another word used frequently by Naveh is mechanical and derivatives of it like mechanized and mechanization. Bertalanffy uses the word "mechanization" to denote the development of a living being where its various organs get more and more specialized and thus unique. However, this is quite different from the meaning of mechanization in the military, where it denotes addition of certain types of equipment (tanks, APC, tracked artillery etc) to combat units. There is no reason to claim that one definition is better than the other. Each can serve well within its respective field. It is however not prudent to move conclusions regarding mechanization in one field and apply them to another field, as Naveh does.

Already on page one, Naveh calls the lower levels of military forces mechanical. Since he refers to Clausewitz, it is clear that he includes a period of time when mechanized equipment did not exist, he must mean something else with mechanical. Either it can be mechanical in the sense that units are specialised or their way of acting can be "mechanical" If it is the first alternative, the threshold must have been very low sometimes. For example, the German storm troops during WWI combined several different types of weapons, equipment, units and skills very low in the organization, sometimes even lower than company level.<sup>14</sup> However, Naveh seems very fond of the Red Army and during WWII it used some divisions of quite specialized nature, like artillery divisions. Also, the Red Army rifle divisions were (after 1941) more of a rifleman formation compared to German, US and British Infantry divisions, which were more of a combined arms formation. However, there is no evidence that the Soviet approach was better.

If we consider the second alternative, that lower level units act (or should act) "mechanically", this seems highly dubious too. Also it is somewhat illogical if Naveh claims this. He is full of praise for the German Heeresdienstvorschrift 300 "Truppenführung".<sup>15</sup> However, this field manual from 1933 makes it clear that mere "mechanical behaviour" is insufficient for any soldier.<sup>16</sup>

In the end, Naveh uses "mechanical" and its siblings in such a way that we can not know what he actually means. But even so, it hardly seems likely that he has something useful to say with these words.

There are many more examples of dubious or ambiguous use of words, however we will have to proceed with other problems.

## Logic

Naveh often uses the word logic and makes claims that certain conclusions are logical consequences of certain premises. However, one of the most basic characteristics of logic is that every link in the chain from the basic fact or axiom to the conclusion can be described. This Naveh almost invariably fails to do. Rather we are forced to rely on his powers of logic. However, this seems dangerous and a few examples will be given to illustrate this.

Naveh correctly concludes that there is no clear and universally accepted definition of Blitzkrieg. However, he does not provide one himself. This would have been necessary to make a clear argumentation against Blitzkrieg, but since Naveh does not give this, we have to read between the lines. It seems that he sees Blitzkrieg as tanks performing encirclement operations. He also claims that one consequence (or even pre-requisite) of the development of Blitzkrieg was the complete extinction of operational thought.<sup>17</sup> Later he claims that the German victory over France 1940 should not be regarded as a Blitzkrieg, because he claims it was a purely operational approach, representing operational thinking at its best.<sup>18</sup> Thus he mixes his definition with what he sets out to show, which hardly merits high marks for logic.

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In footnote 22 (chapter 4) Naveh criticise Barry R. Posen for not provide a definition of Blitzkrieg, but Naveh can hardly claim to have provided one himself and even less of putting forward a coherent argument for why his definition would be preferable.

Naveh asserts that penetration, envelopment and encirclement should be confined to the tactical context and not used at the operational level (page 127f). However, on page 237 Naveh claims that the Korsun-Shevchenkivskii, Vitebsk-Orsha, Lvov-Sandomierz, Iasi-Kishinev, Vistula-Oder and Khingan-Mukden operations have no parallel in scope, creativity and intensity and presents them as models of excellence. However, of these operations, the Korsun-Shevchenkivskii is a typical encirclement operation, as are the Vitebsk-Orsha and Iasi-Kishinev. If encirclement is such a poor operational concept, why do these operations rank as models? That logic escapes me. Furthermore, neither of these operations matches for example Vyazma-Bryansk or Kiev 1941 in scope and intensity. The Korsun-Shevchenkivskii was also not particularly successful. Of the 56,000 German troops that were encircled<sup>19</sup>, 36,262 broke out and a further 4,161 were flown out.<sup>20</sup> This hardly suggests that it was an operation of unsurpassed scope, neither that it was a remarkable example of creativity and intensity.

Naveh does not seem to grasp the limitations of sources and therefore use them illogically. For example, he writes on page 135:

*The fact that the German infantry never achieved a reasonable degree of mechanization<sup>21</sup> determined the tactical disunion between the infantry arm and the armour.<sup>22</sup> Moreover, even in the cases where tanks and infantry found themselves in the same tactical sector, the latter could not sustain the intensity of the fight due to a conspicuous lack of protection and anti-tank artillery. Thus fighting separately without the essential reciprocal support weakened both elements separately.<sup>23</sup>*

If we look at the sources for this passage we get an impression of how Naveh's logic works. The first note (after the word "mechanization") contains a definition of mechanization (but only for the use of the word in this specific context) and two sources on the percentage of the infantry in the panzer divisions that were mechanized. One is Rudolf Steigers "Armour Tactics in the Second World War"<sup>24</sup>. Steiger writes that one company in each infantry regiment in a panzer division was mechanized in 1941. This does however not mean that "less than 10 %" were mechanized, since the infantry regiments in the panzer divisions did normally not have more than ten companies, of which six were rifle and the remaining four were more specialized.<sup>25</sup> Thus it could be said that they had exactly 10 % of its infantry regiments mechanized. However, there were variations in the divisions. The 1. Pz.Div. had two full five-company battalions mechanized and the 10. Pz.Div. had one.<sup>26</sup> On the other hand, the 14., 16. and 19. Pz.Div. did not have a single company mechanized. Altogether, the 20 German Panzer Divisions on 22 June 1941 had 30 mechanized companies in their infantry regiments, or on average 15 %. While it certainly would have been better to have a higher percentage, the mere fact that the Germans had 30 mechanized companies in the panzer divisions put them well ahead of the rest of the world. In fact, only Germany and the United States had any notable production of such vehicles during WWII. Naveh's other source is a letter from Manteuffel to Liddell Hart in 1948. I have not been able to check it, but as a source it can not take precedence over Niehorsters archival based data on the German panzer divisions that I have used.

But if we return to the first sentence in the citation, we see that Naveh says the German infantry *never* achieved a reasonable degree of mechanization. Steiger's data only covers 1939-41, so it can not say that the Germans *never* achieved a "reasonable" (whatever that means) degree of mechanization. Furthermore Naveh goes on to claim that it "determined the tactical disunion between the infantry arm and the armour". The word "determine" is quite strong; hence stringent evidence can be expected. However, his references for this are a few reports from German panzer divisions, that don't give him the support he

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claims. One is a short combat report from the 7. Pz.Div. for 14 May 1940.<sup>27</sup> It only says that the Panzer regiment was in the lead, whether this was caused by the problems asserted by Naveh or a deliberate (and suitable) choice is not possible to tell from the source.\* Another is a report on experiences by the 20. Pz.Div.<sup>28</sup> This says that that the tanks and the infantry frequently cooperated, but the tanks advanced quicker than the infantry was capable to do. These reports cannot be construed to support Naveh's strong claims. They can at most show that everything was not always working perfectly for the Germans, but that can hardly be considered major news. Again it must remember that the Germans were ahead of their opponents, in particular the Red Army. If the German "shortage" of mechanization is construed as evidence for their alleged lack of operational cognition, what can we say of the Red Army's (which Naveh is so impressed of) complete lack of such equipment.†

What is most important here is that how units are equipped, organized and employed is the result of many factors, including economical and political. If it is to be argued that the "problem" of mechanization had certain roots, some kind of evidence that there are no alternative explanations must be presented. This is basic to research and it does not take much logic to realize it. Unfortunately, Naveh pass this with silence.

## Sources

We have already seen that Naveh makes fraudulent use of Bertalanffy as source on systems theory. Unfortunately his use of sources does not improve later in the book. In his chapter on the Blitzkrieg, Naveh makes extensive use of Guderian's memoirs (Panzer Leader) as a source. In fact, Guderian's memoirs appear in no less than 36 of 199 footnotes in this chapter. This is very surprising, since Naveh casts serious doubts about Guderian's descriptions about the events leading to WWII and the development of the German armoured forces.<sup>29</sup> It is no exaggeration to say that Naveh depicts Guderian as a manipulating distorter.

It is of course conceivable that a source is reliable in some areas and unreliable in other. However, he uses Guderian as a source for information in precisely those areas that refer to the development of German forces before the war and the use of them in the war. If Naveh has found Guderian's memoirs useful as a source in some fields and unreliable in others, the least we could expect is some kind of argument for his use of this source. As it is now, one is left with the impression that Guderian is a good source for Naveh when he says something that can be used as support for Naveh's theses and unreliable when he says something that can contradict Naveh.

Another source that Naveh makes extensive use of is F. O. Miksche's book *Blitzkrieg*.<sup>30</sup> This was published in 1941 and is possibly the first analysis written in English on the German Blitzkrieg. However, the book is full of errors, which of course is natural given the paucity of sources available at that time.

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\* The report by the 7. Pz.Div. that Naveh refers to is a day by day summary for the period 10-29 May 1940. It also contains data on casualties for the period that can be of interest. The division reported 1,913 casualties (killed, wounded and missing) while it took 6,849 prisoners. These figures suggest that if there was a problem with tank-infantry cooperation, it must have been of minor importance compared to the problems experienced by the division's opponents. Similarly, the division reported it had destroyed or captured 361 enemy tanks, which contrasted to its own losses of 41. Even given allowance for inflated reporting of the number of enemy tanks destroyed, these figures too suggest that the enemy must have experienced much worse problems than the 7. Pz.Div.

† Except for some lend-lease equipment. However, I doubt that Soviet operational doctrine took lend-lease from capitalistic countries into consideration.



Another problem with his sources in this chapter too is that they simply don't always say anything supporting his statements. For example, he claims that "the German inclination to grab the largest possible number of enemy troops into the cauldrons created by the deep penetrating armoured blows overstretched their resources, split the mechanized formations from the infantry, and severed the succession of operations, thus bringing complete exhaustion upon themselves."<sup>31</sup> The sources he gives are Steiger and Geyer<sup>32</sup>, who says nothing of the sort.

As we shall see below, archival documents are also used in a very dubious way. But there is one thing else that is very strange with his use of sources, in particular archival documents from Bundesarchiv-Militärarchiv in Freiburg Germany. In his chapter on the Blitzkrieg, Naveh makes 62 references to Bundesarchiv-Militärarchiv documents. Of these, 52 refer to panzer operations 1939-41 in Poland, Western Europe and Soviet Union, a subject covered by Rudolf Steiger in his book *Armoured Warfare*, published seven years before Naveh's book and which appears in Naveh's bibliography. Of these 52 references to archival documents, I have found 47 in Steiger's book. Not only are the files the same, but the exact documents too. Also, with one exception, the only documents Naveh refer to from a file are those documents used by Steiger. Furthermore, when there are multi page documents, the page numbers are identical too. And worst of all, when Steiger has written the file code reference wrong, Naveh repeats this mistake too.\* The remaining five notes on this issue can possibly be found in Steiger too, but since his book has close to 700 footnotes, I may simply have missed them.† In any case, Naveh, can of course have looked in other books too for archival references.

After seeing this, one has to wonder if Naveh has actually been at Bundesarchiv in Freiburg. If he has been there, he seems to have done little more than just checking documents that other authors have already used.

## The operational level

Naveh gives his own definition of "operational"<sup>33</sup>. However, this is not without problems. One of his criteria is that "an operational plan should be non-linear in nature, namely it should be hierarchically structured and express depth".<sup>34</sup> Here he again confuse non-linear with hierarchical. Furthermore, one can wonder if it is acceptable with linear plans in the fields of tactics and strategy. In any case, in its mathematical sense, few – if any – military plans are linear. If a plan is linear in the old military 18th century sense, this has no connection to hierarchical.

Another criteria is : "It must reflect a contemplative attitude towards the factor of randomness which expresses the chaotic dimension in the interrelation between contentious systems."<sup>35</sup>

Naveh is quite fond of the word "reflect", unfortunately this makes it more difficult to criticize him, because it is a vague word. In any case, one can again wonder if not tactics or strategy should consider this too. At the tactical level, "the chaotic dimension in the interrelation between contentious systems" is immediately present. While a "contemplative attitude" can certainly be useful in the realm of tactics, it

\* The Bundesarchiv employs the following system for identifying files for WWII panzer divisions: first comes the letters RH (which shows that it is a document from a 1919-45 army agency); next comes the digit 27 (which shows that it refers to a panzer division); this is followed by a dash and the number of the panzer division; last of all comes a slash and the number for the file within the holdings for the particular panzer division. Thus a file code can be RH 27-4/37, which would then be the 37th file for the 4. Pz.Div. However, Steiger has for one file reference switched place between the dash and the slash, thus calling the file RH 27/4-37. This particular file reference has been used by Naveh six times (notes 116, 134, 135, 136, 137, 140) and every time he has made the same error. I have not found this type of error anywhere else.

† At least two of them appear in files given by Steiger in his list of sources.

seems essential in strategy and in no way unique to operations. One is left with the impression that Naveh does not only try to define what an operation is, but rather telling us what he thinks a good operation should be like.

Naveh included the criteria that the planned action should be synergistic. He asserts that this means that "the system should yield a general product that is significantly greater than the linear arithmetic sum of its component's accomplishments." This notion is however not particularly useful. First, it is very difficult, perhaps impossible, to establish "the linear arithmetic sum of its component's accomplishments". Second, even if we could, it established, it would be exceedingly difficult to compare it with the "general product" yielded by the system, because we would easily end up comparing different sorts of output. It could possibly be said that the "general product" is different "from the linear arithmetic sum of its component's accomplishments". To establish which is greater would probably largely be dependent on preferences.

The last of his criteria is: in order to be regarded as operational, a concept, plan or act must be related to a broad and universal theory.

First of all, it is not clear that this would be something good. Second, why this would be particularly important to the operational level, and not to e.g. tactical or strategic matters, is not self-evident and Naveh does not provide any argument. Second, Naveh tries to relate his (and the Soviet) operational theory to general systems theory. This attempt cannot be characterized as anything but a complete failure, since his references do not support his contentions.

There are a few other criteria used by Naveh, but he does not provide an argument why his definition is better than other definitions. Also, even if taken together, one does not see why these aspects are unique to operations and not to the fields of for example tactics and strategy.

Like many other authors who have studied Soviet military publications, Naveh is almost mesmerized both by the operational level as such and the alleged Soviet skill within this realm. Of course it is conceivable that very good theories were developed within the USSR, but never realized in actual warfare. The empirical evidence of the special importance of the operational level is however not apparent. Naveh makes claims about "the absolute dominance of the operational level". Unfortunately, the only "evidence" he presents is his extremely strained analogies with and misinterpretations of general system theory. I have yet to see any reasonably solid methodology employed to establish the relative importance of the various levels of war.

A not uncommon contention is that Soviet operational excellence more than compensated for German tactical virtuosity. However, all publications I have seen that have arrived at this conclusion are based on Soviet secondary sources, which are replete with errors.

## Facts

Naveh is extremely careless with facts. Below will be given a number of examples. They will clearly show that Naveh simply does not know his business. I will give a fuller description of four errors, and a briefer one on a number of other errors or unproven statements.

1. Page 142: "the [Barbarossa] plan allotted similar forces to all three army groups for the purpose of accomplishing their varying missions." He does not give any source, but in the accompanying note

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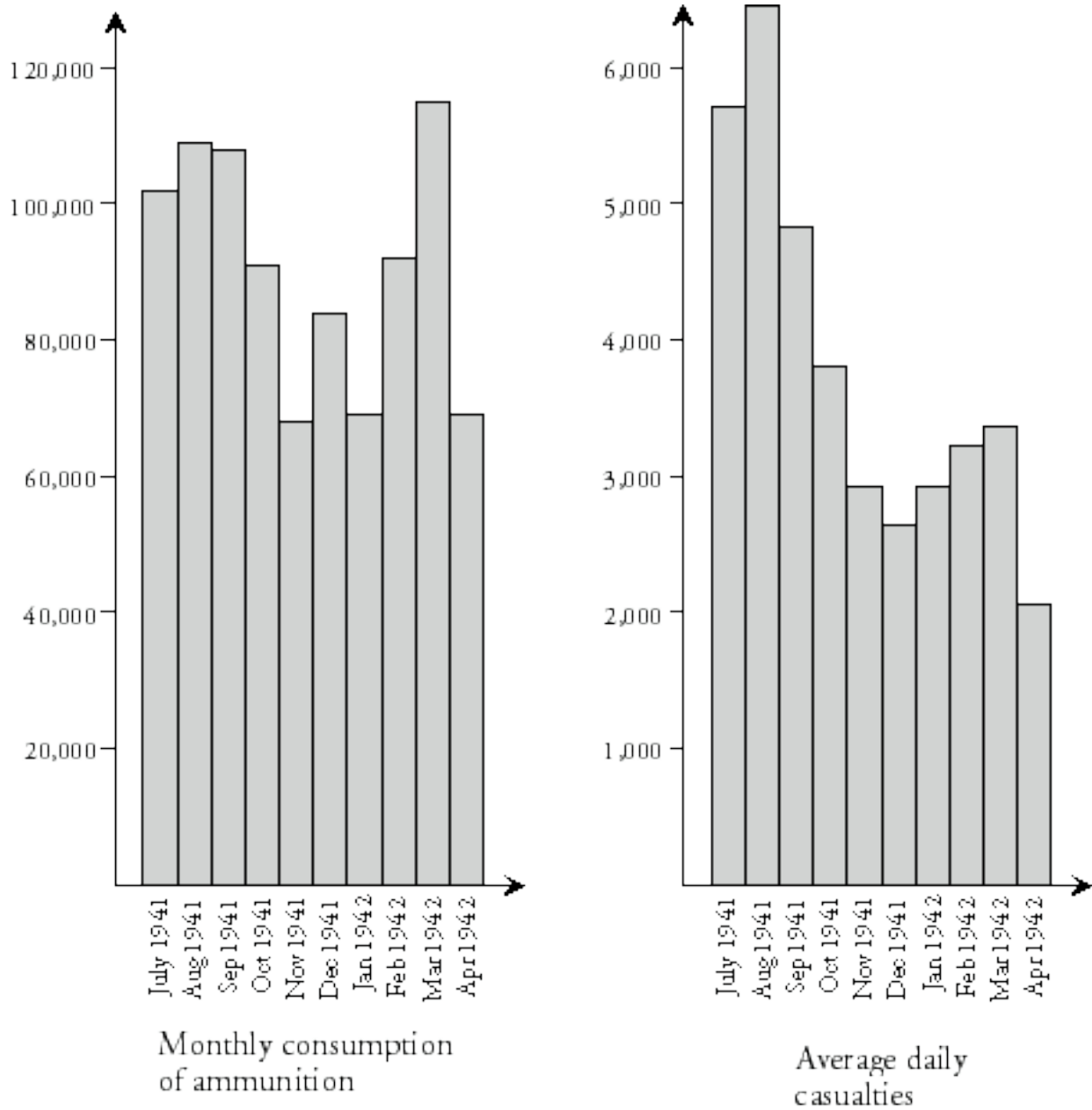
(167), he states: "The order of battle of all three army groups was more or less identical, except the fact that Army Group Centre was allotted two panzer groups instead of one. The reason for this additional allocation of a mobile element derived from the Army Group's central position, and not, as one might think, from the magnitude of its mission." He does of course not give any fact or reference supporting the assertion that it was the central position that was the reason behind the extra panzer group in Army Group Center.

In any case, it can possibly look as if the army groups were of approximately equal size if we look at the number of armies allotted to them. If we equate the panzer groups with armies, we find that Army Group North had three armies, while Centre and South had four each. However, since armies could vary a lot in composition this says little. If we look at the distribution of tanks we find that Army Group North had 18.5 % of the tanks, South had 23.5 % and Centre 58.0 %.<sup>36</sup> This suggests that, contrary to Naveh's unsupported assertion, the Germans did indeed concentrate their efforts in the Centre. Also, the distribution of air power gives the same picture: North 18 %, Centre 51 % and South 31 %. Naveh asserts that the alleged German failure to concentrate the available forces to the Army Group Centre sector was a serious blunder. If a serious blunder has been committed, it is rather Naveh's sloppy research. But there is more to this.

Naveh claims that the German Army Group Centre failed to destroy the Soviet forces in Belorussia. Again, he gives no sources (ZZZ men kolla Reinhardt). But it is clear from Russian sources that the Soviet forces in Belorussia suffered extremely high casualties. According to Krivosheev the manpower losses were no less than 67 % in the defense of Belorussia.<sup>37</sup> The soldiers who managed to escape were probably not bringing much equipment with them. For example Krivosheev states that no less than 4,799 tanks and 9,427 guns and mortars were irrevocably lost in the operation.<sup>38</sup> This suggests that those Soviet soldiers that managed to escape from the fighting in Belorussia did not bring much heavy equipment with them.

This is not the final analysis of the Soviet losses in Belorussia, but it is infinitely better than Naveh's, since he does not provide any substance at all to corroborate his claim that the Germans failed to destroy the Soviet forces in Belorussia. If we accept Krivosheev's manpower figures, they suggest that slightly more than 200,000 Red Army soldiers escaped. Why just these caused the failure of operation Barbarossa, as Naveh claims, is unclear. After all, the Soviet Union must have brought about nine million men into combat during 1941.<sup>39</sup> Why just those approximately 200,000 – most likely without much heavy equipment – had such a decisive role is not made clear by Naveh.

2. Page 137: "In fact during the winter months of 1941-42, the Wehrmacht formations were completely deprived of artillery support." This is an astonishing statement. The left graph below gives the monthly ammunition expenditure on the eastern front.<sup>40</sup>



As can be seen, there is a drop in consumption during the winter, but there is also a drop in German daily casualties as shown in the diagram to the right.<sup>41</sup> It is to be expected that if the intensity of combat declines, both ammunition consumption and casualties will decline too.<sup>42</sup> In any case the difference between the summer months 1941 and the winter months 1941-2 is not very great. July-September saw a monthly average of 106,000 tons and for the period December 1941 – March 1942 the monthly average was 90,000 tons.

It should be emphasized that the artillery consumed about 70 % of the ammunition in an infantry division.<sup>43</sup> Also there were non-divisional artillery units that contributed, which most likely increased the percentage consumed by artillery.

Naveh's source is illuminating, an entry in the war diary of 2nd Panzer Army made on 15 December 1941. How he can conclude that problems for one army shows that all other armies had the same

problems is inexplicable. Even more bizarre is the implicit statement that the German 2nd Panzer Army should have known several months in advance that it would lack artillery support for the rest of the winter. As written above, on sources, most of the archival documents referred to by Naveh actually appear in Rudolf Steiger's "Armoured Warfare" and this is no exception. Naveh gives exactly the same source as Steiger gives to the following passage:

*The drawn artillery no longer existed: "There is no artillery support, since the road situation remains difficult for vehicles and artillery".*

The source given is the war diary for the operations section of 2nd Panzer Army on 15 December 1941, page 6.<sup>44</sup>

If we look at the original source, we find that there is only one sentence that can have any resemblance to this: "The condition of the roads continues to be very difficult due to glassy ice, particularly for heavy vehicles and artillery."<sup>\*</sup>

We see that already Steiger's interpretation is exaggerated, but Naveh carries it to the extreme.

3. Naveh states that "eventually air superiority in the forward operational zones was yielded to the Russians, friendly air support almost vanished, and the Wehrmacht's armoured spearheads were left entirely to the mercy of the Red Air Force" (page 137). Given the sources he refers to and the context from which this passage is taken, it is obvious that he means that this happened during 1941. Also he says on the page: "Russian artillery and air power very soon commanded the skies and the battlefields". However, this goes against everything hitherto known about operation Barbarossa. It would probably most likely be a major surprise to those who served in the Red Air force at the time too. Thus it is relevant to demand a stringent use of sources on Naveh's behalf to back this claim up. What do we find? He only gives references to a few (three) reports. These are reports telling that, in each case, a division on a particular day has been subjected to a Soviet air attack. The only thing these sources tell us is that there were occasions (at least three) when the Red Air force managed to put in attacks on German motorized and panzer divisions. Since the Germans had more than 30 such divisions, and the period Naveh is looking at spanned over half a year (approximately 180 days), we have about 5,000 combat-days for that type of divisions. Naveh has shown that on three of them, the Red Air Force was capable of putting in air attacks. To conclude from this that the Germans lost air superiority and that the panzer divisions were **entirely** at the mercy of the Red Air force is simply to ridicule research methodology.
4. On page 136 Naveh says that the Germans had inadequate operational mobility. Whether this is true depends of course on what is regarded as adequate operational mobility. But no other army, least of all the Red Army (which Naveh seems so fond of) managed to equal the German operational mobility displayed during Barbarossa.<sup>45</sup> Of course Naveh is free to use different yardsticks when evaluating German and Soviet operations, but then this ought to be motivated. Finally, the factors that limited German mobility (tactical as well as operational) were shortages of motor vehicles and fuel. Naveh seems to blame the German shortage of vehicles on the lack of operational cognition. If that is so, why did the Russians, with their supposedly well-developed operational cognition, produce far fewer motor vehicles than the Germans?<sup>46</sup> Also, Naveh criticizes the Germans for not having enough tracked lightly armoured vehicles for the infantry. The Soviet production of similar vehicles was virtually nil. Some American built were received during the war, but not many.

\* German original: Die Straßenlage ist durch Glatteis besonders für schwere Fahrzeuge und Artillerie unverändert schwierig.

On page 17 Naveh writes:

*However, more important is the fact that the combination of deployment and a pyramidal structure of hierarchic command determines the nature of the operational mechanism of hierarchical action.*

This is pure hogwash. Aside from being written in a pompous way, there is no support for this and no useful consequences to be inferred from it.

On page 21 Naveh writes:

*We have already seen that the non-linear nature of the interaction between the system's components determines the columnar form of the operational manoeuvre and the patterns of cooperation between the operational echelons*

Again, the non-linear nature (which may or may not be the case in reality) does not *determine* any form of operational manoeuvre or patterns.

On page 129 Naveh asserts that "the mechanism of mutual confidence emanates from the natural distance existing between the initiator of the operation and its executers". He gives no source or argument for this. Since mutual confidence is a human, psychological phenomenon, systems theory, and certainly not Bertalanffy's version of it, has nothing to teach us on this. To insert the word "mechanism" does not clarify matters. Furthermore, it is not particularly clear what is a natural distance.

On the Gulf War 1991, Naveh makes a number of mistakes. For example on page 327 Naveh claims that Iraq was induced to move its main mass of its army to the Kuwait theatre of operations. This seems hardly true. Possibly Iraq was induced to *keep* its main ground forces in the KTO, but we actually don't know much about the decision process on the Iraqi side. On page 329 he speaks of the coalition forces in the south making a holding effort. Perhaps this was the intention, but the reality was rather that these forces drove through the Iraqi defence, rather than holding it.

We will end this exposition by the following section written by Naveh on page 9:

*Thus, operation constitutes the entire whole or complex of warlike actions governed by an identical concept, and directed towards attainment of the same aim. The quantitative element of matter is provided by the fighting mass. The physical framework for the operational occurrence is comprised of the factors of time and space. The variety of species is reflected through the diversity of arms and forces, and the system's components are represented by the main formations and combat groupings. The interaction between the components is reflected through the manoeuvre, which is based on a unified plan. The substance of the operational plan consists of the strategic aim, which indicates a predetermined definition of the entire operational accomplishment. The division of this aim into operational objectives and tactical missions creates the cognitive tension that moves the system towards its final objective. And, finally, the linkage between the aim and the combined manoeuvre reflects the synergistic postulate, the coherence of the operational action, and its continuity.*

This passage sums up both Naveh's literary style and his way of arguing quite well. The first sentence is fairly OK. It can be accepted as a definition. The second is more dubious. Possibly we can make an

analogy for illustrational purposes between matter and fighting mass, but not much more. The third sentence is acceptable, but the fourth is bizarre. What does the variation of species have to do with military operations and how it can be reflected in anything military (or why it should) is completely unclear. Naveh does not provide any argument. Furthermore, it is doubtful that "the system's components are represented", rather it could be argued that the main formations and combat grouping constitute the system's components. The fifth sentence is quite ambiguous. What is meant by "reflected" is not self-evident, nor how this "reflection" actually looks. It seems more reasonable to assume that the interactions between the military system's components both affects a manoeuvre and is affected by the manoeuvre, but this is hardly much help, rather a truism. The seventh sentence is another example of unproven statements. The "cognitive tension" is in no way a well-defined or established concept in the context Naveh uses it. It is even less of an established fact that the "cognitive tension" is what moves the system towards its final objective. The last sentence says nothing useful.

To complete it, Naveh begins the following passage with: "Therefore, one can rightly claim that the operational level is the implementation of the universal system in the military sphere". At this point one has to wonder if Naveh is making fun of his reader. In any case, the long passage cited above does in no way support Naveh's claim.

## Conclusion

The problems discussed above with Naveh's book are far from all objections I have on the book. However, it simply requires too much time to give all of them a thorough coverage.

The two main issues I have with Naveh is his attempt at building a theory (or rather promoting a theory) without providing any empirical support and his mysterious way of using sources and references.

If we begin with the latter, it seems that Naveh does his utmost to give an impression of having studied a wide range of sources. His bibliography covers no less than 61 pages. However, it seems that he has not spent much time with each of the books, articles and documents he has included in his long list.\* When his statements are checked against the sources he refer to in his footnotes, the poor correspondence between the source and his statements become evident. That he seems to have taken most of his archival references from Rudolf Steiger† does certainly not lend more credence to his statements.

But grave as this problem is, the first objection is actually more fundamental, the lack of empirical support. If Naveh had contended with discussing the developments of theories and ideas, this might have worked, but as he also makes statements on what is a good or bad military concept, it definitely does not work even if his use of sources had not been so dubious.

The use of theories is of course common in science. A theory may be useful, or it may prove misleading. One example of the latter is the phlogiston theory for combustion that was popular in the 18th century, but empirical evidence showed that it was untenable; consequently it was rejected ‡. In the realm of physics and chemistry, empirical evidence has been very useful to reject or corroborate theories. Today, these disciplines have reached a maturity that can often make it possible to reject or support

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\* An exception may be the Russian literature, I have not checked it. However, the vast majority of the Russian titles he has listed in his bibliography were published before 1990. If there is any large group of literature that I would regard as risky to use, it is literature on military matters published in the Soviet Union.

† I emphasize "he seems". After all, there is a theoretical possibility that it is a mere coincidence that 47 of Steiger's references (including identical errors) appears in Naveh's book too.

‡ This theory tried to explain combustion by assuming there was a substance called phlogiston, which "evaporated" from a component when it burned. More flammable items were assumed to contain more phlogiston. However, weight measurements showed that for example metals increased their weight during combustion, which contradicted the phlogiston theory that was subsequently rejected.

conclusions on the basis of theoretical argument alone. Unfortunately it would be a gross exaggeration to say that military theory has reached that level yet, whether supported or not by general system theory.\* Hence, the empirical support is invaluable if one is to claim, like Naveh, that the Soviet operations in 1944-45 are models and the German blitzkrieg in 1939-41 an example of incompetence. It would have been easy to make a comparative study. One good example is the German advance through Byelorussia in 1941 and the Soviet offensive in the opposite direction in 1944. Both were conducted during the same period of the war, in the same terrain, with roughly the same magnitude of force sizes and both seem to have enjoyed benefits of surprise. Unfortunately for Naveh, such a comparison would not place the Red Army's conduct of operations in a favourable light.

In the end, the lack of empirical support and the mistaken belief that systems theory can provide the necessary support for his contentions means that Naveh's conclusions are completely unjustified. To give them a façade of academic jargon does not lend them substance.

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- 1 A study of the footnotes used by Naveh reveals only one case of a reference that is not from Bertalanffy's book. The exception is Peter Senge's "The fifth discipline", which is more of a popular work than a scientific work.
- 2 Bertalanffy was in fact born in Atzgersdorff outside Vienna. He studied at the universities at Innsbruck and Vienna and remained at Vienna until he after WWII emigrated to North America, holding positions at various universities.
- 3 L. Von Bertalanffy, *General System Theory* (Brazillier, New York 2001) p. 55 and p. 56.
- 4 Shimon Naveh, *In Pursuit of Military Excellence – The evolution of Operational Theory* (Frank Cass, London 1997) p. 5.
- 5 Ibid.
- 6 Ibid.
- 7 Ibid, p. 5-6.
- 8 Naveh, p. 28-29, note 78.
- 9 The Lanchester equations exist in two variants, the square law and the linear law. The linear law is:  
 $dN_x/dt = kN_xN_y$   
 $dN_y/dt = cN_xN_y$   
 The square law is;  
 $dN_x/dt = kN_y$   
 $dN_y/dt = cN_x$   
 It seems that the square law is the most often used.
- 10 Naveh, p. 17.
- 11 See Alan Sokal & Jean Bricmont, *Intellectual Impostures* (Profile Books, London 1999) pp. 133-136. This book is well worth reading, it gives many illuminating examples of how science have been abused by academicians. In fact, had Sokal and Bricmont ever read Naveh, they could very well have included several passages from Naveh too in their book.
- 12 Ibid.
- 13 Naveh, p. 21.
- 14 For a good coverage of German storm troops during WWI, see Bruce I. Gudmundsson, *Stormtroop Tactics – Innovation in the German Army 1914-1918* (Praeger, Westport 1995).
- 15 Naveh, p. 116-118.
- 16 Bundesarchiv-Militärarchiv, Freiburg, Germany (hereafter referred to as BA-MA) RHD 4/300/1 (Heeresdienstvorschrift 300/1 "Truppenführung I. Teil" 1933), items 1, 3, 4, 9, 10, 11, 15.
- 17 Naveh, p. 112ff.
- 18 Naveh, p. 125.
- 19 KTB AOK 8 Ia, 11.2.1944, National Archives and Records Administration, Washington D.C. Microfilm Publication T312, Roll 64, F7581864.
- 20 Anlagen zum KTB PzAOK 1 Ia, Meldungen der Gruppe Mattenklott, National Archives and Records Administration, Washington D.C. Microfilm Publication T313, Roll 72, F7310414.
- 21 Here Naveh has inserted footnote 133, which reads: The term "mechanization" in the present context indicates a specific tactical quality, that is reflected in performance of modern heavy infantry. This quality is attained through the combination of motorization, protection against light firearms and light calibre firepower. Unlike the tactical quality defined as armoured, mechanization excels in mobility and flexibility, whereas it lacks in firepower. Steiger, *Armour tactics*, p. 45. remarks that less than 10 per cent of the order of battle of infantry regiments in the panzer divisions were in fact mechanized. H. Von Menteuffel's letter to Liddell Hart, dated 15 May 1949, LHA 9/24/125, pp. 1-2, gives the same data.

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\* This is not to imply that military theory has no value at all. If its limitations are not exceeded it can be of valuable use in for example, education and training, development and clarification of terminology and analytical concepts, as an inspiration for thinking on military matters, etc.



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- 22 Here Naveh has inserted footnote 134, which reads: Ibid.; 20 Panzer Division Abteilung Ia, Erfahrungsbericht des Schützen regiment 59, of 5 November 1941, p. 2 (RH 27-20/99); 4 Panzer Division Abteilung Ia, Anlagen der Gefechtsbericht zum Kriegstagebuch Meldung der 5 Panzer Brigade of 6 Sept. 1941 (RH 27/4-37).
- 23 Here Naveh has inserted footnote 135, which reads: 7 Panzer Division Abteilung Ia, kurzbericht of 14 May 1940 (RH 27-7/191); 4 Panzer Division Abteilung Ia Nr 71/42 geheim of 12 March 1942, p.20 (RH 27/4-37).
- 24 Rudolf Steiger, *Armour Tactics in the Second World War* (Berg, Providence 1991).
- 25 ZZZ Infoga Niehorsters 1941 bok.
- 26 Ibid.
- 27 7. Panzer Division Abteilung Ia, Kurzbericht of 14 May 1940 (RH 27-7/191).
- 28 20. Panzer Division Abteilung Ia, Erfahrungsbericht des Schützen regiment 59, of 5 November 1941, p. 2 (RH 27-20/99).
- 29 Se pages 109-112, 141.
- 30 F. O. Miksche, *Blitzkrieg* (Faber & Faber, London 1941).
- 31 Page 125.
- 32 Steiger, *Armour Tactics* (Oxford 1991) p. 35. Michael Geyer in P. Paret ZZZZZ
- 33 Naveh, p. 13-14.
- 34 Ibid, p. 14.
- 35 Ibid, p. 13.
- 36 ZZZZ Inför Niehorster
- 37 G. F. Krivosheev, *Grif Sekretnosti Sniat* (Voenizdat, Moscow 1993) p. 163. Together with Anders Frankson, I have criticised the accuracy of Krivosheev's figures (see article in Journal of Slavic Military Studies March 1998). However, our point has been that losses were higher than Krivosheev has stated. Thus Krivosheev's loss figures for this operation should be regarded as "not less than", which further sharpens my argument in this particular case.
- 38 Ibid, p. 368.
- 39 Soviet casualties amounted to 4,473,820 according to Krivosheev (op. cit. p. 146), while the Soviet strength on the front was 4.2 million on 1 January 1942 (*Geschichte des Zweiten Weltkrieges*, vol 4, p. 370, Deutscher Militärverlag, East Berlin 1975-85, note that this is a translation of *Istoriya Vtoroi mirovoi Voyny*, a Soviet publication).
- 40 Data taken from "Der Munitionsverbrauch des deutschen Heeres im Osten nach OKH/Gen.Qu./Gruppe Mun/IIa, 10.12.1944 (Beilage zu "Landesverteidigung" Heft 4/1960/61), printed in G. Donat, *Der Munitionsverbrauch im Zweiten Weltkrieg im operativen und Taktischen Rahmen* (Biblio Verlag, Osnabrück 1992), Anlage 23.
- 41 Data taken from Wehrmacht Verlustwesen, Bundesarchiv-Militärarchiv, Freiburg, file RW 6/v. 552 & RW 6/v. 553.
- 42 In fact, the ratio (grim as it may be seen) ammo consumption/casualties is: (July) 0.59; (August) 0.55; (September) 0.76; (October) 0.79; (November) 0.78; (December) 1.07; (January) 0.79; (February) 1.06; (March) 1.14; (April) 1.18. Thus, if anything, we see a rising trend of ammunition consumption. Also, a British study on the fighting in north-west Europe 1944-45 shows the close relation between ammunition consumption and casualties (see Public records office, Kew, London, file CAB 106/1084).
- 43 G. Donat, *Der Munitionsverbrauch im Zweiten Weltkrieg im operativen und Taktischen Rahmen* (Biblio Verlag, Osnabrück 1992), p. 67f.
- 44 Pz.AOK Ia KTB Nr. 1, p. 6, 15.12.1941 (BA-MA RH 21-2/v.277).
- 45 See N. Zetterling & A. Frankson, "Analyzing World War II Eastern Front Battles", *Journal of Slavic Military Studies*, vol 11, no 1 (March 1998) p. 192-8.
- 46 Only by the massive influx of lend-lease vehicles did the Red Army manage to achieve a degree of motorization on the same level as the Germans, but I have never seen any evidence suggesting that the Russians based their development of operational theory and combat formations on lend-lease supply.